# HISTORY

OF THE

# INDUCTIVE SCIENCES.

VOLUME II.

# HISTORY

OF THE

# INDUCTIVE SCIENCES,

FROM

THE EARLIEST TO THE PRESENT TIME.

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*THE THIRD EDITION, WITH ADDITIONS.*

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# BOOK VIII.

*THE SECONDARY MECHANICAL SCIENCES.*

# HISTORY OF ACOUSTICS.

. . . . . . . Go, demand
Of mighty Nature, if 'twas ever meant
That we should pry far off and be unraised,
That we should pore, and dwindle as we pore,
Viewing all objects unremittingly
In disconnexion dead and spiritless;
And still dividing, and dividing still,
Break down all grandeur, still unsatisfied
With the perverse attempt, while littleness
May yet become more little; waging thus
An impious warfare 'gainst the very life
Of our own souls. WORDSWORTH, *Excursion.*

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. . . . . . . Ἐσσυμένη δὲ
Ἠερίην ἀψῖδα διεῤῥοίζησε πεδίλῳ
Εἰς δόμον ἉΡΜΟΝΙΗΣ παμμητόρος, ὁππόθι νύμφη
[illegible] οἶκον ἐνάιε τύπῳ τετράζυγι κόσμου
Αὐτοπαγῆ. NONNUS. *Dionysiac.* xli. 275.

Along the skiey arch the goddess trode,
And sought Harmonia's august abode;
The universal plan, the mystic Four,
Defines the figure of the palace-floor.
Solid and square the ancient fabric stands,
Raised by the labors of unnumbered hands.

# BOOK VIII

## INTRODUCTION.

### *The Secondary Mechanical Sciences.*

IN the sciences of Mechanics and Physical Astronomy, Motion and Force are the direct and primary objects of our attention. But there is another class of sciences in which we endeavor to reduce phenomena, not evidently mechanical, to a known dependence upon mechanical properties and laws. In the cases to which I refer, the facts do not present themselves to the senses as modifications of position and motion, but as *secondary qualities*, which are found to be in some way derived from those primary attributes. Also, in these cases the phenomena are reduced to their mechanical laws and causes in a secondary manner; namely, by treating them as the operation of a *medium* interposed between the object and the organ of sense. These, then, we may call *Secondary Mechanical Sciences.* The sciences of this kind which require our notice are those which treat of the sensible qualities, Sound, Light, and Heat; that is, Acoustics, Optics, and Thermotics.

It will be recollected that our object is not by any means to give a full statement of all the additions which have been successively made to our knowledge on the subjects under review, or a complete list of the persons by whom such additions have been made; but to present a view of the progress of each of those branches of knowledge *as a theoretical science*;—to point out the Epochs of the discovery of those general principles which reduce many facts to one theory; and to note all that is most characteristic and instructive in the circumstances and persons which bear upon such Epochs. A history of any science, written with such objects, will not need to be long; but it will fail in its purpose altogether, if it do not distinctly exhibit some well-marked and prominent features.

We begin our account of the Secondary Mechanical Sciences with Acoustics, because the progress towards right theoretical views, was, in fact, made much earlier in the science of Sound, than in those of Light and of Heat; and also, because a clear comprehension of the theory to which we are led in this case, is the best preparation for the difficulties (by no means inconsiderable) of the reasonings of theorists on the other subjects.

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## CHAPTER I.

### Prelude to the Solution of Problems in Acoustics.

In some measure the true theory of sound was guessed by very early speculators on the subject; though undoubtedly conceived in a very vague and wavering manner. That sound is caused by some motion of the sounding body, and conveyed by some motion of the air to the ear, is an opinion which we trace to the earliest times of physical philosophy. We may take Aristotle as the best expounder of this stage of opinion. In his Treatise *On Sound and Hearing*, he says, "Sound takes place when bodies strike the air, not by the air having a *form* impressed upon it (σχηματιζόμενον), as some think, but by its being moved in a corresponding manner; (probably he means in a manner corresponding to the impulse;) the air being contracted, and expanded, and overtaken, and again struck by the impulses of the breath and of the strings. For when the breath falls upon and strikes the air which is next it, the air is carried forwards with an impetus, and that which is contiguous to the first is carried onwards; so that the same voice spreads every way as far as the motion of the air takes place."

As is the case with all such specimens of ancient physics, different persons would find in such a statement very different measures of truth and distinctness. The admirers of antiquity might easily, by pressing the language closely, and using the light of modern discovery, detect in this passage an exact account of the production and propagation of sound: while others might maintain that in Aristotle's own mind, there were only vague notions, and verbal generalizations. This latter opinion is very emphatically expressed by Bacon.[1] "The collision or thrusting of air, which they will have to be the cause of sound, neither denotes the *form* nor the latent process of sound; but is a term of ignorance and of superficial contemplation." Nor can it be justly denied, that an exact and distinct apprehension of the kind of motion of the air by which sound is diffused, was beyond the reach of the ancient philosophers, and made its way into the world long afterwards. It was by no means easy to reconcile the nature of such motion with obvious phenomena. For the process is not evident as motion; since, as Bacon also observes,[2] it does not visibly agitate the flame of a candle, or a feather, or any light floating substance, by which the slightest motions of the air are betrayed. Still, the persuasion that sound is some motion of the air, continued to keep hold of men's minds, and acquired additional distinctness. The illustration employed by Vitruvius, in the following passage, is even now one of the best we can offer.[3] "Voice is breath, flowing, and made sensible to the hearing by striking the air. It moves in infinite circumferences of circles, as when, by throwing a stone into still water, you produce innumerable circles of waves, increasing from the centre and spreading outwards, till the boundary of the space, or some obstacle, prevents their outlines from going further. In the same manner the voice makes its motion in circles. But in water the circle moves breadthways upon a level plain; the voice proceeds in breadth, and also successively ascends in height."

Both the comparison, and the notice of the difference of the two cases, prove the architect to have had very clear notions on the subject; which he further shows by comparing the resonance of the walls of a building to the disturbance of the outline of the waves of water when they meet with a boundary, and are thrown back. "Therefore, as in the outlines of waves in water, so in the voice, if no obstacle interrupt the foremost, it does not disturb the second and the following ones, so that all come to the ears of persons, whether high up or low down, without resonance. But when they strike against obstacles, the foremost, being thrown back, disturb the lines of those which follow." Similar analogies were employed by the ancients in order to explain the occurrence of Echoes. Aristotle says,[4] "An Echo takes place, when the air, being as one body in consequence of the vessel which bounds it, and being prevented from being thrust forwards, is reflected

[1] *Hist. Son. et Aud.* vol. ix. p 68. [2] *Ibid.* [3] *De Arch.* v. 3. [4] *De Anima* ii. 8

back like a ball." Nothing material was added to such views till modern times.

Thus the first conjectures of those who philosophized concerning sound, led them to an opinion concerning its causes and laws, which only required to be distinctly understood, and traced to mechanical principles, in order to form a genuine science of Acoustics. It was, no doubt, a work which required a long time and sagacious reasoners, to supply what was thus wanting; but still, in consequence of this peculiar circumstance in the early condition of the prevalent doctrine concerning sound, the history of Acoustics assumes a peculiar form. Instead of containing, like the history of Astronomy or of Optics, a series of generalizations, each including and rising above preceding generalizations; in this case, the highest generalization is in view from the first; and the object of the philosopher is to determine its precise meaning and circumstances in each example. Instead of having a series of inductive Truths, successively dawning on men's minds, we have a series of Explanations, in which certain experimental facts and laws are reconciled, as to their mechanical principles and their measures, with the general doctrine already in our possession. Instead of having to travel gradually towards a great discovery, like Universal Gravitation, or Luminiferous Undulations, we take our stand upon acknowledged truths, the production and propagation of sound by the motion of bodies and of air; and we connect these with other truths, the laws of motion and the known properties of bodies, as, for instance, their elasticity. Instead of *Epochs of Discovery*, we have *Solutions of Problems;* and to these we must now proceed.

We must, however, in the first place, notice that these Problems include other subjects than the mere production and propagation of sound generally. For such questions as these obviously occur:—what are the laws and cause of the differences of sounds;—of acute and grave, loud and low, continued and instantaneous;—and, again, of the differences of articulate sounds, and of the quality of different voices and different instruments? The first of these questions, in particular, the real nature of the difference of acute and grave sounds, could not help attracting attention; since the difference of notes in this respect was the foundation of one of the most remarkable mathematical sciences of antiquity. Accordingly, we find attempts to explain this difference in the ancient writers on music. In Ptolemy's *Harmonics*, the third Chapter of the first Book is entitled, "How the acuteness and graveness of notes is produced;" and in this, after noting generally the difference of sounds, and the causes of difference (which he states to be the force of the striking body, the physical constitution of the body struck, and other causes), he comes to the conclusion, that "the things which produce acuteness in sounds, are a greater density and a smaller size; the things which produce graveness, are a greater rarity and a bulkier form." He afterwards explains this so as to include a considerable portion of truth. Thus he says, "That in strings, and in pipes, other things remaining the same, those which are stopped at the smaller distance from the bridge give the most acute note; and in pipes, those notes which come through holes nearest to the mouth-hole are most acute." He even attempts a further generalization, and says that the greater acuteness arises, in fact, from the body being more tense; and that thus "hardness may counteract the effect of greater density, as we see that brass produces a more acute sound than lead." But this author's notions of tension, since they were applied so generally as to include both the tension of a string, and the tension of a piece of solid brass, must necessarily have been very vague. And he seems to have been destitute of any knowledge of the precise nature of the motion or impulse by which sound is produced; and, of course, still more ignorant of the mechanical principles by which these motions are explained. The notion of *vibrations* of the parts of sounding bodies, does not appear to have been dwelt upon as an essential circumstance; though in some cases, as in sounding strings, the fact is very obvious. And the notion of vibrations of the air does not at all appear in ancient writers, except so far as it may be conceived to be implied in the comparison of aërial and watery waves, which we have quoted from Vitruvius. It is, however, very unlikely that, even in the case of water, the motions of the particles were distinctly conceived, for such conception is far from obvious.

The attempts to apprehend distinctly, and to explain mechanically, the phenomena of sound, gave rise to a series of Problems, of which we must now give a brief history. The questions which more peculiarly constitute the Science of Acoustics, are the questions concerning those motions or affections of the air by which it is the medium of hearing. But the motions of sounding bodies have both so much connexion with those of the medium, and so much resemblance to them, that we shall include in our survey researches on that subject also.

## CHAPTER II.

### Problem of the Vibrations of Strings.

THAT the continuation of sound depends on a continued minute and rapid motion, a shaking or trembling, of the parts of the sounding body, was soon seen. Thus Bacon says,[1] "The duration of the sound of a bell or a string when struck, which appears to be prolonged and gradually extinguished, does not proceed from the first percussion; but the trepidation of the body struck perpetually generates a new sound. For if that trepidation be prevented, and the bell or string be stopped, the sound soon dies: as in *spinets*, as soon as the *spine* is let fall so as to touch the string, the sound ceases." In the case of a stretched string, it is not difficult to perceive that the motion is a motion back and forwards across the straight line which the string occupies when at rest. The further examination of the quantitative circumstances of this oscillatory motion was an obvious problem; and especially after oscillations, though of another kind (those of a pendulous body), had attracted attention, as they had done in the school of Galileo. Mersenne, one of the promulgators of Galileo's philosophy in France, is the first author in whom I find an examination of the details of this case (*Harmonicorum Liber*, Paris, 1636). He asserts,[2] that the differences and concords of acute and grave sounds depend on the rapidity of vibrations, and their ratio; and he proves this doctrine by a series of experimental comparisons. Thus he finds[3] that the note of a string is as its length, by taking a string first twice, and then four times as long as the original string, other things remaining the same. This, indeed, was known to the ancients, and was the basis of that numerical indication of the notes which the proposition expresses. Mersenne further proceeds to show the effect of thickness and tension. He finds (Prop. 7) that a string must be four times as thick as another, to give the octave below; he finds, also (Prop. 8), that the tension must be about four times as great in order to produce the octave above. From these proportions various others are deduced, and the *law of the*

[1] *Hist. Son. et Aud* vol. ix. p. 71.

[2] L. i. Prop. 15.

[3] L. ii Prop. 6

*phenomena* of this kind may be considered as determined. Mersenne also undertook to *measure* the phenomena numerically, that is to determine the number of vibrations of the string in each of such cases; which at first might appear difficult, since it is obviously impossible to count with the eye the passages of a sounding string backwards and forwards. But Mersenne rightly assumed, that the number of vibrations is the same so long as the tone is the same, and that the ratios of the numbers of vibrations of different strings may be determined from the numerical relations of their notes. He had, therefore, only to determine the number of vibrations of one certain string, or one known note, to know those of all others. He took a musical string of three-quarters of a foot long, stretched with a weight of six pounds and five eighths, which he found gave him by its vibrations a certain standard note in his organ: he found that a string of the same material and tension, fifteen feet, that is, twenty times as long, made ten recurrences in a second; and he inferred that the number of vibrations of the shorter string must also be twenty times as great; and thus such a string must make in one second of time two hundred vibrations.

This determination of Mersenne does not appear to have attracted due notice; but some time afterwards attempts were made to ascertain the connexion between the sound and its elementary pulsations in a more direct manner. Hooke, in 1681, produced sounds by the striking of the teeth of brass wheels,[4] and Stancari, in 1706, by whirling round a large wheel in air, showed, before the Academy of Bologna, how the number of vibrations in a given note might be known. Sauveur, who, though deaf for the first seven years of his life, was one of the greatest promoters of the science of sound, and gave it its name of *Acoustics*, endeavored also, about the same time, to determine the number of vibrations of a standard note, or, as he called it, Fixed Sound. He employed two methods, both ingenious and both indirect. The first was the method of *beats*. Two organ-pipes, which form a discord, are often heard to produce a kind of *howl*, or *wavy* noise, the sound swelling and declining at small intervals of time. This was readily and rightly ascribed to the coincidences of the pulsations of sound of the two notes after certain cycles. Thus, if the number of vibrations of the notes were as fifteen to sixteen in the same time, every fifteenth vibration of the one would coincide with every six

[4] *Life*, p. xxiii.

teenth vibration of the other, while all the intermediate vibrations of the two tones would, in various degrees, disagree with each other; and thus every such cycle, of fifteen and sixteen vibrations, might be heard as a separate beat of sound. Now, Sauveur wished to take a case in which these beats were so slow as to be counted,[5] and in which the ratio of the vibrations of the notes was known from a knowledge of their musical relations. Thus if the two notes form an interval of a semitone, their ratio will be that above supposed, fifteen to sixteen; and if the beats be found to be six in a second, we know that, in that time, the graver note makes ninety and the acuter ninety-six vibrations. In this manner Sauveur found that an open organ-pipe, five feet long, gave one hundred vibrations in a second.

Sauveur's other method is more recondite, and approaches to a mechanical view of the question.[6] He proceeded on this basis; a string, horizontally stretched, cannot be drawn into a mathematical straight line, but always hangs in a very flat curve, or *festoon*. Hence Sauveur assumed that its transverse vibrations may be conceived to be identical with the lateral swingings of such a festoon. Observing that the string C, in the middle of a harpsichord, hangs in such a festoon to the amount of 1-323rd of an inch, he calculates, by the laws of pendulums, the time of oscillation, and finds it 1-122nd of a second. Thus this C, his *fixed note*, makes one hundred and twenty-two vibrations in a second. It is curious that this process, seemingly so arbitrary, is capable of being justified on mechanical principles; though we can hardly give the author credit for the views which this justification implies. It is, therefore, easy to understand that it agreed with other experiments, in the laws which it gave for the dependence of the tone on the length and tension.

The problem of satisfactorily explaining this dependence, on mechanical principles, naturally pressed upon the attention of mathematicians when the law of the phenomena was thus completely determined by Mersenne and Sauveur. It was desirable to show that both the circumstances and the measure of the phenomena were such as known mechanical causes and laws would explain. But this problem, as might be expected, was not attacked till mechanical principles, and the modes of applying them, had become tolerably familiar.

As the vibrations of a string are produced by its tension, it appeared to be necessary, in the first place, to determine the law of the tension

[5] *Ac. Sc. Hist.* 1700, p. 131. [6] *Ac. Sc. Hist.* 1713

which is called into action by the motion of the string; for it is manifest that, when the string is drawn aside from the straight line into which it is stretched, there arises an additional tension, which aids in drawing it back to the straight line as soon as it is let go. Hooke (*On Spring*, 1678) determined the law of this additional tension, which he expressed in his noted formula, "Ut tensio sic vis," the Force is as the Tension; or rather, to express his meaning more clearly, the Force of tension is as the Extension, or, in a string, as the increase of length. But, in reality, this principle, which is important in many acoustical problems, is, in the one now before us, unimportant; the force which urges the string towards the straight line, depends, with such small extensions as we have now to consider, not on the extension, but on the curvature; and the power of treating the mathematical difficulty of curvature, and its mechanical consequences, was what was requisite for the solution of this problem.

The problem, in its proper aspect, was first attacked and mastered by Brook Taylor, an English mathematician of the school of Newton, by whom the solution was published in 1715, in his *Methodus Incrementorum*. Taylor's solution was indeed imperfect, for it only pointed out a form and a mode of vibration, with which the string *might* move consistently with the laws of mechanics; not the mode in which it *must* move, supposing its form to be any whatever. It showed that the curve might be of the nature of that which is called *the companion to the cycloid;* and, on the supposition of the curve of the string being of this form, the calculation confirmed the previously established laws by which the tone, or the time of vibration, had been discovered to depend on the length, tension, and bulk of the string. The mathematical incompleteness of Taylor's reasoning must not prevent us from looking upon his solution of the problem as the most important step in the progress of this part of the subject: for the difficulty of applying mechanical principles to the question being once overcome, the extension and correction of the application was sure to be undertaken by succeeding mathematicians; and, accordingly, this soon happened. We may add, moreover, that the subsequent and more general solutions require to be considered with reference to Taylor's, in order to apprehend distinctly their import; and further, that it was almost evident to a mathematician, even before the general solution had appeared, that the dependence of the time of vibration on the length and tension, would be the same in the general case as in the Taylorian curve; so that, for the ends of physical philosophy, the solution was not very incomplete.

John Bernoulli, a few years afterwards,[7] solved the problem of vibrating chords on nearly the same principles and suppositions as Taylor; but a little later (in 1747), the next generation of great mathematicians, D'Alembert, Euler, and Daniel Bernoulli, applied the increased powers of analysis to give generality to the mode of treating this question; and especially the calculus of partial differentials, invented for this purpose. But at this epoch, the discussion, so far as it bore on physics, belonged rather to the history of another problem, which comes under our notice hereafter, that of the composition of vibrations; we shall, therefore, defer the further history of the problem of vibrating strings, till we have to consider it in connexion with new experimental facts.

## CHAPTER III.

### Problem of the Propagation of Sound.

WE have seen that the ancient philosophers, for the most part, held that sound was transmitted, as well as produced, by some motion of the air, without defining what kind of motion this was; that some writers, however, applied to it a very happy similitude, the expansive motion of the circular waves produced by throwing a stone into still water; but that notwithstanding, some rejected this mode of conception, as, for instance, Bacon, who ascribed the transmission of sound to certain "spiritual species."

Though it was an obvious thought to ascribe the motion of sound to some motion of air; to conceive what kind of motion could and did produce this effect, must have been a matter of grave perplexity at the time of which we are speaking; and is far from easy to most persons even now. We may judge of the difficulty of forming this conception, when we recollect that John Bernoulli the younger[1] declared, that he could not understand Newton's proposition on this subject. The difficulty consists in this; that the movement of the parts of air, in which sound consists, travels along, but that the parts

[7] *Op.* iii. p. 207.

[1] *Prize Dis. on Light*, 1736.

of air themselves do not so travel. Accordingly Otto Guericke,[2] the inventor of the air-pump, asks, "How can sound be conveyed by the motion of the air? when we find that it is better conveyed through air that is still, than when there is a wind." We may observe, however, that he was partly misled by finding, as he thought, that a bell could be heard in the vacuum of his air-pump; a result which arose, probably, from some imperfection in his apparatus.

Attempts were made to determine, by experiment, the circumstances of the motion of sound; and especially its velocity. Gassendi[3] was one of the first who did this. He employed fire-arms for the purpose, and thus found the velocity to be 1473 Paris feet in a second. Roberval found a velocity so small (560 feet) that it threw uncertainty upon the rest, and affected Newton's reasonings subsequently.[4] Cassini, Huyghens, Picard, Römer, found a velocity of 1172 Paris feet, which is more accurate than the former. Gassendi had been surprised to find that the velocity with which sounds travel, is the same whether they are loud or gentle.

The explanation of this constant velocity of sound, and of its amount, was one of the problems of which a solution was given in the Great Charter of modern science, Newton's *Principia* (1687). There, for the first time, were explained the real nature of the motions and mutual action of the parts of the air through which sound is transmitted. It was shown[5] that a body vibrating in an elastic medium, will propagate *pulses* through the medium; that is, the parts of the medium will move forwards and backwards, and this motion will affect successively those parts which are at a greater and greater distance from the origin of motion. The parts, in going forwards, produce condensation; in returning to their first places, they allow extension; and the play of the elasticities developed by these expansions and contractions, supplies the forces which continue to propagate the motion.

The idea of such a motion as this, is, as we have said, far from easy to apprehend distinctly: but a distinct apprehension of it is a step essential to the physical part of the sciences now under notice; for it is by means of such *pulses*, or *undulations*, that not only sound, but light, and probably heat, are propagated. We constantly meet with evidence of the difficulty which men have in conceiving this undulatory motion, and in separating it from a local motion of the medium as a

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[2] *De Vac. Spat.* p. 138.
[3] Fischer, *Gesch. d. Physik.* vol. i. 171.
[4] Newt. *Prin.* B. ii. P. 50, Schol.
[5] Newt. *Prin.* B. ii. P. 43.

mass. For instance, it is not easy at first to conceive the waters of a great river flowing constantly *down* towards the sea, while waves are rolling *up* the very same part of the stream; and while the great elevation, which makes the tide, is travelling from the sea perhaps with a velocity of fifty miles an hour. The motion of such a wave, or elevation, is distinct from any stream, and is of the nature of undulations in general. The parts of the fluid stir for a short time and for a small distance, so as to accumulate themselves on a neighboring part, and then retire to their former place; and this movement affects the parts in the order of their places. Perhaps if the reader looks at a field of standing corn when gusts of wind are sweeping over it in visible waves, he will have his conception of this matter aided; for he will see that here, where each ear of grain is anchored by its stalk, there can be no permanent local motion of the substance, but only a successive stooping and rising of the separate straws, producing hollows and waves, closer and laxer strips of the crowded ears.

Newton had, moreover, to consider the mechanical consequences which such condensations and rarefactions of the elastic medium, air, would produce in the parts of the fluid itself. Employing known laws of the elasticity of air, he showed, in a very remarkable proposition,[6] the law according to which the particles of air might vibrate. We may observe, that in this solution, as in that of the vibrating string already mentioned, a rule was exhibited according to which the particles *might* oscillate, but not the law to which they *must* conform. It was proved that, by taking the motion of each particle to be perfectly similar to that of a pendulum, the forces, developed by contraction and expansion, were precisely such as the motion required; but it was not shown that no other type of oscillation would give rise to the same accordance of force and motion. Newton's reasoning also gave a determination of the speed of propagation of the pulses: it appeared that sound ought to travel with the velocity which a body would acquire by falling freely through half *the height of a homogeneous atmosphere;* "the height of a homogeneous atmosphere" being the height which the air must have, in order to produce, at the earth's surface, the actual atmospheric pressure, supposing no diminution of density to take place in ascending. This height is about 29,000 feet; and hence it followed that the velocity was 968 feet. This velocity is really considerably less than that of sound; but at the time of which

[6] *Princ.* B. ii. Prop. 48.

we speak, no accurate measure had been established; and Newton persuaded himself, by experiments made in the cloister of Trinity College, his residence, that his calculation was not far from the fact. When, afterwards, more exact experiments showed the velocity to be 1142 English feet, Newton attempted to explain the difference by various considerations, none of which were adequate to the purpose;—as, the dimensions of the solid particles of which the fluid air consists;—or the vapors which are mixed with it. Other writers offered other suggestions; but the true solution of the difficulty was reserved for a period considerably subsequent.

Newton's calculation of the motion of sound, though logically incomplete, was the great step in the solution of the problem; for mathematicians could not but presume that his result was not restricted to the hypothesis on which he had obtained it; and the extension of the solution required only mere ordinary talents. The logical defect of his solution was assailed, as might have been expected. Cranmer (professor at Geneva), in 1741, conceived that he was destroying the conclusiveness of Newton's reasoning, by showing that it applied equally to other modes of oscillation. This, indeed, contradicted the enunciation of the 48th Prop. of the Second Book of the *Principia;* but it confirmed and extended all the general results of the demonstration; for it left even the velocity of sound unaltered, and thus showed that the velocity did not depend mechanically on the type of the oscillation. But the satisfactory establishment of this physical generalization was to be supplied from the vast generalizations of analysis, which mathematicians were now becoming able to deal with. Accordingly this task was performed by the great master of analytical generalization, Lagrange, in 1759, when, at the age of twenty-three, he and two friends published the first volume of the *Turin Memoirs.* Euler, as his manner was, at once perceived the merit of the new solution, and pursued the subject on the views thus suggested. Various analytical improvements and extensions were introduced into the solution by the two great mathematicians; but none of these at all altered the formula by which the velocity of sound was expressed; and the discrepancy between calculation and observation, about one-sixth of the whole, which had perplexed Newton, remained still unaccounted for.

The merit of satisfactorily explaining this discrepancy belongs to Laplace. He was the first to remark[7] that the common law of the

[7] *Méc. Cél.* t. v. l. xii. p. 96.

changes of elasticity in the air, as dependent on its compression, cannot be applied to those rapid vibrations in which sound consists, since the sudden compression produces a degree of heat which additionally increases the elasticity. The ratio of this increase depended on the experiments by which the relation of heat and air is established Laplace, in 1816, published[8] the theorem on which the correction depends. On applying it, the calculated velocity of sound agreed very closely with the best antecedent experiments, and was confirmed by more exact ones instituted for that purpose.

This step completes the solution of the problem of the propagation of sound, as a mathematical induction, obtained from, and verified by, facts. Most of the discussions concerning points of analysis to which the investigations on this subject gave rise, as, for instance, the admissibility of *discontinuous functions* into the solutions of partial differential equations, belong to the history of pure mathematics. Those which really concern the physical theory of sound may be referred to the problem of the motion of air in tubes, to which we shall soon have to proceed; but we must first speak of another form which the problem of vibrating strings assumed.

It deserves to be noticed that the ultimate result of the study of the undulations of fluids seems to show that the comparison of the motion of air in the diffusion of sound with the motion of circular waves from a centre in water, which is mentioned at the beginning of this chapter, though pertinent in a certain way, is not exact. It appears by Mr. Scott's recent investigations concerning waves,[9] that the circular waves are oscillating waves of the Second order, and are *gregarious*. The sound-wave seems rather to resemble the great solitary Wave of Translation of the First order, of which we have already spoken in Book vi. chapter vi.

## CHAPTER IV.

### Problem of different Sounds of the same String.

It had been observed at an early period of acoustical knowledge, that one string might give several sounds. Mersenne and others

[8] *Ann. Phys. et Chim.* t. iii. p. 288. [9] *Brit. Ass. Reports for* 1844, p. 361.

had noticed[1] that when a string vibrates, one which is in unison with it vibrates without being touched. He was also aware that this was true if the second string was an octave or a twelfth below the first. This was observed as a new fact in England in 1674, and communicated to the Royal Society by Wallis.[2] But the later observers ascertained further, that the longer string divides itself into two, or into three equal parts, separated by *nodes*, or points of rest; this they proved by hanging bits of paper on different parts of the string. The discovery so modified was again made by Sauveur[3] about 1700. The sounds thus produced in one string by the vibration of another, have been termed *Sympathetic Sounds.* Similar sounds are often produced by performers on stringed instruments, by touching the string at one of its aliquot divisions, and are then called the *Acute Harmonics.* Such facts were not difficult to explain on Taylor's view of the mechanical condition of the string; but the difficulty was increased when it was noticed that a sounding body could produce these different notes *at the same time.* Mersenne had remarked this, and the fact was more distinctly observed and pursued by Sauveur. The notes thus produced in addition to the genuine note of the string, have been called *Secondary Notes;* those usually heard are, the Octave, the Twelfth, and the Seventeenth above the note itself. To supply a mode of conceiving distinctly, and explaining mechanically, vibrations which should allow of such an effect, was therefore a requisite step in acoustics.

This task was performed by Daniel Bernoulli in a memoir published in 1755.[4] He there stated and proved the Principle of *the coexistence of small vibrations.* It was already established, that a string might vibrate either in a single *swelling* (if we use this word to express the curve between two nodes which Bernoulli calls a *ventre*), or in two or three or any number of equal swellings with immoveable nodes between. Daniel Bernoulli showed further, that these nodes might be combined, each taking place as if it were the only one. This appears sufficient to explain the coexistence of the harmonic sounds just noticed. D'Alembert, indeed, in the article *Fundamental* in the French *Encyclopédie*, and Lagrange in his *Dissertation on Sound* in the *Turin Memoirs*,[5] offer several objections to this explanation; and it cannot be denied that the subject has its difficulties; but

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[1] *Harm.* lib. iv. Prop. 28 (1636). [2] *Ph. Tr.* 1677, April. [3] *A. P.* 1701.
[4] *Berlin Mem.* 1753, p 147. [5] T. i. pp. 64, 103.

still these do not deprive Bernoulli of the merit of having pointed out the principle of Coexistent Vibrations, or divest that principle of its value in physical science.

Daniel Bernoulli's Memoir, of which we speak, was published at a period when the clouds which involve the general analytical treatment of the problem of vibrating strings, were thickening about Euler and D'Alembert, and darkening into a controversial hue; and as Bernoulli ventured to interpose his view, as a solution of these difficulties, which, in a mathematical sense, it is not, we can hardly be surprised that he met with a rebuff. The further prosecution of the different modes of vibration of the same body need not be here considered.

The sounds which are called *Grave Harmonics*, have no analogy with the Acute Harmonics above-mentioned; nor do they belong to this section; for in the case of Grave Harmonics, we have one sound from the co-operation of two strings, instead of several sounds from one string. These harmonics are, in fact, connected with beats, of which we have already spoken; the beats becoming so close as to produce a note of definite musical quality. The discovery of the Grave Harmonics is usually ascribed to Tartini, who mentions them in 1754; but they are first noticed[6] in the work of Sorge *On tuning Organs*, 1744. He there expresses this discovery in a query. "Whence comes it, that if we tune a fifth (2 : 3), a *third* sound is faintly heard, the octave below the lower of the two notes? Nature shows that with 2 : 3, she still requires the unity, to perfect the order 1, 2, 3." The truth is, that these numbers express the frequency of the vibrations, and thus there will be coincidences of the notes 2 and 3, which are of the frequency 1, and consequently give the octave below the sound 2. This is the explanation given by Lagrange,[7] and is indeed obvious.

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## CHAPTER V.

### Problem of the Sounds of Pipes.

It was taken for granted by those who reasoned on sounds, that the sounds of flutes, organ-pipes, and wind-instruments in general, con-

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[6] Chladni. *Acoust.* p. 254. [7] *Mem. Tur.* i. p. 104.

sisted in vibrations of some kind; but to determine the nature and laws of these vibrations, and to reconcile them with mechanical principles, was far from easy. The leading facts which had been noticed were, that the note of a pipe was proportional to its length, and that a flute and similar instruments might be made to produce some of the acute harmonics, as well as the genuine note. It had further been noticed,[1] that pipes closed at the end, instead of giving the series of harmonics 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c., would give only those notes which answer to the odd numbers 1, $\frac{1}{3}$, $\frac{1}{5}$, &c. In this problem also, Newton[2] made the first step to the solution. At the end of the propositions respecting the velocity of sound, of which we have spoken, he noticed that it appeared by taking Mersenne's or Sauveur's determination of the number of vibrations corresponding to a given note, that the pulse of air runs over twice the length of the pipe in the time of each vibration. He does not follow out this observation, but it obviously points to the theory, that the sound of a pipe consists of pulses which travel back and forwards along its length, and are kept in motion by the breath of the player. This supposition would account for the observed dependence of the note on the length of the pipe. The subject does not appear to have been again taken up in a theoretical way till about 1760; when Lagrange in the second volume of the *Turin Memoirs*, and D. Bernoulli in the *Memoirs of the French Academy* for 1762, published important essays, in which some of the leading facts were satisfactorily explained, and which may therefore be considered as the principal solutions of the problem.

In these solutions there was necessarily something hypothetical. In the case of vibrating strings, as we have seen, the Form of the vibrating curve was guessed at only, but the existence and position of the Nodes could be rendered visible to the eye. In the vibrations of air, we cannot see either the places of nodes, or the mode of vibration; but several of the results are independent of these circumstances. Thus both of the solutions explain the fact, that a tube closed at one end is in unison with an open tube of double the length; and, by supposing nodes to occur, they account for the existence of the odd series of harmonics alone, 1, 3, 5, in closed tubes, while the whole series, 1, 2, 3, 4, 5, &c., occurs in open ones. Both views of the nature of the vibration appear to be nearly the same; though Lagrange's is expressed with an analytical generality which renders it obscure, and Bernoulli has perhaps

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[1] D. Bernoulli, *Berlin. Mem.* 1753, p. 150. [2] *Princip.* Schol. Prop. 50.

laid down an hypothesis more special than was necessary. Lagrange[3] considers the vibration of open flutes as "the oscillations of a fibre of air," under the condition that its elasticity at the two ends is, during the whole oscillation, the same as that of the surrounding atmosphere. Bernoulli supposes[4] the whole inertia of the air in the flute to be collected into one particle, and this to be moved by the whole elasticity arising from this displacement. It may be observed that both these modes of treating the matter come very near to what we have stated as Newton's theory; for though Bernoulli supposes all the air in the flute to be moved at once, and not successively, as by Newton's pulse, in either case the whole elasticity moves the whole air in the tube, and requires more time to do this according to its quantity. Since that time, the subject has received further mathematical developement from Euler,[5] Lambert,[6] and Poisson;[7] but no new explanation of facts has arisen. Attempts have however been made to ascertain experimentally the places of the nodes. Bernoulli himself had shown that this place was affected by the amount of the opening, and Lambert[8] had examined other cases with the same view. Savart traced the node in various musical pipes under different conditions; and very recently Mr. Hopkins, of Cambridge, has pursued the same experimental inquiry.[9] It appears from these researches, that the early assumptions of mathematicians with regard to the position of the nodes, are not exactly verified by the facts. When the air in a pipe is made to vibrate so as to have several nodes which divide it into equal parts, it had been supposed by acoustical writers that the part adjacent to the open end was half of the other parts; the outermost node, however, is found experimentally to be *displaced* from the position thus assigned to it, by a quantity depending on several collateral circumstances.

Since our purpose was to consider this problem only so far as it has tended towards its mathematical solution, we have avoided saying anything of the dependence of the mode of vibration on the cause by which the sound is produced; and consequently, the researches on the effects of reeds, embouchures, and the like, by Chladni, Savart, Willis, and others, do not belong to our subject. It is easily seen that the complex effect of the elasticity and other properties of the reed and of the air together, is a problem of which we can hardly

[3] *Mém. Turin*, vol. ii. p. 154.
[4] *Mém. Berlin*, 1753, p. 446.
[5] *Nov. Act. Petrop.* tom. xvi.
[6] *Acad. Berlin*, 1775.
[7] *Journ. Ec. Polyt.* cap. 14.
[8] *Acad. Berlin*, 1775.
[9] *Camb Trans.* vol. v. p. 234.

hope to give a complete solution till our knowledge has advanced much beyond its present condition.

Indeed, in the science of Acoustics there is a vast body of facts to which we might apply what has just been said; but for the sake of pointing out some of them, we shall consider them as the subjects of one extensive and yet unsolved problem.

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## CHAPTER VI.

### Problem of Different Modes of Vibration of Bodies in general.

NOT only the objects of which we have spoken hitherto, strings and pipes, but almost all bodies are capable of vibration. Bells, gongs, tuning-forks, are examples of solid bodies; drums and tambourines, of membranes; if we run a wet finger along the edge of a glass goblet, we throw the fluid which it contains into a regular vibration; and the various character which sounds possess according to the room in which they are uttered, shows that large masses of air have peculiar modes of vibration. Vibrations are generally accompanied by sound, and they may, therefore, be considered as acoustical phenomena, especially as the sound is one of the most decisive facts in indicating the mode of vibration. Moreover, every body of this kind can vibrate in many different ways, the vibrating segments being divided by Nodal Lines and Surfaces of various form and number. The mode of vibration, selected by the body in each case, is determined by the way in which it is held, the way in which it is set in vibration, and the like circumstances.

The general problem of such vibrations includes the discovery and classification of the phenomena; the detection of their formal laws; and, finally, the explanation of these on mechanical principles. We must speak very briefly of what has been done in these ways. The facts which indicate Nodal Lines had been remarked by Galileo, on the sounding board of a musical instrument; and Hooke had proposed to observe the vibrations of a bell by strewing flour upon it. But it was Chladni, a German philosopher, who enriched acoustics with the discovery of the vast variety of symmetrical figures of Nodal Lines, which are exhibited on plates of regular forms, when made to sound. His first investigations on this subject, *Entdeckungen über die Theorie des Klangs*, were published 1787; and in 1802 and 1817 he added other discoveries. In these works he not only related a vast number of new and curious facts, but in some measure reduced some of them to order and law. For instance, he has traced all the vibrations of square plates to a resemblance with those forms of vibration in which Nodal Lines are parallel to one side of the square, and those in which they are parallel to another side; and he has established a notation for the modes of vibration founded on this classification. Thus, 5–2 denotes a form in which there are five nodal lines parallel to one side, and two to another; or a form which can be traced to a disfigurement of such a standard type. Savart pursued this subject still further; and traced, by actual observation, the forms of the Nodal Surfaces which divide solid bodies, and masses of air, when in a state of vibration.

The dependence of such vibrations upon their physical cause, namely, the elasticity of the substance, we can conceive in a general way; but the mathematical theory of such cases is, as might be supposed, very difficult, even if we confine ourselves to the obvious question of the mechanical possibility of these different modes of vibration, and leave out of consideration their dependence upon the mode of excitation. The transverse vibrations of elastic rods, plates, and rings, had been considered by Euler in 1779; but his calculation concerning plates had foretold only a small part of the curious phenomena observed by Chladni;[1] and the several notes which, according to his calculation, the same ring ought to give, were not in agreement with experiment.[2] Indeed, researches of this kind, as conducted by Euler, and other authors,[3] rather were, and were intended for, examples of analytical skill, than explanations of physical facts. James Bernoulli, after the publication of Chladni's experiments in 1787, attempted to solve the problem for plates, by treating a plate as a collection of fibres; but, as Chladni observes, the justice of this mode of conception is disproved, by the disagreement of its results with experiment.

The Institute of France, which had approved of Chladni's labours, proposed, in 1809, the problem now before us as a prize-question:[4]—'To give the mathematical theory of the vibrations of elastic sur

[1] Fischer, vi. 587.

[2] Ib. vi. 596.

[3] See Chladni, p. 474.

[4] See Chladni, p. 357.

faces, and to compare it with experiment." Only one memoir was sent in as a candidate for the prize; and this was not crowned, though honorable mention was made of it.[5] The formulæ of James Bernoulli were, according to M. Poisson's statement, defective, in consequence of his not taking into account the normal force which acts at the exterior boundary of the plate.[6] The author of the anonymous memoir corrected this error, and calculated the note corresponding to various figures of the nodal lines; and he found an agreement with experiment sufficient to justify his theory. He had not, however, proved his fundamental equation, which M. Poisson demonstrated in a Memoir, read in 1814.[7] At a more recent period also, MM. Poisson and Cauchy (as well as a lady, Mlle. Sophie Germain) have applied to this problem the artifices of the most improved analysis. M. Poisson[8] determined the relation of the notes given by the longitudinal and the transverse vibrations of a rod; and solved the problem of vibrating circular plates when the nodal lines are concentric circles. In both these cases, the numerical agreement of his results with experience, seemed to confirm the justice of his fundamental views.[9] He proceeds upon the hypothesis, that elastic bodies are composed of separate particles held together by the attractive forces which they exert upon each other, and distended by the repulsive force of heat. M. Cauchy[10] has also calculated the transverse, longitudinal, and rotatory vibrations of elastic rods, and has obtained results agreeing closely with experiment through a considerable list of comparisons. The combined authority of two profound analysts, as MM. Poisson and Cauchy are, leads us to believe that, for the simpler cases of the vibrations of elastic bodies, Mathematics has executed her task; but most of the more complex cases remain as yet unsubdued.

The two brothers, Ernest and William Weber, made many curious observations on undulations, which are contained in their *Wellenlehre*, (Doctrine of Waves,) published at Leipsig in 1825. They were led to suppose, (as Young had suggested at an earlier period,) that Chladni's figures of nodal lines in plates were to be accounted for by the superposition of undulations.[11] Mr. Wheatstone[12] has undertaken to account for Chladni's figures of vibrating *square* plates by this

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[5] Poisson's *Mém. in Ac. Sc.* 1812, p. 169. [6] Ib. p. 220.

[7] Ib. 1812, p. 2. [8] Ib. t. viii. 1829.

[9] *An. Chim.* tom. xxxvi. 1827, p. 90. [10] *Exercices de Mathématique*, iii. and iv.

[11] *Wellenlehre*, p. 474. [12] *Phil. Trans.* 1833, p. 593.

superposition of two or more simple and obviously allowable modes of nodal division, which have the same time of vibration. He assumes, for this purpose, certain "primary figures," containing only *parallel* nodal lines; and by combining these, first in twos, and then in fours, he obtains most of Chladni's observed figures, and accounts for their transitions and deviations from regularity.

The principle of the superposition of vibrations is so solidly established as a mechanical truth, that we may consider an acoustical problem as satisfactorily disposed of, when it is reduced to that principle, as well as when it is solved by analytical mechanics: but at the same time we may recollect, that the right application and limitation of this law involves no small difficulty; and in this case, as in all advances in physical science, we cannot but wish to have the new ground which has been gained, gone over by some other person in some other manner; and thus secured to us as a permanent possession.

*Savart's Laws.*—In what has preceded, the vibrations of bodies have been referred to certain general classes, the separation of which was suggested by observation; for example, the *transverse*, *longitudinal*, and *rotatory*,[13] vibrations of rods. The transverse vibrations, in which the rod goes backwards and forwards across the line of its length, were the only ones noticed by the earlier acousticians: the others were principally brought into notice by Chladni. As we have already seen in the preceding pages, this classification serves to express important laws; as, for instance, a law obtained by M. Poisson which gives the relation of the notes produced by the transverse and longitudinal vibrations of a rod. But this distinction was employed by M. Felix Savart to express laws of a more general kind; and then, as often happens in the progress of science, by pursuing these laws to a higher point of generality, the distinction again seemed to vanish. A very few words will explain these steps.

It was long ago known that vibrations may be communicated by contact. The distinction of transverse and longitudinal vibrations being established, Savart found that if one rod touched another perpendicularly, the longitudinal vibrations of the first occasion transverse vibrations in the second, and *vice versâ*. This is the more remarkable, since the two sets of vibrations are not equal in rapidity, and therefore cannot sympathize in any obvious manner.[14] Savart found himsel.

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[13] Vibrations tournantes.

[14] *An. Chim.* 1819, tom. xiv. p. 138.

able to generalize this proposition, and to assert that in any combination of rods, strings, and laminæ, at right angles to each other, the longitudinal and transverse vibrations affect respectively the rods in the one and other direction,[15] so that when the horizontal rods, for example, vibrate in the one way, the vertical rods vibrate in the other.

This law was thus expressed in terms of that classification of vibrations of which we have spoken. Yet we easily see that we may express it in a more general manner, without referring to that classification, by saying, that vibrations are communicated so as always to be parallel to their original direction. And by following it out in this shape by means of experiment, M. Savart was led, a short time afterwards, to deny that there is any essential distinction in these different kinds of vibration. "We are thus led," he says[16] in 1822, "to consider *normal* [transverse] vibrations as only one circumstance in a more general motion common to all bodies, analogous to *tangential* [longitudinal and rotatory] vibrations; that is, as produced by small *molecular oscillations*, and differently modified according to the direction which it affects, relatively to the dimensions of the vibrating body."

These "inductions," as he properly calls them, are supported by a great mass of ingenious experiments; and may be considered as well established, when they are limited to molecular oscillations, employing this phrase in the sense in which it is understood in the above statement; and also when they are confined to bodies in which the play of elasticity is not interrupted by parts more rigid than the rest, as the sound-post of a violin.[17] And before I quit the subject, I may notice a consequence which M. Savart has deduced from his views, and which, at first sight, appears to overturn most of the earlier doctrines respecting vibrating bodies. It was formerly held that tense strings and elastic rods could vibrate only in a determinate series of modes of division, with no intermediate steps. But M. Savart maintains,[18] on the contrary, that they produce sounds which are gradually transformed into one another, by indefinite intermediate degrees. The reader may naturally ask, what is the solution of this apparent con-

[15] *An. Chim.* p. 152. [16] Ib. t. xxv. p. 33.

[17] For the suggestion of the necessity of this limitation I am indebted to Mr. Willis.

[18] *An. Chim.* 1826, t. xxxii. p. 384.

tradiction between the earliest and the latest discoveries in acoustics? And the answer must be, that these intermediate modes of vibration are complex in their nature, and difficult to produce; and that those which were formerly believed to be the only possible vibrating conditions, are so eminent above all the rest by their features, their simplicity, and their facility, that we may still, for common purposes, consider them as a class apart; although for the sake of reaching a general theorem, we may associate them with the general mass of cases of molecular vibrations. And thus we have no exception here, as we can have none in any case, to our maxim, that what formed part of the early discoveries of science, forms part of its latest systems.

We have thus surveyed the progress of the science of sound up to recent times, with respect both to the discovery of laws of phenomena, and the reduction of these to their mechanical causes. The former branch of the science has necessarily been inductively pursued; and therefore has been more peculiarly the subject of our attention. And this consideration will explain why we have not dwelt more upon the deductive labors of the great analysts who have treated of this problem.

To those who are acquainted with the high and deserved fame which the labors of D'Alembert, Euler, Lagrange, and others, upon this subject, enjoy among mathematicians, it may seem as if we had not given them their due prominence in our sketch. But it is to be recollected here, as we have already observed in the case of hydrodynamics, that even when the general principles are uncontested, mere mathematical deductions from them do not belong to the history of physical science, except when they point out laws which are intermediate between the general principle and the individual facts, and which observation may confirm.

The business of constructing any science may be figured as the task of forming a road on which our reason can travel through a certain province of the external world. We have to throw a bridge which may lead from the chambers of our own thoughts, from our speculative principles, to the distant shore of material facts. But in all cases the abyss is too wide to be crossed, except we can find some intermediate points on which the piers of our structure may rest. Mere facts, without connexion or law, are only the rude stones hewn from the opposite bank, of which our arches may, at some time, be built. But mere hypothetical mathematical calculations are only plans of projected structures; and those plans which exhibit only one vast and single arch, or which suppose no support but that which our own position supplies, will assuredly never become realities. We must have a firm basis of intermediate generalizations in order to frame a continuous and stable edifice.

In the subject before us, we have no want of such points of intermediate support, although they are in many instances irregularly distributed and obscurely seen. The number of observed laws and relations of the phenomena of sound, is already very great; and though the time may be distant, there seems to be no reason to despair of one day uniting them by clear ideas of mechanical causation, and thus of making acoustics a perfect secondary mechanical science.

The historical sketch just given includes only such parts of acoustics as have been in some degree reduced to general laws and physical causes; and thus excludes much that is usually treated of under that head. Moreover, many of the numerical calculations connected with sound belong to its agreeable effect upon the ear; as the properties of the various systems of *Temperament*. These are parts of Theoretical Music, not of Acoustics; of the Philosophy of the Fine Arts, not of Physical Science; and may be referred to in a future portion of this work, so far as they bear upon our object.

The science of Acoustics may, however, properly consider other differences of sound than those of acute and grave,—for instance, the *articulate* differences, or those by which the various letters are formed. Some progress has been made in reducing this part of the subject to general rules; for though Kempelen's "talking machine" was only a work of art, Mr. Willis's machine,[19] which exhibits the relation among the vowels, gives us a law such as forms a step in science. We may, however, consider this instrument as a *phthongometer*, or measure of vowel quality; and in that point of view we shall have to refer to it again when we come to speak of such measures.

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[19] On the Vowel Sounds, and on Reed Organ-pipes. *Camb. Trans.* iii. 287

# BOOK IX.

## *SECONDARY MECHANICAL SCIENCES.*

(CONTINUED.)

# HISTORY OF OPTICS,

FORMAL AND PHYSICAL.

Ω Διὸς ὑψιμέλαθρον ἔχων κράτος αἰὲν ἀτειρὲς
Ἄστρων, Ἠελίου τε, Σεληναίης τε μέρισμα
Πανδαμάτωρ, πυρίπνου, πᾶσιν ζωοῖσιν ἔναυσμα
Ὑψιφάνης ΑΙΘΗΡ, κόσμου στοιχεῖον, ἄριστον·
Ἀγλαὸν ὦ βλάστημα, σελασφόρον, ἀστεροφεγγὲς
Κικλήσκων λίτομαι σε, κεκραμένον εὔδιον εἶναι.

ORPHEUS. HYMN.

O thou who fillest the palaces of Jove ;
Who flowest round moon, and sun, and stars above ;
Pervading, bright, life-giving element,
Supernal ETHER, fair and excellent ;
Fountain of hope and joy, of light and day,
We own at length thy tranquil, steady sway.

## INTRODUCTION.

### *Formal and Physical Optics.*

THE history of the science of Optics, written at length, would be very voluminous; but we shall not need to make our history so; since our main object is to illustrate the nature of science and the conditions of its progress. In this way Optics is peculiarly instructive; the more so, as its history has followed a course in some respects different from both the sciences previously reviewed. Astronomy, as we have seen, advanced with a steady and continuous movement from one generation to another, from the earliest time, till her career was crowned by the great unforeseen discovery of Newton; Acoustics had her extreme generalization in view from the first, and her history consists in the correct application of it to successive problems; Optics advanced through a scale of generalizations as remarkable as those of Astronomy; but for a long period she was almost stationary; and, at last, was rapidly impelled through all those stages by the energy of two or three discoverers. The highest point of generality which Optics has reached is little different from that which Acoustics occupied at once; but in the older and earlier science we still want that palpable and pointed confirmation of the general principle, which the undulatory theory receives from optical phenomena. Astronomy has amassed her vast fortune by long-continued industry and labor; Optics has obtained hers in a few years by sagacious and happy speculations; Acoustics, having early acquired a competence, has since been employed rather in improving and adorning than in extending her estate.

The successive inductions by which Optics made her advances, might, of course, be treated in the same manner as those of Astronomy, each having its prelude and its sequel. But most of the discoveries in Optics are of a smaller character, and have less employed the minds of men, than those of Astronomy; and it will not be necessary to exhibit them in this detailed manner, till we come to the great generalization by which the theory was established. I shall, therefore, now pass rapidly in review the earlier optical discoveries, without any such division of the series.

Optics, like Astronomy, has for its object of inquiry, first, the laws of phenomena, and next, their causes; and we may hence divide this science, like the other, into *Formal Optics* and *Physical Optics*. The distinction is clear and substantive, but it is not easy to adhere to it in our narrative; for, after the theory had begun to make its rapid advance, many of the laws of phenomena were studied and discovered in immediate reference to the theoretical cause, and do not occupy a separate place in the history of science, as in Astronomy they do. We may add, that the reason why Formal Astronomy was almost complete before Physical Astronomy began to exist, was, that it was necessary to construct the science of Mechanics in the mean time, in order to be able to go on; whereas, in Optics, mathematicians were able to calculate the results of the undulatory theory as soon as it had suggested itself from the earlier facts, and while the great mass of facts were only becoming known.

We shall, then, in the first *nine* chapters of the History of Optics, treat of the Formal Science, that is, the discovery of the laws of phenomena. The classes of phenomena which will thus pass under our notice are numerous; namely, reflection, refraction, chromatic dispersion, achromatization, double refraction, polarization, dipolarization, the colors of thin plates, the colors of thick plates, and the fringes and bands which accompany shadows. All these cases had been studied, and, in most of them, the laws had been in a great measure discovered, before the physical theory of the subject gave to our knowledge a simpler and more solid form.

# FORMAL OPTICS.

## CHAPTER I.

### Primary Induction of Optics.—Rays of Light and Laws of Reflection.

IN speaking of the Ancient History of Physics, we have already noticed that the optical philosophers of antiquity had satisfied themselves that vision is performed in straight lines;—that they had fixed their attention upon those straight lines, or *rays*, as the proper object of the science;—they had ascertained that rays reflected from a bright surface make the *angle of reflection* equal to the *angle of incidence*;—and they had drawn several consequences from these principles.

We may add to the consequences already mentioned, the art of *perspective*, which is merely a corollary from the doctrine of rectilinear visual rays; for if we suppose objects to be referred by such rays to a plane interposed between them and the eye, all the rules of perspective follow directly. The ancients practised this art, as we see in the pictures which remain to us and we learn from Vitruvius,[1] that they also wrote upon it. Agatharchus, who had been instructed by Eschylus in the art of making decorations for the theatre, was the first author on this subject, and Anaxagoras, who was a pupil of Agatharchus, also wrote an *Actinographia*, or doctrine of drawing by rays: but none of these treatises are come down to us. The moderns re-invented the art in the flourishing times of the art of painting, that is, about the end of the fifteenth century; and, belonging to that period also, we have treatises[2] upon it.

But these are only deductive applications of the most elementary optical doctrines; we must proceed to the inductions by which further discoveries were made.

[1] *De Arch.* ix. Mont. i. 707.

[2] Gauricus, 1504.

## CHAPTER II

### Discovery of the Law of Refraction.

WE have seen in the former part of this history that the Greeks had formed a tolerably clear conception of the refraction as well as the reflection of the rays of light; and that Ptolemy had measured the amount of refraction of glass and water at various angles. If we give the names of the *angle of incidence* and the *angle of refraction* respectively to the angles which a ray of light makes with the line perpendicular to surface of glass or water (or any other medium) within and without the medium, Ptolemy had observed that the angle of refraction is always less than the angle of incidence. He had supposed it to be less in a given proportion, but this opinion is false; and was afterwards rightly denied by the Arabian mathematician Alhazen. The optical views which occur in the work of Alhazen are far sounder than those of his predecessors; and the book may be regarded as the most considerable monument which we have of the scientific genius of the Arabians; for it appears, for the most part, not to be borrowed from Greek authorities. The author not only asserts (lib. vii.), that refraction takes place towards the perpendicular, and refers to experiment for the truth of this: and that the quantities of the refraction differ according to the magnitudes of the angles which the directions of the incidental rays (*primæ lineæ*) make with the perpendiculars to the surface; but he also says distinctly and decidedly that the angles of refraction do not follow the proportion of the angles of incidence.

[2nd Ed.] [There appears to be good ground to assent to the assertion of Alhazen's originality, made by his editor Risner, who says, "Euclideum hic vel Ptolemaicum nihil fere est." Besides the doctrine of reflection and refraction of light, the Arabian author gives a description of the eye. He distinguishes three fluids, *humor aqueus*, *crystallinus*, *vitreus*, and four coats of the eye, *tunica adherens*, *cornea*, *uvea*, *tunica reti similis*. He distinguishes also three kinds of vision: "Visibile percipitur aut solo visu, aut visu et syllogismo, aut visu et anticipatâ notione." He has several propositions relating to what we sometimes call the Philosophy of Vision: for instance this: "E visi bili sæpius viso remanet in anima generalis notio," &c.]

The assertion, that the angles of refraction are not proportional to the angles of incidence, was an important remark; and if it had been steadily kept in mind, the next thing to be done with regard to refraction was to go on experimenting and conjecturing till the true law of refraction was discovered; and in the mean time to apply the principle as far as it was known. Alhazen, though he gives directions for making experimental measures of refraction, does not give any Table of the results of such experiments, as Ptolemy had done. Vitello, a Pole, who in the 13th century published an extensive work upon Optics, does give such a table; and asserts it to be deduced from experimen , as I have already said (vol. i.). But this assertion is still liable to doubt in consequence of the table containing impossible observations.

[2nd Ed.] [As I have already stated, Vitello asserts that his Tables were derived from his own observations. Their near agreement with those of Ptolemy does not make this improbable: for where the observations were only made to half a degree, there was not much room for observers to differ. It is not unlikely that the observations of refraction out of air into water and glass, and out of water into glass, were actually made; while the impossible values which accompany them, of the refraction out of water and glass into air, and out of glass into water, were calculated, and calculated from an erroneous rule.]

The principle that a ray refracted in glass or water is turned towards the perpendicular, without knowing the exact law of refraction, enabled mathematicians to trace the effects of transparent bodies in various cases. Thus in Roger Bacon's works we find a tolerably distinct explanation of the effect of a convex glass; and in the work of Vitello the effect of refraction at the two surfaces of a glass globe is clearly traceable.

Notwithstanding Alhazen's assertion of the contrary, the opinion was still current among mathematicians that the angle of refraction was proportional to the angle of incidence. But when Kepler's attention was drawn to the subject, he saw that this was plainly inconsistent with the observations of Vitello for large angles; and he convinced himself by his own experiments that the true law was something different from the one commonly supposed. The discovery of this true law excited in him an eager curiosity; and this point had the more interest for him in consequence of the introduction of a correction for atmospheric refraction into astronomical calculations, which had been made by Tycho, and of the invention of the telescope. In his *Supplement to Vitello*, published in 1604, Kepler attempts to reduce to a rule the measured quantities of refraction. The reader who recollects what we have already narrated, the manner in which Kepler attempted to reduce to law the astronomical observations of Tycho,—devising an almost endless variety of possible formulæ, tracing their consequences with undaunted industry, and relating, with a vivacious garrulity, his disappointments and his hopes,—will not be surprised to find that he proceeded in the same manner with regard to the Tables of Observed Refractions. He tried a variety of constructions by triangles, conic sections, &c., without being able to satisfy himself; and he at last[1] is obliged to content himself with an approximate rule, which makes the refraction partly proportional to the angle of incidence, and partly, to the secant of that angle. In this way he satisfies the observed refractions within a difference of less than half a degree each way. When we consider how simple the law of refraction is, (that the ratio of the sines of the angles of incidence and refraction is constant for the same medium,) it appears strange that a person attempting to discover it, and drawing triangles for the purpose, should fail; but this lot of missing what afterwards seems to have been obvious, is a common one in the pursuit of truth.

The person who did discover the Law of the Sines, was Willebrord Snell, about 1621; but the law was first published by Descartes, who had seen Snell's papers.[2] Descartes does not acknowledge this law to have been first detected by another; and after his manner, instead of establishing its reality by reference to experiment, he pretends to prove *à priori* that it must be true,[3] comparing, for this purpose, the particles of light to balls striking a substance which *accelerates* them.

[2nd Ed.] [Huyghens says of Snell's papers, "Quæ et nos vidimus aliquando, et Cartesium quoque vidisse accepimus, et hinc fortasse mensuram illam quæ in sinibus consistit elicuerit." Isaac Vossius, *De Lucis Naturâ et Proprietate*, says that he also had seen this law in Snell's unpublished optical Treatise. The same writer says, "Quod itaque (Cartesius) habet, refractionum momenta non exigenda esse ad angulos sed ad lineas, id tuo Snellio, acceptum ferre debuisset, cujus nomen *more solito* dissimulavit." "Cartesius got his law from Snell, and *in his usual way*, concealed it."

[1] L. U. K. *Life of Kepler*, p. 115.
[2] Huyghens, *Dioptrica*, p. 2. [3] *Diopt.* p. 53.

Huyghens' assertion, that Snell did not *attend to* the proportion of the sines, is very captious; and becomes absurdly so, when it is made to mean that Snell did not *know* the law of the sines. It is not denied that Snell knew the true law, or that the true law is the law of the sines. Snell does not use the trigonometrical term *sine*, but he expresses the law in a geometrical form more simply. Even if he *had* attended to the law of the sines, he might reasonably have preferred his own way of stating it.

James Gregory also independently discovered the true law of refraction; and, in publishing it, states that he had learnt that it had already been published by Descartes].

But though Descartes does not, in this instance, produce any good claims to the character of an inductive philosopher, he showed considerable skill in tracing the consequences of the principle when once adopted. In particular we must consider him as the genuine author of the explanation of the rainbow. It is true that Fleischer[4] and Kepler had previously ascribed this phenomenon to the rays of sunlight which, falling on drops of rain, are refracted into each drop, reflected at its inner surface, and refracted out again: Antonio de Dominis had found that a glass globe of water, when placed in a particular position with respect to the eye, exhibited bright colors; and had hence explained the circular form of the bow, which, indeed, Aristotle had done before.[5] But none of these writers had shown why there was a narrow bright circle of a definite diameter; for the drops which send rays to the eye after two refractions and a reflection, occupy a much wider space in the heavens. Descartes assigned the reason for this in the most satisfactory manner,[6] by showing that the rays which, after two refractions and a reflection, come to the eye at an angle of about forty-one degrees with their original direction, are far more dense than those in any other position. He showed, in the same manner, that the existence and position of the *secondary bow* resulted from the same laws. This is the complete and adequate account of the state of things, so far as the brightness of the bows only is concerned; the explanation of the colors belongs to the next article of our survey.

The explanation of the rainbow and of its magnitude, afforded by Snell's law of sines, was perhaps one of the leading points in the verification of the law. The principle, being once established, was applied, by the aid of mathematical reasoning, to atmospheric refractions, opti-

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[4] Mont. i. 701. [5] *Meteorol.* iii. 3. [6] *Meteorum*, cap. viii. p. 196

cal instruments, *diacaustic* curves, (that is, the curves of intense light produced by refraction,) and to various other cases; and was, of course, tested and confirmed by such applications. It was, however, impossible to pursue these applications far, without a due knowledge of the laws by which, in such cases, colors are produced. To these we now proceed.

[2nd Ed.] [I have omitted many interesting parts of the history of Optics about this period, because I was concerned with the *inductive* discovery of laws, rather than with mathematical *deductions* from such laws when established, or *applications* of them in the form of instruments. I might otherwise have noticed the discovery of Spectacle Glasses, of the Telescope, of the Microscope, of the Camera Obscura, and the mathematical explanation of these and other phenomena, as given by Kepler and others. I might also have noticed the progress of knowledge respecting the Eye and Vision. We have seen that Alhazen described the structure of the eye. The operation of the parts was gradually made out. Baptista Porta compares the eye to his *Camera Obscura* (*Magia Naturalis*, 1579). Scheiner, in his *Oculus*, published 1652, completed the Theory of the Eye. And Kepler discussed some of the questions even now often agitated; as the causes and conditions of our seeing objects single with two eyes, and erect with inverted images.]

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## CHAPTER III.

### Discovery of the Law of Dispersion by Refraction

EARLY attempts were made to account for the colors of the rain bow, and various other phenomena in which colors are seen to arise from transient and unsubstantial combinations of media. Thus Aristotle explains the colors of the rainbow by supposing[1] that it is light seen through a dark medium: "Now," says he, "the bright seen through the dark appears red, as, for instance, the fire of green wood seen through the smoke, and the sun through mist. Also[2] the weaker is the light, or the visual power, and the nearer the color approaches to the black; becoming first red, then green, then purple. But[3] the

[1] *Meteor.* iii. 3, p. 373. [2] Ib. p. 374. [3] Ib. p. 375

vision is strongest in the outer circle, because the periphery is greater; —thus we shall have a gradation from red, through green, to purple, in passing from the outer to the inner circle." This account would hardly have deserved much notice, if it had not been for a strange attempt to revive it, or something very like it, in modern times. The same doctrine is found in the work of De Dominis.[4] According to him, light is white: but if we mix with the light something dark, the colors arise,—first red, then green, then blue or violet. He applies this to explain the colors of the rainbow,[5] by means of the consideration that, of the rays which come to the eye from the globes of water, some go through a larger thickness of the globe than others, whence he obtains the gradation of colors just described.

Descartes came far nearer the true philosophy of the iridal colors. He found that a similar series of colors was produced by refraction of light bounded by shade, through a prism;[6] and he rightly inferred that neither the curvature of the surface of the drops of water, nor the reflection, nor the repetition of refraction, were necessary to the generation of such colors. In further examining the course of the rays, he approaches very near to the true conception of the case; and we are led to believe that he might have anticipated Newton in his discovery of the unequal refrangibility of different colors, if it had been possible for him to reason any otherwise than in the terms and notions of his preconceived hypotheses. The conclusion which he draws is,[7] that "the particles of the subtile matter which transmit the action of light, endeavor to rotate with so great a force and impetus, that they cannot move in a straight line (whence comes refraction): and that those particles which endeavor to revolve much more strongly produce a red color, those which endeavor to move only a little more strongly produce yellow." Here we have a clear perception that colors and unequal refraction are connected, though the cause of refraction is expressed by a gratuitous hypothesis. And we may add, that he applies this notion rightly, so far as he explains himself,[8] to account for the colors of the rainbow.

It appears to me that Newton and others have done Descartes injustice, in ascribing to De Dominis the true theory of the rainbow. There are two main points of this theory namely, the showing that a *bright* circular band, of a certain definite diameter, arises from the

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[4] Cap. iii. p. 9. See also Göthe, *Farbenl.* vol. ii. p. 251. [5] Göthe, p. 263.
[6] *Meteor.* Sect. viii. p. 190. [7] Sect. vii. p. 192. [8] *Meteor.* Sect. ix.

great intensity of the light returned at a certain angle; and the referring the different *colors* to the *different quantity of the refraction ;* and both these steps appear indubitably to be the discoveries of Descartes And he informs us that these discoveries were not made without some exertion of thought. "At first," he says,[9] "I doubted whether the iridal colors were produced in the same way as those in the prism; but, at last, taking my pen, and carefully calculating the course of the rays which fall on each part of the drop, I found that many more come at an angle of forty-one degrees, than either at a greater or a less angle. So that there is a bright bow terminated by a shade; and hence the colors are the same as those produced through a prism."

The subject was left nearly in the same state, in the work of Grimaldi, *Physico-Mathesis, de Lumine, Coloribus et Iride*, published at Bologna in 1665. There is in this work a constant reference to numerous experiments, and a systematic exposition of the science in an improved state. The author's calculations concerning the rainbow are put in the same form as those of Descartes; but he is further from seizing the true principle on which its coloration depends. He rightly groups together a number of experiments in which colors arise from refraction;[10] and explains them by saying that the color is brighter where the light is denser: and the light is denser on the side from which the refraction turns the ray, because the increments of refraction are greater in the rays that are more inclined.[11] This way of treating the question might be made to give a sort of explanation of most of the facts, but is much more erroneous than a developement of Descartes's view would have been.

At length, in 1672, Newton gave[12] the true explanation of the facts; namely, that light consists of rays of different colors and different refrangibility. This now appears to us so obvious a mode of interpreting the phenomena, that we can hardly understand how they can be conceived in any other manner; but yet the impression which this discovery made, both upon Newton and upon his contemporaries, shows how remote it was from the then accepted opinions. There appears to have been a general persuasion that the coloration was produced, not by any peculiarity in the law of refraction itself, but by some collateral circumstance,—some dispersion or variation of density of the light, in addition to the refraction. Newton's discovery consisted in

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[9] Sect. ix. p. 193. [10] Prop. 35, p. 254. [11] Ib. p. 256.
[12] *Phil. Trans.* t. vii. p. 3075.

teaching distinctly that the law of refraction was to be applied, not to the beam of light in general, but to the colors in particular.

When Newton produced a bright spot on the wall of his chamber, by admitting the sun's light through a small hole in his window-shutter, and making it pass through a prism, he expected the image to be round; which, of course, it would have been, if the colors had been produced by an equal dispersion in all directions; but to his surprise he saw the image, or *spectrum*, five times as long as it was broad He found that no consideration of the different thickness of the glass, the possible unevenness of its surface, or the different angles of rays proceeding from the two sides of the sun, could be the cause of this shape. He found, also, that the rays did not go from the prism to the image in curves; he was then convinced that the different colors were refracted separately, and at different angles; and he confirmed this opinion by transmitting and refracting the rays of each color separately.

The experiments are so easy and common, and Newton's interpretation of them so simple and evident, that we might have expected it to receive general assent; indeed, as we have shown, Descartes had already been led very near the same point. In fact, Newton's opinions were not long in obtaining general acceptance; but they met with enough of cavil and misapprehension to annoy extremely the discoverer, whose clear views and quiet temper made him impatient alike of stupidity and of contentiousness.

We need not dwell long on the early objections which were made to Newton's doctrine. A Jesuit, of the name of Ignatius Pardies, professor at Clermont, at first attempted to account for the elongation of the image by the difference of the angles made by the rays from the two edges of the sun, which would produce a difference in the amount of refraction of the two borders; but when Newton pointed out the calculations which showed the insufficiency of this explanation, he withdrew his opposition. Another more pertinacious opponent appeared in Francis Linus, a physician of Liege; who maintained, that having tried the experiment, he found the sun's image, when the sky was clear, to be round and not oblong; and he ascribed the elongation noticed by Newton, to the effect of clouds. Newton for some time refused to reply to this contradiction of his assertions, though obstinately persisted in; and his answer was at last sent, just about the time of Linus's death, in 1675. But Gascoigne, a friend of Linus, still maintained that he and others had seen what the Dutch physician had described; and Newton, who was pleased with the candor of Gascoigne's letter, suggested that the Dutch experimenters might have taken one of the images reflected from the surfaces of the prism, of which there are several, instead of the proper refracted one. By the aid of this hint, Lucas of Liege repeated Newton's experiments, and obtained Newton's result, except that he never could obtain a spectrum whose length was more than three and a half times its breadth. Newton, on his side, persisted in asserting that the image would be five times as long as it was broad, if the experiment were properly made. It is curious that he should have been so confident of this, as to conceive himself certain that such would be the result in all cases. We now know that the dispersion, and consequently the length, of the spectrum, is very different for different kinds of glass, and it is very probable that the Dutch prism was really less dispersive than the English one.[13] The erroneous assumption which Newton made in this instance, he held by to the last; and was thus prevented from making the discovery of which we have next to speak.

Newton was attacked by persons of more importance than those we have yet mentioned; namely, Hooke and Huyghens. These philosophers, however, did not object so much to the laws of refraction of different colors, as to some expressions used by Newton, which, they conceived, conveyed false notions respecting the composition and nature of light. Newton had asserted that all the different colors are of distinct kinds, and that, by their composition, they form white light. This is true of colors as far as their analysis and composition by refraction are concerned; but Hooke maintained that all natural colors are produced by various combinations of two primary ones, red and violet;[14] and Huyghens held a similar doctrine, taking, however, yellow and blue for his basis. Newton answers, that such compositions as they speak of, are not compositions of simple colors in his sense of the expressions. These writers also had both of them adopted an opinion that light consisted in vibrations; and objected to Newton that his language was erroneous, as involving the hypothesis that light was a body. Newton appears to have had a horror of the word *hypothesis*, and protests against its being supposed that his "theory" rests on such a foundation.

The doctrine of the unequal refrangibility of different rays is clearly exemplified in the effects of lenses, which produce images more or

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[13] Brewster's *Newton*, p. 50.

[14] Brewster's *Newton*, p. 54. *Phil. Trans.* viii. 5084, 6086.

less bordered with color, in consequence of this property. The improvement of telescopes was, in Newton's time, the great practical motive for aiming at the improvement of theoretical optics. Newton's theory showed why telescopes were imperfect, namely, in consequence of the different refraction of different colors, which produces a *chromatic* aberration: and the theory was confirmed by the circumstances of such imperfections. The false opinion of which we have already spoken, that the dispersion must be the same when the refraction is the same, led him to believe that the imperfection was insurmountable, —that *achromatic* refraction could not be obtained: and this view made him turn his attention to the construction of reflecting instead of refracting telescopes. But the rectification of Newton's error was a further confirmation of the general truth of his principles in other respects; and since that time, the soundness of the Newtonian law of refraction has hardly been questioned among physical philosophers.

It has, however, in modern times, been very vehemently controverted in a quarter from which we might not readily have expected a detailed discussion on such a subject. The celebrated Göthe has written a work on *The Doctrine of Colors*, (*Farbenlehre;* Tübingen, 1810,) one main purpose of which is, to represent Newton's opinions, and the work in which they are formally published, (his *Opticks*,) as utterly false and mistaken, and capable of being assented to only by the most blind and obstinate prejudice. Those who are acquainted with the extent to which such an opinion, promulgated by Göthe, was likely to be widely adopted in Germany, will not be surprised that similar language is used by other writers of that nation. Thus Schelling[15] says: "Newton's *Opticks* is the greatest proof of the possibility of a whole structure of fallacies, which, in all its parts, is founded upon observation and experiment." Göthe, however, does not concede even so much to Newton's work. He goes over a large portion of it, page by page, quarrelling with the experiments, diagrams, reasoning, and language, without intermission; and holds that it is not reconcileable with the most simple facts. He declares,[16] that the first time he looked through a prism, he saw the white walls of the room still look white, "and though alone, I pronounced, as by an instinct, that the Newtonian doctrine is false." We need not here point out how inconsistent with the Newtonian doctrine it was, to expect, as Göthe expected, that the wall should be all over colored various colors.

[15] *Vorlesungen*, p. 270.

[16] *Farbenlehre*, vol. ii. p. 678.

Göthe not only adopted and strenuously maintained the opinion that the Newtonian theory was false, but he framed a system of his own to explain the phenomena of color. As a matter of curiosity, it may be worth our while to state the nature of this system; although undoubtedly it forms no part of the *progress* of physical science. Göthe's views are, in fact, little different from those of Aristotle and Antonio de Dominis, though more completely and systematically developed. According to him, colors arise when we see through a dim medium ("ein trübes mittel"). Light in itself is colorless; but if it be seen through a somewhat dim medium, it appears yellow; if the dimness of the medium increases, or if its depth be augmented, we see the light gradually assume a yellow-red color, which finally is heightened to a ruby-red. On the other hand, if darkness is seen through a dim medium which is illuminated by a light falling on it, a blue color is seen, which becomes clearer and paler, the more the dimness of the medium increases, and darker and fuller, as the medium becomes more transparent; and when we come to "the smallest degree of the purest dimness," we see the most perfect violet.[17] In addition to this "doctrine of the dim medium," we have a second principle asserted concerning refraction. In a vast variety of cases, images are accompanied by "accessory images," as when we see bright objects in a looking-glass.[18] Now, when an image is displaced by refraction, the displacement is not complete, clear and sharp, but incomplete, so that there is an accessory image along with the principal one.[19] From these principles, the colors produced by refraction in the image of a bright object on a dark ground, are at once derivable. The accessory image is semitransparent;[20] and hence that border of it which is pushed forwards, is drawn from the dark over the bright, and there the yellow appears; on the other hand, where the clear border laps over the dark ground, the blue is seen;[21] and hence we easily see that the image must appear red and yellow at one end, and blue and violet at the other.

We need not explain this system further, or attempt to show how vague and loose, as well as baseless, are the notions and modes of conception which it introduces. Perhaps it is not difficult to point out the peculiarities in Göthe's intellectual character which led to his singularly unphilosophical views on this subject. One important cir-

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[17] *Farbenlehre*, § 150, p. 151. [18] Ib. § 223.
[19] Ib. § 227. [20] Ib. § 238. [21] Ib. § 239.

cumstance is, that he appears, like many persons in whom the poetical imagination is very active, to have been destitute of the talent and the habit of geometrical thought. In all probability, he never apprehended clearly and steadily those relations of position on which the Newtonian doctrine depends. Another cause of his inability to accept the doctrine probably was, that he had conceived the "composition" of colors in some way altogether different from that which Newton understands by composition. What Göthe expected to see, we cannot clearly collect; but we know, from his own statement, that his intention of experimenting with a prism arose from his speculations on the rules of coloring in pictures; and we can easily see that any notion of the composition of colors which such researches would suggest, would require to be laid aside, before he could understand Newton's theory of the composition of light.

Other objections to Newton's theory, of a kind very different, have been recently made by that eminent master of optical science, Sir David Brewster. He contests Newton's opinion, that the colored rays into which light is separated by refraction are altogether simple and homogeneous, and incapable of being further analysed and modified. For he finds that by passing such rays through colored media (as blue glass for instance), they are not only absorbed and transmitted in very various degrees, but that some of them have their color altered; which effect he conceives as a further analysis of the rays, one component color being absorbed and the other transmitted.[22] And on this subject we can only say, as we have before said, that Newton has incontestably and completely established his doctrine, so far as analysis and decomposition *by refraction* are concerned; but that with regard to any other analysis, which absorbing media or other agents may produce, we have no right from his experiments to assert, that the colors of the spectrum are incapable of *such* decomposition. The whole subject of the colors of objects, both opake and transparent, is still in obscurity. Newton's conjectures concerning the causes of the colors of natural bodies, appear to help us little; and his opinions on that subject are to be separated altogether from the important step which he made in optical science, by the establishment of the true doctrine of refractive dispersion.

[2nd Ed.] [After a careful re-consideration of Sir D. Brewster's asserted analysis of the solar light into three colors by means of

[22] This latter fact has, however, been denied by other experimenters.

absorbing media, I cannot consider that he has established his point as an exception to Newton's doctrine. In the first place, the analysis of light into *three* colors appears to be quite arbitrary, granting all his experimental facts. I do not see why, using other media, he might not just as well have obtained other elementary colors. In the next place, this cannot be called an *analysis* in the same sense as Newton's analysis, except the relation between the two is shown. Is it meant that Newton's experiments prove nothing? Or is Newton's conclusion allowed to be true of light which has not been analysed by absorption? And where are we to find such light, since the atmosphere absorbs? But, I must add, in the third place, that with a very sincere admiration of Sir D. Brewster's skill as an experimenter, I think his experiment requires, not only limitation, but confirmation by other experimenters. Mr. Airy repeated the experiments with about thirty different absorbing substances, and could not satisfy himself that in any case they changed the color of a ray of given refractive power. These experiments were described by him at a meeting of the Cambridge Philosophical Society.]

We now proceed to the corrections which the next generation introduced into the details of this doctrine.

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## CHAPTER IV.

### Discovery of Achromatism.

THE discovery that the laws of refractive dispersion of different substances were such as to allow of combinations which neutralized the dispersion without neutralizing the refraction, is one which has hitherto been of more value to art than to science. The property has no definite bearing, which has yet been satisfactorily explained, upon the *theory* of light; but it is of the greatest importance in its application to the construction of telescopes; and it excited the more notice, in consequence of the prejudices and difficulties which for a time retarded the discovery.

Newton conceived that he had proved by experiment,[1] that light

[1] *Opticks*, B. i. p. ii. Prop. 3.

is white after refraction, when the emergent rays are parallel to the incident, and in no other case. If this were so, the production of colorless images by refracting media would be impossible; and such, in deference to Newton's great authority, was for some time the general persuasion. Euler[2] observed, that a combination of lenses which does not color the image must be possible, since we have an example of such a combination in the human eye; and he investigated mathematically the conditions requisite for such a result. Klingenstierna,[3] a Swedish mathematician, also showed that Newton's rule could not be universally true. Finally, John Dollond,[4] in 1757, repeated Newton's experiment, and obtained an opposite result. He found that when an object was seen through two prisms, one of glass and one of water, of such angles that it did not appear displaced by refraction, it was colored. Hence it followed that, without being colored, the rays might be made to undergo refraction; and that thus, substituting lenses for prisms, a combination might be formed, which should produce an image without coloring it, and make the construction of an *achromatic* telescope possible.

Euler at first hesitated to confide in Dollond's experiments; but he was assured of their correctness by Clairaut, who had throughout paid great attention to the subject; and those two great mathematicians, as well as D'Alembert, proceeded to investigate mathematical formulæ which might be useful in the application of the discovery. The remainder of the deductions, which were founded upon the laws of dispersion of various refractive substances, belongs rather to the history of art than of science. Dollond used at first, for his achromatic object-glass, a lens of crown-glass, and one of flint-glass. He afterwards employed two lenses of the former substance, including between them one of the latter, adjusting the curvatures of his lenses in such a way as to correct the imperfections arising from the spherical form of the glasses, as well as the fault of color. Afterwards, Blair used fluid media along with glass lenses, in order to produce improved object-glasses. This has more recently been done in another form by Mr. Barlow. The inductive laws of refraction being established, their results have been deduced by various mathematicians, as Sir J. Herschel and Professor Airy among ourselves, who have simplified and extended the investigation of the formulæ which determine the best combination of lenses in the object-glasses and eye-glasses of tele-

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[2] *Ac. Berlin.* 1747. [3] *Swedish Trans.* 1754. [4] *Phil. Trans.* 1758

scopes, both with reference to spherical and to *chromatic* aberrations.

According to Dollond's discovery, the colored spectra produced by prisms of two substances, as flint-glass and crown-glass, would be of the same length when the refraction was different. But a question then occurred: When the whole distance from the red to the violet in one spectrum was the same as the whole distance in the other, were the intermediate colors, yellow, green, &c., in corresponding places in the two? This point also could not be determined any otherwise than by experiment. It appeared that such a correspondence did not exist; and, therefore, when the extreme colors were corrected by combinations of the different media, there still remained an uncorrected residue of color arising from the rest of the spectrum. This defect was a consequence of the property, that the spectra belonging to different media were not divided in the *same ratio* by the same colors, and was hence termed the *irrationality* of the spectrum. By using three prisms, or three lenses, three colors may be made to coincide instead of two, and the effects of this irrationality greatly diminished.

For the reasons already mentioned, we do not pursue this subject further,[5] but turn to those optical facts which finally led to a great and comprehensive theory.

[2nd Ed.] [Mr. Chester More Hall, of More Hall, in Essex, is said to have been led by the study of the human eye, which he conceived to be achromatic, to construct achromatic telescopes as early as 1729. Mr. Hall, however, kept his invention a secret. David Gregory, in his *Catoptrics* (1713), had suggested that it would perhaps be an improvement of telescopes, if, in imitation of the human eye, the object-glass were composed of different media. *Encyc. Brit.* art. *Optics.*

It is said that Clairaut first discovered the irrationality of the colored spaces in the spectrum. In consequence of this irrationality, it follows that when two refracting media are so combined as to correct each other's extreme dispersion, (the separation of the red and violet rays,) this first step of correction still leaves a residue of colora-

[5] The discovery of the *fixed lines* in the spectrum, by Wollaston and Fraunhofer, has more recently supplied the means of determining, with extreme accuracy, the corresponding portions of the spectrum in different refracting substances.

tion, arising from the unequal dispersion of the intermediate rays (the green, &c.). These *outstanding* colors, as they were termed by Professor Robison, form the residual, or *secondary* spectrum.

Dr. Blair, by very ingenious devices, succeeded in producing an object-glass, corrected by a fluid lens, in which this aberration of color was completely corrected, and which performed wonderfully well.

The dispersion produced by a prism may be corrected by another prism of the *same substance* and of a different angle. In this case also there is an irrationality in the colored spaces, which prevents the correction of color from being complete; and hence, a new residuary spectrum, which has been called the *tertiary* spectrum, by Sir David Brewster, who first noticed it.

I have omitted, in the notice of discoveries respecting the spectrum, many remarkable trains of experimental research, and especially the investigations respecting the power of various media to absorb the light of different parts of the spectrum, prosecuted by Sir David Brewster with extraordinary skill and sagacity. The observations are referred to in chapter iii. Sir John Herschel, Prof. Miller, Mr. Daniel, Dr. Faraday, and Mr. Talbot, have also contributed to this part of our knowledge.]

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## CHAPTER V.

### Discovery of the Laws of Double Refraction.

THE laws of refraction which we have hitherto described, were simple and uniform, and had a symmetrical reference to the surface of the refracting medium. It appeared strange to men, when their attention was drawn to a class of phenomena in which this symmetry was wanting, and in which a refraction took place which was not even in the plane of incidence. The subject was not unworthy the notice and admiration it attracted; for the prosecution of it ended in the discovery of the general laws of light. The phenomena of which I now speak, are those exhibited by various kinds of crystalline bodies; but observed for a long time in one kind only, namely, the rhombohedral calc-spar; or, as it was usually termed, from the country which supplied the largest and clearest crystals, *Iceland spar*. These rhombohedral crystals are usually very smooth and transparent, and often of considerable size; and it was observed, on looking through them, that all objects appeared double. The phenomena, even as early as 1669, had been considered so curious, that Erasmus Bartholin published a work upon them at Copenhagen,[1] (*Experimenta Crystalli Islandici*, Hafniæ, 1669.) He analysed the phenomena into their laws, so far as to discover that one of the two images was produced by refraction after the usual rule, and the other by an unusual refraction. This latter refraction Bartholin found to vary in different positions; to be regulated by a line parallel to the sides of the rhombohedron; and to be greatest in the direction of a line bisecting two of the angles of the rhombic face of the crystal.

These rules were exact as far as they went; and when we consider how geometrically complex the law is, which really regulates the unusual or extraordinary refraction;—that Newton altogether mistook it, and that it was not verified till the experiments of Haüy and Wollaston in our own time;—we might expect that it would not be soon or easily detected. But Huyghens possessed a key to the secret, in the theory, which he had devised, of the propagation of light by undulations, and which he conceived with perfect distinctness and correctness, so far as its application to these phenomena is concerned. Hence he was enabled to lay down the law of the phenomena (the only part of his discovery which we have here to consider), with a precision and success which excited deserved admiration, when the subject, at a much later period, regained its due share of attention. His Treatise was written[2] in 1678, but not published till 1690.

The laws of the *ordinary* and the *extraordinary* refraction in Iceland spar are related to each other; they are, in fact, similar constructions, made, in the one case, by means of an imaginary sphere, in the other, by means of a spheroid; the spheroid being of such oblateness as to suit the rhombohedral form of the crystal, and the axis of the spheroid being the axis of symmetry of the crystal. Huyghens followed this general conception into particular positions and conditions; and thus obtained rules, which he compared with observation, for cutting the crystal and transmitting the rays in various manners. "I have examined in detail," says he,[3] "the properties of the extraordi-

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[1] Priestley's *Optics*, p. 550. [2] See his *Traité de la Lumière*. Preface.

[3] See Maseres's *Tracts on Optics*, p. 250; or Huyghens, *Tr. sur la Lum.* ch. v Art. 43.

nary refraction of this crystal, to see if each phenomenon which is deduced from theory, would agree with what is really observed. And this being so, it is no slight proof of the truth of our suppositions and principles; but what I am going to add here confirms them still more wonderfully; that is, the different modes of cutting this crystal, in which the surfaces produced give rise to refractions exactly such as they ought to be, and as I had foreseen them, according to the preceding theory."

Statements of this kind, coming from a philosopher like Huyghens, were entitled to great confidence; Newton, however, appears not to have noticed, or to have disregarded them. In his *Opticks*, he gives a rule for the extraordinary refraction of Iceland spar which is altogether erroneous, without assigning any reason for rejecting the law published by Huyghens; and, so far as appears, without having made any experiments of his own. The Huyghenian doctrine of double refraction fell, along with his theory of undulations, into temporary neglect, of which we shall have hereafter to speak. But in 1788, Haüy showed that Huyghens's rule agreed much better than Newton's with the phenomena: and in 1802, Wollaston, applying a method of his own for measuring refraction, came to the same result. "He made," says Young,[4] "a number of accurate experiments with an apparatus singularly well calculated to examine the phenomena, but could find no general principle to connect them, until the work of Huyghens was pointed out to him." In 1808, the subject of double refraction was proposed as a prize-question by the French Institute; and Malus, whose Memoir obtained the prize, says, "I began by observing and measuring a long series of phenomena on natural and artificial faces of Iceland spar. Then, testing by means of these observations the different laws proposed up to the present time by physical writers, I was struck with the admirable agreement of the law of Huyghens with the phenomena, and I was soon convinced that it is really the law of nature." Pursuing the consequences of the law, he found that it satisfied phenomena which Huyghens himself had not observed. From this time, then, the truth of the Huyghenian law was universally allowed, and soon afterwards, the theory by which it had been suggested was generally received.

The property of double refraction had been first studied only in Iceland spar, in which it is very obvious. The same property belongs

[4] *Quart. Rev.* 1809, Nov. p. 338.

though less conspicuously, to many other kinds of crystals. Huyghens had noticed the same fact in rock-crystal;[5] and Malus found it to belong to a large list of bodies besides; for instance, arragonite, sulphate of lime, of baryta, of strontia, of iron; carbonate of lead; zircon, corundum, cymophane, emerald, euclase, felspar, mesotype, peridote, sulphur, and mellite. Attempts were made, with imperfect success, to reduce all these to the law which had been established for Iceland spar. In the first instance, Malus took for granted that the extraordinary refraction depended always upon an oblate spheroid; but M. Biot[6] pointed out a distinction between two classes of crystals in which this spheroid was oblong and oblate respectively, and these he called *attractive* and *repulsive* crystals. With this correction, the law could be extended to a considerable number of cases; but it was afterwards proved by Sir D. Brewster's discoveries, that even in this form, it belonged only to substances of which the crystallization has relation to a single axis of symmetry, as the rhombohedron, or the square pyramid. In other cases, as the rhombic prism, in which the form, considered with reference to its crystalline symmetry, is *biaxal*, the law is much more complicated. In that case, the sphere and the spheroid, which are used in the construction for uniaxal crystals, transform themselves into the two successful convolutions of a single continuous curve surface; neither of the two rays follows the law of ordinary refraction; and the formula which determines their position is very complex. It is, however, capable of being tested by measures of the refractions of crystals cut in a peculiar manner for the purpose, and this was done by MM. Fresnel and Arago. But this complex law of double refraction was only discovered through the aid of the theory of a luminiferous ether, and therefore we must now return to the other facts which led to such a theory.

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## CHAPTER VI.

### Discovery of the Laws of Polarization.

If the Extraordinary Refraction of Iceland spar had appeared strange, another phenomenon was soon noticed in the same

[5] *Traité de la Lumière*, ch. v. Art. 20. [6] Biot, *Traité de Phys.* iii. 330.

substance, which appeared stranger still, and which in the sequel was found to be no less important. I speak of the facts which were afterwards described under the term *Polarization.* Huyghens was the discoverer of this class of facts. At the end of the treatise which we have already quoted, he says,[1] "Before I quit the subject of this crystal, I will add one other marvellous phenomenon, which I have discovered since writing the above; for though hitherto I have not been able to find out its cause, I will not, on that account, omit pointing it out, that I may give occasion to others to examine it." He then states the phenomena; which are, that when two rhombohedrons of Iceland spar are in parallel positions, a ray doubly refracted by the first, is not further divided when it falls on the second: the ordinarily refracted ray is ordinarily refracted *only*, and the extraordinary ray is only extraordinarily refracted by the second crystal, neither ray being doubly refracted. The same is still the case, if the two crystals have their *principal planes* parallel, though they themselves are not parallel. But if the principal plane of the second crystal be perpendicular to that of the first, the reverse of what has been described takes place; the ordinarily refracted ray of the first crystal suffers, at the second, extraordinary refraction *only*, and the extraordinary ray of the first suffers ordinary refraction only at the second. Thus, in each of these positions, the double refraction of each ray at the second crystal is reduced to single refraction, though in a different manner in the two cases. But in any other position of the crystals, each ray, produced by the first, is doubly refracted by the second, so as to produce four rays.

A step in the right conception of these phenomena was made by Newton, in the second edition of his *Opticks* (1717). He represented them as resulting from this;—that the rays of light have "sides," and that they undergo the ordinary or extraordinary refraction, according as these sides are parallel to the principal plane of the crystal, or at right angles to it (Query 26). In this way, it is clear, that those rays which, in the first crystal, had been selected for extraordinary refraction, because their sides were perpendicular to the principal plane, would all suffer extraordinary refraction at the second crystal for the same reason, if its principal plane were parallel to that of the first; and would all suffer ordinary refraction, if the principal plane of the second crystal were perpendicular to that of the first, and con-

[1] *Tr. Opt.* p. 252.

sequently parallel to the sides of the refracted ray. This view of the subject includes some of the leading features of the case, but still leaves several considerable difficulties.

No material advance was made in the subject till it was taken up by Malus,[2] along with the other circumstances of double refraction, about a hundred years afterwards. He verified what had been observed by Huyghens and Newton, on the subject of the variations which light thus exhibits; but he discovered that this modification, in virtue of which light undergoes the ordinary, or the extraordinary, refraction, according to the position of the plane of the crystal, may be impressed upon it many other ways. One part of this discovery was made accidentally.[3] In 1808, Malus happened to be observing the light of the setting sun, reflected from the windows of the Luxembourg, through a rhombohedron of Iceland spar; and he observed that in turning round the crystal, the two images varied in their intensity. Neither of the images completely vanished, because the light from the windows was not properly modified, or, to use the term which Malus soon adopted, was not completely *polarized*. The complete polarization of light by reflection from glass, or any other transparent substance, was found to take place at a certain definite angle, different for each substance. It was found also that in all crystals in which double refraction occurred, the separation of the refracted rays was accompanied by polarization; the two rays, the ordinary and the extraordinary, being always polarized *oppositely*, that is, in planes at right angles to each other. The term *poles*, used by Malus, conveyed nearly the same notion as the term *sides* which had been employed by Newton, with the additional conception of a property which appeared or disappeared according as the *poles* of the particles were or were not in a certain direction; a property thus resembling the *polarity* of magnetic bodies. When a spot of polarized light is looked at through a transparent crystal of Iceland spar, each of the two images produced by the double refraction varies in brightness as the crystal is turned round. If, for the sake of example, we suppose the crystal to be turned round in the direction of the points of the compass, N, E, S, W, and if one image be brightest when the crystal marks N and S, it will disappear when the crystal marks E and W: and on the contrary, the second image will vanish when the crystal marks N and S,

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[2] Malus, *Th. de la Doub. Réf.* p. 296.

[3] Arago, art. *Polarization*, Supp. *Enc. Brit*

and will be brightest when the crystal marks E and W. The first of these images is polarized *in the plane* NS passing through the ray, and the second *in the plane* EW, perpendicular to the other. And these rays are *oppositely* polarized. It was further found that whether the ray were polarized by reflection from glass, or from water, or by double refraction, the modification of light so produced, or the nature of the polarization, was identical in all these cases;—that the alternatives of ordinary and extraordinary refraction and non-refraction, were the same, by whatever crystal they were tested, or in whatever manner the polarization had been impressed upon the light; in short, that the property, when once acquired, was independent of everything except the sides or *poles* of the ray; and thus, in 1811, the term "polarization" was introduced.[4]

This being the state of the subject, it became an obvious question, by what other means, and according to what laws, this property was communicated. It was found that some crystals, instead of giving, by double refraction, two images oppositely polarized, give a single polarized image. This property was discovered in the agate by Sir D. Brewster, and in tourmaline by M. Biot and Dr. Seebeck. The latter mineral became, in consequence, a very convenient part of the apparatus used in such observations. Various peculiarities bearing upon this subject, were detected by different experimenters. It was in a short time discovered, that light might be polarized by refraction, as well as by reflection, at the surface of uncrystallized bodies, as glass; the plane of polarization being perpendicular to the plane of refraction; further, that when a portion of a ray of light was polarized by reflection, a corresponding portion was polarized by transmission, the planes of the two polarizations being at right angles to each other. It was found also that the polarization which was incomplete with a single plate, either by reflection or refraction, might be made more and more complete by increasing the number of plates.

Among an accumulation of phenomena like this, it is our business to inquire what general laws were discovered. To make such discoveries without possessing the general theory of the facts, required no ordinary sagacity and good fortune. Yet several laws were detected at this stage of the subject. Malus, in 1811, obtained the important generalization that, whenever we obtain, by any means, a polarized ray of light, we produce also another ray, polarized in a contrary

[4] *Mém. Inst.* 1810.

direction; thus when reflection gives a polarized ray, the companion-ray is refracted polarized oppositely, along with a quantity of unpolarized light. And we must particularly notice *Sir D. Brewster's rule* for the *polarizing angle* of different bodies.

Malus[5] had said that the angle of reflection from transparent bodies which most completely polarizes the reflected ray, does not follow any discoverable rule with regard to the order of refractive or dispersive powers of the substances. Yet the rule was in reality very simple. In 1815, Sir D. Brewster stated[6] as the law, which in all cases determines this angle, that "the index of refraction is the tangent of the angle of polarization." It follows from this, that the polarization takes place when the reflected and refracted rays are at right angles to each other. This simple and elegant rule has been fully confirmed by all subsequent observations, as by those of MM. Biot and Seebeck; and must be considered one of the happiest and most important discoveries of the laws of phenomena in Optics.

The rule for polarization by one reflection being thus discovered, tentative formulæ were proposed by Sir D. Brewster and M. Biot, for the cases in which several reflections or refractions take place. Fresnel also in 1817 and 1818, traced the effect of reflection in modifying the direction of polarization, which Malus had done inaccurately in 1810. But the complexity of the subject made all such attempts extremely precarious, till the theory of the phenomena was understood, a period which now comes under notice. The laws which we have spoken of were important materials for the establishment of the theory; but in the mean time, its progress at first had been more forwarded by some other classes of facts, of a different kind, and of a longer standing notoriety, to which we must now turn our attention.

## CHAPTER VII.

### Discovery of the Laws of the Colours of Thin Plates.

THE facts which we have now to consider are remarkable, inasmuch as the colours are produced merely by the smallness of dimensions of the bodies employed. The light is not analysed by any peculiar

[5] *Mém. Inst.* 1810.
[6] *Phil. Trans.* 1815

property of the substances, but dissected by the minuteness of their parts. On this account, these phenomena give very important indications of the real structure of light; and at an early period, suggested views which are, in a great measure, just.

Hooke appears to be the first person who made any progress in discovering the laws of the colors of thin plates. In his *Micrographia*, printed by the Royal Society in 1664, he describes, in a detailed and systematic manner, several phenomena of this kind, which he calls "fantastical colors." He examined them in *Muscovy glass* or mica, a transparent mineral which is capable of being split into the exceedingly thin films which are requisite for such colors; he noticed them also in the fissures of the same substance, in bubbles blown of water, rosin, gum, glass; in the films on the surface of tempered steel; between two plane pieces of glass; and in other cases. He perceived also,[1] that the production of each color required a plate of determinate thickness, and he employed this circumstance as one of the grounds of his theory of light.

Newton took up the subject where Hooke had left it; and followed it out with his accustomed skill and clearness, in his *Discourse on Light and Colors*, communicated to the Royal Society in 1675. He determined, what Hooke had not ascertained, the thickness of the film which was requisite for the production of each color; and in this way explained, in a complete and admirable manner, the colored rings which occur when two lenses are pressed together, and the *scale of color* which the rings follow; a step of the more consequence, as the same scale occurs in many other optical phenomena.

It is not our business here to state the hypothesis with regard to the properties of light which Newton founded on these facts;—the "fits of easy transmission and reflection." We shall see hereafter that his attempted induction was imperfect; and his endeavor to account, by means of the laws of thin plates, for the colors of natural bodies, is altogether unsatisfactory. But notwithstanding these failures in the speculations on this subject, he did make in it some very important steps; for he clearly ascertained that when the thickness of the plate was about 1-178000th of an inch, or three times, five times, seven times that magnitude, there was a bright color produced; but blackness, when the thickness was exactly intermediate between those magnitudes. He found, also, that the thicknesses which gave red and vio-

[1] *Micrographia*, p. 53.

let[2] were as fourteen to nine; and the intermediate colors of course corresponded to intermediate thicknesses, and therefore, in his apparatus, consisting of two lenses pressed together, appeared as rings of intermediate sizes. His mode of confirming the rule, by throwing upon this apparatus differently colored homogeneous light, is striking and elegant. "It was very pleasant," he says, "to see the rings gradually swell and contract as the color of the light was changed."

It is not necessary to enter further into the detail of these phenomena, or to notice the rings seen by transmission, and other circumstances. The important step made by Newton in this matter was, the showing that the rays of light, in these experiments, as they pass onwards go periodically through certain cycles of modification, each period occupying nearly the small fraction of an inch mentioned above; and this interval being different for different colors. Although Newton did not correctly disentangle the conditions under which this periodical character is manifestly disclosed, the discovery that, under some circumstances, such a periodical character does exist, was likely to influence, and did influence, materially and beneficially, the subsequent progress of Optics towards a connected theory.

We must now trace this progress; but before we proceed to this task, we will briefly notice a number of optical phenomena which had been collected, and which waited for the touch of sound theory to introduce among them that rule and order which mere observation had sought for in vain.

## CHAPTER VIII.

### Attempts to discover the Laws of other Phenomena.

THE phenomena which result from optical combinations, even of a comparatively simple nature, are extremely complex. The theory which is now known accounts for these results with the most curious exactness, and points out the laws which pervade the apparent confusion; but without this key to the appearances, it was scarcely possible that any rule or order should be detected. The undertaking was of

[2] *Opticks*, p. 184.

the same kind as it would have been, to discover all the inequalities of the moon's motion without the aid of the doctrine of gravity. We will enumerate some of the phenomena which thus employed and perplexed the cultivators of optics.

The fringes of shadows were one of the most curious and noted of such classes of facts. These were first remarked by Grimaldi[1] (1665), and referred by him to a property of light which he called *Diffraction.* When shadows are made in a dark room, by light admitted through a very small hole, these appearances are very conspicuous and beautiful. Hooke, in 1672, communicated similar observations to the Royal Society, as "a new property of light not mentioned by any optical writer before;" by which we see that he had not heard of Grimaldi's experiments. Newton, in his *Opticks*, treats of the same phenomena, which he ascribes to the *inflexion* of the rays of light. He asks (Qu. 3), "Are not the rays of light, in passing by the edges and sides of bodies, bent several times backward and forward with a motion like that of an eel? And do not the three fringes of colored light in shadows arise from three such bendings?" It is remarkable that Newton should not have noticed, that it is impossible, in this way, to account for the facts, or even to express their laws; since the light which produces the fringes must, on this theory, be propagated, even after it leaves the neighborhood of the opake body, in curves, and not in straight lines. Accordingly, all who have taken up Newton's notion of inflexion, have inevitably failed in giving anything like an intelligible and coherent character to these phenomena. This is, for example, the case with Mr. (now Lord) Brougham's attempts in the *Philosophical Transactions* for 1796. The same may be said of other experimenters, as Mairan[2] and Du Four,[3] who attempted to explain the facts by supposing an atmosphere about the opake body. Several authors, as Maraldi,[4] and Comparetti,[5] repeated or varied these experiments in different ways.

Newton had noticed certain rings of color produced by a glass speculum, which he called "colors of thick plates," and which he attempted to connect with the colors of thin plates. His reasoning is by no means satisfactory; but it was of use, by pointing out this as a case in which his "fits" (the small periods, or cycles in the rays of light, of

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[1] *Physico-Mathesis, de Lumine, Coloribus et Iride.* Bologna, 1665.

[2] *Ac. Par.* 1738. [3] *Mémoires Présentés,* vol. v. [4] *Ac. Par.* 1723.

[5] *Observationes Opticæ de Luce Inflexâ et Coloribus.* Padua, 1787

which we have spoken) continued to occur for a considerable length of the ray. But other persons, attempting to repeat his experiments, confounded with them extraneous phenomena of other kinds; as the Duc de Chaulnes, who spread muslin before his mirror,[6] and Dr. Herschel, who scattered hair-powder before his.[7] The colors produced by the muslin were those belonging to shadows of *gratings*, afterwards examined more successfully by Fraunhofer, when in possession of the theory. We may mention here also the colors which appear on finely-striated surfaces, and on mother-of-pearl, feathers, and similar substances. These had been examined by various persons (as Boyle, Mazeas, Lord Brougham), but could still, at this period, be only looked upon as insulated and lawless facts.

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## CHAPTER IX.

### Discovery of the Laws of Phenomena of Dipolarized Light.

BESIDES the above-mentioned perplexing cases of colors produced by common light, cases of *periodical colors produced by polarized light* began to be discovered, and soon became numerous. In August, 1811, M. Arago communicated to the Institute of France an account of colors seen by passing polarized light through mica, and *analysing*[1] it with a prism of Iceland spar. It is remarkable that the light which produced the colors in this case was the light polarized by the sky, a cause of polarization not previously known. The effect which the mica thus produced was termed *depolarization;*—not a very happy term, since the effect is not the destruction of the polarization, but the combination of a new polarizing influence with the former. The word *dipolarization*, which has since been proposed, is a much more appropriate expression. Several other curious phenomena of the same kind were observed in quartz, and in flint-glass. M. Arago was not able to reduce these phenomena to laws, but he had a full conviction of their value, and ventures to class them with the great steps in

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[6] *Ac. Par.* 1755.

[7] *Phil. Trans.* 1807.

[1] The prism of Iceland spar produces the colors by separating the transmitted rays according to the laws of double refraction. Hence it is said to *analyse* the light.

this part of optics. "To Bartholin we owe the knowledge of double refraction; to Huyghens, that of the accompanying polarization; to Malus, polarization by reflection; to Arago, depolarization." Sir D. Brewster was at the same time engaged in a similar train of research; and made discoveries of the same nature, which, though not published till some time after those of Arago, were obtained without a knowledge of what had been done by him. Sir D. Brewster's *Treatise on New Philosophical Instruments*, published in 1813, contains many curious experiments on the "depolarizing" properties of minerals. Both these observers noticed the changes of color which are produced by changes in the position of the ray, and the alternations of color in the two oppositely polarized images; and Sir D. Brewster discovered that, in topaz, the phenomena had a certain reference to lines which he called the *neutral* and *depolarizing* axes. M. Biot had endeavored to reduce the phenomena to a law; and had succeeded so far, that he found that in the plates of sulphate of lime, the place of the tint, estimated in Newton's *scale* (see *ante*, chap. vii.), was as the square of the sine of the inclination. But the laws of these phenomena became much more obvious when they were observed by Sir D. Brewster with a larger field of view.[2] He found that the colors of topaz, under the circumstances now described, exhibited themselves in the form of elliptical rings, crossed by a black bar, "the most brilliant class of phenomena," as he justly says, "in the whole range of optics." In 1814, also, Wollaston observed the circular rings with a black cross, produced by similar means in calc-spar; and M. Biot, in 1815, made the same observation. The rings in several of these cases were carefully measured by M. Biot and Sir D. Brewster, and a great mass of similar phenomena was discovered. These were added to by various persons, as M. Seebeck, and Sir John Herschel.

Sir D. Brewster, in 1818, discovered a general relation between the crystalline form and the optical properties, which gave an incalculable impulse and a new clearness to these researches. He found that there was a correspondence between the degree of symmetry of the optical phenomena and the crystalline form; those crystals which are uniaxal in the crystallographical sense, are also uniaxal in their optical properties, and give circular rings; those which are of other forms are, generally speaking, biaxal; they give oval and knotted *isochromatic* lines, with two *poles*. He also discovered a rule for the tint at each point

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[2] *Phil. Trans.* 1814.

in such cases; and thus explained, so far as an empirical law of phenomena went, the curious and various forms of the colored curves. This law, when simplified by M. Biot,[3] made the tint proportional to the product of the distances of the point from the two poles. In the following year, Sir J. Herschel confirmed this law by showing, from actual measurement, that the curve of the isochromatic lines in these cases was the curve termed the *lemniscata*, which has, for each point, the product of the distances from two fixed poles equal to a constant quantity.[4] He also reduced to rule some other apparent anomalies in phenomena of the same class.

M. Biot, too, gave a rule for the directions of the planes of polarization of the two rays produced by double refraction in biaxal crystals, a circumstance which has a close bearing upon the phenomena of dipolarization. His rule was, that the one plane of polarization bisects the dihedral angle formed by the two planes which pass through the optic axes, and that the other is perpendicular to such a plane. When, however, Fresnel had discovered from the theory the true laws of double refraction, it appeared that the above rule is inaccurate, although in a degree which observation could hardly detect without the aid of theory.[5]

There were still other classes of optical phenomena which attracted notice; especially those which are exhibited by plates of quartz cut perpendicular to the axis. M. Arago had observed, in 1811, that this substance produced a *twist* of the plane of polarization to the right or left hand, the amount of this twist being different for different colors; a result which was afterwards traced to a modification of light different both from common and from polarized light, and subsequently known as *circular polarization.* Sir J. Herschel had the good fortune and sagacity to discover that this peculiar kind of polarization in quartz was connected with an equally peculiar modification of crystallization, the *plagihedral* faces which are seen, on some crystals, obliquely disposed, and, as it were, following each other round the crystal from left to right, or from right to left. Sir J. Herschel found that the *right-handed* or *left-handed* character of the circular polarization corresponded, in all cases, to that of the crystal.

In 1815, M. Biot, in his researches on the subject of circular polarization, was led to the unexpected and curious discovery, that this pro-

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[3] *Mém. Inst.* 1818, p. 192. [4] *Phil. Trans.* 1819.

[5] Fresnel. *Mém. Inst.* 1827, p. 162.

perty, which seemed to require for its very conception a crystalline structure in the body, belonged nevertheless to several fluids, and in different directions for different fluids. Oil of turpentine, and an essential oil of laurel, gave the plane of polarization a rotation to the left hand; oil of citron, syrup of sugar, and a solution of camphor gave a rotation to the right hand. Soon after, the like discovery was made independently by Dr. Seebeck, of Berlin.

It will easily be supposed that all these brilliant phenomena could not be observed, and the laws of many of the phenomena discovered, without attempts on the part of philosophers to combine them all under the dominion of some wide and profound theory. Endeavors to ascend from such knowledge as we have spoken of, to the general theory of light, were, in fact, made at every stage of the subject, and with a success which at last won almost all suffrages. We are now arrived at the point at which we are called upon to trace the history of this theory; to pass from the laws of phenomena to their causes;—from Formal to Physical Optics. The undulatory theory of light, the only discovery which can stand by the side of the theory of universal gravitation, as a doctrine belonging to the same order, for its generality, its fertility, and its certainty, may properly be treated of with that ceremony which we have hitherto bestowed only on the great advances of astronomy; and I shall therefore now proceed to speak of the Prelude to this epoch, the Epoch itself, and its Sequel, according to the form of the preceding Book which treats of astronomy.

[2nd Ed.] [I ought to have stated, in the beginning of this chapter, that Malus discovered the depolarization of *white light* in 1811. He found that a pencil of light which, being polarized, refused to be reflected by a surface properly placed, recovered its power of being reflected after being transmitted through certain crystals and other transparent bodies. Malus intended to pursue this subject, when his researches were terminated by his death, Feb. 7, 1812. M. Arago, about the same time, announced his important discovery of the depolarization of *colors* by crystals.

I may add, to what is above said of M. Biot's discoveries respecting the circular polarizing power of fluids, that he pursued his researches so as to bring into view some most curious relations among the elements of bodies. It appeared that certain substances, as sugar of canes, had a right-handed effect, and certain other substances, as gum, a left-handed effect; and that the molecular value of this effect was not altered by dilution. It appeared also that a certain element of the substance of fruits, which had been supposed to be gum, and which is changed into sugar by the operation of acids, is not gum, and has a very energetic right-handed effect. This substance M. Biot called *dextrine*, and he has since traced its effects into many highly curious and important results.

# PHYSICAL OPTICS.

## CHAPTER X.

### Prelude to the Epoch of Young and Fresnel.

BY *Physical* Optics we mean, as has already been stated, the theories which explain optical phenomena on mechanical principles. No such explanation could be given till true mechanical principles had been obtained; and, accordingly, we must date the commencement of the essays towards physical optics from Descartes, the founder of the modern mechanical philosophy. His hypothesis concerning light is, that it consists of small particles emitted by the luminous body. He compares these particles to balls, and endeavors to explain, by means of this comparison, the laws of reflection and refraction.[1] In order to account for the production of colors by refraction, he ascribes to these balls an alternating rotatory motion.[2] This form of the *emission theory*, was, like most of the physical speculations of its author, hasty and gratuitous; but was extensively accepted, like the rest of the Cartesian doctrines, in consequence of the love which men have for sweeping and simple dogmas, and deductive reasonings from them. In a short time, however, the rival optical *theory of undulations* made its appearance. Hooke in his *Micrographia* (1664) propounds it, upon occasion of his observations, already noticed, (chap. viii.,) on the colors of thin plates. He there asserts[3] light to consist in a "quick, short, vibrating motion," and that it is propagated in a homogeneous medium, in such a way that "every pulse or vibration of the luminous body will generate a sphere, which will continually increase and grow bigger, just after the same manner (though indefinitely swifter) as the waves or rings on the surface of water do swell into bigger and bigger circles about a point in it."[4] He applies this to the explanation of refraction,

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[1] *Diopt.* c. ii. 4. [2] *Meteor.* c. viii. 6. [3] *Micrographia,* p. 56
[4] *Micrographia,* p. 57.

by supposing that the rays in a denser medium move more easily, and hence that the pulses become oblique; a far less satisfactory and consistent hypothesis than that of Huyghens, of which we shall next have to speak. But Hooke has the merit of having also combined with his theory, though somewhat obscurely, the *Principle of Interferences*, in the application which he makes of it to the colors of thin plates. Thus[5] he supposes the light to be reflected at the first surface of such plates; and he adds, "after two refractions and one reflection (from the second surface) there is propagated a kind of fainter ray," which comes behind the other reflected pulse; "so that hereby (the surfaces AB and EF being so near together that the eye cannot discriminate them from one), this compound or duplicated pulse does produce on the retina the sensation of a yellow." The reason for the production of this particular color, in the case of which he here speaks, depends on his views concerning the kind of pulses appropriate to each color; and, for the same reason, when the thickness is different, he finds that the result will be a red or a green. This is a very remarkable anticipation of the explanation ultimately given of these colors; and we may observe that if Hooke could have measured the thickness of his thin plates, he could hardly have avoided making considerable progress in the doctrine of interferences.

But the person who is generally, and with justice, looked upon as the great author of the undulatory theory, at the period now under notice, is Huyghens, whose *Traité de la Lumière*, containing a developement of his theory, was written in 1678, though not published till 1690. In this work he maintained, as Hooke had done, that light consists in undulations, and expands itself spherically, nearly in the same manner as sound does; and he referred to the observations of Römer on Jupiter's satellites, both to prove that this difference takes place successively, and to show its exceeding swiftness. In order to trace the effect of an undulation, Huyghens considers that every point of a wave diffuses its motion in all directions; and hence he draws the conclusion, so long looked upon as the turning-point of the combat between the rival theories, that the light will not be *diffused* beyond the rectilinear space, when it passes through an aperture; "for," says he,[6] "although the *partial* waves, produced by the particles comprised in the aperture, do diffuse themselves beyond the rectilinear space, these waves do not *concur* anywhere except in front of the

[5] *Micrographia*, p. 66. [6] *Tracts on Optics*, p. 209

aperture." He rightly considers this observation as of the most essential value. "This," he says, "was not known by those who began to consider the waves of light, among whom are Mr. Hooke in his *Micrography*, and Father Pardies; who, in a treatise of which he showed me a part, and which he did not live to finish, had undertaken to prove. by these waves, the effects of reflection and refraction. But the principal foundation, which consists in the remark I have just made, was wanting in his demonstrations."

By the help of this view, Huyghens gave a perfectly satisfactory and correct explanation of the laws of reflection and refraction; and he also applied the same theory, as we have seen, to the double refraction of Iceland spar with great sagacity and success. He conceived that in this crystal, besides the spherical waves, there might be others of a spheroidal form, the axis of the spheroid being symmetrically disposed with regard to the faces of the rhombohedron, for to these faces the optical phenomena are symmetrically related. He found[7] that the position of the refracted ray, determined by such spheroidal undulations, would give an oblique refraction, which would coincide in its laws with the refraction observed in Iceland spar: and, as we have stated, this coincidence was long after fully confirmed by other observers.

Since Huyghens, at this early period, expounded the undulatory theory with so much distinctness, and applied it with so much skill, it may be asked why we do not hold him up as the great Author of the induction of undulations of light;—the person who marks the epoch of the theory? To this we reply, that though Huyghens discovered strong presumptions in favor of the undulatory theory, it was not *established* till a later era, when the fringes of shadows, rightly understood, made the waves visible, and when the hypothesis which had been assumed to account for double refraction, was found to contain also an explanation of polarization. It is *then* that this theory of light assumes its commanding form; and the persons who gave it this form, we must make the great names of our narrative; without, however, denying the genius and merit of Huyghens, who is, undoubtedly, the leading character in the prelude to the discovery.

The undulatory theory, from this time to our own, was unfortunate in its career. It was by no means destitute of defenders, but these were not experimenters; and none of them thought of applying it to

[7] *Tracts on Optics*, 237

Grimaldi's experiments on fringes, of which we have spoken a little while ago. And the great authority of the period, Newton, adopted the opposite hypothesis, that of emission, and gave it a currency among his followers which kept down the sounder theory for above a century.

Newton's first disposition appears to have been by no means averse to the assumption of an ether as the vehicle of luminiferous undulations. When Hooke brought against his prismatic analysis of light some objections, founded on his own hypothetical notions, Newton, in his reply, said,[8] "The hypothesis has a much greater affinity with his own hypothesis than he seems to be aware of; the vibrations of the ether being as useful and necessary in this as in his." This was in 1672; and we might produce, from Newton's writing, passages of the same kind, of a much later date. Indeed it would seem that, to the last, Newton considered the assumption of an ether as highly probable, and its vibrations important parts of the phenomena of light; but he also introduced into his system the hypothesis of emission, and having followed this hypothesis into mathematical detail, while he has left all that concerns the ether in the form of queries and conjectures, the emission theory has naturally been treated as the leading part of his optical doctrines.

The principal propositions of the *Principia* which bear upon the question of optical theory are those of the fourteenth Section of the first Book,[9] in which the law of the sines in refraction is proved on the hypothesis that the particles of bodies act on light only at very small distances; and the proposition of the eighth Section of the second Book;[10] in which it is pretended to be demonstrated that the motion propagated in a fluid must diverge when it has passed through an aperture. The former proposition shows that the law of refraction, an optical truth which mainly affected the choice of a theory, (for about reflection there is no difficulty on any mechanical hypothesis,) follows from the theory of emission: the latter proposition was intended to prove the inadmissibility of the rival hypothesis, that of undulations. As to the former point,—the hypothetical explanation of refraction, on the assumptions there made,—the conclusion is quite satisfactory; but the reasoning in the latter case, (respecting the propagation of undulations,) is certainly inconclusive and vague; and something better might the more reasonably have been expected, since Huyghens had at least

[8] *Phil. Trans.* vii. 5087. [9] *Principia*, Prop. 94, *et seq.* [10] Ib. Prop. 42.

endeavored to prove the opposite proposition. But supposing we leave these properties, the rectilinear course, the reflection, and the refraction of light, as problems in which neither theory has a decided advantage, what is the next material point? The colors of thin plates. Now, how does Newton's theory explain these? By a new and special supposition;—that of *fits of easy transmission and reflection:* a supposition which, though it truly expresses these facts, is not borne out by any other phenomena. But, passing over this, when we come to the peculiar laws of polarization in Iceland spar, how does Newton's meet this? Again by a special and new supposition;—that the rays of light have *sides.* Thus we find no fresh evidence in favor of the emission hypothesis springing out of the fresh demands made upon it. It may be urged, in reply, that the same is true of the undulatory theory; and it must be allowed that, at the time of which we now speak, its superiority in this respect was not manifested; though Hooke, as we have seen, had caught a glimpse of the explanation, which this theory supplies, of the colors of thin plates.

At a later period, Newton certainly seems to have been strongly disinclined to believe light to consist in undulations merely. "Are not," he says, in Question twenty-eight of the *Opticks*, "all hypotheses erroneous, in which light is supposed to consist in pression or motion propagated through a fluid medium?" The arguments which most weighed with him to produce this conviction, appear to have been the one already mentioned,—that, on the undulatory hypothesis, undulations passing through an aperture would be diffused; and again, —his conviction, that the properties of light, developed in various optical phenomena, "depend not upon new modifications, but upon the original and unchangeable properties of the rays." (Question twenty-seven.)

But yet, even in this state of his views, he was very far from abandoning the machinery of vibrations altogether. He is disposed to use such machinery to produce his "fits of easy transmission." In his seventeenth Query, he says,[11] "when a ray of light falls upon the surface of any pellucid body, and is there refracted or reflected; may not waves of vibrations or tremors be thereby excited in the refracting or reflecting medium at the point of incidence? . . . . and do not these vibrations overtake the rays of light, and by overtaking them successively do they not put them into the fits of easy reflection and easy

[11] *Opticks*, p. 322.

transmission described above?" Several of the other queries imply the same persuasion, of the necessity for the assumption of an ether and its vibrations. And it might have been asked, whether any good reason could be given for the hypothesis of an ether as a *part* of the mechanism of light, which would not be equally valid in favor of this being the *whole* of the mechanism, especially if it could be shown that nothing more was wanted to produce the results.

The emission theory was, however, embraced in the most strenuous manner by the disciples of Newton. That propositions existed in the *Principia* which proceeded on this hypothesis, was, with many of these persons, ground enough for adopting the doctrine; and it had also the advantage of being more ready of conception, for though the propagation of a wave is not very difficult to conceive, at least by a mathematician, the motion of a particle is still easier.

On the other hand, the undulation theory was maintained by no less a person than Euler; and the war between the two opinions was carried on with great earnestness. The arguments on one side and on the other soon became trite and familiar, for no person explained any new class of facts by either theory. Thus it was urged by Euler against the system of emission,[12]—that the perpetual emanation of light from the sun must have diminished the mass;—that the stream of matter thus constantly flowing must affect the motions of the planets and comets; that the rays must disturb each other;—that the passage of light through transparent bodies is, on this system, inconceivable: all such arguments were answered by representations of the exceeding minuteness and velocity of the matter of light. On the other hand, there was urged against the theory of waves, the favorite Newtonian argument, that on this theory the light passing through an aperture ought to be diffused, as sound is. It is curious that Euler does not make to this argument the reply which Huyghens had made before. The fact really was, that he was not aware of the true ground of the difference of the result in the cases of sound and light; namely, that any ordinary aperture bears an immense ratio to the length of an undulation of light, but does not bear a very great ratio to the length of an undulation of sound. The demonstrable consequence of this difference is, that light darts through such an orifice in straight rays, while sound is diffused in all directions. Euler, not perceiving this difference, rested his answer mainly upon a circumstance by no means

[12] Fischer, iv. 449.

unimportant, that the partitions usually employed are not impermeable to sound, as opake bodies are to light. He observes that the sound does not all come through the aperture; for we hear, though the aperture be stopped. These were the main original points of attack and defence, and they continued nearly the same for the whole of the last century; the same difficulties were over and over again proposed, and the same solutions given, much in the manner of the disputations of the schoolmen of the middle ages.

The struggle being thus apparently balanced, the scale was naturally turned by the general ascendancy of the Newtonian doctrines; and the emission theory was the one most generally adopted. It was still more firmly established, in consequence of the turn generally taken by the scientific activity of the latter half of the eighteenth century; for while nothing was added to our knowledge of optical laws, the chemical effects of light were studied to a considerable extent by various inquirers;[13] and the opinions at which these persons arrived, they found that they could express most readily, in consistency with the reigning chemical views, by assuming the materiality of light. It is, however, clear, that no reasonings of the inevitably vague and doubtful character which belong to these portions of chemistry, ought to be allowed to interfere with the steady and regular progress of induction and generalization, founded on relations of space and number, by which procedure the mechanical sciences are formed. We reject, therefore, all these chemical speculations, as belonging to other subjects; and consider the history of optical theory as a blank, till we arrive at some very different events, of which we have now to speak

[13] As Scheele, Selle, Lavoisier, De Luc, Richter, Leonhardi, Gren, Girtanner, Link, Hagen, Voigt, De la Metherie, Scherer, Dizé, Brugnatelli. See Fischer, vii. p. 20.

# CHAPTER XI.

## Epoch of Young and Fresnel.

### *Sect.* I.—*Introduction.*

THE man whose name must occupy the most distinguished place in the history of Physical Optics, in consequence of what he did in reviving and establishing the undulatory theory of light, is Dr. Thomas Young. He was born in 1773, at Milverton in Somersetshire, of Quaker parents; and after distinguishing himself during youth by the variety and accuracy of his attainments, he settled in London as a physician in 1801; but continued to give much of his attention to general science. His optical theory, for a long time, made few proselytes; and several years afterwards, Auguste Fresnel, an eminent French mathematician, an engineer officer, took up similar views, proved their truth, and traced their consequences, by a series of labors almost independent of those of Dr. Young. It was not till the theory was thus re-echoed from another land, that it was able to take any strong hold on the attention of the countrymen of its earlier promulgator.

The theory of undulations, like that of universal gravitation, may be divided into several successive steps of generalization. In both cases, all these steps were made by the same persons; but there is this difference;—all the parts of the law of universal gravitation were worked out in one burst of inspiration by its author, and published at one time;—in the doctrine of light, on the other hand, the different steps of the advance were made and published at separate times, with intervals between. We see the theory in a narrower form, and in detached portions, before the widest generalizations and principles of unity are reached; we see the authors struggling with the difficulties before we see them successful. They appear to us as men like ourselves, liable to perplexity and failure, instead of coming before us, as Newton does in the history of Physical Astronomy, as the irresistible and almost supernatural hero of a philosophical romance.

The main subdivisions of the great advance in physical optics, of which we have now to give an account, are the following:—

1. The explanation of the *periodical colors* of thin plates, thick plates, fringed shadows, striated surfaces, and other phenomena of the same kind, by means of the doctrine of the *interference* of undulations.

2. The explanation of the phenomena of *double refraction* by the propagation of undulations in a medium of which the optical *elasticity* is different in different directions.

3. The conception of *polarization* as the result of the vibrations being *transverse;* and the consequent explanation of the production of polarization, and the necessary connexion between polarization and double refraction, on mechanical principles.

4. The explanation of the phenomena of *dipolarization*, by means of the interference of the *resolved parts* of the vibrations after double refraction.

The history of each of these discoveries will be given separately to a certain extent; by which means the force of proof arising from their combination will be more apparent.

*Sect. 2.—Explanation of the Periodical Colors of Thin Plates and Shadows by the Undulatory Theory.*

The explanation of periodical colors by the principle of interference of vibrations, was the first step which Young made in his confirmation of the undulatory theory. In a paper on Sound and Light, dated Emmanuel College, Cambridge, 8th July, 1799, and read before the Royal Society in January following, he appears to incline strongly to the Huyghenian theory; not however offering any new facts or calculations in its favor, but pointing out the great difficulties of the Newtonian hypothesis. But in a paper read before the Royal Society, November 12, 1801, he says, "A further consideration of the colors of *thin plates* has converted that prepossession which I before entertained for the undulatory theory of light, into a very strong conviction of its truth and efficiency; a conviction which has since been most strikingly confirmed by an analysis of the colors of *striated surfaces*." He here states the general principle of interferences in the form of a proposition. (Prop. viii.) "When two undulations from different origins coincide either perfectly or very nearly in direction, their joint effect is a combination of the motions belonging to them." He explains, by the help of this proposition, the colors which were observed in Coventry's micrometers, in which instrument lines were drawn on glass at a distance of 1-500th of an inch. The interference of the undulations of the rays reflected from the two sides of these fine lines, produced periodical colors. In the same manner, he accounts for the colors of thin plates, by the interference of the light partially reflected from the two surfaces of the plates. We have already seen that Hooke had long before suggested the same explanation; and Young says at the end of his paper, "It was not till I had satisfied myself respecting all these phenomena, that I found in Hooke's *Micrographia* a passage which might have led me earlier to a similar opinion." He also quotes from Newton many passages which assume the existence of an ether; of which, as we have already seen, Newton suggests the necessity in these very phenomena, though he would apply it in combination with the emission of material light. In July, 1802, Young explained, on the same principle, some facts in indistinct vision, and other similar appearances. And in 1803,[1] he speaks more positively still. "In making," he says, "some experiments on the fringes of colors accompanying shadows, I have found so simple and so demonstrative a proof of the general law of interference of two portions of light, which I have already endeavored to establish, that I think it right to lay before the Royal Society a short statement of the facts which appear to me to be thus decisive." The two papers just mentioned certainly ought to have convinced all scientific men of the truth of the doctrine thus urged; for the number and exactness of the explanations is very remarkable. They include the colored fringes which are seen with the shadows of fibres; the colors produced by a dew between two pieces of glass, which, according to the theory, should appear when the thickness of the plate is *six* times that of thin plates, and which do so; the changes resulting from the employment of other fluids than water; the effect of inclining the plates; also the fringes and bands which accompany shadows, the phenomena observed by Grimaldi, Newton, Maraldi, and others, and hitherto never at all reduced to rule. Young observes, very justly, "whatever may be thought of the theory, we have got a simple and general law" of the phenomena. He moreover calculated the length of an undulation from the measurements of fringes of shadows, as he had done before from the colors of thin plates; and found a very close accordance of the results of the various cases with one another.

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[1] *Phil. Trans.* Memoir, read Nov. 24.

There is one difficulty, and one inaccuracy, in Young's views at this period, which it may be proper to note. The difficulty was, that he found it necessary to suppose that light, when reflected at a rarer medium, is retarded by half an undulation. This assumption, though often urged at a later period as an argument against the theory, was fully justified as the mechanical principles of the subject were unfolded; and the necessity of it was clear to Young from the first. On the strength of this, says he, "I ventured to predict, that if the reflections were of the same kind, made at the surfaces of a thin plate, of a density intermediate between the densities of the mediums surrounding it, the central spot would be white; and I have now the pleasure of stating, that I have fully verified this prediction by interposing a drop of oil of sassafras between a prism of flint-glass and a lens of crown-glass."

The inaccuracy of his calculations consisted in his considering the external fringe of shadows to be produced by the interference of a ray *reflected* from the *edge* of the object, with a ray which passes clear of it; instead of supposing *all the parts* of the wave of light to corroborate or interfere with one another. The mathematical treatment of the question on the latter hypothesis was by no means easy. Young was a mathematician of considerable power in the solution of the problems which came before him: though his methods possessed none of the analytical elegance which, in his time, had become general in France. But it does not appear that he ever solved the problem of undulations as applied to fringes, with its true conditions. He did, however, rectify his conceptions of the nature of the interference; and we may add, that the numerical error of the consequences of the defective hypothesis is not such as to prevent their confirming the undulatory theory.[2]

But though this theory was thus so powerfully recommended by experiment and calculation, it met with little favor in the scientific world. Perhaps this will be in some measure accounted for, when we come, in the next chapter, to speak of the mode of its reception by

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[2] I may mention, in addition to the applications which Young made of the principle of interferences, his *Eriometer*, an instrument invented for the purpose of measuring the thickness of the fibres of wood; and the explanation of the supernumerary bands of the rainbow. These explanations involve calculations founded on the length of an undulation of light, and were confirmed by experiment, as far as experiment went.

the supposed judges of science and letters. Its author went on laboring at the completion and application of the theory in other parts of the subject; but his extraordinary success in unravelling the complex phenomena of which we have been speaking, appears to have excited none of the notice and admiration which properly belonged to it, till Fresnel's Memoir *On Diffraction* was delivered to the Institute, in October, 1815.

MM. Arago and Poinsot were commissioned to make a report upon this Memoir; and the former of these philosophers threw himself upon the subject with a zeal and intelligence which peculiarly belonged to him. He verified the laws announced by Fresnel: "laws," he says, "which appear to be destined to make an epoch in science." He then cast a rapid glance at the history of the subject, and recognized, at once, the place which Young occupied in it. Grimaldi, Newton, Maraldi, he states, had observed the facts, and tried in vain to reduce them to rule or cause. "Such[3] was the state of our knowledge on this difficult question, when Dr. Thomas Young made the very remarkable experiment which is described in the *Philosophical Transactions* for 1803;" namely, that to obliterate all the bands within the shadow, we need only stop the ray which is going to graze, or has grazed, one border of the object. To this, Arago added the important observation, that the same obliteration takes place, if we stop the ray, with a transparent plate; except the plate be very thin, in which case the bands are displaced, and not extinguished. "Fresnel," says he, "guessed the effect which a thin plate would produce, when I had told him of the effect of a thick glass." Fresnel himself declares[4] that he was not, at the time, aware of Young's previous labors. After stating nearly the same reasonings concerning fringes which Young had put forward in 1801, he adds, "it is therefore the meeting, the actual crossing of the rays, which produces the fringes. This consequence, which is only, so to speak, the translation of the phenomena, seems to me entirely opposed to the hypothesis of emission, and confirms the system which makes light consist in the vibrations of a peculiar fluid." And thus the Principle of Interferences, and the theory of undulations, so far as that principle depends upon the theory, was a second time established by Fresnel in France, fourteen years after it had been discovered, fully proved, and repeatedly published by Young in England.

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*An. Chim.* 1815, Febr.

[4] Ib. tom. xvii. p. 402.

In this Memoir of Fresnel's, he takes very nearly the same course as Young had done; considering the interference of the direct light with that reflected at the edge, as the cause of the external fringes; and he observes, that in this reflection it is necessary to suppose half an undulation lost: but a few years later, he considered the propagation of undulations in a more true and general manner, and obtained the solution of this difficulty of the half-undulation. His more complete Memoir on *Diffraction* was delivered to the Institute of France, July 29, 1818; and had the prize awarded it in 1819:[5] but by the delays which at that period occurred in the publication of the *Parisian Academical Transactions*, it was not published[6] till 1826, when the theory was no longer generally doubtful or unknown in the scientific world. In this Memoir, Fresnel observes, that we must consider the effect of *every portion* of a wave of light upon a distant point, and must, on this principle, find the illumination produced by any number of such waves together. Hence, in general, the process of integration is requisite; and though the integrals which here offer themselves are of a new and difficult kind, he succeeded in making the calculation for the cases in which he experimented. His *Table of the Correspondences of Theory and Observation*,[7] is very remarkable for the closeness of the agreement; the errors being generally less than one hundredth of the whole, in the distances of the black bands. He justly adds, "A more striking agreement could not be expected between experiment and theory. If we compare the smallness of the differences with the extent of the breadths measured; and if we remark the great variations which $a$ and $b$ (the distance of the object from the luminous point and from the screen) have received in the different observations, we shall find it difficult not to regard the integral which has led us to these results as the faithful expression of the law of the phenomena."

A mathematical theory, applied, with this success, to a variety of cases of very different kinds, could not now fail to take strong hold of the attention of mathematicians; and accordingly, from this time, the undulatory doctrine of diffraction has been generally assented to, and the mathematical difficulties which it involves, have been duly studied and struggled with.

Among the remarkable applications of the undulatory doctrine to diffraction, we may notice those of Joseph Fraunhofer, a mathemati-

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[5] *Ann. Chim.* May, 1819. [6] *Mém. Inst.* for 1821–2. [7] *Mém. Inst.* p. 420–424.

cal optician of Munich. He made a great number of experiments on the shadows produced by small holes, and groups of small holes, very near each other. These were published[8] in his *New Modifications of Light*, in 1823. The greater part of this Memoir is employed in tracing the laws of phenomena of the extremely complex and splendid appearances which he obtained; but at the conclusion he observes, "It is remarkable that the laws of the reciprocal influence and of the diffraction of the rays, can be deduced from the principles of the undulatory theory: knowing the conditions, we may, by means of an extremely simple equation, determine the extent of a luminous wave for each of the different colors; and in every case, the calculation corresponds with observation." This mention of "an extremely simple equation," appears to imply that he employed only Young's and Fresnel's earlier mode of calculating interferences, by considering two portions of light, and not the method of integration. Both from the late period at which they were published, and from the absence of mathematical details, Fraunhofer's labors had not any strong influence on the establishment of the undulatory theory; although they are excellent verifications of it, both from the goodness of the observations, and the complexity and beauty of the phenomena.

We have now to consider the progress of the undulatory theory in another of its departments, according to the division already stated.

### *Sect. 3.—Explanation of Double Refraction by the Undulatory Theory.*

We have traced the history of the undulatory theory applied to diffraction, into the period when Young came to have Fresnel for his fellow-laborer. But in the mean time, Young had considered the theory in its reference to other phenomena, and especially to those of *double refraction.*

In this case, indeed, Huyghens's explanation of the facts of Iceland spar, by means of spheroidal undulations, was so complete, and had been so fully confirmed by the measurements of Haüy and Wollaston, that little remained to be done, except to connect the Huyghenian hypothesis with the mechanical views belonging to the theory, and to extend his law to other cases. The former part of this task Young executed, by remarking that we may conceive the *elasticity* of the

[8] In Schumacher's *Astronomische Abhandlungen*, in French; earlier in German

crystal, on which the velocity of propagation of the luminiferous undulation depends, to be different, in the direction of the crystallographic axis, and in the direction of the planes at right angles to this axis; and from such a difference, he deduces the existence of spheroidal undulations. This suggestion appeared in the *Quarterly Review* for November, 1809, in a critique upon an attempt of Laplace to account for the same phenomena. Laplace had proposed to reduce the double refraction of such crystals as Iceland spar, to his favorite machinery of forces which are sensible at small distances only. The peculiar forces which produce the effect in this case, he conceives to emanate from the crystallographic axis: so that the velocity of light within the crystal will depend only on the situation of the ray with respect to this axis. But the establishment of this condition is, as Young observes, the main difficulty of the problem. How are we to conceive refracting forces, independent of the surface of the refracting medium, and regulated only by a certain internal line? Moreover, the law of force which Laplace was obliged to assume, namely, that it varied as the square of the sine of the angle which the ray made with the axis, could hardly be reconciled with mechanical principles. In the critique just mentioned, Young appears to feel that the undulatory theory, and perhaps he himself, had not received justice at the hands of men of science; he complains that a person so eminent in the world of science as Laplace then was, should employ his influence in propagating error, and should disregard the extraordinary confirmations which the Huyghenian theory had recently received.

The extension of this view, of the different elasticity of crystals in different directions, to other than uniaxal crystals, was a more complex and difficult problem. The general notion was perhaps obvious, after what Young had done; but its application and verification involved mathematical calculations of great generality, and required also very exact experiments. In fact, this application was not made till Fresnel, a pupil of the Polytechnic School, brought the resources of the modern analysis to bear upon the problem;—till the phenomena of dipolarized light presented the properties of biaxal crystals in a vast variety of forms;—and till the theory received its grand impulse by the combination of the explanation of polarization with the explanation of double refraction. To the history of this last-mentioned great step we now proceed.

*Sect. 4.—Explanation of Polarization by the Undulatory Theory.*

Even while the only phenomena of *polarization* which were known were those which affect the two images in Iceland spar, the difficulty which these facts seemed at first to throw in the way of the undulatory theory was felt and acknowledged by Young. Malus's discovery of polarization by reflection increased the difficulty, and this Young did not attempt to conceal. In his review of the papers containing this discovery[9] he says, "The discovery related in these papers appears to us to be by far the most important and interesting which has been made in France concerning the properties of light, at least since the time of Huyghens; and it is so much the more deserving of notice, as it greatly influences the general balance of evidence in the comparison of the undulatory and projectile theories of the nature of light." He then proceeds to point out the main features in this comparison, claiming justly a great advantage for the theory of undulations on the two points we have been considering, the phenomena of diffraction and of double refraction. And he adds, with reference to the embarrassment introduced by polarization, that we are not to expect the course of scientific discovery to run smooth and uninterrupted; but that we are to lay our account with partial obscurity and seeming contradiction, which we may hope that time and enlarged research will dissipate. And thus he steadfastly held, with no blind prejudice, but with unshaken confidence, his great philosophical trust, the fortunes of the undulatory theory. It is here, after the difficulties of polarization had come into view, and before their solution had been discovered, that we may place the darkest time of the history of the theory; and at this period Young was alone in the field.

It does not appear that the light dawned upon him for some years. In the mean time, Young found that his theory would explain dipolarized colors; and he had the satisfaction to see Fresnel re-discover, and M. Arago adopt, his views on diffraction. He became engaged in friendly intercourse with the latter philosopher, who visited him in England in 1816. On January the 12th, 1817, in writing to this gentleman, among other remarks on the subject of optics, he says, "I have also been reflecting on the possibility of giving an imperfect explanation of the affection of light which constitutes polarization, with

[9] *Quart. Rev.* May, 1810.

out departing from the genuine doctrine of undulation." He then proceeds to suggest the possibility of "a *transverse* vibration, propagated in the direction of the radius, the motions of the particles being in a certain constant direction with respect to that radius; and this," he adds, "is *polarization*." From his further explanation of his views, it appears that he conceived the motions of the particles to be oblique to the direction of the ray, and not perpendicular, as the theory was afterwards framed; but still, here was the essential condition for the explanation of the facts of polarization,—the transverse nature of the vibrations. This idea at once made it possible to conceive how the rays of light could have *sides;* for the direction in which the vibration was transverse to the ray, might be marked by peculiar properties. And after the idea was once started, it was comparatively easy for men like Young and Fresnel to pursue and modify it till it assumed its true and distinct form.

We may judge of the difficulty of taking firmly hold of the conception of transverse vibrations of the ether, as those which constitute light, by observing how long the great philosophers of whom we are speaking lingered within reach of it, before they ventured to grasp it. Fresnel says, in 1821, "When M. Arago and I had remarked (in 1816) that two rays polarized at right angles always give the same quantity of light by their union, I thought this might be explained by supposing the vibrations to be transverse, and to be at right angles when the rays are polarized at right angles. But this supposition was so contrary to the received ideas on the nature of the vibrations of elastic fluids," that Fresnel hesitated to adopt it till he could reconcile it better to his mechanical notions. "Mr. Young, more bold in his conjectures, and less confiding in the views of geometers, published it before me, though perhaps he thought it after me." And M. Arago was afterwards wont to relate[10] that when he and Fresnel had obtained their joint experimental results of the non-interference of oppositely-polarized pencils, and when Fresnel pointed out that transverse vibrations were the only possible translation of this fact into the undulatory theory, he himself protested that he had not courage to publish such a conception; and accordingly, the second part of the Memoir was published in Fresnel's name alone. What renders this more remarkable is, that it occurred when M. Arago had in his possession the very letter of Young, in which he proposed the same suggestion.

[10] I take the liberty of stating this from personal knowledge.

Young's first published statement of the doctrine of transverse vibrations was given in the explanation of the phenomena of dipolarization, of which we shall have to speak in the next Section. But the primary and immense value of this conception, as a step in the progress of the undulatory theory, was the connexion which it established between polarization and double refraction; for it held forth a promise of accounting for polarization, if any conditions could be found which might determine what was the direction of the transverse vibrations. The analysis of these conditions is, in a great measure, the work of Fresnel; a task performed with profound philosophical sagacity and great mathematical skill.

Since the double refraction of uniaxal crystals could be explained by undulations of the form of a spheroid, it was perhaps not difficult to conjecture that the undulations of biaxal crystals would be accounted for by undulations of the form of an ellipsoid, which differs from the spheroid in having its three axes unequal, instead of two only; and consequently has that very relation to the other, in respect of symmetry, which the crystalline and optical phenomena have. Or, again, instead of supposing two different degrees of elasticity in different directions, we may suppose three such different degrees in directions at right angles to each other. This kind of generalization was tolerably obvious to a practised mathematician.

But what shall call into play all these elasticities at once, and produce waves governed by each of them? And what shall explain the different polarization of the rays which these separate waves carry with them? These were difficult questions, to the solution of which mathematical calculation had hitherto been unable to offer any aid.

It was here that the conception of transverse vibrations came in, like a beam of sunlight, to disclose the possibility of a mechanical connexion of all these facts. If transverse vibrations, travelling through a uniform medium, come to a medium not uniform, but constituted so that the elasticity shall be different in different directions, in the manner we have described, what will be the course and condition of the waves in the second medium? Will the effects of such waves agree with the phenomena of doubly-refracted light in biaxal crystals? Here was a problem, striking to the mathematician for its generality and difficulty, and of deep interest to the physical philosopher, because the fate of a great theory depended upon its solution.

The solution, obtained by great mathematical skill, was laid before the French Institute by Fresnel in November, 1821, and was carried further in two Memoirs presented in 1822. Its import is very curious The undulations which, coming from a distant centre, fall upon such a medium as we have described, are, it appears from the principles of mechanics, propagated in a manner quite different from anything which had been anticipated. The "surface of the waves" (that is, the surface which would bound undulations diverging from a point), is a very complex, yet symmetrical curve surface; which, in the case of uniaxal crystals, resolves itself into a sphere and a spheroid; but which, in general, forms a continuous double envelope of the central point to which it belongs, intersecting itself, and returning into itself. The directions of the rays are determined by this curve surface in biaxal crystals, as in uniaxal crystals they are determined by the sphere and the spheroid; and the result is, that in biaxal crystals, *both* rays suffer *extraordinary* refraction according to determinate laws. And the positions of the planes of polarization of the two rays follow from the same investigation; the plane of polarization in every case being supposed to be that which is perpendicular to the transverse vibrations. Now it appeared that the polarization of the two rays, as determined by Fresnel's theory, would be in directions, not indeed exactly accordant with the law deduced by M. Biot from experiment, but deviating so little from those directions, that there could be small doubt that the empirical formula was wrong, and the theoretical one right.

The theory was further confirmed by an experiment showing that, in a biaxal crystal (topaz), neither of the rays was refracted according to the ordinary law, though it had hitherto been supposed that one of them was so; a natural inaccuracy, since the error was small.[11] Thus this beautiful theory corrected, while it explained, the best of the observations which had previously been made; and offered itself to mathematicians with an almost irresistible power of conviction. The explanation of laws so strange and diverse as those of double refraction and polarization, by the same general and symmetrical theory, could not result from anything but the truth of the theory.

"Long," says Fresnel,[12] "before I had conceived this theory, I had convinced myself, by a pure contemplation of the facts, that it was not possible to discover the true explanation of double refraction, without explaining, at the same time, the phenomena of polarization, which always goes along with it; and accordingly, it was after having found

[11] *An. Ch.* xxviii. p. 264. [12] *Sur la Double Réf*, *Mém. Inst.* 1826, p. 174.

what mode of vibration constituted polarization, that I caught sight of the mechanical causes of double refraction."

Having thus got possession of the principle of the mechanism of polarization, Fresnel proceeded to apply it to the other cases of polarized light, with a rapidity and sagacity which reminds us of the spirit in which Newton traced out the consequences of the principle of universal gravitation. In the execution of his task, indeed, Fresnel was forced upon several precarious assumptions, which make, even yet, a wide difference between the theory of gravitation and that of light. But the mode in which these were confirmed by experiment, compels us to admire the happy apparent boldness of the calculator.

The subject of *polarization by reflection* was one of those which seemed most untractable; but, by means of various artifices and conjectures, it was broken up and subdued. Fresnel began with the simplest case, the reflection of light polarized in the plane of reflection; which he solved by means of the laws of collision of elastic bodies. He then took the reflection of light polarized perpendicularly to this plane; and here, adding to the general mechanical principles a hypothetical assumption, that the communication of the resolved motion parallel to the refracting surface, takes place according to the laws of elastic bodies, he obtains his formula. These results were capable of comparison with experiment; and the comparison, when made by M. Arago, confirmed the formulæ. They accounted, too, for Sir D. Brewster's law concerning the polarizing angle (see Chap. vi.); and this could not but be looked upon as a striking evidence of their having some real foundation. Another artifice which MM. Fresnel and Arago employed, in order to trace the effect of reflection upon common light, was to use a ray polarized in a plane making half a right angle with the plane of reflection; for the quantities of the oppositely[13] polarized light in such an incident ray are equal, as they are in common light; but the relative quantities of the oppositely polarized light in the reflected ray are indicated by the new plane of polarization; and thus these relative quantities become known for the case of common light. The results thus obtained were also confirmed by facts; and in this manner, all that was doubtful in the process of Fresnel's reasoning, seemed to be authorized by its application to real cases.

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[13] It will be recollected all along, that *oppositely* polarized rays are those which are polarized in two planes *perpendicular* to each other. See above, chap. vi.

These investigations were published[14] in 1821. In succeeding years, Fresnel undertook to extend the application of his formulæ to a case in which they ceased to have a meaning, or, in the language of mathematicians, became *imaginary;* namely, to the case of internal reflection at the surface of a transparent body. It may seem strange to those who are not mathematicians, but it is undoubtedly true, that in many cases in which the solution of a problem directs impossible arithmetical or algebraical operations to be performed, these directions may be so interpreted as to point out a true solution of the question. Such an interpretation Fresnel attempted[15] in the case of which we now speak; and the result at which he arrived was, that the reflection of light through a rhomb of glass of a certain form (since called *Fresnel's rhomb*), would produce a polarization of a kind altogether different from those which his theory had previously considered, namely, that kind which we have spoken of as *circular polarization.* The complete confirmation of this curious and unexpected result by trial, is another of the extraordinary triumphs which have distinguished the history of the theory at every step since the commencement of Fresnel's labors.

But anything further which has been done in this way, may be treated of more properly in relating the verification of the theory. And we have still to speak of the most numerous and varied class of facts to which rival theories of light were applied, and of the establishment of the undulatory doctrine in reference to that department; I mean the phenomena of depolarized, or rather, as I have already said, *di*polarized light.

### *Sect. 5.—Explanation of Dipolarization by the Undulatory Theory.*

When Arago, in 1811, had discovered the colors produced by polarized light passing through certain crystals,[16] it was natural that attempts should be made to reduce them to theory. M. Biot, animated by the success of Malus in detecting the laws of double refraction, and Young, knowing the resources of his own theory, were the first persons to enter upon this undertaking. M. Biot's theory, though in the end displaced by its rival, is well worth notice in the history of the subject. It was what he called the doctrine of *moveable polarization.* He conceived that when the molecules of light pass through

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[14] *An. Chim.* t. xvii. [15] *Bullet. des Sc.* Feb. 1823. [16] See chap. ix.

thin crystalline plates, the plane of polarization undergoes an oscillation which carries it backwards and forwards through a certain angle, namely, twice the angle contained between the original plane of polarization and the principal section of the crystal. The intervals which this oscillation occupies are lengths of the path of the ray, very minute, and different for different colors, like Newton's fits of easy transmission; on which model, indeed, the new theory was evidently framed.[17] The colors produced in the phenomena of dipolarization really do depend, in a periodical manner, on the length of the path of the light through the crystal, and a theory such as M. Biot's was capable of being modified, and was modified, so as to include the leading features of the facts as then known; but many of its conditions being founded on special circumstances in the experiments, and not on the real conditions of nature, there were in it several incongruities, as well as the general defect of its being an arbitrary and unconnected hypothesis.

Young's mode of accounting for the brilliant phenomena of dipolarization appeared in the *Quarterly Review* for 1814. After noticing the discoveries of MM. Arago, Brewster, and Biot, he adds, "We have no doubt that the surprise of these gentlemen will be as great as our own satisfaction in finding that they are perfectly reducible, like other causes of recurrent colors, to the general laws of the interference of light which have been established in this country;" giving a reference to his former statements. The results are then explained by the interference of the ordinary and extraordinary ray. But, as M. Arago properly observes, in his account of this matter,[18] "It must, however, be added that Dr. Young had not explained either in what circumstances the interference of the rays can take place, nor why we see no colors unless the crystallized plates are exposed to light previously polarized." The explanation of these circumstances depends on the laws of interference of polarized light which MM. Arago and Fresnel established in 1816. They then proved, by direct experiment, that when polarized light was treated so as to bring into view the most marked phenomena of interference, namely, the bands of shadows; pencils of light which have a common origin, and which are polarized in the parallel planes, interfere completely, while those which are

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[17] See MM. Arago and Biot's Memoirs, *Mém. Inst.* for 1811; the whole volume for 1812 is a Memoir of M. Biot's (published 1814); also *Mém. Inst.* for 1817; M. Biot's Mem. read in 1818, published in 1819 and for 1818.

[18] *Enc. Brit.* Supp. art. *Polarization.*

polarized in *opposite* (that is, perpendicular,) planes do not interfere at all.[19] Taking these principles into the account, Fresnel explained very completely, by means of the interference of undulations, all the circumstances of colors produced by crystallized plates; showing the necessity of the *polarization* in the first instance; the *dipolarizing* effect of the crystal; and the office of the *analysing plate*, by which certain portions of each of the two rays in the crystal are made to interfere and produce color. This he did, as he says,[20] without being aware, till Arago told him, that Young had, to some extent, anticipated him.

When we look at the history of the emission-theory of light, we see exactly what we may consider as the natural course of things in the career of a false theory. Such a theory may, to a certain extent, explain the phenomena which it was at first contrived to meet; but every new class of facts requires a new supposition,—an addition to the machinery; and as observation goes on, these incoherent appendages accumulate, till they overwhelm and upset the original frame work. Such was the history of the hypothesis of solid epicycles; such has been the history of the hypothesis of the material emission of light. In its simple form, it explained reflection and refraction; but the colors of thin plates added to it the hypothesis of fits of easy transmission and reflection; the phenomena of diffraction further invested the particles with complex hypothetical laws of attraction and repulsion; polarization gave them sides; double refraction subjected them to peculiar forces emanating from the axes of crystals; finally, dipolarization loaded them with the complex and unconnected contrivance of moveable polarization; and even when all this had been assumed, additional mechanism was wanting. There is here no unexpected success, no happy coincidence, no convergence of principles from remote quarters; the philosopher builds the machine, but its parts do not fit; they hold together only while he presses them: this is not the character of truth.

In the undulatory theory, on the other hand, all tends to unity and simplicity. We explain reflection and refraction by undulations; when we come to thin plates, the requisite "fits" are already involved in our fundamental hypothesis, for they are the length of an undulation; the phenomena of diffraction also require such intervals; and the intervals thus required agree exactly with the others in magnitude,

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[19] *Ann. Chim.* tom. x.

[20] Ib. tom. xvii. p. 402.

so that no new property is needed. Polarization for a moment checks us; but not long; for the direction of our vibrations is hitherto arbitrary;—we allow polarization to decide it. Having done this for the sake of polarization, we find that it also answers an entirely different purpose, that of giving the law of double refraction. Truth may give rise to such a coincidence; falsehood cannot. But the phenomena become more numerous, more various, more strange; no matter: the Theory is equal to them all. It makes not a single new physical hypothesis; but out of its original stock of principles it educes the counterpart of all that observation shows. It accounts for, explains, simplifies, the most entangled cases; corrects known laws and facts; predicts and discloses unknown ones; becomes the guide of its former teacher, Observation; and, enlightened by mechanical conceptions, acquires an insight which pierces through shape and color to force and cause.

We thus reach the philosophical *moral* of this history, so important in reference to our purpose; and here we shall close the account of the discovery and promulgation of the undulatory theory. Any further steps in its development and extension, may with propriety be noticed in the ensuing chapters, respecting its reception and verification.

[2nd Ed.] [In the *Philosophy of the Inductive Sciences*, B. xi. ch. iii. Sect. 11, I have spoken of the *Consilience of Inductions* as one of the characters of scientific truth. We have several striking instances of such consilience in the history of the undulatory theory. The phenomena of fringes of shadows and colored bands in crystals *jump together* in the Theory of Vibrations. The phenomena of polarization and double refraction *jump together* in the Theory of Crystalline Vibrations. The phenomena of polarization and of the interference of polarized rays *jump together* in the Theory of Transverse Vibrations.

The proof of what is above said of the undulatory theory is contained in the previous history. This theory has "accounted for, explained, and simplified the most entangled cases;" as the cases of fringes of shadows; shadows of gratings; colored bands in biaxal crystals, and in quartz. There are no optical phenomena more entangled than these. It has "corrected experimental laws," as in the case of M. Biot's law of the direction of polarization in biaxal crystals. It has done this, "without making any new physical hypothesis;" for the transverse direction of vibrations, the different optical elasticities of crystals in different directions, and (if it be adopted) the hypothesis of finite intervals of the particles (see chap. x. and hereafter, chap. xiii.), are only limitations of what was indefinite in the earlier form of the hypothesis. And so far as the properties of visible radiant light are concerned, I do not think it at all too much to say, as M. Schwerd has said, that "the undulation theory accounts for the phenomena as completely as the theory of gravitation does for the facts of the solar system."

This we might say, even if some facts were not yet fully explained; for there were till very lately, if there are not still, such unexplained facts with regard to the theory of gravitation, presented to us by the solar system. With regard to the undulatory theory, these exceptions are, I think, disappearing quite as rapidly and as completely as in the case of gravitation. It is to be observed that no presumption against the theory can with any show of reason be collected from the cases in which classes of phenomena remain unexplained, the theory having never been applied to them by any mathematician capable of tracing its results correctly. The history of the theory of gravitation may show us abundantly how necessary it is to bear in mind this caution; and the results of the undulatory theory cannot be traced without great mathematical skill and great labor, any more than those of gravitation.

This remark applies to such cases as that of the *transverse fringes of grooved surfaces.* The general phenomena of these cases are perfectly explained by the theory. But there is an interruption in the light in an oblique direction, which has not yet been explained; but looking at what has been done in other cases, it is impossible to doubt that this phenomenon depends upon the results of certain integrations, and would be explained if these were rightly performed.

The phenomena of *crystallized surfaces*, and especially their effects upon the plane of polarization, were examined by Sir D. Brewster, and laws of the phenomena made out by him with his usual skill and sagacity. For a time these were unexplained by the theory. But recently Mr. Mac Cullagh has traced the consequences of the theory in this case,[21] and obtained a law which represents with much exactness, Sir D. Brewster's observation.

The phenomena which Sir D. Brewster, in 1837, called a *new property of light*, (certain appearances of the spectrum when the pupil of the eye is half covered with a thin glass or crystal,) have been explained by Mr. Airy in the *Phil. Trans.* for 1840.

Mr. Airy's explanation of the phenomena termed by Sir D. Brew

[21] Prof. Lloyd's *Report, Brit. Assoc.* 1834, p. 374.

ster a *new property of light*, is completed in the *Philosophical Magazine* for November, 1846. It is there shown that a dependence of the breadth of the bands upon the aperture of the pupil, which had been supposed to result from the theory, and which does not appear in the experiment, did really result from certain limited conditions of the hypothesis, which conditions do not belong to the experiment; and that when the problem is solved without those limitations, the discrepance of theory and observation vanishes; so that, as Mr. Airy says, "this very remarkable experiment, which long appeared inexplicable, seems destined to give one of the strongest confirmations to the Undulatory Theory."

I may remark also that there is no force in the objection which has been urged against the admirers of the undulatory theory, that by the fulness of their assent to it, they discourage further researches which may contradict or confirm it. We must, in this point of view also, look at the course of the theory of gravitation and its results. The acceptance of that theory did not prevent mathematicians and observers from attending to the apparent exceptions, but on the contrary, stimulated them to calculate and to observe with additional zeal, and still does so. The acceleration of the Moon, the mutual disturbances of Jupiter and Saturn, the motions of Jupiter's Satellites, the effect of the Earth's oblateness on the Moon's motion, the motions of the Moon about her own centre, and many other phenomena, were studied with the greater attention, *because* the general theory was deemed so convincing: and the same cause makes the remaining exceptions objects of intense interest to astronomers and mathematicians. The mathematicians and optical experimenters who accept the undulatory theory, will of course follow out their conviction in the same manner. Accordingly, this has been done and is still doing, as in Mr. Airy's mathematical investigation of the effect of an annular aperture; Mr. Earnshaw's, of the effect of a triangular aperture; Mr. Talbot's explanation of the effect of interposing a film of mica between a part of the pupil and the pure spectrum, so nearly approaching to the phenomena which have been spoken of as a new Polarity of Light; besides other labors of eminent mathematicians, elsewhere mentioned in these pages.

The phenomena of the *absorption* of light have no especial bearing upon the undulatory theory. There is not much difficulty in explaining the *possibility* of absorption upon the theory. When the light is absorbed, it ceases to belong to the theory.

For, as I have said, the theory professes only to explain the phenomena of *radiant visible* light. We know very well that light has other bearings and properties. It produces chemical effects. The optical polarity of crystals is connected with the chemical polarity of their constitution. The natural colors of bodies, too, are connected with their chemical constitution. Light is also connected with heat. The undulatory theory does not undertake to explain these properties and their connexion. If it did, it would be a Theory of Heat and of Chemical Composition, as well as a Theory of Light.

Dr. Faraday's recent experiments have shown that the magnetic polarity is directly connected with that optical polarity by which the plane of polarization is affected. When the lines of magnetic force pass through certain transparent bodies, they communicate to them a certain kind of circular polarizing power; yet different from the circular polarizing power of quartz, and certain fluids mentioned in chapter ix.

Perhaps I may be allowed to refer to this discovery as a further illustration of the views I have offered in the *Philosophy of the Inductive Sciences* respecting the *Connexion of Co-existent Polarities.* (B. v. Chap. ii.)]

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## CHAPTER XII.

### SEQUEL TO THE EPOCH OF YOUNG AND FRESNEL. RECEPTION OF THE UNDULATORY THEORY.

WHEN Young, in 1800, published his assertion of the Principle of Interferences, as the true theory of optical phenomena, the condition of England was not very favorable to a fair appreciation of the value of the new opinion. The men of science were strongly pre-occupied in favor of the doctrine of emission, not only from a national interest in Newton's glory, and a natural reverence for his authority, but also from deference towards the geometers of France, who were looked up to as our masters in the application of mathematics to physics, and who were understood to be Newtonians in this as in other subjects. A general tendency to an atomic philosophy, which had begun to appear from the time of Newton, operated powerfully; and the hypothesis of emission was so easily conceived, that, when recommended by high authority, it easily became popular; while the hypothesis of luminiferous undulations, unavoidably difficult to comprehend, even by the aid of steady thought, was neglected, and all but forgotten.

Yet the reception which Young's opinions met with was more harsh than he might have expected, even taking into account all these considerations. But there was in England no visible body of men, fitted by their knowledge and character to pronounce judgment on such a question, or to give the proper impulse and bias to public opinion. The Royal Society, for instance, had not, for a long time, by custom or institution, possessed or aimed at such functions. The writers of "Reviews" alone, self-constituted and secret tribunals, claimed this kind of authority. Among these publications, by far the most distinguished about this period was the *Edinburgh Review;* and, including among its contributors men of eminent science and great talents, employing also a robust and poignant style of writing (often certainly in a very unfair manner), it naturally exercised great influence. On abstruse doctrines, intelligible to few persons, more than on other subjects, the opinions and feelings expressed in a Review must be those of the individual reviewer. The criticism on some of Young's early papers on optics was written by Mr. (afterwards Lord) Brougham, who, as we have seen, had experimented on diffraction, following the Newtonian view, that of inflexion. Mr. Brougham was perhaps at this time young enough[1] to be somewhat intoxicated with the appearance of judicial authority in matters of science, which his office of anonymous reviewer gave him · and even in middle-life, he was sometimes considered to be prone to indulge himself in severe and sarcastic expressions. In January, 1803, was published[2] his critique on Dr. Young's Bakerian Lecture, *On the Theory of Light and Colors*, in which lecture the doctrine of undulations and the law of interferences was maintained. This critique was an uninterrupted strain of blame and rebuke. "This paper," the reviewer said, "contains nothing which deserves the name either of experiment or discovery." He charged the writer with "dangerous relaxations of the principles of physical logic." "We wish," he cried, "to recall philosophers to the strict and severe methods of investigation," describing them as those pointed out by Bacon, Newton, and the like. Finally, Dr. Young's speculations

[1] His age was twenty-four.

[2] *Edin. Review*, vol. i. p. 450.

were spoken of as a hypothesis, which is a mere work of fancy; and the critic added, "we cannot conclude our review without entreating the attention of the Royal Society, which has admitted of late so many hasty and unsubstantial papers into its *Transactions*;" which habit he urged them to reform. The same aversion to the undulatory theory appears soon after in another article by the same reviewer, on the subject of Wollaston's measures of the refraction of Iceland spar; he says. "We are much disappointed to find that so acute and ingenious an experimentalist should have adopted the wild optical theory of vibrations." The reviewer showed ignorance as well as prejudice in the course of his remarks; and Young drew up an answer, which was ably written, but being published separately had little circulation. We can hardly doubt that these Edinburgh reviews had their effect in confirming the general disposition to reject the undulatory theory.

We may add, however, that Young's mode of presenting his opinions was not the most likely to win them favor; for his mathematical reasonings placed them out of the reach of popular readers, while the want of symmetry and system in his symbolical calculations, deprived them of attractiveness for the mathematician. He himself gave a very just criticism of his own style of writing, in speaking on another of his works:[3] "The mathematical reasoning, for want of mathematical symbols, was not understood, even by tolerable mathematicians. From a dislike of the affectation of algebraical formality which he had observed in some foreign authors, he was led into something like an affectation of simplicity, which was equally inconvenient to a scientific reader."

Young appears to have been aware of his own deficiency in the power of drawing public favor, or even notice, to his discoveries. In 1802, Davy writes to a friend, "Have you seen the theory of my colleague, Dr. Young, on the undulations of an ethereal medium as the cause of light? It is not likely to be a popular hypothesis, after what has been said by Newton concerning it. He would be very much flattered if you could offer any observations upon it, *whether for or against it*." Young naturally felt confident in his power of refuting objections, and wanted only the opportunity of a public combat.

Dr. Brewster, who was, at this period, enriching optical knowledge with so vast a train of new phenomena and laws, shared the general aversion to the undulatory theory, which, indeed, he hardly overcame

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[3] See *Life of Young*, p. 54.

thirty years later. Dr. Wollaston was a person whose character led him to look long at the laws of phenomena, before he attempted to determine their causes; and it does not appear that he had decided the claims of the rival theories in his own mind. Herschel (I now speak of the son) had at first the general mathematical prejudice in favor of the emission doctrine. Even when he had himself studied and extended the laws of dipolarized phenomena, he translated them into the language of the theory of moveable polarization. In 1819, he refers to, and corrects, this theory; and says, it is now "relieved from every difficulty, and entitled to rank with the fits of easy transmission and reflection as a general and simple physical law;" a just judgment, but one which now conveys less of praise than he then intended. At a later period, he remarked that we cannot be certain that if the theory of emission had been as much cultivated as that of undulation, it might not have been as successful; an opinion which was certainly untenable after the fair trial of the two theories in the case of diffraction, and extravagant after Fresnel's beautiful explanation of double refraction and polarization. Even in 1827, in a *Treatise on Light*, published in the *Encyclopædia Metropolitana*, he gives a section to the calculations of the Newtonian theory; and appears to consider the rivalry of the theories as still subsisting. But yet he there speaks with a proper appreciation of the advantages of the new doctrine. After tracing the prelude to it, he says, "But the unpursued speculations of Newton, and the opinions of Hooke, however distinct, must not be put in competition, and, indeed, ought scarcely to be mentioned, with the elegant, simple, and comprehensive theory of Young,—a theory which, if not founded in nature, is certainly one of the happiest fictions that the genius of man ever invented to grasp together natural phenomena, which, at their first discovery, seemed in irreconcileable opposition to it. It is, in fact, in all its applications and details, one succession of *felicities;* insomuch, that we may almost be induced to say, if it be not true, it deserves to be so."

In France, Young's theory was little noticed or known, except perhaps by M. Arago, till it was revived by Fresnel. And though Fresnel's assertion of the undulatory theory was not so rudely received as Young's had been, it met with no small opposition from the older mathematicians, and made its way slowly to the notice and comprehension of men of science. M. Arago would perhaps have at once adopted the conception of transverse vibrations, when it was suggested by his fellow-laborer, Fresnel, if it had not been that he was a member of the Institute, and had to bear the brunt of the war, in the frequent discussions on the undulatory theory; to which theory Laplace, and other leading members, were so vehemently opposed, that they would not even listen with toleration to the arguments in its favor. I do not know how far influences of this kind might operate in producing the delays which took place in the publication of Fresnel's papers. We have seen that he arrived at the conception of transverse vibrations in 1816, as the true key to the understanding of polarization. In 1817 and 1818, in a memoir read to the Institute, he analysed and explained the perplexing phenomena of quartz, which he ascribed to a *circular polarization*. This memoir had not been printed, nor any extract from it inserted in the scientific journals, in 1822, when he confirmed his views by further experiments.[4] His remarkable memoir, which solved the extraordinary and capital problem of the connexion of double refraction and crystallization, though written in 1821, was not published till 1827. He appears by this time to have sought other channels of publication. In 1822, he gave,[5] in the *Annales de Chimie et de Physique*, an explanation of refraction on the principles of the undulatory theory; alleging, as the reason for doing so, that the theory was still little known. And in succeeding years there appeared in the same work, his theory of reflection. His memoir on this subject (*Mémoire sur la Loi des Modifications que la Réflexion imprime à la Lumière Polarisée,*) was read to the Academy of Sciences in 1853. But the original paper was mislaid, and, for a time, supposed to be lost; it has since been recovered among the papers of M. Fourier, and printed in the eleventh volume of the Memoirs of the Academy.[6] Some of the speculations to which he refers, as communicated to the Academy, have never yet appeared.[7]

Still Fresnel's labors were, from the first, duly appreciated by some of the most eminent of his countrymen. His *Memoir on Diffraction* was, as we have seen, crowned in 1819: and, in 1822, a Report upon his *Memoir on Double Refraction* was drawn up by a commission consisting of MM. Ampère, Fourier, and Arago. In this report[8] Fresnel's theory is spoken of as confirmed by the most delicate tests. The reporters add, respecting his "theoretical ideas on the particular kind of undulations which, according to him, constitute light," that "it would be impossible for them to pronounce at present a decided judg-

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[4] Hersch. *Light*, p. 539. [5] *Ann. de Chim.* 1822, tom. xxi. p. 235.

[6] Lloyd. *Report on Optics*, p. 363. (Fourth Rep. of Brit. Ass.)

[7] Ib. p. 316, *note*. [8] *Ann. Chim.* tom. xx. p. 343.

ment," but that "they have not thought it right to delay any longer making known a work of which the difficulty is attested by the fruitless efforts of the most skilful philosophers, and in which are exhibited in the same brilliant degree, the talent for experiment and the spirit of invention."

In the meantime, however, a controversy between the theory of undulations and the theory of moveable polarization which M. Biot had proposed with a view of accounting for the colors produced by dipolarizing crystals, had occurred among the French men of science. It is clear that in some main features the two theories coincide; the intervals of interference in the one theory being represented by the intervals of the oscillations in the other. But these intervals in M. Biot's explanations were arbitrary hypotheses, suggested by these very facts themselves; in Fresnel's theory, they were essential parts of the general scheme. M. Biot, indeed, does not appear to have been averse from a coalition; for he allowed[9] to Fresnel that "the theory of undulations took the phenomena at a higher point and carried them further." And M. Biot could hardly have dissented from M. Arago's account of the matter, that Fresnel's views "*linked together*"[10] the oscillations of moveable polarization. But Fresnel, whose hypothesis was all of one piece, could give up no part of it, although he allowed the usefulness of M. Biot's formulæ. Yet M. Biot's speculations fell in better with the views of the leading mathematicians of Paris. We may consider as evidence of the favor with which they were looked upon, the large space they occupy in the volumes of the Academy for 1811, 1812, 1817, and 1818. In 1812, the entire volume is filled with a memoir of M. Biot's on the subject of moveable polarization. This doctrine also had some advantage in coming early before the world in a didactic form, in his *Traité de Physique*, which was published in 1816, and was the most complete treatise on general physics which had appeared up to that time. In this and others of this author's writings, he expresses facts so entirely in the terms of his own hypothesis, that it is difficult to separate the two. In the sequel M. Arago was the most prominent of M. Biot's opponents; and in his report upon Fresnel's memoir on the colors of crystalline plates, he exposed the weaknesses of the theory of moveable polarization with some severity. The details of this controversy need not occupy us; but we may observe that this may be considered as the last struggle

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[9] *Ann. Chim.* tom. xvii. p. 251.

[10] "Nouait."

in favor of the theory of emission among mathematicians of eminence. After this crisis of the war, the theory of moveable polarization lost its ground; and the explanations of the undulatory theory, and the calculations belonging to it, being published in the *Annales de Chimie et de Physique*, of which M. Arago was one of the conductors, soon diffused it over Europe.

It was probably in consequence of the delays to which we have referred, in the publication of Fresnel's memoirs, that as late as December, 1826, the Imperial Academy at St. Petersburg proposed, as one of their prize-questions for the two following years, this,—"To deliver the optical system of waves from all the objections which have (as it appears) with justice been urged against it, and to apply it to the polarization and double refraction of light." In the programme to this announcement, Fresnel's researches on the subject are not alluded to, though his memoir on diffraction is noticed; they were, therefore, probably not known to the Russian Academy.

Young was always looked upon as a person of marvellous variety of attainments and extent of knowledge; but during his life he hardly held that elevated place among great discoverers which posterity will probably assign him. In 1802, he was constituted Foreign Secretary of the Royal Society, an office which he held during life; in 1827 he was elected one of the eight Foreign Members of the Institute of France; perhaps the greatest honor which men of science usually receive. The fortune of his life in some other respects was of a mingled complexion. His profession of a physician occupied, sufficiently to fetter, without rewarding him; while he was Lecturer at the Royal Institution, he was, in his lectures, too profound to be popular; and his office of Superintendent of the *Nautical Almanac* subjected him to much minute labor, and many petulant attacks of pamphleteers. On the other hand, he had a leading part in the discovery of the long sought key to the Egyptian hieroglyphics; and thus the age which was marked by two great discoveries, one in science and one in literature, owed them both in a great measure to him. Dr. Young died in 1829, when he had scarcely completed his fifty-sixth year. Fresnel was snatched from science still more prematurely, dying, in 1827, at the early age of thirty-nine.

We need not say that both these great philosophers possessed, in an eminent degree, the leading characteristics of the discoverer's mind, perfect clearness of view, rich fertility of invention, and intense love of knowledge. We cannot read without great interest a letter of

Fresnel to Young,[11] in November, 1824: "For a long time that sensibility, or that vanity, which people call love of glory, is much blunted in me. I labor much less to catch the suffrages of the public, than to obtain an inward approval which has always been the sweetest reward of my efforts. Without doubt I have often wanted the spur of vanity to excite me to pursue my researches in moments of disgust and discouragement. But all the compliments which I have received from MM. Arago, De Laplace, or Biot, never gave me so much pleasure as the discovery of a theoretical truth, or the confirmation of a calculation by experiment."

Though Young and Fresnel were in years the contemporaries of many who are now alive, we must consider ourselves as standing towards them in the relation of posterity. The Epoch of Induction in Optics is past; we have now to trace the Verification and Application of the true theory.

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## CHAPTER XIII.

### Confirmation and Extension of the Undulatory Theory.

AFTER the undulatory theory had been developed in all its main features, by its great authors, Young and Fresnel, although it bore marks of truth that could hardly be fallacious, there was still here, as in the case of other great theories, a period in which difficulties were to be removed, objections answered, men's minds familiarized to the new conceptions thus presented to them; and in which, also, it might reasonably be expected that the theory would be extended to facts not at first included in its domain. This period is, indeed, that in which we are living; and we might, perhaps with propriety, avoid the task of speaking of our living contemporaries. But it would be unjust to the theory not to notice some of the remarkable events, characteristic of such a period, which have already occurred; and this may be done very simply.

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I was able to give this, and some other extracts, from the then unedited correspondence of Young and Fresnel, by the kindness of (the Dean of Ely) Professor Peacock, of Trinity College, Cambridge, whose Life of Dr. Young has since been published.

In the case of this great theory, as in that of gravitation, by far the most remarkable of these confirmatory researches were conducted by the authors of the discovery, especially Fresnel. And in looking at what he conceived and executed for this purpose, we are, it appears to me, strongly reminded of Newton, by the wonderful inventiveness and sagacity with which he devised experiments, and applied to them mathematical reasonings.

1. *Double Refraction of Compressed Glass.*—One of these confirmatory experiments was the production of double refraction by the *compression* of glass. Fresnel observes,[1] that though Sir D. Brewster had shown that glass under compression produced colors resembling those which are given by doubly-refracting crystals, "very skilful physicists had not considered those experiments as a sufficient proof of the bifurcation of the light." In the hypothesis of moveable polarization, it is added, there is no apparent connexion between these phenomena of coloration and double refraction; but on Young's theory, that the colors arise from two rays which have traversed the crystal with different velocities, it appears almost unavoidable to admit also a difference of path in the two rays.

"Though," he says, "I had long since adopted this opinion, it did not appear to me so completely demonstrated, that it was right to neglect an experimental verification of it;" and therefore, in 1819, he proceeded to satisfy himself of the fact, by the phenomena of diffraction. The trial left no doubt on the subject; but he still thought it would be interesting actually to produce two images in glass by compression; and by a highly-ingenious combination, calculated to exaggerate the effect of the double refraction, which is very feeble, even when the compression is most intense, he obtained two distinct images. This evidence of the dependence of dipolarizing structure upon a doubly-refracting state of particles, thus excogitated out of the general theory, and verified by trial, may well be considered, as he says, "as a new occasion of proving the infallibility of the principle of interferences."

2. *Circular Polarization.*—Fresnel then turned his attention to another set of experiments, related to this indeed, but by a tie so recondite, that nothing less than his clearness and acuteness of view could have detected any connexion. The optical properties of quartz had been perceived to be peculiar, from the period of the discovery

[1] *Ann. de Chim.* 1822, tom. xx. p. 377.

of dipolarized colors by MM. Arago and Biot. At the end of the Notice just quoted, Fresnel says,[2] "As soon as my occupations permit me, I propose to employ a pile of prisms similar to that which I have described, in order to study the double refraction of the rays which traverse crystals of quartz in the direction of the axis." He then ventures, without hesitation, to describe beforehand what the phenomena will be. In the *Bulletin des Sciences*[3] for December, 1822, it is stated that experiment had confirmed what he had thus announced.

The phenomena are those which have since been spoken of as *circular polarization;* and the term first occurs in this notice.[4] They are very remarkable, both by their resemblances to, and their differences from, the phenomena of *plane-polarized* light. And the manner in which Fresnel was led to this anticipation of the facts is still more remarkable than the facts themselves. Having ascertained by observation that two differently-polarized rays, totally reflected at the internal surface of glass, suffer different *retardations* of their undulations, he applied the formulæ which he had obtained for the polarizing effect of reflection to this case. But in this case the formulæ expressed an impossibility; yet as algebraical formulæ, even in such cases, have often some meaning, "I interpreted," he says,[5] "in the manner which appeared to me most natural and most probable, what the analysis indicated by this imaginary form;" and by such an interpretation he collected the law of the difference of undulation of the two rays. He was thus able to predict that by two internal reflections in a *rhomb*, or parallelopiped of glass, of a certain form and position, a polarized ray would acquire a circular undulation of its particles; and this constitution of the ray, it appeared, by reasoning further, would show itself by its possessing peculiar properties, partly the same as those of polarized light, and partly different. This extraordinary anticipation was exactly confirmed; and thus the apparently bold and strange guess of the author was fully justified, or at least assented to, even by the most cautious philosophers. "As I cannot appreciate the mathematical evidence for the nature of circular polarization," says Prof. Airy,[6] "I shall mention the experimental evidence on which I receive it." The conception has since been universally adopted.

But Fresnel, having thus obtained circularly-polarized rays, saw

[2] *Ann. de Chim.* 1822, tom. xx. p. 382.

[3] Ib. *Ann. de Chim.* 1822, tom. xx. p. 191. [4] Ib. p. 194.

*Bullet. des Sc.* 1823, p. 33. [6] *Camb. Trans.* vol. iv. p. 81, 1831 that he could account for the phenomena of quartz, already observed by M. Arago, as we have noticed in Chap. ix., by supposing two circularly-polarized rays to pass, with different velocities, along the axis. The curious succession of colors, following each other in right-handed or left-handed circular order, of which we have already spoken, might thus be hypothetically explained.

But was this hypothesis of two circularly-polarized rays, travelling along the axis of such crystals, to be received, merely because it accounted for the phenomena? Fresnel's ingenuity again enabled him to avoid such a defect in theorizing. If there were two such rays, they might be visibly separated[7] by the same artifice, of a pile of prisms properly achromatized, which he had used for compressed glass. The result was, that he did obtain a visible separation of the rays; and this result has since been confirmed by others, for instance, Professor Airy.[8] The rays were found to be in all respects identical with the circularly-polarized rays produced by the internal reflections in Fresnel's rhomb. This kind of double refraction gave a hypothetical explanation of the laws which M. Biot had obtained for the phenomena of this class; for example,[9] the rule, that the deviation of the plane of polarization of the emergent ray is inversely as the square of the length of an undulation for each kind of rays. And thus the phenomena produced by light passing along the axis of quartz were reduced into complete conformity with the theory.

[2nd Ed.] [I believe, however, Fresnel did not deduce the phenomenon from the mathematical formula, without the previous suggestion of experiment. He *observed* appearances which implied a difference of retardation in the two differently-polarized rays at total reflection; as Sir D. Brewster observed in reflection of metals phenomena having a like character. The general fact being observed, Fresnel used the theory to discover the law of this retardation, and to determine a construction in which, one ray being a quarter of an undulation retarded more than the other, circular polarization would be produced. And this anticipation was verified by the construction of his *rhomb*.

As a still more curious verification of this law, another of Fresnel's experiments may be mentioned. He found the proper angles for a circularly-polarizing glass rhomb on the supposition that there were

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[7] *Bull. des Sc.* 1822, p. 193.

[8] *Cambridge Trans.* iv. p. 80.

[9] *Bull. des Sc.* 1822, p. 197.

*four* internal reflections instead of two; two of the four taking place when the surface of the glass was dry, and two when it was wet. The rhomb was made; and when all the points of reflection were dry, the light was not circularly polarized; when two points were wet, the light was circularly polarized; and when all four were wet, it was not circularly polarized.]

3. *Elliptical Polarization in Quartz.*—We now come to one of the few additions to Fresnel's theory which have been shown to be necessary. He had accounted fully for the colors produced by the rays which travel *along the axis* of quartz crystals; and thus, for the colors and changes of the central spot which is produced when polarized light passes through a transverse plate of such crystals. But this central spot is surrounded by rings of colors. How is the theory to be extended to these?

This extension has been successfully made by Professor Airy.[10] His hypothesis is, that as rays passing along the axis of a quartz crystal are circularly polarized, rays which are oblique to the axis are elliptically polarized, the amount of ellipticity depending, in some unknown manner, upon the obliquity; and that each ray is separated by double refraction into two rays polarized elliptically; the one right-handed, the other left-handed. By means of these suppositions, he not only was enabled to account for the simple phenomena of single plates of quartz; but for many most complex and intricate appearances which arise from the superposition of two plates, and which at first sight might appear to defy all attempts to reduce them to law and symmetry; such as spirals, curves approaching to a square form, curves broken in four places. "I can hardly imagine," he says,[11] very naturally, "that any other supposition would represent the phenomena to such extreme accuracy. I am not so much struck with the accounting for the continued dilatation of circles, and the general representation of the forms of spirals, as with the explanations of the minute deviations from symmetry; as when circles become almost square, and crosses are inclined to the plane of polarization. And I believe that any one who shall follow my investigation, and imitate my experiments, will be surprised at their perfect agreement."

4. *Differential Equations of Elliptical Polarization.*—Although circular and elliptical polarization can be clearly conceived, and their existence, it would seem, irresistibly established by the phenomena, it

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[10] *Camb. Trans.* iv. p. 83, &c. [11] *Camb. Trans.* iv. p. 122.

is extremely difficult to conceive any arrangement of the particles of bodies by which such motions can mechanically be produced; and this difficulty is the greater, because some fluids and some gases impress a circular polarization upon light; in which cases we cannot imagine any definite arrangement of the particles, such as might form the mechanism requisite for the purpose. Accordingly, it does not appear that any one has been able to suggest even a plausible hypothesis on that subject. Yet, even here, something has been done. Professor Mac Cullagh, of Dublin, has discovered that by slightly modifying the *analytical expressions* resulting from the common case of the propagation of light, we may obtain other expressions which would give rise to such motions as produce circular and elliptical polarization. And though we cannot as yet assign the mechanical interpretation of the language of analysis thus generalized, this generalization brings together and explains by one common numerical supposition, two distinct classes of facts;—a circumstance which, in all cases, entitles an hypothesis to a very favorable consideration.

Mr. Mac Cullagh's assumption consists in adding to the two equations of motion which are expressed by means of second differentials, two other terms involving third differentials in a simple and symmetrical manner. In doing this, he introduces a coefficient, of which the magnitude determines both the amount of rotation of the polarization of a ray passing along the axis, as observed and measured by Biot, and the ellipticity of the polarization of a ray which is oblique to the axis, according to Mr. Airy's theory, of which ellipticity that philosopher also had obtained certain measures. The agreement between the two sets of measures[12] thus brought into connexion is such as very strikingly to confirm Mr. Mac Cullagh's hypothesis. It appears probable, too, that the confirmation of this hypothesis involves, although in an obscure and oracular form, a confirmation of the undulatory theory, which is the starting-point of this curious speculation.

5. *Elliptical Polarization of Metals.*—The effect of metals upon the light which they reflect, was known from the first to be different from that which transparent bodies produce. Sir David Brewster, who has recently examined this subject very fully,[13] has described the modification thus produced, as *elliptic polarization.* In employing this term, "he seems to have been led," it has been observed,[14] "by a

[12] *Royal I. A. Trans.* 1836. [13] *Phil. Trans.* 1830.

[14] Lloyd, *Report on Optics*, p. 372. (Brit. Assoc.)

desire to avoid as much as possible all reference to theory The laws which he has obtained, however, belong to elliptically-polarized light in the sense in which the term was introduced by Fresnel." And the identity of the light produced by metallic reflection with the elliptically-polarized light of the wave-theory, is placed beyond all doubt, by an observation of Professor Airy, that the rings of uniaxal crystals, produced by Fresnel's elliptically-polarized light, are exactly the same as those produced by Brewster's metallic light.

6. *Newton's Rings by Polarized Light.*—Other modifications of the phenomena of thin plates by the use of polarized light, supplied other striking confirmations of the theory. These were in one case the more remarkable, since the result was foreseen by means of a rigorous application of the conception of the vibratory motion of light, and confirmed by experiment. Professor Airy, of Cambridge, was led by his reasonings to see, that if Newton's rings are produced between a lens and a plate of metal, by polarized light, then, up to the polarizing angle, the central spot will be black, and instantly beyond this, it will be white. In a note,[15] in which he announced this, he says, "This I anticipated from Fresnel's expressions; it is confirmatory of them, and defies emission." He also predicted that when the rings were produced between two substances of very different refractive powers, the centre would twice pass from black to white and from white to black, by increasing the angle; which anticipation was fulfilled by using a diamond for the higher refraction.[16]

7. *Conical Refraction.*—In the same manner, Professor Hamilton of Dublin pointed out that according to the Fresnelian doctrine of double refraction, there is a certain direction of a crystal in which a single ray of light will be refracted so as to form a *conical pencil.* For the direction of the refracted ray is determined by a plane which touches the wave surface, the rule being that the ray must pass from the centre of the surface to the point of contact; and though in general this contact gives a single point only, it so happens, from the peculiar inflected form of the wave surface, which has what is called *a cusp*, that in one particular position, the plane can touch the surface in an entire circle. Thus the general rule which assigns the path of

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[15] Addressed to myself, dated May 23, 1831. I ought, however, to notice, that this experiment had been made by M. Arago, fifteen years earlier, and published: though not then recollected by Mr. Airy.

[16] *Camb. Trans.* vol. ii. p. 409.

the refracted ray, would, in this case, guide it from the centre of the surface to every point in the circumference of the circle, and thus make it a cone. This very curious and unexpected result, which Professor Hamilton thus obtained from the theory, his friend Professor Lloyd verified as an experimental fact. We may notice, also, that Professor Lloyd found the light of the conical pencil to be polarized according to a law of an unusual kind; but one which was easily seen to be in complete accordance with the theory.

8. *Fringes of Shadows.*—The phenomena of the *fringes of shadows* of small holes and groups of holes, which had been the subject of experiment by Fraunhofer, were at a later period carefully observed in a vast variety of cases by M. Schwerd of Spires, and published in a separate work, [17] *Beugungs-erscheinungen* (Phenomena of Inflection), 1836. In this Treatise, the author has with great industry and skill calculated the integrals which, as we have seen, are requisite in order to trace the consequences of the theory; and the accordance which he finds between these and the varied and brilliant results of observation is throughout exact. "I shall," says he, in the preface,[18] "prove by the present Treatise, that all inflection-phenomena, through openings of any form, size, and arrangement, are not only explained by the undulation-theory, but that they can be represented by analytical expressions, determining the intensity of the light in any point whatever." And he justly adds, that the undulation-theory accounts for the phenomena of light, as completely as the theory of gravitation does for the facts of the solar system.

9. *Objections to the Theory.*—We have hitherto mentioned only cases in which the undulatory theory was either entirely successful in explaining the facts, or at least hypothetically consistent with them and with itself. But other objections were started, and some difficulties were long considered as very embarrassing. Objections were made to the theory by some English experimenters, as Mr. Potter, Mr. Barton, and others. These appeared in scientific journals, and were afterwards answered in similar publications. The objections depended partly on the measure of the *intensity* of light in the different points of the phenomena (a datum which it is very difficult to obtain with accuracy

[17] *Die Beugungs-erscheinungen, aus dem Fundamental-gesetz der Undulations-Theorie analytisch entwickelt und in Bildern dargestellt,* von F. M. Schwerd. Mannheim, 1835.

[18] Dated Speyer, Aug. 1835.

by experiment), and partly on misconceptions of the theory; and I believe there are none of them which would now be insisted on.

We may mention, also, another difficulty, which it was the habit of the opponents of the theory to urge as a reproach against it, long after it had been satisfactorily explained: I mean the *half-undulation* which Young and Fresnel had found it necessary, in some cases, to assume as gained or lost by one of the rays. Though they and their followers could not analyse the mechanism of reflection with sufficient exactness to trace out all the circumstances, it was not difficult to see, upon Fresnel's principles, that reflection from the interior and exterior surface of glass must be of opposite kinds, which might be expressed by supposing one of these rays to lose half an undulation. And thus there came into view a justification of the step which had originally been taken upon empirical grounds alone.

10. *Dispersion, on the Undulatory Theory.*—A difficulty of another kind occasioned a more serious and protracted embarrassment to the cultivators of this theory. This was the apparent impossibility of accounting, on the theory, for the prismatic dispersion of color. For it had been shown by Newton that the amount of refraction is different for every color; and the amount of refraction depends on the velocity with which light is propagated. Yet the theory suggested no reason why the velocity should be different for different colors: for, by mathematical calculation, vibrations of all degrees of rapidity (in which alone colors differ) are propagated with the same speed. Nor does analogy lead us to expect this variety. There is no such difference between quick and slow waves of air. The sounds of the deepest and the highest bells of a peal are heard at any distance in the same order. Here, therefore, the theory was at fault.

But this defect was far from being a fatal one. For though the theory did not explain, it did not contradict, dispersion. The suppositions on which the calculations had been conducted, and the analogy of sound, were obviously in no small degree precarious. The velocity of propagation might differ for different rates of undulation, in virtue of many causes which would not affect the general theoretical results.

Many such hypothetical causes were suggested by various eminent mathematicians, as solutions of this conspicuous difficulty. But without dwelling upon these conjectures, it may suffice to notice that hypothesis upon which the attention of mathematicians was soon concentrated. This was the *hypothesis of finite intervals* between the particles of the ether. The length of one of those undulations which produce light, is a very small quantity, its mean value being 1-50,000th of an inch; but in the previous investigations of the consequences of the theory, it had been assumed that the distance from each other, of the particles of the ether, which, by their attractions or repulsions, caused the undulations to be propagated, is indefinitely less than this small quantity;—so that its amount might be neglected in the cases in which the length of the undulation was one of the quantities which determined the result. But this assumption was made arbitrarily, as a step of simplification, and because it was imagined that, in this way, a nearer approach was made to the case of a continuous fluid ether, which the supposition of distinct particles imperfectly represented. It was still free for mathematicians to proceed upon the opposite assumption, of particles of which the distances were finite, either as a mathematical basis of calculation, or as a physical hypothesis; and it remained to be seen if, when this was done, the velocity of light would still be the same for different lengths of undulation, that is, for different colors. M. Cauchy, calculating, upon the most general principles, the motion of such a collection of particles as would form an elastic medium, obtained results which included the new extension of the previous hypothesis. Professor Powell, of Oxford, applied himself to reduce to calculation, and to compare with experiment, the result of these researches. And it appeared that, on M. Cauchy's principles, a variation in the velocity of light is produced by a variation in the length of the wave, provided that the interval between the molecules of the ether bears a sensible ratio to the length of an undulation.[19] Professor Powell obtained also, from the general expressions, a formula expressing the relation between the refractive index of a ray, and the length of a wave, or the color of light.[20] It then became his task to ascertain whether this relation obtained experimentally; and he found a very close agreement between the numbers which resulted from the formula and those observed by Fraunhofer, for ten different kinds of media, namely, certain glasses and fluids.[21] To these he afterwards added ten other cases of crystals observed by M. Rudberg.[22] Mr. Kelland, of Cambridge, also calculated, in a manner somewhat different, the results of the same hypothesis of finite intervals;[23] and, obtaining

[19] *Phil. Mag.* vol. vi. p. 266.

[20] Ib. vol. vii. 1835, p. 266.

[21] *Phil. Trans.* 1835, p. 249.

[22] Ib. 1836, p. 17.

[23] *Camb. Trans.* vol. vi. p. 153.

formulæ not exactly the same as Professor Powell, found also an agreement between these and Fraunhofer's observations.

It may be observed, that the refractive indices observed and employed in these comparisons, were not those determined by the color of the ray, which is not capable of exact identification, but those more accurate measures which Fraunhofer was enabled to make, in consequence of having detected in the spectrum the black lines which he called B, C, D, E, F, G, H. The agreement between the theoretical formulæ and the observed numbers is remarkable, throughout all the series of comparisons of which we have spoken. Yet we must at present hesitate to pronounce upon the hypothesis of finite intervals, as proved by these calculations; for though this hypothesis has given results agreeing so closely with experiment, it is not yet clear that other hypotheses may not produce an equal agreement. By the nature of the case, there must be a certain gradation and continuity in the succession of colors in the spectrum, and hence, any supposition which will account for the general fact of the whole dispersion, may possibly account for the amount of the intermediate dispersions because these must be interpolations between the extremes. The result of this hypothetical calculation, however, shows very satisfactorily that there is uot, in the fact of dispersion, anything which is at all formidable to the undulatory theory.

11. *Conclusion.*—There are several other of the more recondite points of the theory which may be considered as, at present, too undecided to allow us to speak historically of the discussions which they have occasioned.[24] For example, it was conceived, for some time, that the vibrations of polarized light are perpendicular to the plane of polarization. But this assumption was not an essential part of the theory; and all the phenomena would equally allow us to suppose the vibrations to be *in* the polarization plane; the main requisite being, that light polarized in planes at right angles to each other, should also have the vibrations at right angles. Accordingly, for some time, this point was left undecided by Young and Fresnel, and, more recently, some mathematicians have come to the opinion that ether vibrates in the plane of polarization. The theory of transverse vibrations is equally stable, whichever supposition may be finally confirmed.

We may speak, in the same manner, of the suppositions which, from

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[24] For an account of these, see Professor Lloyd's *Report on Physical Optics* (Brit. Assoc. Report, 1834.)

the time of Young and Fresnel, the cultivators of this theory have been led to make respecting the mechanical constitution of the ether, and the forces by which transverse vibrations are produced. It was natural that various difficulties should arise upon such points, for transverse vibrations had not previously been made the subject of mechanical calculation, and the forces which occasion them must act in a different manner from those which were previously contemplated. Still, we may venture to say, without entering into these discussions, that it has appeared, from all the mathematical reasonings which have been pursued, that there is not, in the conception of transverse vibrations, anything inconsistent either with the principles of mechanics, or with the best general views which we can form, of the forces by which the universe is held together.

I willingly speak as briefly as the nature of my undertaking allows, of those points of the undulatory theory which are still under deliberation among mathematicians. With respect to these, an intimate acquaintance with mathematics and physics is necessary to enable any one to understand the steps which are made from day to day; and still higher philosophical qualifications would be requisite in order to pronounce a judgment upon them. I shall, therefore, conclude this survey by remarking the highly promising condition of this great department of science, in respect to the character of its cultivators. Nothing less than profound thought and great mathematical skill can enable any one to deal with this theory, in any way likely to promote the interests of science. But there appears, in the horizon of the scientific world, a considerable class of young mathematicians, who are already bringing to these investigations the requisite talents and zeal; and who, having acquired their knowledge of the theory since the time when its acceptation was doubtful, possess, without effort, that singleness and decision of view as to its fundamental doctrines, which it is difficult for those to attain whose minds have had to go through the hesitation, struggle, and balance of the epoch of the establishment of the theory. In the hands of this new generation, it is reasonable to suppose the Analytical Mechanics of light will be improved as much as the Analytical Mechanics of the solar system was by the successors of Newton. We have already had to notice many of this younger race of undulationists. For besides MM. Cauchy, Poisson, and Ampère, M. Lamé has been more recently following these researches in France.[25] In

[25] Prof. Lloyd's *Report*, p. 392.

Belgium, M. Quetelet has given great attention to them; and, in our own country, Sir William Hamilton, and Professor Lloyd, of Dublin, have been followed by Mr. Mac Cullagh. Professor Powell, of Oxford, has continued his researches with unremitting industry; and, at Cambridge, Professor Airy, who did much for the establishment and diffusion of the theory before he was removed to the post of Astronomer Royal, at Greenwich, has had the satisfaction to see his labors continued by others, even to the most recent time; for Mr. Kelland,[26] whom we have already mentioned, and Mr. Archibald Smith,[27] the two persons who, in 1834 and 1836, received the highest mathematical honors which that university can bestow, have both of them published investigations respecting the undulatory theory.

We may be permitted to add, as a reflection obviously suggested by these facts, that the cause of the progress of science is incalculably benefited by the existence of a body of men, trained and stimulated to the study of the higher mathematics, such as exist in the British universities, who are thus prepared, when an abstruse and sublime theory comes before the world with all the characters of truth, to appreciate its evidence, to take steady hold of its principles, to pursue its calculations, and thus to convert into a portion of the permanent treasure and inheritance of the civilized world, discoveries which might otherwise expire with the great geniuses who produced them, and be lost for ages, as, in former times, great scientific discoveries have sometimes been.

The reader who is acquainted with the history of recent optical discovery, will see that we have omitted much which has justly excited admiration; as, for example, the phenomena produced by glass under heat or pressure, noticed by MM. Lobeck, and Biot, and Brewster, and many most curious properties of particular minerals. We have omitted, too, all notice of the phenomena and laws of the absorption of light, which hitherto stand unconnected with the theory. But in this we have not materially deviated from our main design; for our end, in what we have done, has been to trace the advances of Optics

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[26] *On the Dispersion of Light, as explained by the Hypothesis of Finite Intervals.* Camb. Trans. vol. vi. p. 153.

[27] *Investigation of the Equation to Fresnel's Wave Surface,* ib. p. 85. See also, in the same volume, *Mathematical Considerations on the Problem of the Rainbow,* showing it to belong to Physical Optics, by R. Potter, Esq., of Queen's College.

towards perfection as a theory; and this task we have now nearly executed as far as our abilities allow.

We have been desirous of showing that the *type* of this progress, in the histories of the two great sciences, Physical Astronomy and Physical Optics, is the same. In both we have many *Laws of Phenomena* detected and accumulated by acute and inventive men; we have *Preludial* guesses which touch the true theory, but which remain for a time imperfect, undeveloped, unconfirmed: finally we have the *Epoch* when this true theory, clearly apprehended by great philosophical geniuses, is recommended by its fully explaining what it was first meant to explain, and confirmed by its explaining what it was *not* meant to explain. We have then its *Progress* struggling for a little while with adverse prepossessions and difficulties; finally overcoming all these, and moving onwards, while its triumphal procession is joined by all the younger and more vigorous men of science.

It would, perhaps, be too fanciful to attempt to establish a parallelism between the prominent persons who figure in these two histories. If we were to do this, we must consider Huyghens and Hooke as standing in the place of Copernicus, since, like him, they announced the true theory, but left it to a future age to give it development and mechanical confirmation; Malus and Brewster, grouping them together, correspond to Tycho Brahe and Kepler, laborious in accumulating observations, inventive and happy in discovering laws of phenomena; and Young and Fresnel combined, make up the Newton of optical science.

[2nd Ed.] [In the *Report on Physical Optics*, (*Brit. Ass. Reports*, 1834,) by Prof. Lloyd, the progress of the mathematical theory after Fresnel's labors is stated more distinctly than I have stated it, to the following effect. Ampère, in 1828, proved Fresnel's mathematical results directly, which Fresnel had only proved indirectly, and derived from his proof Fresnel's beautiful geometrical construction. Prof. MacCullagh not long after gave a concise demonstration of the same theorem, and of the other principal points of Fresnel's theory. He represents the elastic force by means of an ellipsoid whose axes are inversely proportional to those of Fresnel's generating ellipsoid, and deduces Fresnel's construction geometrically. In the third Supplement to his *Essay on the Theory of Systems of Rays* (*Trans. R. I. Acad.* vol xvii.), Sir W. Hamilton has presented that portion of Fresnel's theory which relates to the fundamental problem of the determination of the velocity and polarization of a plane wave, in a very elegant and analytical form. This he does by means of what he calls the *characteristic function* of the optical system to which the problem belongs. From this function is deduced the *surface of wave-slowness* of the medium; and by means of this surface, the direction of the rays refracted into the medium. From this construction also Sir W. Hamilton was led to the anticipation of *conical refraction*, mentioned above.

The investigations of MM. Cauchy and Lamé refer to the laws by which the particles of the ether act upon each other and upon the particles of other bodies;—a field of speculation which appears to me not yet ripe for the final operations of the analyst.

Among the mathematicians who have supplied defects in Fresnel's reasoning on this subject, I may mention Mr. Tovey, who treated it in several papers in the *Philosophical Magazine* (1837–40). Mr. Tovey's early death must be deemed a loss to mathematical science.

Besides investigating the motion of symmetrical systems of particles which may be supposed to correspond to biaxal crystals, Mr. Tovey considered the case of unsymmetrical systems, and found that the undulations propagated would, in the general case, be elliptical; and that in a particular case, circular undulations would take place, such as are propagated along the axis of quartz. It appears to me, however, that he has not given a definite meaning to those limitations of his general hypothesis which conduct him to this result. Perhaps if the hypothetical conditions of this result were traced into detail, they would be found to reside in a *screw-like* arrangement of the elementary particles, in some degree such as crystals of quartz themselves exhibit in their forms, when they have plagihedral faces at both ends.

Such crystals of quartz are, some like a right-handed and some like a left-handed screw; and, as Sir John Herschel discovered, the circular polarization is right-handed or left-handed according as the plagihedral form is so. In Mr. Tovey's hypothetical investigation it does not appear upon what part of the hypothesis this difference of right and left-handed depends. The definition of this part of the hypothesis is a very desirable step.

When crystals of Quartz are right-handed at one end, they are right-handed at the other end: but there is a different kind of plagihedral form, which occurs in some other crystals, for instance, in Apatite: in these the plagihedral faces are right-handed at the one extremity and left-handed at the other. For the sake of distinction, we may call the former *homologous* plagihedral faces, since, at both ends, they have the same name; and the latter *heterologous* plagihedral faces.

The nomologous plagihedral faces of Quartz crystals are accompanied by homologous circular polarization of the same name. I do not know that heterologous circular polarization has been observed in any crystal, but it has been discovered by Dr. Faraday to occur in glass, &c., when subjected to powerful magnetic action.

Perhaps it was presumptuous in me to attempt to draw such comparisons, especially with regard to living persons, as I have done in the preceding pages of this Book. Having published this passage, however, I shall not now suppress it. But I may observe that the immense number and variety of the beautiful optical discoveries which we owe to Sir David Brewster makes the comparison in his case a very imperfect representation of his triumphs over nature; and that, besides his place in the history of the Theory of Optics, he must hold a most eminent position in the history of Optical Crystallography, whenever the discovery of a True Optical Theory of Crystals supplies us with the *Epoch* to which his labors in this field form so rich a *Prelude*. I cordially assent to the expression employed by Mr. Airy in the *Phil. Trans.* for 1840, in which he speaks of Sir David Brewster as "the Father of Modern Experimental Optics."]

# BOOK X.

*SECONDARY MECHANICAL SCIENCES.*

(CONTINUED.)

# HISTORY

OF

# THERMOTICS AND ATMOLOGY.

Et primum faciunt ignem se vortere in auras
Aëris; hinc imbrem gigni terramque creari
Ex imbri; retroque a terrâ cuncta revorti,
Humorem primum, post aëra deinde calorem;
Nec cessare hæc inter se mutare, meare,
De cœlo ad terram de terrâ ad sidera mundi.

LUCRETIUS, i. 783.

Water, and Air, and Fire, alternate run
Their endless circle, multiform, yet one.
For, moulded by the fervor's latent beams,
Solids flow loose, and fluids flash to steams,
And elemental flame, with secret force,
Pursues through earth, air, sky, its stated course.

## INTRODUCTION

### *Of Thermotics and Atmology.*

I EMPLOY the term *Thermotics*, to include all the doctrines respecting Heat, which have hitherto been established on proper scientific grounds. Our survey of the history of this branch of science must be more rapid and less detailed than it has been in those subjects of which we have hitherto treated: for our knowledge is, in this case, more vague and uncertain than in the others, and has made less progress towards a general and certain theory. Still, the narrative is too important and too instructive to be passed over.

The distinction of Formal Thermotics and Physical Thermotics,—of the discovery of the mere Laws of Phenomena, and the discovery of their causes,—is applicable here, as in other departments of our knowledge. But we cannot exhibit, in any prominent manner, the latter division of the science now before us; since no general theory of heat has yet been propounded, which affords the means of calculating the circumstances of the phenomena of conduction, radiation, expansion, and change of solid, liquid, and gaseous form. Still, on each of these subjects there have been proposed, and extensively assented to, certain general views, each of which explains its appropriate class of phenomena; and, in some cases, these principles have been clothed in precise and mathematical conditions, and thus made bases of calculation.

These principles, thus possessing a generality of a limited kind, connecting several observed laws of phenomena, but yet not connecting all the observed classes of facts which relate to heat, will require our separate attention. They may be described as the Doctrine of Conduction, the Doctrine of Radiation, the Doctrine of Specific Heat, and the Doctrine of Latent Heat; and these, and similar doctrines respecting heat, make up the science which we may call *Thermotics proper*.

But besides these collections of principles which regard heat by itself, the relations of heat and moisture give rise to another extensive and important collection of laws and principles, which I shall treat of in connexion with Thermotics, and shall term *Atmology*, borrowing the term from the Greek word (ἄτμος,) which signifies *vapor*. The *Atmosphere* was so named by the Greeks, as being a sphere of vapor; and, undoubtedly, the most general and important of the phenomena which take place in the air, by which the earth is surrounded, are those in which water, of one *consistence* or other (ice, water, or steam,) is concerned. The knowledge which relates to what takes place in the atmosphere has been called *Meteorology*, in its collective form: but such knowledge is, in fact, composed of parts of many different sciences. And it is useful for our purpose to consider separately those portions of Meteorology which have reference to the laws of aqueous vapor, and these we may include under the term Atmology.

The instruments which have been invented for the purpose of measuring the moisture of the air, that is, the quantity of vapor which exists in it, have been termed *Hygrometers;* and the doctrines on which these instruments depend, and to which they lead, have been called *Hygrometry;* but this term has not been used in quite so extensive a sense as that which we intend to affix to *Atmology.*

In treating of Thermotics, we shall first describe the earlier progress of men's views concerning Conduction, Radiation, and the like, and shall then speak of the more recent corrections and extensions, by which they have been brought nearer to theoretical generality.

# THERMOTICS PROPER.

## CHAPTER I.

### The Doctrines of Conduction and Radiation.

*Section* 1.—*Introduction of the Doctrine of Conduction.*

BY *conduction* is meant the propagation of heat from one part to another of a continuous body; or from one body to another in contact with it; as when one end of a poker stuck in the fire heats the other end, or when this end heats the hand which takes hold of it. By *radiation* is meant the diffusion of heat from the surface of a body to points not in contact. It is clear in both these cases, that, in proportion as the hot portion is hotter, it produces a greater effect in warming the cooler portion; that is, it *communicates more Heat* to it, if *Heat* be the abstract conception of which this effect is the measure. The simplest rule which can be proposed is, that the heat thus communicated in a given instant is proportional to the excess of the heat of the hot body over that of the contiguous bodies; there are no obvious phenomena which contradict the supposition that this is the true law; and it was thence assumed by Newton as the true law for radiation and by other writers for conduction. This assumption was confirmed approximately, and afterwards corrected, for the case of Radiation; in its application to Conduction, it has been made the basis of calculation up to the present time. We may observe that this statement takes for granted that we have attained to a measure of heat (or of *temperature*, as heat thus measured is termed), corresponding to the law thus assumed; and, in fact, as we shall have occasion to explain in speaking of the *measures* of sensible qualities, the thermometrical scale of heat according to the expansion of liquids (which is the measure of temperature here adopted), was constructed with a reference to Newton's law of radiation of heat; and thus the law is necessarily consistent with the scale.

In any case in which the parts of a body are unequally hot, the temperature will vary *continuously* in passing from one part of the body to another; thus, a long bar of iron, of which one end is kept red hot, will exhibit a *gradual* diminution of temperature at successive points, proceeding to the other end. The law of temperature of the parts of such a bar might be expressed by the ordinates of a *curve* which should run alongside the bar. And, in order to trace mathematically the consequences of the assumed law, some of those processes would be necessary, by which mathematicians are enabled to deal with the properties of curves; as the method of infinitesimals, or the differential calculus; and the truth or falsehood of the law would be determined, according to the usual rules of inductive science, by a comparison of results so deduced from the principle, with the observed phenomena.

It was easily perceived that this comparison was the task which physical inquirers had to perform; but the execution of it was delayed for some time; partly, perhaps, because the mathematical process presented some difficulties. Even in a case so simple as that above mentioned, of a linear bar with a stationary temperature at one end, *partial differentials* entered; for there were three variable quantities, the time, as well as the place of each point and its temperature. And at first, another scruple occurred to M. Biot when, about 1804, he undertook this problem.[1] "A difficulty," says Laplace,[2] in 1809, "presents itself, which has not yet been solved. The quantities of heat received and communicated in an instant (by any point of the bar) must be infinitely small quantities of the same order as the excess of the heat of a slice of the body over that of the contiguous slice; therefore the *excess* of the heat received by any slice over the heat communicated, is an infinitely small quantity of the second order; and the accumulation in a finite time (which depends on this excess) cannot be finite." I conceive that this difficulty arises entirely from an arbitrary and unnecessary assumption concerning the relation of the infinitesimal parts of the body. Laplace resolved the difficulty by further reasoning founded upon the same assumption which occasioned

[1] Biot, *Traité de Phys.* iv. p. 669. [2] Laplace, *Mém. Inst.* for 1809, p. 332.

it; but Fourier, who was the most distinguished of the cultivators of this mathematical doctrine of conduction, follows a course of reasoning in which the difficulty does not present itself. Indeed it is stated by Laplace, in the Memoir above quoted,[3] that Fourier had already obtained the true fundamental equations by views of his own.

The remaining part of the history of the doctrine of conduction is principally the history of Fourier's labors. Attention having been drawn to the subject, as we have mentioned, the French Institute, in January, 1810, proposed, as their prize question, "To give the mathematical theory of the laws of the propagation of heat, and to compare this theory with exact observations." Fourier's Memoir (the sequel of one delivered in 1807,) was sent in September, 1811; and the prize (3000 francs) adjudged to it in 1812. In consequence of the political confusion which prevailed in France, or of other causes, these important Memoirs were not published by the Academy till 1824; but extracts had been printed in the *Bulletin des Sciences* in 1808, and in the *Annales de Chimie* in 1816; and Poisson and M. Cauchy had consulted the manuscript itself.

It is not my purpose to give, in this place,[4] an account of the analytical processes by which Fourier obtained his results. The skill displayed in these Memoirs is such as to make them an object of just admiration to mathematicians; but they consist entirely of deductions from the fundamental principle which I have noticed,—that the quantity of heat conducted from a hotter to a colder point is proportional to the excess of heat, modified by the *conductivity*, or conducting power of each substance. The equations which flow from this principle assume nearly the same forms as those which occur in the most general problems of hydrodynamics. Besides Fourier's solution, Laplace, Poisson, and M. Cauchy have also exercised their great analytical skill in the management of these formulæ. We shall briefly speak of the comparison of the results of these reasonings with experiment, and notice some other consequences to which they lead. But before we can do this, we must pay some attention to the subject of radiation.

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[3] Laplace, *Mém. Inst.* for 1809, p. 538.

[4] I have given an account of Fourier's mathematical results in the *Reports of the British Association for* 1835.

### *Sect. 2.—Introduction of the Doctrine of Radiation.*

A HOT body, as a mass of incandescent iron, emits heat, as we perceive by our senses when we approach it; and by this emission of heat the hot body cools down. The first step in our systematic knowledge of the subject was made in the *Principia*. "It was in the destiny of that great work," says Fourier, "to exhibit, or at least to indicate, the causes of the principal phenomena of the universe." Newton assumed, as we have already said, that the rate at which a body cools, that is, parts with its heat to surrounding bodies, is proportional to its heat; and on this assumption he rested the verification of his scale of temperatures. It is an easy deduction from this law, that if times of cooling be taken in arithmetical progression, the heat will decrease in geometrical progression. Kraft, and after him Richman, tried to verify this law by direct experiments on the cooling of vessels of warm water; and from these experiments, which have since been repeated by others, it appears that for differences of temperature which do not exceed 50 degrees (boiling water being 100), this geometrical progression represents, with tolerable (but not with complete) accuracy, the process of cooling.

This principle of radiation, like that of conduction, required to be followed out by mathematical reasoning. But it required also to be corrected in the first place, for it was easily seen that the rate of cooling depended, not on the absolute temperature of the body, but on the excess of its temperature above the surrounding objects to which it communicated its heat in cooling. And philosophers were naturally led to endeavor to explain or illustrate this process by some physical notions. Lambert in 1755 published[5] an *Essay on the Force of Heat*, in which he assimilates the communication of heat to the flow of a fluid out of one vessel into another by an excess of pressure; and mathematically deduces the laws of the process on this ground. But some additional facts suggested a different view of the subject. It was found that heat is propagated by radiation according to straight lines, like light; and that it is, as light is, capable of being reflected by mirrors, and thus brought to a focus of intenser action. In this manner the radiative effect of a body could be more precisely traced. A fact, however, came under notice, which, at first sight, appeared to

[5] *Act. Helvet.* tom. ii. p. 172.

offer some difficulty. It appeared that cold was reflected no less than heat. A mass of ice, when its effect was concentrated on a thermometer by a system of mirrors, made the thermometer fall, just as a vessel of hot water placed in a similar situation made it rise. Was cold, then, to be supposed a real substance, no less than heat?

The solution of this and similar difficulties was given by Pierre Prevost, professor at Geneva, whose theory of radiant heat was proposed about 1790. According to this theory, heat, or *caloric*, is constantly radiating from every point of the surface of all bodies in straight lines; and it radiates the more copiously, the greater is the quantity of heat which the body contains. Hence a constant exchange of heat is going on among neighboring bodies; and a body grows hotter or colder, according as it receives more caloric than it emits, or the contrary. And thus a body is cooled by rectilinear rays from a cold body, because along these paths it sends rays of heat in greater abundance than those which return the same way. This *theory of exchanges* is simple and satisfactory, and was soon generally adopted; but we must consider it rather as the simplest mode of expressing the dependence of the communication of heat on the excess of temperature, than as a proposition of which the physical truth is clearly established.

A number of curious researches on the effect of the different kinds of surface of the heating and of the heated body, were made by Leslie and others. On these I shall not dwell; only observing that the relative amount of this radiative and receptive energy may be expressed by numbers, for each kind of surface; and that we shall have occasion to speak of it under the term *exterior conductivity;* it is thus distinguished from *interior conductivity*, which is the relative rate at which heat is conducted in the interior of bodies.[6]

### *Sect.* 3.—*Verifications of the Doctrines of Conduction and Radiation.*

The interior and exterior conductivity of bodies are numbers, which enter as elements, or *coefficients*, into the mathematical calculations founded on the doctrines of conduction and radiation. These coeffi-

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[6] The term employed by Fourier, *conductibility* or *conducibility*, suggests expressions altogether absurd, as if the bodies could be called *conductible*, or *conducible*, with respect to heat: I have therefore ventured upon a slight alteration of the word, and have used the abstract term which analogy would suggest, if we suppose bodies to be *conductive* in this respect.

cients are to be determined for each case by appropriate experiments: when the experimenters had obtained these data, as well as the mathematical solutions of the problems, they could test the truth of their fundamental principles by a comparison of the theoretical and actual results in properly-selected cases. This was done for the law of conduction in the simple cases of metallic bars heated at one end, by M. Biot,[7] and the accordance with experiment was sufficiently close. In the more complex cases of conduction which Fourier considered, it was less easy to devise a satisfactory mode of comparison. But some rather curious relations which he demonstrated to exist among the temperatures at different points of an *armille*, or ring, afforded a good criterion of the value of the calculations, and confirmed their correctness.[8]

We may therefore presume these doctrines of radiation and conduction to be sufficiently established; and we may consider their application to any remarkable case to be a portion of the history of science. We proceed to some such applications.

### *Sect.* 4.—*The Geological and Cosmological Application of Thermotics.*

By far the most important case to which conclusions from these doctrines have been applied, is that of the globe of the earth, and of those laws of climate to which the modifications of temperature give rise; and in this way we are led to inferences concerning other parts of the universe. If we had any means of observing these terrestrial and cosmical phenomena to a sufficient extent, they would be valuable facts on which we might erect our theories; and they would thus form part, not of the corollaries, but of the foundations of our doctrine of heat. In such a case, the laws of the propagation of heat, as discovered from experiments on smaller bodies, would serve to explain these phenomena of the universe, just as the laws of motion explain the celestial movements. But since we are almost entirely without any definite indications of the condition of the other bodies in the solar system as to heat; and since, even with regard to the earth, we know only the temperature of the parts at or very near the surface, our knowledge of the part which heat plays in the earth and the heavens must be in a great measure, not a generalization of observed facts, but a deduction from theoretical principles. Still, such knowledge, whether obtained

[7] *Tr. de Phys.* iv. 671.

[8] *Mém. Inst.* 1819, p. 192, published 1824.

from observation or from theory, must possess great interest and importance. The doctrines of this kind which we have to notice refer principally to the effect of the sun's heat on the earth, the laws of climate,—the thermotical condition of the interior of the earth,—and that of the planetary spaces.

1. *Effect of Solar Heat on the Earth.*—That the sun's heat passes into the interior of the earth in a variable manner, depending upon the succession of days and nights, summers and winters, is an obvious consequence of our first notions on this subject. The mode in which it proceeds into the interior, after descending below the surface, remained to be gathered, either from the phenomena, or from reasoning. Both methods were employed.[9] Saussure endeavored to trace its course by digging, in 1785, and thus found that at the depth of about thirty-one feet, the annual variation of temperature is about 1-12th what it is at the surface. Leslie adopted a better method, sinking the bulbs of thermometers deep in the earth, while their stems appeared above the surface. In 1813, '16, and '17, he observed thus the temperatures at the depths of one, two, four, and eight feet, at Abbotshall, in Fifeshire. The results showed that the extreme annual oscillations of the temperature diminish as we descend. At the depth of one foot, the yearly range of oscillation was twenty-five degrees (Fahrenheit); at two feet it was twenty degrees; at four feet it was fifteen degrees; at eight feet it was only nine degrees and a half. And the time at which the heat was greatest was later and later in proceeding to the lower points. At one foot, the maximum and minimum were three weeks after the solstice of summer and of winter; at two feet, they were four or five weeks; at four feet, they were two months; and at eight feet, three months. The mean temperature of all the thermometers was nearly the same. Similar results were obtained by Ott at Zurich in 1762, and by Herrenschneider at Strasburg in 1821, '2, '3.[10]

These results had already been explained by Fourier's theory of conduction. He had shown[11] that when the surface of a sphere is affected by a periodical heat, certain alternations of heat travel uniformly into the interior, but that the extent of the alternation diminishes in geometrical progression in this descent. This conclusion applies to the effect of days and years on the temperature of the earth, and shows that such facts as those observed by Leslie are both exemplifications of

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[9] Leslie, art. *Climate*, Supp. *Enc. Brit.* 179. [10] Pouillet, *Météorol.* t. ii. p. 643.

[11] *Mém. Inst.* for 1821 (published 1826), p. 162.

the general circumstances of the earth, and are perfectly in accordance with the principles on which Fourier's theory rests.

2. *Climate.*—The term *climate*, which means *inclination*, was applied by the ancients to denote that inclination of the axis of the terrestrial sphere from which result the inequalities of days in different latitudes. This inequality is obviously connected also with a difference of thermotical condition. Places near the poles are colder, on the whole, than places near the equator. It was a natural object of curiosity to determine the law of this variation.

Such a determination, however, involves many difficulties, and the settlement of several preliminary points. How is the temperature of any place to be estimated? and if we reply, by its *mean* temperature, how are we to learn this mean? The answers to such questions require very multiplied observations, exact instruments, and judicious generalizations; and cannot be given here. But certain first approximations may be obtained without much difficulty; for instance, the mean temperature of any place may be taken to be the temperature of deep springs, which is probably identical with the temperature of the soil below the reach of the annual oscillations. Proceeding on such facts, Mayer found that the mean temperature of any place was nearly proportional to the square of the cosine of the latitude. This, as a law of phenomena, has since been found to require considerable correction; and it appears that the mean temperature does not depend on the latitude alone, but on the distribution of land and water, and on other causes. M. de Humboldt has expressed these deviations[12] by his map of *isothermal lines*, and Sir D. Brewster has endeavored to reduce them to a law by assuming two *poles of maximum cold*.

The expression which Fourier finds[13] for the distribution of heat in a homogeneous sphere, is not immediately comparable with Mayer's empirical formula, being obtained on a certain hypothesis, namely, that the equator is kept constantly at a fixed temperature. But there is still a general agreement; for, according to the theory, there is a diminution of heat in proceeding from the equator to the poles in such a case; the heat is propagated from the equator and the neighboring parts, and radiates out from the poles into the surrounding space. And thus, in the case of the earth, the solar heat enters in the tropical

[12] British Assoc. 1833. Prof. Forbes's *Report on Meteorology*, p. 215.

[13] Fourier. *Mém. Inst.* tom. v. p. 173.

parts, and constantly flows towards the polar regions, by which it is emitted into the planetary spaces.

Climate is affected by many thermotic influences, besides the conduction and radiation of the solid mass of the earth. The atmosphere, for example, produces upon terrestrial temperatures effects which it is easy to see are very great; but these it is not yet in the power of calculation to appreciate;[14] and it is clear that they depend upon other properties of air besides its power to transmit heat. We must therefore dismiss them, at least for the present.

3. *Temperature of the Interior of the Earth.*—The question of the temperature of the interior of the earth has excited great interest, in consequence of its bearing on other branches of knowledge. The various facts which have been supposed to indicate the fluidity of the central parts of the terrestrial globe, belong, in general, to geological science; but so far as they require the light of thermotical calculations in order to be rightly reasoned upon, they properly come under our notice here.

The principal problem of this kind which has been treated of is this: —If in the globe of the earth there be a certain original heat, resulting from its earlier condition, and independent of the action of the sun, to what results will this give rise? and how far do the observed temperatures of points below the surface lead us to such a supposition? It has, for instance, been asserted, that in many parts of the world the temperature, as observed in mines and other excavations, increases in descending, at the rate of one degree (centesimal) in about forty yards. What inference does this justify?

The answer to this question was given by Fourier and by Laplace. The former mathematician had already considered the problem of the cooling of a large sphere, in his Memoirs of 1807, 1809, and 1811. These, however, lay unpublished in the archives of the Institute for many years. But in 1820, when the accumulation of observations which indicated an increase of the temperature of the earth as we descend, had drawn observation to the subject, Fourier gave, in the Bulletin of the Philomathic Society,[15] a summary of his results, as far as they bore on this point. His conclusion was, that such an increase of temperature in proceeding towards the centre of the earth, can arise from nothing but the remains of a primitive heat;—that the heat which the sun's action would communicate, would, in its final and

[14] *Mém. Inst.* tom. vii. p. 584. [15] *Bullet. des Sc.* 1820, p. 58.

permanent state, be uniform in the same vertical line, as soon as we get beyond the influence of the superficial oscillations of which we have spoken ;—and that, before the distribution of temperature reaches this limit, it will decrease, not increase, in descending. It appeared also, by the calculation, that this remaining existence of the primitive heat in the interior of the earth's mass, was quite consistent with the absence of all perceptible traces of it at the surface ; and that the same state of things which produces an increase of one degree of heat in descending forty yards, does not make the surface a quarter of a degree hotter than it would otherwise be. Fourier was led also to some conclusions, though necessarily very vague ones, respecting the time which the earth must have taken to cool from a supposed original state of incandescence to its present condition, which time it appeared must have been very great; and respecting the extent of the future cooling of the surface, which it was shown must be insensible. Everything tended to prove that, within the period which the history of the human race embraces, no discoverable change of temperature had taken place from the progress of this central cooling. Laplace further calculated the effect[16] which any contraction of the globe of the earth by cooling would produce on the length of the day. He had already shown, by astronomical reasoning, that the day had not become shorter by 1-200th of a second, since the time of Hipparchus ; and thus his inferences agreed with those of Fourier. As far as regards the smallness of the perceptible effect due to the past changes of the earth's temperature, there can be no doubt that all the curious conclusions just stated are deduced in a manner quite satisfactory, from the fact of a general increase of heat in descending below the surface of the earth ; and thus our principles of speculative science have a bearing upon the history of the past changes of the universe, and give us information concerning the state of things in portions of time otherwise quite out of our reach.

4. *Heat of the Planetary Spaces.*—In the same manner, this portion of science is appealed to for information concerning parts of space which are utterly inaccessible to observation. The doctrine of heat leads to conclusions concerning the temperatures of the spaces which surround the earth, and in which the planets of the solar system revolve. In his Memoir, published in 1827,[17] Fourier states that he conceives it to follow from his principles, that these planetary spaces

[16] *Conn. des Tems*, 1823.

[17] *Mém. Inst.* tom. vii. p 580

are not absolutely cold, but have a "proper heat" independent of the sun and of the planets. If there were not such a heat, the cold of the polar regions would be much more intense than it is, and the alternations of cold and warmth, arising from the influence of the sun, would be far more extreme and sudden than we find them. As the cause of this heat in the planetary spaces, he assigns the radiation of the innumerable stars which are scattered through the universe.

Fourier says,[18] "We conclude from these various remarks, and principally from the mathematical examination of the question," that this is so. I am not aware that the mathematical calculation which bears peculiarly upon this point has anywhere been published. But it is worth notice, that Svanberg has been led[19] to the opinion of the same temperature in these spaces which Fourier had adopted (50 centigrade below zero), by an entirely different course of reasoning, founded on the relation of the atmosphere to heat.

In speaking of this subject, I have been led to notice incomplete and perhaps doubtful applications of the mathematical doctrine of conduction and radiation. But this may at least serve to show that Thermotics is a science, which, like Mechanics, is to be established by experiments on masses capable of manipulation, but which, like that, has for its most important office the solution of geological and cosmological problems. I now return to the further progress of our thermotical knowledge.

### *Sect. 5.—Correction of Newton's Law of Cooling.*

In speaking of the establishment of Newton's assumption, that the temperature communicated is proportional to the excess of temperature, we stated that it was approximately verified, and afterwards corrected (chap. i., sect. 1.), This correction was the result of the researches of MM. Dulong and Petit in 1817, and the researches by which they were led to the true law, are an admirable example both of laborious experiment and sagacious induction. They experimented through a very great range of temperature (as high as two hundred and forty degrees centigrade), which was necessary because the inaccuracy of Newton's law becomes considerable only at high temperatures. They removed the effect of the surrounding medium, by making their experiments in a vacuum. They selected with great

[18] *Mém. Inst.* tom, vii. p. 581. [19] Berzel. *Jahres Bericht*, xi. p. 50.

judgment the conditions of their experiments and comparisons, making one quantity vary while the others remained constant. In this manner they found, that *the quickness of cooling for a constant excess of temperature, increases in geometrical progression, when the temperature of the surrounding space increases in arithmetical progression;* whereas, according to the Newtonian law, this quickness would not have varied at all. Again, this variation being left out of the account, it appeared that *the quickness of cooling, so far as it depends on the excess of temperature of the hot body, increases as the terms of a geometrical progression diminished by a constant number, when the temperature of the hot body increases in arithmetical progression.* These two laws, with the coefficients requisite for their application to particular substances, fully determine the conditions of cooling in a vacuum.

Starting from this determination, MM. Dulong and Petit proceeded to ascertain the effect of the medium, in which the hot body is placed, upon its rate of cooling; for this effect became a *residual phenomenon*,[20] when the cooling in the vacuum was taken away. We shall not here follow this train of research; but we may briefly state, that they were led to such laws as this;—that the rapidity of cooling due to any gaseous medium in which the body is placed, is the same, so long as the excess of the body's temperature is the same, although the temperature itself vary;—that the cooling power of a gas varies with the elasticity, according to a determined law; and other similar rules.

In reference to the process of their induction, it is worthy of notice, that they founded their reasonings upon Prevost's law of exchanges; and that, in this way, the second of their laws above stated, respecting the quickness of cooling, was a mathematical consequence of the first. It may be observed also, that their temperatures are measured by means of the air-thermometer, and that if they were estimated on another scale, the remarkable simplicity and symmetry of their results would disappear. This is a strong argument for believing such a measure of temperature to have a natural prerogative of simplicity. This belief is confirmed by other considerations; but these, depending on the laws of *expansion* by heat, cannot be here referred to; and we must proceed to finish our survey of the mathematical theory of heat, as founded on the phenomena of radiation and conduction, which alone have as yet been traced up to general principles.

We may observe, before we quit this subject, that this correction of

[20] See *Phil. Ind. Sciences*, B. xiii. c. 7, Sect. iv.

Newton's law will materially affect the mathematical calculations on the subject, which were made to depend on that law both by Fourier, Laplace, and Poisson. Probably, however, the general features of the results will be the same as on the old supposition. M. Libri, an Italian mathematician, has undertaken one of the problems of this kind, that of the armil, with Dulong and Petit's law for his basis, in a Memoir read to the Institute of France in 1825, and since published at Florence.[21]

### *Sect. 6.— Other Laws of Phenomena with respect to Radiation.*

The laws of radiation as depending upon the surface of radiating bodies, and as affecting screens of various kinds interposed between the hot body and the thermometer, were examined by several inquirers. I shall not attempt to give an account of the latter course of research, and of the different laws which luminous and non-luminous heat have been found to follow in reference to bodies, whether transparent or opaque, which intercept them. But there are two or three laws of the phenomena, depending upon the effects of the surfaces of bodies, which are important.

1. In the first place, the powers of bodies to *emit* and to *absorb* heat, as far as depends upon their surface, appear to be in the same proportion. If we blacken the surface of a canister of hot water, it radiates heat more copiously; and in the same measure, it is more readily heated by radiation.

2. In the next place, as the radiative power increases, the power of reflection diminishes, and the contrary. A bright metal vessel reflects much heat; on this very account it does not emit much; and hence a hot fluid which such a vessel contains, remains hot longer than it does in an unpolished case.

3. The heat is emitted from every point of the surface of a hot body in all directions; but by no means in all directions with equal intensity. The intensity of the heating ray is as the sine of the angle which it makes with the surface.

The last law is entirely, the two former in a great measure, due to the researches of Leslie, whose *Experimental Inquiry into the Nature and Propagation of Heat*, published in 1804, contains a great number of curious and striking results and speculations. The laws now just

[21] *Mém. de Math. et de Phys.* 1829.

stated bear, in a very important manner, upon the formation of the theory; and we must now proceed to consider what appears to have been done in this respect; taking into account, it must still be borne in mind, only the phenomena of conduction and radiation.

### *Sect. 7.—Fourier's Theory of Radiant Heat.*

The above laws of phenomena being established, it was natural that philosophers should seek to acquire some conception of the physical action by which they might account, both for these laws, and for the general fundamental facts of Thermotics; as, for instance, the fact that all bodies placed in an inclosed space assume, in time, the temperature of the inclosure. Fourier's explanation of this class of phenomena must be considered as happy and successful; for he has shown that the supposition to which we are led by the most simple and general of the facts, will explain, moreover, the less obvious laws. It is an obvious and general fact, that bodies which are included in the space tend to acquire the same temperature. And this identity of temperature of neighboring bodies requires an hypothesis, which, it is found, also accounts for Leslie's law of the sine, in radiation.

This hypothesis is, that the radiation takes place, not from the surface alone of the hot body, but from all particles situated within a certain small depth of the surface. It is easy to see[22] that, on this supposition, a ray emitted obliquely from an internal particle, will be less intense than one sent forth perpendicular to the surface, because the former will be intercepted in a greater degree, having a greater length of path within the body; and Fourier shows, that whatever be the law of this intercepting power, the result will be, that the radiative intensity is as the sine of the angle made by the ray with the surface.

But this law is, as I have said, likewise necessary, in order that neighboring bodies may tend to assume the same temperature: for instance, in order that a small particle placed within a spherical shell, should finally assume the temperature of the shell. If the law of the sines did not obtain, the final temperature of such a particle would depend upon its place in the inclosure;[23] and within a shell of ice we should have, at certain points, the temperature of boiling water and of melting iron.

This proposition may at first appear strange and unlikely; but it may

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[22] *Mem. Inst.* t. v. 1821, p. 204.

[23] *An. Chim.* iv. 1817, p. 129.

be shown to be a necessary consequence of the assumed principle, by very simple reasoning, which I shall give in a general form in a Note.[24]

This reasoning is capable of being presented in a manner quite satisfactory, by the use of mathematical symbols, and proves that Leslie's law of the sines is rigorously and mathematically true on Fourier's hypothesis. And thus Fourier's theory of *molecular extra-radiation* acquires great consistency.

### *Sect. 8.—Discovery of the Polarization of Heat.*

The laws of which the discovery is stated in the preceding Sections of this Chapter, and the explanations given of them by the theories of conduction and radiation, all tended to make the conception of a material heat, or *caloric*, communicated by an actual flow and emission, familiar to men's minds; and, till lately, had led the greater part of thermotical philosophers to entertain such a view, as the most probable opinion concerning the nature of heat. But some steps have recently been made in thermotics, which appear to be likely to overturn this belief, and to make the doctrine of emission as untenable with regard to heat, as it had been found to be with regard to light. I speak of the discovery of the polarization of heat. It being ascertained that rays of heat are polarized in the same manner as rays of

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[24] The following reasoning may show the connexion of the law of the sines in radiant heat with the general principle of ultimate identity of neighboring temperatures. The equilibrium and identity of temperature between an including shell and an included body, cannot obtain upon the whole, except it obtain between each pair of parts of the two surfaces of the body and of the shell; that is, any part of the one surface, in its exchanges with any part of the other surface, must give and receive the same quantity of heat. Now the quantity exchanged, so far as it depends on the receiving surface, will, by geometry, be proportional to the sine of the obliquity of that surface: and as, in the exchanges, each may be considered as receiving, the quantity transferred must be proportional to the sines of the two obliquities; that is, to that of the giving as well as of the receiving surface.

Nor is this conclusion disturbed by the consideration, that all the rays of heat which fall upon a surface are not absorbed, some being reflected according to the nature of the surface. For, by the other above-mentioned laws of phenomena, we know that, in the same measure in which the surface loses the power of admitting, it loses the power of emitting, heat; and the superficial parts gain, by absorbing their own radiation, as much as they lose by not absorbing the incident heat; so that the result of the preceding reasoning remains unaltered.

light, we cannot retain the doctrine that heat radiates by the emanation of material particles, without supposing those particles of caloric to have poles; an hypothesis which probably no one would embrace for, besides that the ill fortune which attended that hypothesis in the case of light must deter speculators from it, the intimate connexion of heat and light would hardly allow us to suppose polarization in the two cases to be produced by two different kinds of machinery.

But, without here tracing further the influence which the polarization of heat must exercise upon the formation of our theories of heat, we must briefly notice this important discovery, as a law of phenomena.

The analogies and connexions between light and heat are so strong, that when the polarization of light had been discovered, men were naturally led to endeavor to ascertain whether heat possessed any corresponding property. But partly from the difficulty of obtaining any considerable effect of heat separated from light, and partly from the want of a thermometrical apparatus sufficiently delicate, these attempts led, for some time, to no decisive result. M. Berard took up the subject in 1813. He used Malus's apparatus, and conceived that he found heat to be polarized by reflection at the surface of glass, in the same manner as light, and with the same circumstances.[25] But when Professor Powell, of Oxford, a few years later (1830), repeated these experiments with a similar apparatus, he found[26] that though the heat which is conveyed along with light is, of course, polarizable, "simple radiant heat," as he terms it, did not offer the smallest difference in the two rectangular azimuths of the second glass, and thus showed no trace of polarization.

Thus, with the old thermometers, the point remained doubtful. But soon after this time, MM. Melloni and Nobili invented an apparatus, depending on certain galvanic laws, of which we shall have to speak hereafter, which they called a *thermomultiplier;* and which was much more sensitive to changes of temperature than any previously-known instrument. Yet even with this instrument, M. Melloni failed; and did not, at first, detect any perceptible polarization of heat by the tourmaline;[27] nor did M. Nobili,[28] in repeating M. Berard's experiment. But in this experiment the attempt was made to polarize heat by reflection from glass, as light is polarized: and the quantity

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[25] *Ann. Chim.* March, 1813. [26] *Edin. Journ. of Science*, 1830, vol. ii. p. 303.
[27] *Ann. de Chimie*, vol. lv. [28] *Bibliothèque Universelle*

reflected is so small that the inevitable errors might completely disguise the whole difference in the two opposite positions. When Prof. Forbes, of Edinburgh, (in 1834,) employed mica in the like experiments, he found a very decided polarizing effect; first, when the heat was transmitted through several films of mica at a certain angle, and afterwards, when it was reflected from them. In this case, he found that with non-luminous heat, and even with the heat of water below the boiling point, the difference of the heating power in the two positions of opposite polarity (parallel and *crossed*) was manifest. He also detected by careful experiments,[29] the polarizing effect of tourmaline. This important discovery was soon confirmed by M. Melloni. Doubts were suggested whether the different effect in the opposite positions might not be due to other circumstances; but Professor Forbes easily showed that these suppositions were inadmissible; and the property of a difference of *sides*, which at first seemed so strange when ascribed to the rays of light, also belongs, it seems to be proved, to the rays of heat. Professor Forbes also found, by interposing a plate of mica to intercept the ray of heat in an intermediate point, an effect was produced in certain positions of the mica analogous to what was called *depolarization* in the case of light; namely, a partial destruction of the differences which polarization establishes.

Before this discovery, M. Melloni had already proved by experiment that heat is *refracted* by transparent substances as light is. In the case of light, the *depolarizing* effect was afterwards found to be really, as we have seen, a *dipolarizing* effect, the ray being divided into two rays by *double refraction*. We are naturally much tempted to put the same interpretation upon the dipolarizing effect in the case of heat; but perhaps the assertion of the analogy between light and heat to this extent is as yet insecure.

It is the more necessary to be cautious in our attempt to identify the laws of light and heat, inasmuch as along with all the resemblances of the two agents, there are very important differences. The power of transmitting light, *the diaphaneity* of bodies, is very distinct from their power of transmitting heat, which has been called *diathermancy* by M. Melloni. Thus both a plate of alum and a plate of rock-salt transmit nearly the whole light; but while the first stops nearly the whole heat, the second stops very little of it; and a plate of opake

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[29] *Ed. R. S. Transactions*, vol. xiv.; and *Phil. Mag.* 1835, vol. v. p. 209. Ib vol. vii. p. 349.

quartz, nearly impenetrable by light, allows a large portion of the heat to pass. By passing the rays through various media, the heat may be, as it were, *sifted* from the light which accompanies it.

[2nd Ed.] [The diathermancy of bodies is distinct from their diaphaneity, in so far that the same bodies do not exercise the same powers of selection and suppression of certain rays on heat and on light; but it appears to be proved by the investigations of modern thermotical philosophers (MM. De la Roche, Powell, Melloni, and Forbes), that there is a close analogy between the absorption of certain colors by transparent bodies, and the absorption of certain kinds of heat by diathermanous bodies. Dark sources of heat emit rays which are analogous to blue and violet rays of light; and highly luminous sources emit rays which are analogous to red rays. And by measuring the angle of total reflection for heat of different kinds, it has been shown that the former kind of calorific rays are really less refrangible than the latter.[30]

M. Melloni has assumed this analogy as so completely established, that he has proposed for this part of thermotics the name *Thermochroology* (Qu. *Chromothermotics?*); and along with this term, many others derived from the Greek, and founded on the same analogy. If it should appear, in the work which he proposes to publish on this subject, that the doctrines which he has to state cannot easily be made intelligible without the use of the terms he suggests, his nomenclature will obtain currency; but so large a mass of etymological innovations is in general to be avoided in scientific works.

M. Melloni's discovery of the extraordinary power of *rock-salt* to transmit heat, and Professor Forbes's discovery of the extraordinary power of *mica* to polarize and depolarize heat, have supplied thermotical inquirers with two new and most valuable instruments.[31]]

Moreover, besides the laws of conduction and radiation, many other laws of the phenomena of heat have been discovered by philosophers; and these must be taken into account in judging any theory of heat. To these other laws we must now turn our attention.

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[30] See Prof. Forbes's *Third Series of Researches on Heat, Edinb. R.S. Trans.* vol. xiv.

[31] For an account of many thermotical researches, which I have been obliged to pass unnoticed here, see two Reports by Prof. Powell on the present state of our knowledge respecting Radiant Heat, in the *Reports of the British Association* for 1832 and 1840.

## CHAPTER II.

### The Laws of Changes occasioned by Heat.

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*Sect.* 1.—*Expansion by Heat.—The Law of Dalton and Gay-Lussac for Gases.*

ALMOST all bodies expand by heat; solids, as metals, in a small degree; fluids, as water, oil, alcohol, mercury, in a greater degree. This was one of the facts first examined by those who studied the nature of heat, because this property was used for the measure of heat. In the *Philosophy of the Inductive Sciences*, Book iv., Chap. iv., I have stated that secondary qualities, such as Heat, must be measured by their effects: and in Sect. 4 of that Chapter I have given an account of the successive attempts which have been made to obtain measures of heat. I have there also spoken of the results which were obtained by comparing the rate at which the expansion of different substances went on, under the same degrees of heat; or as it was called, the different *thermometrical march* of each substance. Mercury appears to be the liquid which is most uniform in its thermometrical march; and it has been taken as the most common material of our thermometers; but the expansion of mercury is not proportional to the heat. De Luc was led, by his experiments, to conclude "that the dilatations of mercury follow an accelerated march for equal augmentations of heat." Dalton conjectured that water and mercury both expand as the square of the *real temperature* from the point of greatest contraction: the real temperature being measured so as to lead to such a result. But none of the rules thus laid down for the expansion of solids and fluids appear to have led, as yet, to any certain general laws.

With regard to gases, thermotical inquirers have been more successful. Gases expand by heat; and their expansion is governed by a law which applies alike to all degrees of heat, and to all gaseous fluids. The law is this: that *for equal increments of temperature they expand by the same fraction of their own bulk;* which fraction is *three-eighths* in proceeding from freezing to boiling water. This law was discovered by Dalton and M. Gay-Lussac independently of each other;[1] and is usually called by both their names, *the law of Dalton and Gay-Lussac*. The latter says,[2] "The experiments which I have described, and which have been made with great care, prove incontestably that oxygen, hydrogen, azotic acid, nitrous acid, ammoniacal acid, muriatic acid, sulphurous acid, carbonic acid, gases, expand equally by equal increments of heat." "Therefore," he adds with a proper inductive generalization, "the result does not depend upon physical properties, and I collect that *all gases expand equally by heat*." He then extends this to vapors, as ether. This must be one of the most important foundation-stones of any sound theory of heat.

[2nd Ed.] Yet MM. Magnus and Regnault conceive that they have overthrown this law of Dalton and Gay-Lussac, and shown that the different gases do not expand alike for the same increment of heat. Magnus found the ratio to be for atmospheric air, 1·366; for hydrogen, 1·365; for carbonic acid, 1·369; for sulphurous-acid gas, 1·385. But these differences are not greater than the differences obtained for the same substances by different observers; and as this law is referred to in Laplace's hypothesis, hereafter to be discussed, I do not treat the law as disproved.

Yet that the rate of expansion of gas in certain circumstances is different for different substances, must be deemed very probable, after Dr. Faraday's recent investigations *On the Liquefaction and Solidification of Bodies generally existing as Gases*,[3] by which it appears that the elasticity of vapors *in contact with their fluids* increases at different rates in different substances. "That the force," he says, "of vapor increases in a geometrical ratio for equal increments of heat is true for all bodies, but the ratio is not the same for all. . . . For an increase of pressure from two to six atmospheres, the following number of degrees require to be added to the bodies named:—water 69°, sulphureous acid 63°, cyanogen 64°·5, ammonia 60°, arseniuretted hydrogen 54°, sulphuretted hydrogen 56°·5, muriatic acid 43°, carbonic acid 32°·5, nitrous oxide 30°."]

We have already seen that the opinion that the air-thermometer is a true measure of heat, is strongly countenanced by the symmetry which, by using it, we introduce into the laws of radiation. If we

[1] *Manch. Mem.* vol. v. 1802; and *Ann. Chim.* xliii. p. 137.

[2] Ib. p. 272.

[3] *Phil. Trans.* 1845, Pt. 1.

accept the law of Dalton and Gay-Lussac, it follows that this result is independent of any peculiar properties in the air employed; and thus this measure has an additional character of generality and simplicity which make it still more probable that it is the true standard. This opinion is further supported by the attempts to include such facts in a theory; but before we can treat of such theories, we must speak of some other doctrines which have been introduced.

### *Sect. 2.—Specific Heat.—Change of Consistence.*

In the attempts to obtain measures of heat, it was found that bodies had different capacities for heat; for the same quantity of heat, however measured, would raise, in different degrees, the temperature of different substances. The notion of different capacities for heat was thus introduced, and each body was thus assumed to have a specific *capacity for heat*, according to the quantity of heat which it required to raise it through a given scale of heat.[4] The term "capacity for heat" was introduced by Dr. Irvine, a pupil of Dr. Black. For this term, Wilcke, the Swedish physicist, substituted "specific heat;" in analogy with "specific gravity."

It was found, also, that the capacity of the same substance was different in the same substance at different temperatures. It appears from experiments of MM. Dulong and Petit, that, in general, the capacity of liquids and solids increases as we ascend in the scale of temperature.

But one of the most important thermotic facts is, that by the sudden contraction of any mass, its temperature is increased. This is peculiarly observable in gases, as, for example, common air. The amount of the increase of temperature by sudden condensation, or of the cold produced by sudden rarefaction, is an important datum, determining the velocity of sound, as we have already seen, and affecting many points of meteorology. The coefficient which enters the calculation in the former case depends on the ratio of two specific heats of air under different conditions; one belonging to it when, varying in density, the pressure is constant by which the air is contained; the other, when, varying in density, it is contained in a constant space.

A leading fact, also, with regard to the operation of heat on bodies

[4] See Crawfurd, *On Heat*, for the History of Specific Heat.

is, that it changes their *form*, as it is often called, that is, their condition as solid, liquid, or air. Since the term "form" is employed in too many and various senses to be immediately understood when it is intended to convey this peculiar meaning, I shall use, instead of it, the term *consistence*, and shall hope to be excused, even when I apply this word to gases, though I must acknowledge such phraseology to be unusual. Thus there is a change of consistence when solids become liquid, or liquids gaseous; and the laws of such changes must be fundamental facts of our thermotical theories. We are still in the dark as to many of the laws which belong to this change; but one of them, of great importance, has been discovered, and to that we must now proceed.

### *Sect. 3.—The Doctrine of Latent Heat.*

The Doctrine of Latent Heat refers to such changes of consistence as we have just spoken of. It is to this effect; that during the conversion of solids into liquids, or of liquids into vapors, there is communicated to the body heat which is not indicated by the thermometer. The heat is absorbed, or becomes *latent;* and, on the other hand, on the condensation of the vapor to a liquid, or the liquid to a solid consistency, this heat is again given out and becomes *sensible.* Thus a pound of ice requires twenty times as long a time, in a warm room, to raise its temperature seven degrees, as a pound of ice-cold water does. A kettle placed on a fire, in four minutes had its temperature raised to the boiling point, 212°: and this temperature continued stationary for twenty minutes, when the whole was boiled away. Dr. Black inferred from these facts that a large quantity of heat is absorbed by the ice in becoming water, and by the water in becoming steam. He reckoned from the above experiments, that ice, in melting, absorbs as much heat as would raise ice-cold water through 140° of temperature: and that water, in evaporating, absorbs as much heat as would raise it through 940°.

That snow requires a great quantity of heat to melt it; that water requires a great quantity of heat to convert it into steam; and that this heat is not indicated by a rise in the thermometer, are facts which it is not difficult to observe; but to separate these from all extraneous conditions, to group the cases together, and to seize upon the general law by which they are connected, was an effort of inductive insight, which has been considered, and deservedly, as one of the most striking events in the modern history of physics. Of this step the principal merit appears to belong to Black.

[2nd Ed.] [In the first edition I had mentioned the names of De Luc and of Wilcke, in connexion with the discovery of Latent Heat, along with the name of Black. De Luc had observed, in 1755, that ice, in melting, did not rise above the freezing-point of temperature till the whole was melted. De Luc has been charged with plagiarizing Black's discovery, but, I think, without any just ground. In his *Idées sur la Météorologique* (1787), he spoke of Dr. Black as "the first who had attempted the determinations of the quantities of latent heat." And when Mr. Watt pointed out to him that from this expression it might be supposed that Black had not discovered the fact itself, he acquiesced, and redressed the equivocal expression in an Appendix to the volume.[5]

Black never published his own account of the doctrine of Latent Heat: but he delivered it every year after 1760 in his Lectures. In 1770, a surreptitious publication of his Lectures was made by a London bookseller, and this gave a view of the leading points of Dr. Black's doctrine. In 1772, Wilcke, of Stockholm, read a paper to the Royal Society of that city, in which the absorption of heat by melting ice is described; and in the same year, De Luc of Geneva published his *Recherches sur les Modifications de l'Atmosphere*, which has been alleged to contain the doctrine of latent heat, and which the author asserts to have been written in ignorance of what Black had done. At a later period, De Luc, adopting, in part, Black's expression, gave the name of *latent fire* to the heat absorbed.[6]

It appears that Cavendish determined the amount of heat produced by condensing steam, and by thawing snow, as early as 1765. He had perhaps already heard something of Black's investigations, but did not accept his term "latent heat."]

The consequences of Black's principle are very important, for upon it is founded the whole doctrine of evaporation; besides which, the principle of latent heat has other applications. But the relations of aqueous vapor to air are so important, and have been so long a sub-

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[5] See his *Letter* to the Editors of the *Edinburgh Review*, No. xii. p. 502, of the *Review*.

[6] See *Ed. Rev.* No. vi. p. 20.

[7] See Mr. V. Harcourt's *Address* to the Brit. Assoc. in 1839, and the *Appendix*.

ject of speculation, that we may with advantage dwell a little upon them. The part of science in which this is done may be called, as we have said, Atmology; and to that division of Thermotics the following chapters belong.

# ATMOLOGY.

## CHAPTER III.

### The Relation of Vapor and Air.

*Sect.* I.—*The Boylean Law of the Air's Elasticity.*

IN the Sixth Book (Chap. IV. Sect. I.) we have already seen how the conception on the laws of fluid equilibrium was, by Pascal and others, extended to air, as well as water. But though air presses and is pressed as water presses and is pressed, pressure produces upon air an effect which it does not, in any obvious degree, produce upon water. Air which is pressed is also *compressed*, or made to occupy a smaller space; and is consequently also made more dense, or *condensed;* and on the other hand, when the pressure upon a portion of air is diminished, the air expands or is rarefied. These broad facts are evident. They are expressed in a general way by saying that air is an *elastic* fluid, yielding in a certain degree to pressure, and recovering its previous dimensions when the pressure is removed.

But when men had reached this point, the questions obviously offered themselves, in what degree and according to what law air yields to pressure; when it is compressed, what relation does the density bear to the pressure? The use which had been made of tubes containing columns of mercury, by which the pressure of portions of air was varied and measured, suggested obvious modes of devising experiments by which this question might be answered. Such experiments accordingly were made by Boyle about 1650; and the result at which he arrived was, that when air is thus compressed, the density is *as* the pressure. Thus if the pressure of the atmosphere in its common state be equivalent to 30 inches of mercury, as shown by the barometer; if air included in a tube be pressed by 30 additional inches of mercury, its density will be doubled, the air being compressed into one half the space. If the pressure be increased threefold, the density is also trebled; and so on. The same law was soon afterwards (in 1676) proved experimentally by Mariotte. And this law of the air's elasticity, that the density is as the pressure, is sometimes called the *Boylean Law*, and sometimes the *Law of Boyle and Mariotte.*

Air retains its aerial character permanently; but there are other aerial substances which appear as such, and then disappear or change into some other condition. Such are termed *vapors.* And the discovery of their true relation to air was the result of a long course of researches and speculations.

[2nd Ed.] [It was found by M. Cagniard de la Tour (in 1823), that at a certain temperature, a liquid, under sufficient pressure, becomes clear transparent vapor or gas, having the same bulk as the liquid. This condition Dr. Faraday calls the *Cagniard de la Tour* state, (the *Tourian* state?) It was also discovered by Dr. Faraday that carbonic-acid gas, and many other gases, which were long conceived to be permanently elastic, are really reducible to a liquid state by pressure.[1] And in 1835, M. Thilorier found the means of reducing liquid carbonic acid to a solid form, by means of the cold produced in evaporation More recently Dr. Faraday has added several substances usually gaseous to the list of those which could previously be shown in the liquid state, and has reduced others, including ammonia, nitrous oxide, and sulphuretted hydrogen, to a solid consistency.[2] After these discoveries, we may, I think, reasonably doubt whether all bodies are not capable of existing in the three *consistencies* of solid, liquid, and air.

We may note that the law of Boyle and Mariotte is not exactly true near the limit at which the air passes to the liquid state in such cases as that just spoken of. The diminution of bulk is then more rapid than the increase of pressure.

The transition of fluids from a liquid to an airy consistence appears to be accompanied by other curious phenomena. See Prof. Forbes's papers on the *Color of Steam under certain circumstances*, and on the *Colors of the Atmosphere*, in the *Edin. Trans.* vol. xiv.]

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[1] *Phil. Trans.* 1823. Ib. Pt. I. 1845.

*Sect. 2.—Prelude to Dalton's Doctrine of Evaporation.*

VISIBLE clouds, smoke, distillation, gave the notion of Vapor; vapor was at first conceived to be identical with air, as by Bacon.[3] It was easily collected, that by heat, water might be converted into vapor. It was thought that air was thus produced, in the instrument called the *æolipile*, in which a powerful blast is caused by a boiling fluid; but Wolfe showed that the fluid was not converted into air, by using camphorated spirit of wine, and condensing the vapor after it had been formed. We need not enumerate the doctrines (if very vague hypotheses may be so termed) of Descartes, Dechales, Borelli.[4] The latter accounted for the rising of vapor by supposing it a mixture of fire and water; and thus, fire being much lighter than air, the mixture also was light. Boyle endeavored to show that vapors do not permanently float *in vacuo*. He compared the mixture of vapor with air to that of salt with water. He found that the pressure of the atmosphere affected the heat of boiling water; a very important fact. Boyle proved this by means of the air-pump; and he and his friends were much surprised to find that when air was removed, water only just warm boiled violently. Huyghens mentions an experiment of the same kind made by Papin about 1673.

The ascent of vapor was explained in various ways in succession, according to the changes which physical science underwent. It was a problem distinctly treated of, at a period when hydrostatics had accounted for many phenomena; and attempts were naturally made to reduce this fact to hydrostatical principles. An obvious hypothesis, which brought it under the dominion of these principles, was, to suppose that the water, when converted into vapor, was divided into small hollow globules;—thin pellicles including air or heat. Halley gave such an explanation of evaporation; Leibnitz calculated the dimensions of these little bubbles; Derham managed (as he supposed) to examine them with a magnifying glass; Wolfe also examined and calculated on the same subject. It is curious to see so much confidence in so lame a theory; for if water became hollow globules in order to rise as vapor, we require, in order to explain the formation of these globules, new laws of nature, which are not even hinted at by

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[3] Bacon's *Hist. Nat.* Cent. i. p. 27.

[4] They may be seen in Fischer, *Geschichte der Physik*, vol. ii. p. 175.

the supporters of the doctrine, though they must be far more complex than the hydrostatical law by which a hollow sphere floats.

Newton's opinion was hardly more satisfactory; he[5] explained evaporation by the repulsive power of heat; the parts of vapors, according to him, being small, are easily affected by this force, and thus become lighter than the atmosphere.

Muschenbroek still adhered to the theory of globules, as the explanation of evaporation; but he was manifestly discontented with it; and reasonably apprehended that the pressure of the air would destroy the frail texture of these bubbles. He called to his aid a rotation of the globules (which Descartes also had assumed); and, not satisfied with this, threw himself on electrical action as a reserve. Electricity, indeed, was now in favor, as hydrostatics had been before; and was naturally called in, in all cases of difficulty. Desaguliers, also, uses this agent to account for the ascent of vapor, introducing it into a kind of sexual system of clouds; according to him, the male fire (heat) does a part, and the female fire (electricity) performs the rest. These are speculations of small merit and no value.

In the mean time, Chemistry made great progress in the estimation of philosophers, and had its turn in the explanation of the important facts of evaporation. Bouillet, who, in 1742, placed the particles of water in the interstices of those of air, may be considered as approaching to the chemical theory. In 1743, the Academy of Sciences of Bourdeaux proposed the ascent of vapors as the subject of a prize; which was adjudged in a manner very impartial as to the choice of a theory; for it was divided between Kratzenstein, who advocated the bubbles, (the coat of which he determined to be 1-50,000th of an inch thick,) and Hamberger, who maintained the truth to be the adhesion of particles of water to those of air and fire. The latter doctrine had become much more distinct in the author's mind when seven years afterwards (1750) he published his *Elementa Physices.* He then gave the explanation of evaporation in a phrase which has since been adopted,—the *solution of water in air;* which he conceived to be of the same kind as other chemical solutions.

This theory of solution was further advocated and developed by Le Roi;[6] and in his hands assumed a form which has been extensively adopted up to our times, and has, in many instances, tinged the language commonly used. He conceived that air, like other solvents,

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[5] *Opticks,* Qu. 31.

[6] *Ac. R. Sc.* Paris, 1750.

might be *saturated ;* and that when the water was beyond the amount required for saturation, it appeared in a visible form. The saturating quantity was held to depend mainly on warmth and wind.

This theory was by no means devoid of merit; for it brought together many of the phenomena, and explained a number of the experiments which Le Roi made. It explained the facts of the transparency of vapor, (for perfect solutions are transparent,) the precipitation of water by cooling, the disappearance of the visible moisture by warming it again, the increased evaporation by rain and wind; and other observed phenomena. So far, therefore, the introduction of the notion of the chemical solution of water in air was apparently very successful. But its defects are of a very fatal kind; for it does not at all apply to the facts which take place when air is excluded.

In Sweden, in the mean time,[7] the subject had been pursued in a different, and in a more correct manner. Wallerius Ericsen had, by various experiments, established the important fact, that water evaporates in a *vacuum*. His experiments are clear and satisfactory; and he inferred from them the falsity of the common explanation of evaporation by the solution of water in *air*. His conclusions are drawn in a very intelligent manner. He considers the question whether water can be changed into air, and whether the atmosphere is, in consequence, a mere collection of vapors; and on good reasons, decides in the negative, and concludes the existence of permanently-elastic air different from vapor. He judges, also, that there are two causes concerned, one acting to produce the first ascent of vapors, the other to support them afterwards. The first, which acts in a vacuum, he conceives to be the mutual repulsion of the particles; and since this force is independent of the presence of other substances, this seems to be a sound induction. When the vapors have once ascended into the air, it may readily be granted that they are carried higher, and driven from side to side by the currents of the atmosphere. Wallerius conceives that the vapor will rise till it gets into air of the same density as itself, and being then in equilibrium, will drift to and fro.

The two rival theories of evaporation, that of *chemical solution* and that of *independent vapor*, were, in various forms, advocated by the next generation of philosophers. De Saussure may be considered as the leader on one side, and De Luc on the other. The former maintained the solution theory, with some modifications of his own. De

[7] Fischer, *Gesch. Phys.* vol. v. p. 63.

Luc denied all solution, and held vapor to be a combination of the particles of water with fire, by which they became lighter than air. According to him, there is always fire enough present to produce this combination, so that evaporation goes on at all temperatures.

This mode of considering independent vapor as a combination of fire with water, led the attention of those who adopted that opinion to the thermometrical changes which take place when vapor is formed and condensed. These changes are important, and their laws curious. The laws belong to the induction of latent heat, of which we have just spoken; but a knowledge of them is not absolutely necessary in order to enable us to understand the manner in which steam exists in air.

De Luc's views led him[8] also to the consideration of the effect of pressure on vapor. He explains the fact that pressure will condense vapor, by supposing that it brings the particles within the distance at which the repulsion arising from fire ceases. In this way, he also explains the fact, that though external pressure does thus condense steam, the mixture of a body of air, by which the pressure is equally increased, will not produce the same effect; and therefore, vapors can exist in the atmosphere. They make no fixed proportion of it; but at the same temperature we have the same pressure arising *from them*, whether they are in air or not. As the heat increases, vapor becomes capable of supporting a greater and greater pressure, and at the boiling heat, it can support the pressure of the atmosphere.

De Luc also marked very precisely (as Wallerius had done) the difference between vapor and air; the former being capable of change of *consistence* by cold or pressure, the latter not so. Pictet, in 1786, made a hygrometrical experiment, which appeared to him to confirm De Luc's views; and De Luc, in 1792, published a concluding essay on the subject in the *Philosophical Transactions*. Pictet's *Essay on Fire*, in 1791, also demonstrated that "all the train of hygrometrical phenomena takes place just as well, indeed rather quicker, in a vacuum than in air, provided the same quantity of moisture is present." This essay, and De Luc's paper, gave the death-blow to the theory of the solution of water in air.

Yet this theory did not fall without an obstinate struggle. It was taken up by the new school of French chemists, and connected with their views of heat. Indeed, it long appears as the prevalent opinion

[8] Fischer, vol. vii. p. 453. *Nouvelles Idées sur la Météorologie*, 1787.

Girtanner,[9] in his *Grounds of the Antiphlogistic Theory*, may be considered as one of the principal expounders of this view of the matter. Hube, of Warsaw, was, however, the strongest of the defenders of the theory of solution, and published upon it repeatedly about 1790. Yet he appears to have been somewhat embarrassed with the increase of the air's elasticity by vapor. Parrot, in 1801, proposed another theory, maintaining that De Luc had by no means successfully attacked that of solution, but only De Saussure's superfluous additions to it.

It is difficult to see what prevented the general reception of the doctrine of independent vapor; since it explained all the facts very simply, and the agency of air was shown over and over again to be unnecessary. Yet, even now, the solution of water in air is hardly exploded. M. Gay Lussac,[10] in 1800, talks of the quantity of water "held in solution" by the air; which, he says, varies according to its temperature and density by a law which has not yet been discovered. And Professor Robison, in the article "Steam," in the *Encyclopædia Britannica* (published about 1800), says,[11] "Many philosophers imagine that spontaneous evaporation, at low temperatures, is produced in this way (by elasticity alone). But we cannot be of this opinion; and must still think that this kind of evaporation is produced by the dissolving power of the air." He then gives some reasons for his opinion. "When moist air is suddenly rarefied, there is always a precipitation of water. But by this new doctrine the very contrary should happen, because the tendency of water to appear in the elastic form is promoted by removing the external pressure." Another main difficulty in the way of the doctrine of the mere mixture of vapor and air was supposed to be this; that if they were so mixed, the heavier fluid would take the lower part, and the lighter the higher part, of the space which they occupied.

The former of these arguments was repelled by the consideration that in the rarefaction of air, its specific heat is changed, and thus its temperature reduced below the constituent temperature of the vapor which it contains. The latter argument is answered by a reference to Dalton's law of the mixture of gases. We must consider the establishment of this doctrine in a new section, as the most material step to the true notion of evaporation.

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[9] Fischer, vol. vii. 473. [10] *Ann. Chim.* tom. xliii

[11] Robison's *Works*, ii. 37.

### *Sect. 3.—Dalton's Doctrine of Evaporation.*

A portion of that which appears to be the true notion of evaporation was known, with greater or less distinctness, to several of the physical philosophers of whom we have spoken. They were aware that the vapor which exists in air, in an invisible state, may be condensed into water by cold: and they had noticed that, in any state of the atmosphere, there is a certain temperature lower than that of the atmosphere, to which, if we depress bodies, water forms upon them in fine drops like dew; this temperature is thence called the *dew-point*. The vapor of water which exists anywhere may be reduced below the degree of heat which is necessary to constitute it vapor, and thus it ceases to be vapor. Hence this temperature is also called the *constituent temperature*. This was generally known to the meteorological speculators of the last century, although, in England, attention was principally called to it by Dr. Wells's *Essay on Dew*, in 1814. This doctrine readily explains how the cold produced by rarefaction of air, descending below the constituent temperature of the contained vapor, may precipitate a dew; and thus, as we have said, refutes one obvious objection to the theory of independent vapor.

The other difficulty was first fully removed by Mr. Dalton. When his attention was drawn to the subject of vapor, he saw insurmountable objections to the doctrine of a chemical union of water and air. In fact, this doctrine was a mere nominal explanation; for, on closer examination, no chemical analogies supported it. After some reflection, and in the sequel of other generalizations concerning gases, he was led to the persuasion, that when air and steam are mixed together, each follows its separate laws of equilibrium, the particles of each being elastic with regard to those of their own kind only: so that steam may be conceived as flowing among the particles of air[12] "like a stream of water among pebbles;" and the resistance which air offers to evaporation arises, not from its weight, but from the inertia of its particles.

It will be found that the theory of independent vapor, understood with these conditions, will include all the facts of the case;—gradual evaporation in air; sudden evaporation in a vacuum; the increase of

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[12] *Manchester Memoirs*, vol. v. p. 581

the air's elasticity by vapor; condensation by its various causes; and other phenomena.

But Mr. Dalton also made experiments to prove his fundamental principle, that if two different gases communicate, they will diffuse themselves through each other;[13]—slowly, if the opening of communication be small. He observes also, that all the gases had equal solvent powers for vapor, which could hardly have happened, had chemical affinity been concerned. Nor does the density of the air make any difference.

Taking all these circumstances into the account, Mr. Dalton abandoned the idea of solution. "In the autumn of 1801," he says, "I hit upon an idea which seemed to be exactly calculated to explain the phenomena of vapor: it gave rise to a great variety of experiments," which ended in fixing it in his mind as a true idea. "But," he adds, "the theory was almost universally misunderstood, and consequently reprobated."

Mr. Dalton answers various objections. Berthollet had urged that we can hardly conceive the particles of an elastic substance added to those of another, without increasing its elasticity. To this Mr. Dalton replies by adducing the instance of magnets, which repel each other, but do not repel other bodies. One of the most curious and ingenious objections is that of M. Gough, who argues, that if each gas is elastic with regard to itself alone, we should hear, produced by one stroke, four sounds; namely, *first*, the sound through aqueous vapor; *second*, the sound through azotic gas; *third*, the sound through oxygen gas; *fourth*, the sound through carbonic acid. Mr. Dalton's answer is, that the difference of time at which these sounds would come is very small; and that, in fact, we do hear, sounds double and treble.

In his *New System of Chemical Philosophy*, Mr. Dalton considers the objections of his opponents with singular candor and impartiality. He there appears disposed to abandon that part of the theory which negatives the mutual repulsion of the particles of the two gases, and to attribute their diffusion through one another to the different size of the particles, which would, he thinks,[14] produce the same effect.

In selecting, as of permanent importance, the really valuable part of this theory, we must endeavor to leave out all that is doubtful or unproved. I believe it will be found that in all theories hitherto promul-

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[13] *New System of Chemical Philosophy*, vol. i. p. 151.

[14] *New System*, vol. i. p. 188.

gated, all assertions respecting the properties of the particles of bodies, their sizes, distances, attractions, and the like, are insecure and superfluous. Passing over, then, such hypotheses, the inductions which remain are these;—that two gases which are in communication will, by the elasticity of each, diffuse themselves in one another, quickly or slowly; and—that the quantity of steam contained in a certain space of air is the same, whatever be the air, whatever be its density, and even if there be a vacuum. These propositions may be included together by saying, that one gas is *mechanically mixed* with another; and we cannot but assent to what Mr. Dalton says of the latter fact,—"this is certainly the touchstone of the mechanical and chemical theories." This *doctrine of the mechanical mixture of gases* appears to supply answers to all the difficulties opposed to it by Berthollet and others, as Mr. Dalton has shown;[15] and we may, therefore, accept it as well established.

This doctrine, along with the *principle of the constituent temperature of steam*, is applicable to a large series of meteorological and other consequences. But before considering the applications of theory to natural phenomena, which have been made, it will be proper to speak of researches which were carried on, in a great measure, in consequence of the use of steam in the arts: I mean the laws which connect its elastic force with its constituent temperature.

### *Sect. 4.—Determination of the Laws of the Elastic Force of Steam.*

The expansion of aqueous vapor at different temperatures is governed, like that of all other vapors, by the law of Dalton and Gay-Lussac, already mentioned; and from this, its elasticity, when its expansion is resisted, will be known by the law of Boyle and Mariotte; namely, by the rule that the pressure of airy fluids is as the condensation. But it is to be observed, that this process of calculation goes on the supposition that the steam is cut off from contact with water, so that no more steam can be generated; a case quite different from the common one, in which the steam is more abundant as the heat is greater. The examination of the force of vapor, when it is in contact with water, must be briefly noticed.

During the period of which we have been speaking, the progress of the investigation of the laws of aqueous vapor was much accelerated

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[15] *New System*, vol. i. p. 160, &c.

by the growing importance of the steam-engine, in which those laws operated in a practical form. James Watts, the main improver of that machine, was thus a great contributor to speculative knowledge, as well as to practical power. Many of his improvements depended on the laws which regulate the quantity of heat which goes to the formation or condensation of steam; and the observations which led to these improvements enter into the induction of latent heat. Measurements of the force of steam, at all temperatures, were made with the same view. Watts's attention had been drawn to the steam-engine in 1759, by Robison, the former being then an instrument-maker, and the latter a student at the University of Glasgow.[16] In 1761 or 1762, he tried some experiments on the force of steam in a Papin's Digester;[17] and formed a sort of working model of a steam-engine, feeling already his vocation to develope the powers of that invention. His knowledge was at that time principally derived from Desaguliers and Belidor, but his own experiments added to it rapidly. In 1764 and 1765, he made a more systematical course of experiments, directed to ascertain the force of steam. He tried this force, however, only at temperatures above the boiling-point; and inferred it at lower degrees from the supposed continuity of the law thus obtained. His friend Robison, also, was soon after led, by reading the account of some experiments of Lord Charles Cavendish, and some others of Mr. Nairne, to examine the same subject. He made out a table of the correspondence of the elasticity and the temperature of vapor, from thirty-two to two hundred and eighty degrees of Fahrenheit's thermometer.[18] The thing here to be remarked, is the establishment of a law of the pressure of steam, down to the freezing-point of water. Ziegler of Basle, in 1769, and Achard of Berlin, in 1782, made similar experiments. The latter examined also the elasticity of the vapor of alcohol. Betancourt, in 1792, published his Memoir on the expansive force of vapors; and his tables were for some time considered the most exact.

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[16] Robison's *Works*, vol. ii. p. 113.

[17] Denis Papin, who made many of Boyle's experiments for him, had discovered that if the vapor be prevented from rising, the water becomes hotter than the usual boiling-point; and had hence invented the instrument called *Papin's Digester*. It is described in his book, *La manière d'amolir les os et de faire cuire toutes sorts de viandes en fort peu de temps et à peu de frais.* Paris, 1682.

[18] These were afterwards published in the *Encyclopædia Britannica;* in the article "Steam," written by Robison.

Prony, in his *Architecture Hydraulique* (1796), established a mathematical formula,[19] on the experiments of Betancourt, who began his researches in the belief that he was first in the field, although he afterwards found that he had been anticipated by Ziegler. Gren compared the experiments of Betancourt and De Luc with his own. He ascertained an important fact, that when water *boils*, the elasticity of the steam is equal to that of the atmosphere. Schmidt at Giessen endeavored to improve the apparatus used by Betancourt; and Biker, of Rotterdam, in 1800, made new trials for the same purpose.

In 1801, Mr. Dalton communicated to the Philosophical Society of Manchester his investigations on this subject; observing truly, that though the forces at high temperatures are most important when steam is considered as a mechanical agent, the progress of philosophy is more immediately interested in accurate observations on the force at low temperatures. He also found that his elasticities for equidistant temperatures resembled a *geometrical progression*, but with a ratio constantly diminishing. Dr. Ure, in 1818, published in the *Philosophical Transactions* of London, experiments of the same kind, valuable from the high temperatures at which they were made, and for the simplicity of his apparatus. The law which he thus obtained approached, like Dalton's, to a *geometrical progression*. Dr. Ure says, that a formula proposed by M. Biot gives an error of near nine inches out of seventy-five, at a temperature of 266 degrees. This is very conceivable, for if the formula be wrong at all, the geometrical progress rapidly inflames the error in the higher portions of the scale. The elasticity of steam, at high temperatures, has also been experimentally examined by Mr. Southern, of Soho, and Mr. Sharpe, of Manchester. Mr. Dalton has attempted to deduce certain general laws from Mr. Sharpe's experiments; and other persons have offered other rules, as those which govern the force of steam with reference to the temperature: but no rule appears yet to have assumed the character of an established scientific truth. Yet the law of the expansive force of steam is not only required in order that the steam-engine may be employed with safety and to the best advantage; but must also be an important point in every consistent thermotical theory.

[2nd Ed.] [To the experiments on steam made by private physicists, are to be added the experiments made on a grand scale by order of the governments of France and of America, with a view to

[19] *Architecture Hydraulique*, Seconde Partie, p. 163.

legislation on the subject of steam-engines. The French experiments were made in 1823, under the direction of a commission consisting of some of the most distinguished members of the Academy of Sciences; namely, MM. de Prony, Arago, Girard, and Dulong. The American experiments were placed in the hands of a committee of the Franklin Institute of the State of Pennsylvania, consisting of Prof. Bache and others, in 1830. The French experiments went as high as 435° of Fahrenheit's thermometer, corresponding to a pressure of 60 feet of mercury, or 24 atmospheres. The American experiments were made up to a temperature of 346°, which corresponded to 274 inches of mercury, more than 9 atmospheres. The extensive range of these experiments affords great advantages for determining the law of the expansive force. The French Academy found that their experiments indicated an increase of the elastic force according to the *fifth* power of a binominal $1 + mt$, where $t$ is the temperature. The American Institute were led to a *sixth* power of a like binominal. Other experimenters have expressed their results, not by powers of the temperature, but by geometrical ratios. Dr. Dalton had supposed that the expansion of mercury being as the square of the true temperature above its freezing-point, the expansive force of steam increases in geometrical ratio for equal increments of temperature. And the author of the article *Steam* in the Seventh Edition of the *Encyclopædia Britannica* (Mr. J. S. Russell), has found that the experiments are best satisfied by supposing mercury, as well as steam, to expand in a geometrical ratio for equal increments of the true temperature.

It appears by such calculation, that while dry gas increases in the ratio of 8 to 11, by an increase of temperature from freezing to boiling water; steam in contact with water, by the same increase of temperature above boiling water, has its expansive force increased in the proportion of 1 to 12. By an equal increase of temperature, mercury expands in about the ratio of 8 to 9.

Recently, MM. Magnus of Berlin, Holzmann and Regnault, have made series of observations on the relation between temperature and elasticity of steam.[20]

Prof. Magnus measured his temperatures by an air-thermometer; a process which, I stated in the first edition, seemed to afford the best promise of simplifying the law of expansion. His result is, that the

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[20] See Taylor's *Scientific Memoirs*, Aug. 1845, vol iv. part xiv., and *Ann. de Chimie.*

elasticity proceeds in a geometric series when the temperature proceeds in an arithmetical series nearly; the differences of temperature for equal augmentations of the ratio of elasticity being somewhat greater for the higher temperatures.

The forces of the vapors of other liquids in contact with their liquids, determined by Dr. Faraday, as mentioned in Chap. ii. Sect. 1, are analogous to the elasticity of steam here spoken of.]

### *Sect. 5.—Consequences of the Doctrine of Evaporation.—Explanation of Rain, Dew, and Clouds.*

The discoveries concerning the relations of heat and moisture which were made during the last century, were principally suggested by meteorological inquiries, and were applied to meteorology as fast as they rose. Still there remains, on many points of this subject, so much doubt and obscurity, that we cannot suppose the doctrines to have assumed their final form; and therefore we are not here called upon to trace their progress and connexion. The principles of atmology are pretty well understood; but the difficulty of observing the conditions under which they produce their effects in the atmosphere is so great, that the precise theory of most meteorological phenomena is still to be determined.

We have already considered the answers given to the question: According to what rules does transparent aqueous vapor resume its form of visible water? This question includes, not only the problems of Rain and Dew, but also of Clouds; for clouds are not vapor, but water, vapor being always invisible. An opinion which attracted much notice in its time, was that of Hutton, who, in 1784, endeavored to prove that if two masses of air saturated with transparent vapor at different temperatures are mixed together, the precipitation of water in the form either of cloud or of drops will take place. The reason he assigned for the opinion was this: that the temperature of the mixture is a mean between the two temperatures, but that the force of the vapor in the mixture, which is the mean of the forces of the two component vapors, will be greater than that which corresponds to the mean temperature, since the force increases faster than the temperature;[21] and hence some part of the vapor will be precipitated. This doctrine, it will be seen, speaks of vapor as "saturating" air, and is

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[21] *Edin. Trans.* vol. i. p. 42.

therefore, in this form, inconsistent with Dalton's principle; but it is not difficult to modify the expression so as to retain the essential part of the explanation.

*Dew.*—The principle of a "constituent temperature" of steam, and the explanation of the "dew-point," were known, as we have said (chap. iii. sect. 3,) to the meteorologists of the last century; but we perceive how incomplete their knowledge was, by the very gradual manner in which the consequences of this principle were traced out. We have already noticed, as one of the books which most drew attention to the true doctrine, in this country at least, Dr. Wells's *Essay on Dew*, published in 1814. In this work the author gives an account of the progress of his opinions;[22] "I was led," he says, "in the autumn of 1784, by the event of a rude experiment, to think it probable that the formation of dew is attended with the production of cold." This was confirmed by the experiments of others. But some years after, "upon considering the subject more closely, I began to suspect that Mr. Wilson, Mr. Six, and myself, had all committed an error in regarding the cold which accompanies the dew, as an *effect* of the formation of the dew." He now considered it rather as the *cause:* and soon found that he was able to account for the circumstances of this formation, many of them curious and paradoxical, by supposing the bodies on which dew is deposited, to be cooled down, by radiation into the clear night-sky, to the proper temperature. The same principle will obviously explain the formation of mists over streams and lakes when the air is cooler than the water; which was put forward by Davy, even in 1819, as a new doctrine, or at least not familiar.

*Hygrometers.*—According as air has more or less of vapor in comparison with that which its temperature and pressure enable it to contain, it is more or less humid; and an instrument which measures the degrees of such a gradation is a *hygrometer.* The hygrometers which were at first invented, were those which measured the moisture by its effect in producing expansion or contraction in certain organic substances; thus De Saussure devised a hair-hygrometer, De Luc a whalebone-hygrometer, and Dalton used a piece of whipcord. All these contrivances were variable in the amount of their indications under the same circumstances; and, moreover, it was not easy to know the physical meaning of the degree indicated. The dew-point, or constituent temperature of the vapor which exists in the air, is, on

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[22] *Essay on Dew*, p. 1.

the other hand, both constant and definite. The determination of this point, as a datum for the moisture of the atmosphere, was employed by Le Roi, and by Dalton (1802), the condensation being obtained by cold water:[23] and finally, Mr. Daniell (1812) constructed an instrument, where the condensing temperature was produced by evaporation of ether, in a very convenient manner. This invention (*Daniell's Hygrometer*) enables us to determine the quantity of vapor which exists in a given mass of the atmosphere at any time of observation.

[2nd Ed.] [As a happy application of the Atmological Laws which have been discovered, I may mention the completion of the theory and use of the *Wet-bulb Hygrometer;* an instrument in which, from the depression of temperature produced by wetting the bulb of a thermometer, we infer the further depression which would produce *dew*. Of this instrument the history is thus summed up by Prof. Forbes:—"Hutton invented the method; Leslie revived and extended it, giving probably the earliest, though an imperfect theory; Gay-Lussac, by his excellent experiments and reasoning from them, completed the theory, so far as perfectly dry air is concerned; Ivory extended the theory; which was reduced to practice by Auguste and Bohnenberger, who determined the constant with accuracy. English observers have done little more than confirm the conclusions of our industrious Germanic neighbors; nevertheless the experiments of Apjohn and Prinsep must ever be considered as conclusively settling the value of the coefficient near the one extremity of the scale, as those of Kæmtz have done for the other."[24]

Prof. Forbes's two Reports *On the Recent Progress and Present State of Meteorology* given among the *Reports of the British Association* for 1832 and 1840, contain a complete and luminous account of recent researches on this subject. It may perhaps be asked why I have not given Meteorology a place among the Inductive Sciences; but if the reader refers to these accounts, or any other adequate view of the subject, he will see that Meteorology is not a single Inductive Science, but the application of several sciences to the explanation of terrestrial and atmospheric phenomena. Of the sciences so applied, Thermotics and Atmology are the principal ones. But others also come into play; as Optics, in the explanation of Rainbows, Halos,

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[23] Daniell, *Met. Ess.* p. 142. *Manch. Mem.* vol. v. p. 581.

[24] *Second Report on Meteorology*, p. 101.

Parhelia, Coronæ, Glories, and the like; Electricity, in the explanation of Thunder and Lightning, Hail, Aurora Borealis; to which others might be added.]

*Clouds.*—When vapor becomes visible by being cooled below its constituent temperature, it forms itself into a very fine watery powder, the diameter of the particles of which this powder consists being very small: they are estimated by various writers, from 1-100,000th to 1-20,000th of an inch.[25] Such particles, even if solid, would descend very slowly; and very slight causes would suffice for their suspension, without recurring to the hypothesis of vesicles, of which we have already spoken. Indeed that hypothesis will not explain the fact, except we suppose these vesicles filled with a rarer air than that of the atmosphere; and, accordingly, though this hypothesis is still maintained by some,[26] it is asserted as a fact of observation, proved by optical or other phenomena, and not deduced from the suspension of clouds. Yet the latter result is still variously explained by different philosophers: thus, M. Gay-Lussac[27] accounts for it by upward currents of air, and Fresnel explains it by the heat and rarefaction of air in the interior of the cloud.

*Classification of Clouds.*—A classification of clouds can then only be consistent and intelligible when it rests upon their atmological conditions. Such a system was proposed by Mr. Luke Howard, in 1802–3. His primary modifications are, *Cirrus*, *Cumulus*, and *Stratus*, which the Germans have translated by terms equivalent in English to *feather-cloud*, *heap-cloud*, and *layer-cloud*. The cumulus increases by accumulations on its top, and floats in the air with a horizontal base; the stratus grows from below, and spreads along the earth; the cirrus consists of fibres in the higher regions of the atmosphere, which grow every way. Between the simple modifications are intermediate ones, *cirro-cumulus* and *cirro-stratus*; and, again, compound ones, the *cumulo-stratus* and the *nimbus*, or *rain-cloud*. These distinctions have been generally accepted all over Europe: and have rendered a description of all the processes which go on in the atmosphere far more definite and clear than it could be made before their use.

I omit a mass of facts and opinions, supposed laws of phenomena and assigned causes, which abound in meteorology more than in any other science. The slightest consideration will show us what a great

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[25] Kæmtz, *Met.* i. 393

[26] Ib. i. 393. Robison, ii. 13

[27] *Ann. Chim.* xxv. 1822.

amount of labor, of persevering and combined observation, the progress of this branch of knowledge requires. I do not even speak of the condition of the more elevated parts of the atmosphere. The diminution of temperature as we ascend, one of the most marked of atmospheric facts, has been variously explained by different writers. Thus Dalton[28] (1808) refers it to a principle "that each atom of air, in the same perpendicular column, is possessed of the same degree of heat," which principle he conceives to be entirely empirical in this case. Fourier says[29] (1817), "This phenomenon results from several causes: one of the principal is the progressive extinction of the rays of heat in the successive strata of the atmosphere."

Leaving, therefore, the application of thermotical and atmological principles in particular cases, let us consider for a moment the general views to which they have led philosophers.

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## CHAPTER IV.

### Physical Theories of Heat.

WHEN we look at the condition of that branch of knowledge which, according to the phraseology already employed, we must call *Physical Thermotics*, in opposition to Formal Thermotics, which gives us detached laws of phenomena, we find the prospect very different from that which was presented to us by physical astronomy, optics, and acoustics. In these sciences, the maintainers of a distinct and comprehensive theory have professed at least to show that it explains and includes the principal laws of phenomena of various kinds; in Thermotics, we have only attempts to explain a part of the facts. We have here no example of an hypothesis which, assumed in order to explain one class of phenomena, has been found also to account exactly for another; as when central forces led to the precession of the equinoxes, or when the explanation of polarization explained also double refraction; or when the pressure of the atmosphere, as measured by the barometer, gave the true velocity of sound. Such coincidences, or *consiliences*, as I have elsewhere called them, are the test of truth; and thermotical theories cannot yet exhibit credentials of this kind.

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[28] *New Syst. of Chem.* vol. i. p. 125.

[29] *Ann. Chim.* vi. 285.

On looking back at our view of this science, it will be seen that it may be distinguished into two parts; the Doctrines of Conduction and Radiation, which we call Thermotics proper; and the Doctrines respecting the relation of Heat, Airs, and Moisture, which we have termed Atmology. These two subjects differ in their bearing on our hypothetical views.

*Thermotical Theories.*—The phenomena of radiant heat, like those of radiant light, obviously admit of general explanation in two different ways;—by the emission of material particles, or by the propagation of undulations. Both these opinions have found supporters. Probably most persons, in adopting Prevost's theory of exchanges, conceive the radiation of heat to be the radiation of matter. The undulation hypothesis, on the other hand, appears to be suggested by the production of heat by friction, and was accordingly maintained by Rumford and others. Leslie[1] appears, in a great part of his *Inquiry*, to be a supporter of some undulatory doctrine, but it is extremely difficult to make out what his undulating medium is; or rather, his opinions wavered during his progress. In page 31, he asks, "What is this calorific and frigorific fluid?" and after keeping the reader in suspense for a moment, he replies,

"Quod petis hic est.

It is merely the ambient AIR." But at page 150, he again asks the question, and, at page 188, he answers, "It is the same subtile matter that, according to its different modes of existence, constitutes either heat or light." A person thus vacillating between two opinions, one of which is palpably false, and the other laden with exceeding difficulties which he does not even attempt to remove, had little right to protest against[2] "the sportive freaks of some intangible *aura*;" to rank all other hypotheses than his own with the "occult qualities of the schools;" and to class the "prejudices" of his opponents with the tenets of those who maintained the *fuga vacui* in opposition to Torricelli. It is worth while noticing this kind of rhetoric, in order to observe, that it may be used just as easily on the wrong side as on the right.

Till recently, the theory of material heat, and of its propagation by emission, was probably the one most in favor with those who had studied mathematical thermotics. As we have said, the laws of con-

[1] *An Experimental Inquiry into the Nature and Propagation of Heat*, 1804.
[2] Ib. p. 47.

duction, in their ultimate analytical form, were almost identical with the laws of motion of fluids. Fourier's principle also, that the radiation of heat takes place from points below the surface, and is intercepted by the superficial particles, appears to favor the notion of material emission.

Accordingly, some of the most eminent modern French mathematicians have accepted and extended the hypothesis of a material caloric. In addition to Fourier's doctrine of molecular extra-radiation, Laplace and Poisson have maintained the hypothesis of *molecular intra-radiation*, as the mode in which conduction takes place; that is, they say that the particles of bodies are to be considered as *discrete*, or as points separated from each other, and acting on each other at a distance; and the conduction of heat from one part to another, is performed by radiation between all neighboring particles. They hold that, without this hypothesis, the differential equations expressing the conditions of conduction cannot be made homogeneous: but this assertion rests, I conceive, on an error, as Fourier has shown, by dispensing with the hypothesis. The necessity of the hypothesis of discrete molecular action in bodies, is maintained in all cases by M. Poisson; and he has asserted Laplace's theory of capillary attraction to be defective on this ground, as Laplace asserted Fourier's reasoning respecting heat to be so. In reality, however, this hypothesis of discrete molecules cannot be maintained as a physical truth; for the law of molecular action, which is assumed in the reasoning, after answering its purpose in the progress of calculation, vanishes in the result; the conclusion is the same, whatever law of the intervals of the molecules be assumed. The definite integral, which expresses the whole action, no more proves that this action is actually made of the differential parts by means of which it was found, than the processes of finding the weight of a body by integration, prove it to be made up of differential weights. And therefore, even if we were to adopt the emission theory of heat, we are by no means bound to take along with it the hypothesis of discrete molecules.

But the recent discovery of the refraction, polarization, and depolarization of heat, has quite altered the theoretical aspect of the subject, and, almost at a single blow, ruined the emission theory. Since heat is reflected and refracted like light, analogy would lead us to conclude that the mechanism of the processes is the same in the two cases. And when we add to these properties the property of polarization, it is scarcely possible to believe otherwise than that heat consists in trans verse vibrations; for no wise philosopher would attempt an explanation by ascribing poles to the emitted particles, after the experience which Optics affords, of the utter failure of such machinery.

But here the question occurs, If heat consists in vibrations, whence arises the extraordinary identity of the laws of its propagation with the laws of the flow of matter? How is it that, in conducted heat, this vibration creeps slowly from one part of the body to another, the part first heated remaining hottest; instead of leaving its first place and travelling rapidly to another, as the vibrations of sound and light do? The answer to these questions has been put in a very distinct and plausible form by that distinguished philosopher, M. Ampère, who published a *Note on Heat and Light considered as the results of Vibratory Motion*,[8] in 1834 and 1835; and though this answer is an hypothesis, it at least shows that there is no fatal force in the difficulty.

M. Ampère's hypothesis is this; that bodies consist of solid molecules, which may be considered as arranged at intervals in a very rare ether; and that the vibrations of the molecules, causing vibrations of the ether and caused by them, constitute heat. On these suppositions, we should have the phenomena of conduction explained; for if the molecules at one end of a bar be hot, and therefore in a state of vibration, while the others are at rest, the vibrating molecules propagate vibrations in the ether, but these vibrations do not produce heat, except in proportion as they put the quiescent molecules of the bar in vibration; and the ether being very rare compared with the molecules, it is only by the repeated impulses of many successive vibrations that the nearest quiescent molecules are made to vibrate; after which they combine in communicating the vibration to the more remote molecules. "We then find necessarily," M. Ampère adds, "the same equations as those found by Fourier for the distribution of heat, setting out from the same hypothesis, that the temperature or heat transmitted is proportional to the difference of the temperatures."

Since the undulatory hypothesis of heat can thus answer all obvious objections, we may consider it as upon its trial, to be confirmed or modified by future discoveries; and especially by an enlarged knowledge of the laws of the polarization of heat.

[2nd Ed.] [Since the first edition was written, the analogies between light and heat have been further extended, as I have already stated. It

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[8] *Bibliothèque Universelle de Genève*, vol. xlix. p. 225. *Ann. Chim.* tom. lvii p. 434.

has been discovered by MM. Biot and Melloni that quartz impresses a circular polarization upon heat; and by Prof. Forbes that mica, of a certain thickness, produces phenomena such as would be produced by the impression of circular polarization of the supposed transversal vibrations of radiant heat; and further, a rhomb of rock-salt, of the shape of the glass rhomb which verified Fresnel's extraordinary anticipation of the circular polarization of light, verified the expectation, founded upon other analogies, of the polarization of heat. By passing polarized heat through various thicknesses of mica, Prof. Forbes has attempted to calculate the length of an undulation for heat.

These analogies cannot fail to produce a strong disposition to believe that light and heat, essences so closely connected that they can hardly be separated, and thus shown to have so many curious properties in common, are propagated by the same machinery; and thus we are led to an Undulatory Theory of Heat.

Yet such a Theory has not yet by any means received full confirmation. It depends upon the analogy and the connexion of the Theory of Light, and would have little weight if those were removed. For the separation of the rays in double refraction, and the phenomena of periodical intensity, the two classes of facts out of which the Undulatory Theory of Optics principally grew, have neither of them been detected in thermotical experiments. Prof. Forbes has assumed alternations of heat for increasing thicknesses of mica, but in his experiments we find only one *maximum*. The occurrence of alternate maxima and minima under the like circumstances would exhibit visible waves of heat, as the fringes of shadows do of light, and would thus add much to the evidence of the theory.

Even if I conceived the Undulatory Theory of Heat to be now established, I should not venture, as yet, to describe its establishment as an event in the history of the Inductive Sciences. It is only at an interval of time after such events have taken place that their history and character can be fully understood, so as to suggest lessons in the Philosophy of Science.]

*Atmological Theories.*—Hypotheses of the relations of heat and air almost necessarily involve a reference to the forces by which the composition of bodies is produced, and thus cannot properly be treated of, till we have surveyed the condition of chemical knowledge. But we may say a few words on one such hypothesis; I mean the hypothesis on the subject of the atmological laws of heat, proposed by Laplace, in the twelfth Book of the *Mécanique Céleste*, and published in 1823.

It will be recollected that the main laws of pheuomena for which we have to account, by meaus of such an hypothesis, are the following :—

(1.) The law of Boyle and Mariotte, that the elasticity of an air varies as its density. See Chap. iii., Sect. 1 of this Book.

(2.) The Law of Gay-Lussac and Dalton, that all airs expand equally by heat. See Chap. ii. Sect. 1.

(3.) The production of heat by sudden compression. See Chap. ii. Sect. 2.

(4.) Dalton's principle of the mechanical mixture of airs. See Chap. iii. Sect. 3.

(5.) The Law of expansion of solids and fluids by heat. See Chap. ii. Sect. 1.

(6.) Changes of consistence by heat, and the doctrine of latent heat. See Chap. ii. Sect. 3.

(7.) The Law of the expansive force of steam. See Chap. iii. Sect. 4.

Besides these, there are laws of which it is doubtful whether they are or are not included in the preceding, as the low temperature of the air in the higher parts of the atmosphere. (See Chap. iii. Sect. 5.)

Laplace's hypothesis[4] is this :—that bodies consist of particles, each of which gathers round it, by its attraction, a quantity of caloric : that the particles of the bodies attract each other, besides attracting the caloric, aud that the particles of the caloric repel each other.

In gases, the particles of the bodies are so far removed, that their mutual attraction is insensible, and the matter tends to expand by the mutual repulsion of the caloric. He conceives this caloric to be constantly radiating among the particles; the density of this internal radiation is the *temperature*, and he proves that, on this supposition, the elasticity of the air will be as the density, and as this temperature. Hence follow the three first rules above stated. The same suppositions lead to Dalton's principle of mixtures (4), though without involving his mode of conception ; for Laplace says that whatever the mutual action of two gases be, the whole pressure will be equal to the sum of the separate pressures.[5] Expansion (5), and the changes of consistence (6), are explained by supposing[6] that in solids, the mutual attraction of the particles of the body is the greatest force ; in liquids, the attraction of the particles for the caloric ; in airs, the repulsion of

[4] *Méc. Cél.* t. v p. 89. [5] Ib. p. 110. [6] Ib. p. 92.

the caloric. But the doctrine of latent heat again modifies[7] the hypothesis, and makes it necessary to include latent heat in the calculation; yet there is not, as we might suppose there would be if the theory were the true one, any confirmation of the hypothesis resulting from the new class of laws thus referred to. Nor does it appear that the hypothesis accounts for the relation between the elasticity and the temperature of steam.

It will be observed that Laplace's hypothesis goes entirely upon the materiality of heat, and is inconsistent with any vibratory theory; for, as Ampère remarks, "It is clear that if we admit heat to consist in vibrations, it is a contradiction to attribute to heat (or caloric) a repulsive force of the particles which would be a cause of vibration."

An unfavorable judgment of Laplace's Theory of Gases is suggested by looking for that which, in speaking of Optics, was mentioned as the great characteristic of a true theory; namely, that the hypotheses, which were assumed in order to account for one class of facts, are found to explain another class of a different nature:—the consilience of inductions. Thus, in thermotics, the law of an intensity of radiation proportional to the sine of the angle of the ray with the surface, which is founded on direct experiments of radiation, is found to be necessary in order to explain the tendency of neighboring bodies to equality of temperature; and this leads to the higher generalization, that heat is radiant from points below the surface. But in the doctrine of the relation of heat to gases, as delivered by Laplace, there is none of this unexpected confirmation; and though he explains some of the leading laws, his assumptions bear a large proportion to the laws explained. Thus, from the assumption that the repulsion of gases arises from the mutual repulsion of the particles of caloric, he finds that the pressure in any gas is as the square of the density and of the quantity of caloric;[8] and from the assumption that the temperature is the internal radiation, he finds that this temperature is as the density and the square of the caloric.[9] Hence he obtains the law of Boyle and Mariotte, and that of Dalton and Gay-Lussac. But this view of the subject requires other assumptions when we come to latent heat; and accordingly, he introduces, to express the latent heat, a new quantity.[10] Yet this quantity produces no effect on his calculations, nor does he apply his reasoning to any problem in which latent heat is concerned.

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[7] *Méc. Cél.* t. v. p. 93.

[8] $P = 2\,\pi\,H\,K\,\rho^2 c^2$ (1) p. 107.

[9] $q'\,\Pi\,(a) = \rho c^2$ (2) p. 108.

[10] The quantity $i$, p. 113.

Without, then, deciding upon this theory, we may venture to say that it is wanting in all the prominent and striking characteristics which we have found in those great theories which we look upon as clearly and indisputably established.

*Conclusion.*—We may observe, moreover, that heat has other bearings and effects, which, as soon as they have been analysed into numerical laws of phenomena, must be attended to in the formation of thermotical theories. Chemistry will probably supply many such; those which occur to us, we must examine hereafter. But we may mention as examples of such, MM. De la Rive and Marcet's law, that the specific heat of all gases is the same;[11] and MM. Dulong and Petit's law, that single atoms of all simple bodies have the same capacity for heat.[12] Though we have not yet said anything of the relation of different gases, or explained the meaning of *atoms* in the chemical sense, it will easily be conceived that these are very general and important propositions.

Thus the science of Thermotics, imperfect as it is, forms a highly instructive part of our survey; and is one of the cardinal points on which the doors of those chambers of physical knowledge must turn which hitherto have remained closed. For, on the one hand, this science is related by strong analogies and dependencies to the most complete portions of our knowledge, our mechanical doctrines and optical theories; and on the other, it is connected with properties and laws of a nature altogether different,—those of chemistry; properties and laws depending upon a new system of notions and relations, among which clear and substantial general principles are far more difficult to lay hold of, and with which the future progress of human knowledge appears to be far more concerned. To these notions and relations we must now proceed; but we shall find an intermediate stage, in certain subjects which I shall call the *Mechanico-chemical* Sciences; viz., those which have to do with Magnetism, Electricity, and Galvanism.

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[11] *Ann. Chim.* xxxv. (1827.) [12] Ib. x. 397.

# BOOK XI.

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*THE MECHANICO-CHEMICAL SCIENCES.*

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# HISTORY OF ELECTRICITY.

Parva metu primo: mox sese extollit in auras,
Ingrediturque solo, et caput inter nubila condit.

*Æn.* iv. 176.

A timid breath at first, a transient touch,
How soon it swells from little into much!
Runs o'er the ground, and springs into the air,
And fills the tempest's gloom, the lightning's glare;
While denser darkness than the central storm
Conceals the secrets of its inward form.

## INTRODUCTION.

### *Of the Mechanico-Chemical Sciences.*

UNDER the title of Mechanico-Chemical Sciences, I include the laws of Magnetism, Electricity, Galvanism, and the other classes of phenomena closely related to these, as Thermo-electricity. This group of subjects forms a curious and interesting portion of our physical knowledge; and not the least of the circumstances which give them their interest, is that double bearing upon mechanical and chemical principles, which their name is intended to imply. Indeed, at first sight they appear to be purely Mechanical Sciences; the attractions and repulsions, the pressure and motion, which occur in these cases, are referrible to mechanical conceptions and laws, as completely as the weight or fall of terrestrial bodies, or the motion of the moon and planets. And if the phenomena of magnetism and electricity had directed us only to such laws, the corresponding sciences must have been arranged as branches of mechanics. But we find that, on the other side, these phenomena have laws and bearings of a kind altogether different. Magnetism is associated with Electricity by its mechanical analogies; and, more recently, has been discovered to be still more closely connected with it by physical influence; electric is identified with galvanic agency; but in galvanism, decomposition, or some action of that kind, universally appears; and these appearances lead to very general laws. Now composition and decomposition are the subjects of Chemistry; and thus we find that we are insensibly but irresistibly led into the domain of that science. The highest generalizations to which we can look, in advancing from the elementary facts of electricity and galvanism, must involve chemical notions; we must therefore, in laying out the platform of these sciences, make provision for that convergence of mechanical and chemical theory, which they are to exhibit as we ascend.

We must begin, however, with stating the mechanical phenomena of these sciences, and the reduction of such phenomena to laws. In this point of view, the phenomena of which we have to speak are those in which bodies exhibit attractions and repulsions, peculiarly determined by their nature and circumstances; as the magnet, and a piece of amber when rubbed. Such results are altogether different from the universal attraction which, according to Newton's discovery, prevails among all particles of matter, and to which cosmical phenomena are owing. But yet the difference of these special attractions, and of cosmical attraction, was at first so far from being recognized, that the only way in which men could be led to conceive or assent to an action of one body upon another at a distance, in cosmical cases, was by likening it to magnetic attraction, as we have seen in the history of Physical Astronomy. And we shall, in the first part of our account, not dwell much upon the peculiar conditions under which bodies are magnetic or electric, since these conditions are not readily reducible to mechanical laws; but, taking the magnetic or electric character for granted, we shall trace its effects.

The habit of considering magnetic action as the type or general case of attractive and repulsive agency, explains the early writers having spoken of Electricity as a kind of Magnetism. Thus Gilbert, in his book *De Magnete* (1600), has a chapter,[1] *De coitione Magnitica, primumque de Succini attractione, sive verius corporum ad Succinum applicatione.* The manner in which he speaks, shows us how mysterious the fact of attraction then appeared; so that, as he says, "the magnet and amber were called in aid by philosophers as illustrations, when our sense is in the dark in abstruse inquiries, and when our reason can go no further. Gilbert speaks of these phenomena like a genuine inductive philosopher, reproving[2] those who before him had "stuffed the booksellers' shops by copying from one another extravagant stories concerning the attraction of magnets and amber, without giving any reason from experiment." He himself makes some important steps in the subject. He distinguishes magnetic from *electric* forces,[3] and is the inventor of the latter name, derived from ἤλεκτρον, *electron*, amber. He observes rightly, that the electric force attracts all light bodies, while the magnetic force attracts iron only; and he devises a satisfactory apparatus by which this is shown. He gives[4] a considerable list of bodies which possess the electric property; "Not only amber and agate attract small bodies, as some think, but diamond, sapphire, carbuncle, opal, amethyst, Bristol gem, beryl, crystal, glass, glass of antimony, spar of various kinds, sulphur, mastic, sealing-wax," and other substances which he mentions. Even his speculations on the general laws of these phenomena, though vague and erroneous, as

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[1] Lib. ii. cap. 2. [2] *De Magnete*, p. 48. [3] Ib. p. 52. [4] Ib. p. 48.

at that period was unavoidable, do him no discredit when compared with the doctrines of his successors a century and a half afterwards. But such speculations belong to a succeeding part of this history.

In treating of these Sciences, I will speak of Electricity in the first place; although it is thus separated by the interposition of Magnetism from the succeeding subjects (Galvanism, &c.) with which its alliance seems, at first sight, the closest, and although some general notions of the laws of magnets were obtained at an earlier period than a knowledge of the corresponding relations of electric phenomena: for the theory of electric attraction and repulsion is somewhat more simple than of magnetic; was, in fact, the first obtained; and was of use in suggesting and confirming the generalization of magnetic laws.

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## CHAPTER I.

### Discovery of Laws of Electric Phenomena.

WE have already seen what was the state of this branch of knowledge at the beginning of the seventeenth century, and the advances made by Gilbert. We must now notice the additions which it subsequently received, and especially those which led to the discovery of general laws, and the establishment of the theory; events of this kind being those of which we have more peculiarly to trace the conditions and causes. Among the facts which we have thus especially to attend to, are the electric attractions of small bodies by amber and other substances when rubbed. Boyle, who repeated and extended the experiments of Gilbert, does not appear to have arrived at any new general notions; but Otto Guericke of Magdeburg, about the same time, made a very material step, by discovering that there was an electric force of repulsion as well as of attraction. He found that when a globe of sulphur had attracted a feather, it afterwards repelled it, till the feather had been in contact with some other body. This, when verified under a due generality of circumstances, forms a capital fact in our present subject. Hawkesbee, who wrote in 1709 (*Physico-Mechanical Experiments,*) also observed various of the effects of attraction and repulsion upon threads hanging loosely. But the person who appears to have first fully seized the general law of these facts, is

Dufay, whose experiments appear in the Memoirs of the French Academy, in 1733, 1734, and 1737.[1] "I discovered," he says, "a very simple principle, which accounts for a great part of the irregularities, and, if I may use the term, the caprices that seem to accompany most of the experiments in electricity. This principle is, that electric bodies attract all those that are not so, and repel them as soon as they are become electric by the vicinity or contact of the electric body. . . . Upon applying this principle to various experiments of electricity, any one will be surprised at the number of obscure and puzzling facts which it clears up." By the help of this principle, he endeavors to explain several of Hawkesbee's experiments.

A little anterior to Dufay's experiments were those of Grey, who, in 1729, discovered the properties of *conductors*. He found that the attraction and repulsion which appear in electric bodies are exhibited also by other bodies in contact with the electric. In this manner he found that an ivory ball, connected with a glass tube by a stick, a wire, or a packthread, attracted and repelled a feather, as the glass itself would have done. He was then led to try to extend this communication to considerable distances, first by ascending to an upper window and hanging down his ball, and, afterwards, by carrying the string horizontally supported on loops. As his success was complete in the former case, he was perplexed by failure in the latter; but when he supported the string by loops of silk instead of hempen cords. he found it again become a conductor of electricity. This he ascribed at first to the smaller thickness of the silk, which did not carry off so much of the electric virtue; but from this explanation he was again driven, by finding that wires of brass still thinner than the silk destroyed the effect. Thus Grey perceived that the efficacy of the support depended on its being silk, and he soon found other substances which answered the same purpose. The difference, in fact, depended on the supporting substance being electric, and therefore not itself a conductor; for it soon appeared from such experiments, and especially[2] from those made by Dufay, that substances might be divided into *electrics per se*, and *non-electrics*, or *conductors*. These terms were introduced by Desaguliers,[3] and gave a permanent currency to the results of the labors of Grey and others.

Another very important discovery belonging to this period is, that

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[1] Priestley's *History of Electricity*, p. 45, and the Memoirs quoted

[2] *Mem. Acad. Par.* 1734. [3] Priestley, p. 66.

of the two kinds of electricity. This also was made by Dufay. "Chance," says he, "has thrown in my way another principle more universal and remarkable than the preceding one, and which casts a new light upon the subject of electricity. The principle is, that there are two distinct kinds of electricity, very different from one another; one of which I call *vitreous*, the other *resinous*, electricity. The first is that of glass, gems, hair, wool, &c.; the second is that of amber, gum-lac, silk, &c. The characteristic of these two electricities is, that they repel themselves and attract each other." This discovery does not, however, appear to have drawn so much attention as it deserved. It was published in 1735; (in the Memoirs of the Academy *for* 1733:) and yet in 1747, Franklin and his friends at Philadelphia, who had been supplied with electrical apparatus and information by persons in England well acquainted with the then present state of the subject, imagined that they were making observations unknown to European science, when they were led to assert two conditions of bodies, which were in fact the opposite electricities of Dufay, though the American experimenters referred them to a single element, of which electrized bodies might have either excess or defect. "Hence," Franklin says, "have arisen some new terms among us: we say B," who receives a spark from glass, "and bodies in like circumstances, is electrized *positively*; A," who communicates his electricity to glass, "*negatively*; or rather B is electrized *plus*, A *minus*." Dr. (afterwards Sir William) Watson had, about the same time, arrived at the same conclusions, which he expresses by saying that the electricity of A was *more rare*, and that of B *more dense*, than it naturally would have been.[4] But that which gave the main importance to this doctrine was its application to some remarkable experiments, of which we must now speak.

Electric action is accompanied, in many cases, by light and a crackling sound. Otto Guericke[5] observes that his sulphur-globe, when rubbed in a dark place, gave faint flashes, such as take place when sugar is crushed. And shortly after, a light was observed at the surface of the mercury in the barometer, when shaken, which was explained at first by Bernoulli, on the then prevalent Cartesian principles; but. afterwards, more truly by Hawkesbee, as an electrical phenomenon. Wall, in 1708, found sparks produced by rubbing amber, and Hawkesbee observed the light and the *snapping*, as he calls it, under various modifications. But the electric spark from a living body, which, as

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[4] Priestley, p. 115. [5] *Experimenta Magdeburgica*, 1672, lib. iv. cap. 15.

Priestley says,[6] "makes a principal part of the diversion of gentlemen and ladies who come to see experiments in electricity," was first observed by Dufay and the Abbé Nollet. Nollet says[7] he "shall never forget the surprise which the first electric spark ever drawn from the human body excited, both in M. Dufay and in himself." The drawing of a spark from the human body was practised in various forms, one of which was familiarly known as the "electrical kiss." Other exhibitions of electrical light were the electrical star, electrical rain, and the like.

As electricians determined more exactly the conditions of electrical action, they succeeded in rendering more intense those sudden actions which the spark accompanies, and thus produced the electric *shock*. This was especially done in the *Leyden phial*. This apparatus received its name, while the discovery of its property was attributed to Cunæus, a native of Leyden, who, in 1746, handling a vessel containing water in communication with the electrical machine, and happening thus to bring the inside and the outside into connexion, received a sudden shock in his arms and breast. It appears, however,[8] that a shock had been received under nearly the same circumstances in 1745, by Von Kleist, a German prelate, at Camin, in Pomerania. The strangeness of this occurrence, and the suddenness of the blow, much exaggerated the estimate which men formed of its force. Muschenbroek, after taking one shock, declared he would not take a second for the kingdom of France; though Boze, with a more magnanimous spirit, wished[9] that he might die by such a stroke, and have the circumstances of the experiment recorded in the Memoirs of the Academy. But we may easily imagine what a new fame and interest this discovery gave to the subject of electricity. It was repeated in all parts of the world, with various modifications: and the shock was passed through a line of several persons holding hands; Nollet, in the presence of the king of France, sent it through a circle of 180 men of the guards, and along a line of men and wires of 900 toises;[10] and experiments of the same kind were made in England, principally under the direction of Watson, on a scale so large as to excite the admiration of Muschenbroek; who says, in a letter to Watson, "Magnificentissimis tuis experimentis superasti conatus omnium." The result was, that the transmission of electricity through a length of 12,000 feet was, to sense, instantaneous.

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[6] P. p. 47. [7] Priestley, p. 47. Nollet, *Leçons de Physique*, vol. vi. p. 408
[8] Fischer, v. 490. [9] Fischer, p. 84. [10] Ibid. v. 512.

The essential circumstances of the electric shock were gradually unravelled. Watson found that it did not increase in proportion either to the contents of the phial or the size of the globe by which the electricity was excited; that the outside coating of the glass (which, in the first form of the experiment, was only a film of water), and its contents, might be varied in different ways. To Franklin is due the merit of clearly pointing out most of the circumstances on which the efficacy of the Leyden phial depends. He showed, in 1747,[11] that the inside of the bottle is electrized positively, the outside negatively; and that the shock is produced by the restoration of the equilibrium, when the outside and inside are brought into communication suddenly. But in order to complete this discovery, it remained to be shown that the electric matter was collected entirely at the surface of the glass, and that the opposite electricities on the two opposite sides of the glass were accumulated by their mutual attraction. Monnier the younger discovered that the electricity which bodies can receive, depends upon their surface rather than their mass, and Franklin[12] soon found that "the whole force of the bottle, and power of giving a shock, is in the glass itself." This they proved by decanting the water out of an electrized into another bottle, when it appeared that the second bottle did not become electric, but the first remained so. Thus it was found "that the non-electrics, in contact with the glass, served only to unite the force of the several parts."

So far as the effect of the coating of the Leyden phial is concerned, this was satisfactory and complete: but Franklin was not equally successful in tracing the action of the electric matter upon itself, in virtue of which it is accumulated in the phial; indeed, he appears to have ascribed the effect to some property of the glass. The mode of describing this action varied, accordingly as two electric *fluids* were supposed (with Dufay,) or one, which was the view taken by Franklin. On this latter supposition the parts of the electric fluid repel each other, and the excess in one surface of the glass expels the fluid from the other surface. This kind of action, however, came into much clearer view in the experiments of Canton, Wilcke, and Æpinus. It was principally manifested in the attractions and repulsions which objects exert when they are in the neighborhood of electrized bodies; or in the *electrical atmosphere*, using the phraseology of the time. At present we say that bodies are electrized *by induction*, when they are

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[11] *Letters*, p. 13.

[12] *Letters*, iv. Sect. 16.

thus made electric by the electric attraction and repulsion of other bodies. Canton's experiments were communicated to the Royal Society in 1753, and show that the electricity on each body acts upon the electricity of another body, at a distance, with a repulsive energy. Wilcke, in like manner, showed that parts of non-electrics, plunged in electric atmospheres, acquire an electricity opposite to that of such atmospheres. And Æpinus devised a method of examining the nature of the electricity at any part of the surface of a body, by means of which he ascertained its distribution, and found that it agreed with such a law of self-repulsion. His attempt to give mathematical precision to this induction was one of the most important steps towards electrical theory, and must be spoken of shortly, in that point of view. But in the mean time we may observe, that this doctrine was applied to the explanation of the Leyden jar; and the explanation was confirmed by charging a plate of air, and obtaining a shock from it, in a manner which the theory pointed out.

Before we proceed to the history of the theory, we must mention some other of the laws of phenomena which were noticed, and which theory was expected to explain. Among the most celebrated of these, were the effect of sharp points in conductors, and the phenomena of electricity in the atmosphere. The former of these circumstances was one of the first which Franklin observed as remarkable. It was found that the points of needles and the like throw off and draw off the electric virtue; thus a bodkin, directed towards an electrized ball, at six or eight inches' distance, destroyed its electric action. The latter subject, involving the consideration of thunder and lightning, and of many other meteorological phenomena, excited great interest. The comparison of the electric spark to lightning had very early been made; but it was only when the discharge had been rendered more powerful in the Leyden jar, that the comparison of the effects became very plausible. Franklin, about 1750, had offered a few somewhat vague conjectures[13] respecting the existence of electricity in the clouds; but it was not till Wilcke and Æpinus had obtained clear notions of the effect of electric matter at a distance, that the real condition of the clouds could be well understood. In 1752, however,[14] D'Alibard, and other French philosophers, were desirous of verifying Franklin's conjecture of the analogy of thunder and electricity. This they did by erecting a pointed iron rod, forty feet high,

[13] Letter v.

[14] Franklin, p. 107

at Marli: the rod was found capable of giving out electrical sparks when a thunder-cloud passed over the place. This was repeated in various parts of Europe, and Franklin suggested that a communication with the clouds might be formed by means of a kite. By these, and similar means, the electricity of the atmosphere was studied by Canton in England, Mazeas in France, Beccaria in Italy, and others elsewhere. These essays soon led to a fatal accident, the death of Richman at Petersburg, while he was, on Aug. 6th, 1753, observing the electricity collected from an approaching thunder-cloud, by means of a rod which he called an electrical gnomon: a globe of blue fire was seen to leap from the rod to the head of the unfortunate professor, who was thus struck dead.

[2nd Ed.] [As an important application of the doctrines of electricity, I may mention the contrivances employed to protect ships from the effects of lightning. The use of conductors in such cases is attended with peculiar difficulties. In 1780 the French began to turn their attention to this subject, and Le Roi was sent to Brest and the various sea-ports of France for that purpose. Chains temporarily applied in the rigging had been previously suggested, but he endeavored to place, he says, such conductors in ships as might be fixed and durable. He devised certain long linked rods, which led from a point in the mast-head along a part of the rigging, or in divided stages along the masts, and were fixed to plates of metal in the ship's sides communicating with the sea. But these were either unable to stand the working of the rigging, or otherwise inconvenient, and were finally abandoned.[15]

The conductor commonly used in the English Navy, till recently, consisted of a flexible copper chain, tied, when occasion required, to the mast-head, and reaching down into the sea; a contrivance recommended by Dr. Watson in 1762. But notwithstanding this precaution, the shipping suffered greatly from the effects of lightning.

Mr. Snow Harris (now Sir William Snow Harris), whose electrical labors are noticed above, proposed to the Admiralty, in 1820, a plan which combined the conditions of ship-conductors, so desirable, yet so difficult to secure:—namely, that they should be permanently fixed, and sufficiently large, and yet should in no way interfere with the motion of the rigging, or with the sliding masts. The method which he proposed was to make the masts themselves conductors of electricity,

[15] See Le Roi's Memoir in the *Hist. Acad. Sc.* for 1790.

by incorporating with them, in a peculiar way, two laminæ of sheet-copper, uniting these with the metallic masses in the hull by other laminæ, and giving the whole a free communication with the sea. This method was tried experimentally, both on models and to a large extent in the navy itself; and a Commission appointed to examine the result reported themselves highly satisfied with Mr. Harris's plan, and strongly recommended that it should be fully carried out in the Navy.[16]]

It is not here necessary to trace the study of atmospheric electricity any further: and we must now endeavor to see how these phenomena and laws of phenomena which we have related, were worked up into consistent theories; for though many experimental observations and measures were made after this time, they were guided by the theory, and may be considered as having rather discharged the office of confirming than of suggesting it.

We may observe also that we have now described the period of most extensive activity and interest in electrical researches. These naturally occurred while the general notions and laws of the phenomena were becoming, and were not yet become, fixed and clear. At such a period, a large and popular circle of spectators and amateurs feel themselves nearly upon a level, in the value of their trials and speculations, with more profound thinkers: at a later period, when the subject is become a science, that is, a study in which all must be left far behind who do not come to it with disciplined, informed, and logical minds, the cultivators are far more few, and the shout of applause less tumultuous and less loud. We may add, too, that the experiments, which are the most striking to the senses, lose much of their impressiveness with their novelty. Electricity, to be now studied rightly, must be reasoned upon mathematically; how slowly such a mode of study makes its way, we shall see in the progress of the theory, which we must now proceed to narrate.

[2nd Ed.] [A new mode of producing electricity has excited much notice lately. In October, 1840, one of the workmen in attendance upon a boiler belonging to the Newcastle and Durham Railway reported that the boiler was full of fire; the fact being, that when he placed his hand near it an electrical spark was given out. This drew the attention of Mr. Armstrong and Mr. Pattinson, who made the circumstance publicly known.[17] Mr. Armstrong pursued the investigation

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[16] See Mr. Snow Harris's paper in *Phil. Mag.* March, 1841.

[17] *Phil. Mag.* Oct. 1840.

with great zeal, and after various conjectures was able to announce[13] that the electricity was excited at the point where the steam is subject to friction in its emission. He found too that he could produce a like effect by the emission of condensed air. Following out his views, he was able to construct, for the Polytechnic Institution in London, a "Hydro-electric Machine," of greater power than any electrical machine previously made. Dr. Faraday took up the investigation as the subject of the Eighteenth Series of his *Researches*, sent to the Royal Society Jan. 26, 1842; and in this he illustrated, with his usual command of copious and luminous experiments, a like view;—that the electricity is produced by the friction of the particles of the water carried along by the stream. And thus this is a new manifestation of that electricity, which, to distinguish it from voltaic electricity, is sometimes called *Friction Electricity* or *Machine Electricity*. Dr. Faraday has, however, in the course of this investigation, brought to light several new electrical relations of bodies.]

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## CHAPTER II.

### The Progress of Electrical Theory.

THE cause of electrical phenomena, and the mode of its operation, were naturally at first spoken of in an indistinct and wavering manner. It was called the electric *fire*, the electric *fluid*; its effects were attributed to *virtues*, *effluvia*, *atmospheres*. When men's mechanical ideas became somewhat more distinct, the motions and tendencies to motion were ascribed to *currents*, in the same manner as the cosmical motions had been in the Cartesian system. This doctrine of currents was maintained by Nollet, who ascribed all the phenomena of electrized bodies to the contemporaneous afflux and efflux of electrical matter. It was an important step towards sound theory, to get rid of this notion of moving fluids, and to consider attraction and repulsion as statical forces; and this appears to have been done by others about the same time. Dufay[1] considered that he had proved the existence of two electricities, the vitreous and the resinous, and conceived each

[13] *Phil. Mag.* Jan. 1842, dated Dec. 9, 1841.

[1] *Ac. Par.* 1733, p. 467.

of these to be a fluid which repelled its own parts and attracted those of the other: this is, in fact, the outline of the theory which recently has been considered as the best established; but from various causes it was not at once, or at least not generally adopted. The hypothesis of the excess and defect of a single fluid is capable of being so treated as to give the same results with the hypothesis of two opposite fluids, and happened to obtain the preference for some time. We have already seen that this hypothesis, according to which electric phenomena arose from the excess and defect of a generally diffused fluid, suggested itself to Watson and Franklin about 1747. Watson found that when an electric body was excited, the electricity was not created, but collected; and Franklin held, that when the Leyden jar was charged, the quantity of electricity was unaltered, though its distribution was changed. Symmer[2] maintained the existence of two fluids; and Cigna supplied the main defect which belonged to this tenet in the way in which Dufay held it, by showing that the two opposite electricities were usually produced at the same time. Still the apparent simplicity of the hypothesis of one fluid procured it many supporters. It was that which Franklin adopted, in his explanation of the Leyden experiment; and though after the first conception of an electrical charge as a disturbance of equilibrium, there was nothing in the development or details of Franklin's views which deserved to win for them any peculiar authority, his reputation, and his skill as a writer, gave a considerable influence to his opinions. Indeed, for a time he was considered, over a large part of Europe, as the creator of the science, and the terms[3] *Franklinism*, *Franklinist*, *Franklinian system*, occur in almost every page of continental publications on the subject. Yet the electrical phenomena to the knowledge of which Franklin added least, those of induction, were those by which the progress of the theory was most promoted. These, as we have already said, were at first explained by the hypothesis of electrical atmospheres. Lord Mahon wrote a treatise, in which this hypothesis was mathematically treated; yet the hypothesis was very untenable, for it would not account for the most obvious cases of induction, such as the Leyden jar, except the atmosphere was supposed to penetrate glass.

The phenomena of electricity by induction, when fairly considered by a person of clear notions of the relations of space and force, were seen to accommodate themselves very generally to the conception

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[2] *Phil. Trans.* 1759. Priestley, p. 160.

introduced by Dufay;[4] of two electricities each repelling itself and attracting the other. If we suppose that there is only one fluid, which repels itself and attracts all other matter, we obtain, in many cases, the same general results as if we suppose two fluids; thus, if an electrized body, overcharged with the single fluid, act upon a ball, it drives the electric fluid in the ball to the further side by its repulsion, and then attracts the ball by attracting the matter of the ball more than it repels the fluid which is upon the ball. If we suppose two fluids, the positively electrized body draws the negative fluid to the nearer side of the ball, repels the positive fluid to the opposite side, and attracts the ball on the whole, because the attracted fluid is nearer than that which is repelled. The verification of either of these hypotheses, and the determination of their details, depended necessarily upon experiment and calculation. It was under the hypothesis of a single fluid that this trial was first properly made. Æpinus of Petersburg published, in 1759, his *Tentamen Theoriæ Electricitatis et Magnetismi;* in which he traces mathematically the consequences of the hypothesis of an electric fluid, attracting all other matter, but repelling itself; the law of force of this repulsion and attraction he did not pretend to assign precisely, confining himself to the supposition that the mutual force of the particles increases as the distance decreases. But it was found, that in order to make this theory tenable, an additional supposition was required, namely, that the particles of bodies repel each other as much as they attract the electric fluid.[5] For if two bodies, A and B, be in their natural electrical condition, they neither attract nor repel each other. Now, in this case, the fluid in A attracts the matter in B and repels the fluid in B with equal energy, and thus no tendency to motion results from the fluid in A; and if we further suppose that the *matter* in A attracts the fluid in B and *repels the matter* in B with equal energy, we have the resulting mutual inactivity of the two bodies explained; but without the latter supposition, there would be a mutual attraction: or we may put the truth more simply thus; two negatively electrized bodies repel each other; if negative electrization were merely the abstraction of the fluid which is the repulsive element, this result could not follow except there were a repulsion in the bodies themselves, independent of the fluid. And thus Æpinus found himself compelled to assume this mutual repulsion of material particles; he had, in fact, the alter-

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[4] *Mém. A. P.* 1733, p. 467. [5] Robison, vol. iv. p. 18.

native of this supposition, or that of two fluids, to choose between, for the mathematical results of both hypotheses are the same. Wilcke, a Swede, who had at first asserted and worked out the Æpinian theory in its original form, afterwards inclined to the opinion of Symmer; and Coulomb, when, at a later period, he confirmed the theory by his experiments and determined the law of force, did not hesitate to prefer[6] the theory of two fluids, "because," he says, "it appears to me contradictory to admit at the same time, in the particles of bodies, an attractive force in the inverse ratio of the squares of the distances, which is demonstrated by universal gravitation, and a repulsive force in the same inverse ratio of the squares of the distances; a force which would necessarily be infinitely great relatively to the action of gravitation." We may add, that by forcing us upon this doctrine of the universal repulsion of matter, the theory of a single fluid seems quite to lose that superiority in the way of simplicity which had originally been its principal recommendation.

The mathematical results of the supposition of Æpinus, which are, as Coulomb observes,[7] the same as of that of the two fluids, were traced by the author himself, in the work referred to, and shown to agree, in a great number of cases, with the observed facts of electrical induction, attraction, and repulsion. Apparently this work did not make its way very rapidly through Europe; for in 1771, Henry Cavendish stated[8] the same hypothesis in a paper read before the Royal Society; which he prefaces by saying, "Since I first wrote the following paper, I find that this way of accounting for the phenomena of electricity is not new. Æpinus, in his *Tentamen Theoriæ Electricitatis et Magnetismi*, has made use of the same, or nearly the same hypothesis that I have; and the conclusions he draws from it agree nearly with mine as far as he goes."

The confirmation of the theory was, of course, to be found in the agreement of its results with experiment; and in particular, in the facts of electrical induction, attraction, and repulsion, which suggested the theory. Æpinus showed that such a confirmation appeared in a number of the most obvious cases; and to these, Cavendish added others, which, though not obvious, were of such a nature that the calculations, in general difficult or impossible, could in these instances be easily performed; as, for example, cases in which there are plates or globes at the two extremities of a long wire. In all these cases of

[6] *Mém. Ac. P.* 1788, p. 671. [7] *Ac. P.* 1788, p. 672.
[8] *Phil. Trans.* 1771, vol. lxi.

electrical action the theory was justified. But in order to give it full confirmation, it was to be considered whether any other facts, not immediately assumed in the foundation of the theory, were explained by it; a circumstance which, as we have seen, gave the final stamp of truth to the theories of astronomy and optics. Now we appear to have such confirmation, in the effect of points, and in the phenomena of the electrical discharge. The theory of neither of these was fully understood by Cavendish, but he made an approach to the true view of them. If one part of a conducting body be a sphere of small radius, the electric fluid upon the surface of this sphere will, it appears by calculation, be more dense, and tend to escape more energetically, in proportion as the radius of the sphere is smaller; and, therefore, if we consider a point as part of the surface of a sphere of imperceptible radius, it follows from the theory that the effort of the fluid to escape at that place will be enormous; so that it may easily be supposed to overcome the resisting causes. And the discharge may be explained in nearly the same manner; for when a conductor is brought nearer and nearer to an electrized body, the opposite electricity is more and more accumulated by attraction on the side next to the electrized body; its tension becomes greater by the increase of its quantity and the diminution of the distance, and at last it is too strong to be contained, and leaps out in the form of a spark.

The light, sound, and mechanical effects produced by the electric discharge, made the electric *fluid* to be not merely considered as a mathematical hypothesis, useful for reducing phenomena to formulæ (as for a long time the magnetic fluid was), but caused it to be at once and universally accepted as a physical reality, of which we learn the existence by the common use of the senses, and of which measures and calculations are only wanted to teach us the laws.

The applications of the theory of electricity which I have principally considered above, are those which belong to conductors, in which the electric fluid is perfectly moveable, and can take that distribution which the forces require. In non-conducting or electric bodies, the conditions to which the fluid is subject are less easy to determine; but by supposing that the fluid moves with great difficulty among the particles of such bodies,—that nevertheless it may be dislodged and accumulated in parts of the surface of such bodies, by friction and other modes of excitement; and that the earth is an inexhaustible reservoir of electric matter,—the principal facts of excitation and the like receive a tolerably satisfactory explanation.

The theory of Æpinus, however, still required to have the law of action of the particles of the fluid determined. If we were to call to mind how momentous an event in physical astronomy was the determination of the law of the cosmical forces, the inverse square of the distance, and were to suppose the importance and difficulty of the analogous step in this case to be of the same kind, this would be to mistake the condition of science at that time. The leading idea, the conception of the possibility of explaining natural phenomena by means of the action of forces, on rigorously mechanical principles, had already been promulgated by Newton, and was, from the first, seen to be peculiarly applicable to electrical phenomena; so that the very material step of clearly proposing the problem, often more important than the solution of it, had already been made. Moreover the confirmation of the truth of the assumed cause in the astronomical case depended on taking the right law; but the electrical theory could be confirmed, in a general manner at least, without this restriction. Still it was an important discovery that the law of the inverse square prevailed in these as well as in cosmical attractions.

It was impossible not to conjecture beforehand that it would be so. Cavendish had professed in his calculations not to take the exponent of the inverse power, on which the force depended, to be strictly 2, but to leave it indeterminate between 1 and 3; but in his applications of his results, he obviously inclines to the assumption that it is 2. Experimenters tried to establish this in various ways. Robison,[9] in 1769, had already proved that the law of force is very nearly or exactly the inverse square; and Meyer[10] had discovered, but not published, the same result. The clear and satisfactory establishment of this truth is due to Coulomb, and was one of the first steps in his important series of researches on this subject. In his first paper [11] in the *Memoirs* of the Academy for 1785, he proves this law for small globes; in his second Memoir he shows it to be true for globes one and two feet in diameter. His invention of the *torsion-balance*, which measures very small forces with great certainty and exactness, enabled him to set this question at rest for ever.

The law of force being determined for the particles of the electric fluid, it now came to be the business of the experimenter and the

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[9] *Works*, iv. p. 68.

[10] *Biog. Univ.* art *Coulomb*, by Biot.

[11] *Mém. A. P.* 1785, pp. 569, 578.

mathematician to compare the results of the theory in detail with those of experimental measures. Coulomb undertook both portions of the task. He examined the electricity of portions of bodies by means of a little disk (his *tangent plane*) which he applied to them and then removed, and which thus acted as a sort of electric *taster*. His numerical results (the intensity being still measured by the torsion-balance) are the fundamental facts of the theory of the electrical fluid Without entering into detail, we may observe that he found the electricity to be entirely collected at the surface of conductors (which Beccaria had before shown to be the case), and that he examined and recorded the electric intensity at the surface of globes, cylinders, and other conducting bodies, placed within each other's influence in various ways.

The mathematical calculation of the distribution of two fluids, all the particles of which attract and repel each other according to the above law, was a problem of no ordinary difficulty; as may easily be imagined, when it is recollected that the attraction and repulsion determine the distribution, and the distribution reciprocally determines the attraction and repulsion. The problem was of the same nature as that of the figure of the earth; and its rigorous solution was beyond the powers of the analysis of Coulomb's time. He obtained, however, approximate solutions with much ingenuity; for instance, in a case in which it was obvious that the electric fluid would be most accumulated at and near the equator of a certain sphere, he calculated the action of the sphere on two suppositions: first, that the fluid was all collected precisely at the equator; and next, that it was uniformly diffused over the surface; and he then assumed the actual case to be intermediate between these two. By such artifices he was able to show that the results of his experiments and of his calculations gave an agreement sufficiently near to entitle him to consider the theory as established on a solid basis.

Thus, at this period, mathematics was behind experiment; and a problem was proposed, in which theoretical numerical results were wanted for comparison with observation, but could not be accurately obtained; as was the case in astronomy also, till the time of the approximate solution of the Problem of Three Bodies, and the consequent formation of the Tables of the Moon and Planets on the theory of universal gravitation. After some time, electrical theory was relieved from this reproach, mainly in consequence of the progress which astronomy had occasioned in pure mathematics. About 1801, there appeared in the *Bulletin des Sciences*,[12] an exact solution of the problem of the distribution of electric fluid on a spheroid, obtained by M. Biot, by the application of the peculiar methods which Laplace had invented for the problem of the figure of the planets. And in 1811, M. Poisson applied Laplace's artifices to the case of two spheres acting upon one another in contact, a case to which many of Coulomb's experiments were referrible; and the agreement of the results of theory and observation, thus extricated from Coulomb's numbers, obtained above forty years previously, was very striking and convincing.[13] It followed also from Poisson's calculations, that when two electrized spheres are brought near each other, the accumulation of the opposite electricities on their nearest points increases without limit as the spheres approach to contact; so that before the contact takes place, the external resistance will be overcome, and a *spark* will pass.

Though the relations of non-conductors to electricity, and various other circumstances, leave many facts imperfectly explained by the theory, yet we may venture to say that, as a theory which gives the laws of the phenomena, and which determines the distribution of those elementary forces, on the surface of electrized bodies, from which elementary forces (whether arising from the presence of a fluid or not,) the total effects result, the doctrine of Dufay and Coulomb, as developed in the analysis of Poisson, is securely and permanently established. This part of the subject has been called *statical electricity*. In the establishment of the theory of this branch of science, we must, I conceive, allow to Dufay more merit than is generally ascribed to him; since he saw clearly, and enunciated in a manner which showed that he duly appreciated their capital character, the two chief principles,—the conditions of electrical attraction and repulsion, and the apparent existence of two kinds of electricity. His views of attraction are, indeed, partly expressed in terms of the Cartesian hypothesis of vortices, then prevalent in France; but, at the time when he wrote, these forms of speech indicated scarcely anything besides the power of attraction. Franklin's real merit as a discoverer was, that he was one of the first who distinctly conceived the electrical *charge* as a derangement of equilibrium. The great fame which, in his day, he enjoyed, arose from the clearness and spirit with which he narrated his discoveries; from his dealing with electricity in the imposing form of thunder and lightning; and partly, perhaps, from his character as an

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[12] No. li.

[13] *Mém. A. P.* 1811.

American and a politician; for he was already, in 1736, engaged in public affairs as clerk to the General Assembly of Pennsylvania, though it was not till a later period of his life that his admirers had the occasion of saying of him

> Eripuit cœlis fulmen sceptrumque tyrannis;
>
> Born to control all lawless force, all fierce and baleful sway,
> The thunder's bolt, the tyrant's rod, alike he wrenched away.

Æpinus and Coulomb were two of the most eminent physical philosophers of the last century, and labored in the way peculiarly required by that generation; whose office it was to examine the results, in particular subjects, of the general conception of attraction and repulsion, as introduced by Newton. The reasonings of the Newtonian period had, in some measure, anticipated all possible theories resembling the electrical doctrine of Æpinus and Coulomb; and, on that account, this doctrine could not be introduced and confirmed in a sudden and striking manner, so as to make a great epoch. Accordingly, Dufay, Symmer, Watson, Franklin, Æpinus and Coulomb, have all a share in the process of induction. With reference to these founders of the theory of electricity, Poisson holds the same place which Laplace holds with reference to Newton.

The reception of the Coulombian theory (so we must call it, for the Æpinian theory implies one fluid only,) has hitherto not been so general as might have been reasonably expected from its very beautiful accordance with the facts which it contemplates. This has partly been owing to the extreme abstruseness of the mathematical reasoning which it employs, and which put it out of the reach of most experimenters and writers of works of general circulation. The theory of Æpinus was explained by Robison in the *Encyclopædia Britannica;* the analysis of Poisson has recently been presented to the public in the *Encyclopædia Metropolitana*, but is of a kind not easily mastered even by most mathematicians. On these accounts probably it is, that in English compilations of science, we find, even to this day, the two theories of one and of two fluids stated as if they were nearly on a par in respect of their experimental evidence. Still we may say that the Coulombian theory is probably assented to by all who have examined it, at least as giving the laws of phenomena; and I have not heard of any denial of it from such a quarter, or of any attempt to show it to be erroneous by detailed and measured experiments. Mr. Snow Harris has recently[14] described some important experiments and measures; but his apparatus was of such a kind that the comparison of the results with the Coulombian theory was not easy ; and indeed the mathematical problems which Mr. Harris's combinations offered, require another Poisson for their solution. Still the more obvious results are such as agree with the theory, even in the cases in which their author considered them to be inexplicable. For example, he found that by doubling the quantity of electricity of a conductor, it attracted a body with four times the force ; but the body not being insulated, would have its electricity also doubled by induction, and thus the fact was what the theory required.

Though it is thus highly probable that the Coulombian theory of electricity (or the Æpinian, which is mathematically equivalent) will stand as a true representation of the law of the elementary actions, we must yet allow that it has not received that complete evidence, by means of experiments and calculations added to those of its founders, which the precedents of other permanent sciences have led us to look for. The experiments of Coulomb, which he used in the establishment of the theory, were not very numerous, and they were limited to a peculiar form of bodies, namely spheres. In order to form the proper *sequel* to the promulgation of this theory, to give a full *confirmation*, and to ensure its general *reception*, we ought to have experiments more numerous and more varied (such as those of Mr. Harris are) shown to agree in all respects with results calculated from the theory. This would, as we have said, be a task of labor and difficulty ; but the person who shall execute it will deserve to be considered as one of the real founders of the true doctrine of electricity. To show that the coincidence between theory and observation, which has already been proved for spherical conductors, obtains also for bodies of other forms, will be a step in electricity analogous to what was done in astronomy, when it was shown that the law of gravitation applied to comets as well as to planets.

But although we consider the views of Æpinus or Coulomb in a very high degree probable as a *formal theory*, the question is very different when we come to examine them as a *physical theory* ;—that is, when we inquire whether there really is a material electric fluid or fluids.

*Question of One or Two Fluids.*—In the first place as to the question whether the fluids are one or two ;—Coulomb's introduction of

[14] *Phil. Trans.* 1834, p. 2.

the hypothesis of two fluids has been spoken of as a reform of the theory of Æpinus; it would probably have been more safe to have called his labors an advance in the calculation, and in the comparison of hypothesis with experiment, than to have used language which implied that the question, between the rival hypotheses of one or two fluids, could be treated as settled. For, in reality, if we assume, as Æpinus does, the mutual repulsion of all the particles of matter, in addition to the repulsion of the particles of the electric fluid for one another and their attraction for the particles of matter, the one fluid of Æpinus will give exactly the same results as the two fluids of Coulomb. The mathematical formulæ of Coulomb and of Poisson express the conditions of the one case as well as of the other; the interpretation only being somewhat different. The place of the forces of the resinous fluid is supplied by the excess of the forces ascribed to the matter above the forces of the fluid, in the parts where the electric fluid is deficient.

The obvious argument against this hypothesis is, that we ascribe to the particles of matter a mutual repulsion, in addition to the mutual attraction of universal gravitation, and that this appears incongruous. Accordingly, Æpinus says, that when he was first driven to this proposition it horrified him.[15] But we may answer it in this way very satisfactorily:—If we suppose the mutual repulsion of matter to be somewhat less than the mutual attraction of matter and electric fluid, it will follow, as a consequence of the hypothesis, that besides all obvious electrical action, the particles of matter would attract each other with forces varying inversely as the square of the distance. Thus gravitation itself becomes an electrical phenomenon, arising from the residual excess of attraction over repulsion; and the fact which is urged against the hypothesis becomes a confirmation of it. By this consideration the prerogative of simplicity passes over to the side of the hypothesis of one fluid; and the rival view appears to lose at least all its superiority.

Very recently, M. Mosotti[16] has calculated the results of the Æpinian theory in a far more complete manner than had previously been performed; using Laplace's coefficients, as Poisson had done for the Cou-

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[15] Neque diffiteor cum ipsa se mihi offerret . . . . me ad ipsam quodammodo exhorruisse. *Tentamen Theor. Elect.* p. 39.

[16] *Sur les Forces qui régissent la Constitution Intérieure des Corps.* Turin. 1836.

lombian theory. He finds that, from the supposition of a fluid and of particles of matter exercising such forces as that theory assumes (with the very allowable additional supposition that the particles are small compared with their distances), it follows that the particles would exert a force, repulsive at the smallest distances, a little further on vanishing, afterwards attractive, and at all sensible distances attracting in proportion to the inverse square of the distance. Thus there would be a position of stable equilibrium for the particles at a very small distance from each other, which may be, M. Mosotti suggests, that equilibrium on which their physical structure depends. According to this view, the resistance of bodies to compression and to extension, as well as the phenomena of statical electricity and the mutual gravitation of matter, are accounted for by the same hypothesis of a single fluid or ether. A theory which offers a prospect of such a generalization is worth attention; but a very clear and comprehensive view of the doctrines of several sciences is requisite to prepare us to estimate its value and probable success.

*Question of the Material Reality of the Electric Fluid.*—At first sight the beautiful accordance of the experiments with calculations founded upon the attractions and repulsions of the two hypothetical fluids, persuade us that the hypotheses must be the real state of things. But we have already learned that we must not trust to such evidence too readily. It is a curious instance of the mutual influence of the histories of two provinces of science, but I think it will be allowed to be just, to say that the discovery of the polarization of heat has done much to shake the theory of the electric fluids as a physical reality. For the doctrine of a material caloric appeared to be proved (from the laws of conduction and radiation) by the same kind of mathematical evidence (the agreement of laws respecting the elementary actions with those of fluids), which we have for the doctrine of material electricity. Yet we now seem to see that heat cannot be matter, since its rays have *sides*, in a manner in which a stream of particles of matter cannot have sides without inadmissible hypotheses. We see, then, that it will not be contrary to precedent, if our electrical theory, representing with perfect accuracy the *laws* of the actions, in all their forms, simple and complex, should yet be fallacious as a view of the *cause* of the actions.

Any true view of electricity must include, or at least be consistent with, the other classes of the phenomena, as well as this statical electrical action; such as the conditions of excitation and retention of electricity; to which we may add, the connexion of electricity with magnetism and with chemistry;—a vast field, as yet dimly seen. Now, even with regard to the simplest of these questions, the cause of the retention of electricity at the surface of bodies, it appears to be impossible to maintain Coulomb's opinion, that this is effected by the resistance of air to the passage of electricity. The other questions are such as Coulomb did not attempt to touch; they refer, indeed, principally to laws not suspected at his time. How wide and profound a theory must be which deals worthily with these, we shall obtain some indications in the succeeding part of our history.

But it may be said on the other side, that we have the evidence of our senses for the reality of an electric fluid;—we see it in the spark; we hear it in the explosion; we feel it in the shock; and it produces the effects of mechanical violence, piercing and tearing the bodies through which it passes. And those who are disposed to assert a real fluid on such grounds, may appear to be justified in doing so, by one of Newton's "Rules of Philosophizing," in which he directs the philosopher to assume, in his theories, "causes which are true." The usual interpretation of a "vera causa," has been, that it implies causes which, independently of theoretical calculations, are known to exist by their mechanical effects; as gravity was familiarly known to exist on the earth, before it was extended to the heavens. The electric fluid might seem to be such a *vera causa*.

To this I should venture to reply, that this reasoning shows how delusive the Newtonian rule, so interpreted, may be. For a moment's consideration will satisfy us that none of the circumstances, above adduced, can really prove material currents, rather than vibrations, or other modes of agency. The spark and shock are quite insufficient to supply such a proof. Sound is vibrations,—light is vibrations; vibrations may affect our nerves, and may rend a body, as when glasses are broken by sounds. Therefore all these supposed indications of the reality of the electric fluid are utterly fallacious. In truth, this mode of applying Newton's rule consists in elevating our first rude and unscientific impressions into a supremacy over the results of calculation, generalization, and systematic induction.[16]

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[16] On the subject of this Newtonian Rule of Philosophizing, see further *Phil. Ind. Sc.* B. xii. c. 13. I have given an account of the history and evidence of the Theory of Electricity in the *Reports of the British Association* for 1835. I may seem there to have spoken more favorably of the Theory as a Physical

Thus our conclusion with regard to this subject is, that if we wish to form a stable physical theory of electricity, we must take into account not only the laws of statical electricity, which we have been chiefly considering, but the laws of other kinds of agency, different from the electric, yet connected with it. For the electricity of which we have hitherto spoken, and which is commonly excited by friction, is identical with galvanic action, which is a result of chemical combinations, and belongs to chemical philosophy. The connexion of these different kinds of electricity with one another leads us into a new domain; but we must, in the first place, consider their mechanical laws. We now proceed to another branch of the same subject, Magnetism.

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Theory than I have done here. This difference is principally due to a consideration of the present aspect of the Theory of Heat.

# BOOK XII.

*MECHANICO-CHEMICAL SCIENCES.*

(CONTINUED.)

# HISTORY OF MAGNETISM

Effice, ut interea fera munera militiaï
Per maria ac terras omneis sopita quiescant.
Nam tu sola potes tranquilla pace juvare
Mortales; quoniam belli fera munera Mavors
Armipotens regit, in gremium qui sæpe tuum se
Rejicit, æterno devictus vulnere amoris;
Atque ita suspiciens tereti cervice reposta,
Pascit amore avidos inhians in te, Dea, visus,
Eque tuo pendet resupini spiritus ore.
Hunc tu, Diva, tuo recubantem corpore sancto
Circumfusa super, suaves ex ore loquelas
Funde, petens placidam Romanis, incluta, pacem.

Lucret. i. 31.

O charming Goddess, whose mysterious sway
The unseen hosts of earth and sky obey;
To whom, though cold and hard to all besides,
The Iron God by strong affection glides,
Flings himself eager to thy close embrace,
And bends his head to gaze upon thy face;
Do thou, what time thy fondling arms are thrown
Around his form, and he is all thy own,
Do thou, thy Rome to save, thy power to prove,
Beg him to grant a boon for thy dear love;
Beg him no more in battle-fields to deal,
Or crush the nations with his mailed heel,
But, touched and softened by a worthy flame,
Quit sword and spear, and seek a better fame.
Bid him to make all war and slaughter cease,
And ply his genuine task in arts of peace;
And by thee guided o'er the trackless surge,
Bear wealth and joy to ocean's farthest verge

## CHAPTER I.

### Discovery of Laws of Magnetic Phenomena.

THE history of Magnetism is in a great degree similar to that of Electricity, and many of the same persons were employed in the two trains of research. The general fact, that the magnet attracts iron, was nearly all that was known to the ancients, and is frequently mentioned and referred to; for instance, by Pliny, who wonders and declaims concerning it, in his usual exaggerated style.[1] The writers of the Stationary Period, in this subject as in others, employed themselves in collecting and adorning a number of extravagant tales, which the slightest reference to experiment would have disproved; as, for example, that a magnet, when it has lost its virtue, has it restored by goat's blood. Gilbert, whose work *De Magnete* we have already mentioned, speaks with becoming indignation and pity of this bookish folly, and repeatedly asserts the paramount value of experiments. He himself, no doubt, acted up to his own precepts; for his work contains all the fundamental facts of the science, so fully examined indeed, that even at this day we have little to add to them. Thus, in his first Book, the subjects of the third, fourth, and fifth Chapters are,—that the magnet has poles,—that we may call these poles the north and the south pole,—that in two magnets the north pole of each attracts the south pole and repels the north pole of the other. This is, indeed, the cardinal fact on which our generalizations rest; and the reader will perceive at once its resemblance to the leading phenomena of statical electricity.

But the doctrines of magnetism, like those of heat, have an additional claim on our notice from the manner in which they are exemplified in the globe of the earth. The subject of *terrestrial magnetism* forms a very important addition to the general facts of magnetic attraction and repulsion. The property of the magnet by which it directs its poles exactly or nearly north and south, when once discovered, was of immense importance to the mariner. It does not

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[1] *Hist. Nat.* lib. xxxvi. c. 25.

appear easy to trace with certainty the period of this discovery Passing over certain legends of the Chinese, as at any rate not bearing upon the progress of European science,[2] the earliest notice of this property appears to be contained in the Poem of Guyot de Provence, who describes the needle as being magnetized, and then placed in or on a straw, (floating on water, as I presume :)

> Puis se torne la pointe toute
> Contre l'estoile sans doute ;

that is, it turns towards the pole-star. This account would make the knowledge of this property in Europe anterior to 1200. It was afterwards found[3] that the needle does not point exactly towards the north. Gilbert was aware of this deviation, which he calls the *variation*, and also, that it is different in different places.[4] He maintained on theoretical principles also,[5] that at the same place the variation is constant; probably in his time there were not any recorded observations by which the truth of this assertion could be tested; it was afterwards found to be false. The alteration of the variation in proceeding from one place to another was, it will be recollected, one of the circumstances which most alarmed the companions of Columbus in 1492. Gilbert says,[6] "Other learned men have, in long navigations, observed the differences of magnetic variations, as Thomas Hariot, Robert Hues, Edward Wright, Abraham Kendall, all Englishmen: others have invented magnetic instruments and convenient modes of observation, such as are requisite for those who take long voyages, as William Borough in his Book concerning the variation of the compass, William Barlo in his supplement, William Norman in his *New Attractive.* This is that Robert Norman (a good seaman and an ingenious artificer,) who first discovered the *dip* of magnetic iron." This important discovery was made[7] in 1576. From the time when the difference of the variation of the compass in different places became known, it was important to mariners to register the variation in all parts of the world. Halley was appointed to the command of a ship in the Royal Navy by the Government of William and Mary, with orders "to seek by observation the discovery of the rule for the variation of the compass." He published Magnetic Charts, which

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[2] *Enc. Met.* art. *Magnetism*, p. 736. [3] Before 1269. *Enc. Met.* p. 737.
[4] *De Magnete*, lib. iv. c. 1. [5] c. 3. [6] Lib. i. c. 1. [7] *Enc Met.* p. 738.

have been since corrected and improved by various persons. The most recent are those of Mr. Yates in 1817, and of M. Hansteen. The dip, as well as the variation, was found to be different in different places. M. Humboldt, in the course of his travels, collected many such observations. And both the observations of variation and of dip seemed to indicate that the earth, as to its effect on the magnetic needle, may, approximately at least, be considered as a magnet, the poles of which are not far removed from the earth's poles of rotation. Thus we have a *magnetic equator*, in which the needle has no dip, and which does not deviate far from the earth's equator; although, from the best observations, it appears to be by no means a regular circle. And the phenomena, both of the dip and of the variation, in high northern latitudes, appear to indicate the existence of a pole below the surface of the earth to the north of Hudson's Bay. In his second remarkable expedition into those regions, Captain Ross is supposed to have reached the place of this pole; the dipping-needle there pointing vertically downwards, and the variation-compass turning towards this point in the adjacent regions. We shall hereafter have to consider the more complete and connected views which have been taken of terrestrial magnetism.

In 1633, Gellibrand discovered that the variation is not constant, as Gilbert imagined, but that at London it had diminished from eleven degrees east in 1580, to four degrees in 1633. Since that time the variation has become more and more westerly; it is now about twenty-five degrees west, and the needle is supposed to have begun to travel eastward again.

The next important fact which appeared with respect to terrestrial magnetism was, that the position of the needle is subject to a small *diurnal* variation: this was discovered in 1722, by Graham, a philosophical instrument-maker, of London. The daily variation was established by one thousand observations of Graham, and confirmed by four thousand more made by Canton, and is now considered to be out of dispute. It appeared also, by Canton's researches, that the diurnal variation undergoes an annual inequality, being nearly a quarter of a degree in June and July, and only half that quantity in December and January.

Having thus noticed the principal facts which belong to terrestrial magnetism, we must return to the consideration of those phenomena which gradually led to a consistent magnetic theory. Gilbert observed that both smelted iron and hammered iron have the magnetic virtue, though in a weaker degree than the magnet itself,[8] and he asserted distinctly that the magnet is merely an ore of iron, (lib. i. c. 16, Quod magnes et vena ferri idem sunt.) He also noted the increased energy which magnets acquire by being *armed;* that is, fitted with a cap of polished iron at each pole.[9] But we do not find till a later period any notice of the distinction which exists between the magnetical properties of soft iron and of hard steel;—the latter being susceptible of being formed into *artificial magnets*, with permanent poles; while soft iron is only *passively magnetic*, receiving a temporary polarity from the action of a magnet near it, but losing this property when the magnet is removed. About the middle of the last century, various methods were devised of making artificial magnets, which exceeded in power all magnetic bodies previously known.

The remaining experimental researches had so close an historical connexion with the theory, that they will be best considered along with it, and to that, therefore, we now proceed.

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## CHAPTER II.

### Progress of Magnetic Theory.

THEORY OF MAGNETIC ACTION.—The assumption of a fluid, as a mode of explaining the phenomena, was far less obvious in magnetic than in electric cases, yet it was soon arrived at. After the usual philosophy of the middle ages, the "forms" of Aquinas, the "efflux" of Cusanus, the "vapors" of Costæus, and the like, which are recorded by Gilbert,[1] we have his own theory, which he also expresses by ascribing the effects to a "formal efficiency;"—a "*form* of primary globes; the proper entity and existence of their homogeneous parts, which we may call a primary and radical and astral *form:*"—of which forms there is one in the sun, one in the moon, one in the earth, the latter being the magnetic virtue.

Without attempting to analyse the precise import of these expressions, we may proceed to Descartes's explanation of magnetic phenomena. The mode in which he presents this subject[2] is, perhaps, the

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[8] Lib. i. c. 9—13.

[9] Lib. ii. c. 17.

[1] Gilb. lib. ii. c. 3, 4.

[2] *Prin. Phil.* pars c. iv. 146.

most persuasive of his physical attempts. If a magnet be placed among iron filings, these arrange themselves in curved lines, which proceed from one pole of the magnet to the other. It was not difficult to conceive these to be the traces of currents of ethereal matter which circulate through the magnet, and which are thus rendered sensible even to the eye. When phenomena could not be explained by means of one vortex, several were introduced. Three Memoirs on Magnetism, written on such principles, had the prize adjudged[3] by the French Academy of Sciences in 1746.

But the Cartesian philosophy gradually declined; and it was not difficult to show that the *magnetic curves*, as well as other phenomena would, in fact, result from the attraction and repulsion of two poles. The analogy of magnetism with electricity was so strong and clear, that similar theories were naturally proposed for the two sets of facts; the distinction of bodies into conductors and electrics in the one case, corresponding to the distinction of soft and hard steel, in their relations to magnetism. Æpinus published a theory of magnetism and electricity at the same time (1759); and the former theory, like the latter, explained the phenomena of the opposite poles as results of the excess and defect of a magnetic "fluid," which was dislodged and accumulated in the ends of the body, by the repulsion of its own particles, and by the attraction of iron or steel, as in the case of induced electricity. The Æpinian theory of magnetism, as of electricity, was recast by Coulomb, and presented in a new shape, with two fluids instead of one. But before this theory was reduced to calculation, it was obviously desirable, in the first place, to determine the law of force.

In magnetic, as in electric action, the determination of the law of attraction of the particles was attended at first with some difficulty, because the action which a finite magnet exerts is a compound result of the attractions and repulsions of many points. Newton had imagined the attractive force of magnetism to be inversely as the cube of the distance; but Mayer in 1760, and Lambert a few years later, asserted the law to be, in this as in other forces, the inverse square. Coulomb has the merit of having first clearly confirmed this law, by the use of his torsion-balance.[4] He established, at the same time, other very important facts, for instance, "that the directive magnetic force, which the earth exerts upon a needle, is a constant quantity, parallel

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[3] Coulomb, 1789, p. 482.

[4] *Mem. A. P.* 1784, 2d Mem. p. 593.

to the magnetic meridian, and passing through the same point of the needle whatever be its position." This was the more important, because it was necessary, in the first place, to allow for the effect of the terrestrial force, before the mutual action of the magnets could be extricated from the phenomena.[5] Coulomb then proceeded to correct the theory of magnetism.

Coulomb's reform of the Æpinian theory, in the case of magnetism, as in that of electricity, substituted two fluids (an *austral* and a *boreal* fluid,) for the single fluid; and in this way removed the necessity under which Æpinus found himself, of supposing all the particles of iron and steel and other magnetic bodies to have a peculiar repulsion for each other, exactly equal to their attraction for the magnetic fluid. But in the case of magnetism, another modification was necessary. It was impossible to suppose here, as in the electrical phenomena, that one of the fluids was accumulated on one extremity of a body, and the other fluid on the other extremity; for though this might appear, at first sight, to be the case in a magnetic needle, it was found that when the needle was cut into two halves, the half in which the austral fluid had seemed to predominate, acquired immediately a boreal pole opposite to its austral pole, and a similar effect followed in the other half. The same is true, into however many parts the magnetic body be cut. The way in which Coulomb modified the theory so as to reconcile it with such facts, is simple and satisfactory. He supposes[6] the magnetic body to be made up of "molecules or integral parts," or, as they were afterwards called by M. Poisson, "magnetic elements." In each of these elements, (which are extremely minute,) the fluids can be separated, so that each element has an austral and a boreal pole; but the austral pole of an element which is adjacent to the boreal pole of the next neutralizes, or nearly neutralizes, its effect; so that the sensible magnetism appears only towards the extremities of the body, as it would do if the fluids could permeate the body freely. We shall have exactly the same result, as to sensible magnetic force, on the one supposition and on the other, as Coulomb showed.[7]

The theory, thus freed from manifest incongruities, was to be reduced to calculation, and compared with experiment; this was done in Coulomb's Seventh Memoir.[8] The difficulties of calculation in this, as in the electric problem, could not be entirely surmounted by the analysis of Coulomb; but by various artifices, he obtained theoretically the rela-

[5] p. 603. [6] *Mem. A. P.* 1789, p. 488. [7] *Mem. A. P.* p. 492. [8] *A. P.* 1789.

tive amount of magnetism at several points of a needle,[9] and the proposition that the directive force of the earth on similar needles saturated with magnetism, was as the cube of their dimensions; conclusions which agreed with experiment.

The agreement thus obtained was sufficient to give a great probability to the theory; but an improvement of the methods of calculation and a repetition of experiments, was, in this as in other cases, desirable, as a confirmation of the labors of the original theorist. These requisites, in the course of time, were supplied. The researches of Laplace and Legendre on the figure of the earth had (as we have already stated,) introduced some very peculiar analytical artifices, applicable to the attractions of spheroids; and these methods were employed by M. Biot in 1811, to show that on an elliptical spheroid, the thickness of the fluid in the direction of the radius would be as the distance from the centre.[10] But the subject was taken up in a more complete manner in 1824 by M. Poisson, who obtained general expressions for the attractions or repulsions of a body of any form whatever, magnetized by influence, upon a given point; and in the case of spherical bodies was able completely to solve the equations which determine these forces.[11]

Previously to these theoretical investigations, Mr. Barlow had made a series of experiments on the effect of an iron sphere upon a compass needle; and had obtained empirical formulæ for the amount of the deviation of the needle, according to its dependence upon the position and magnitude of the sphere. He afterwards deduced the same formulæ from a theory which was, in fact, identical with that of Coulomb, but which he considered as different, in that it supposed the magnetic fluids to be entirely collected at the surface of the sphere. He had indeed found, by experiment, that the surface was the only part in which there was any sensible magnetism; and that a thin shell of iron would produce the same effect as a solid ball of the same diameter.

But this was, in fact, a most complete verification of Coulomb's theory. For though that theory did not suppose the magnetism to be collected solely at the surface, as Mr. Barlow found it, it followed from the theory, that the *sensible* magnetic intensity assumed the same distribution (namely, a surface distribution,) as if the fluids could permeate the whole body, instead of the "magnetic elements" only. Coulomb, indeed, had not expressly noticed the result, that the sensible

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[9] p. 485. [10] *Bull. des Sc.* No. li. [11] *A. P.* for 1821 and 2, published 1826.

magnetism would be confined to the surface of bodies; but he had found that, in a long needle, the magnetic fluid might be supposed to be concentrated very near the extremities, just as it is in a long electric body. The theoretical confirmation of this rule among the other consequences of the theory,—that the sensible magnetism would be collected at the surface,—was one of the results of Poisson's analysis. For it appeared that if the sum of the electric elements of the body was equal to the whole body, there would be no difference between the action of a solid sphere and very thin shell.

We may, then, consider the Coulombian theory to be fully established and verified, as a representation of the laws of magnetical phenomena. We may add, as a remarkable and valuable example of an ulterior step in the course of sciences, the application of the laws of the distribution of magnetism to the purposes of navigation. It had been found that the mass of iron which exists in a ship produces a deviation in the direction of the compass-needle, which was termed "local attraction," and which rendered the compass an erroneous guide. Mr. Barlow proposed to correct this by a plate of iron placed near the compass; the plate being of comparatively small mass, but, in consequence of its expanded form, and its proximity to the needle, of equivalent effect to the disturbing cause.

[2nd Ed.] [This proposed arrangement was not successful, because as the ship turns into different positions, it may be considered as revolving round a vertical axis; and as this does not coincide with the magnetic axis, the relative magnetic position of the disturbing parts of the ship, and of the correcting plate, will be altered, so that they will not continue to counteract each other. In high magnetic latitudes the correcting plate was used with success.

But when iron ships became common, a correction of the effect of the iron upon the ship's compass in the general case became necessary. Mr. Airy devised the means of making this correction. By placing a magnet and a mass of iron in certain positions relative to the compass, the effect of the rest of the iron in the ship is completely counteracted in all positions.[12]]

But we have still to trace the progress of the theory of terrestrial magnetism.

*Theory of Terrestrial Magnetism.*—Gilbert had begun a plausible course of speculation on this point. "We must reject," he says,[13] "in

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[12] See *Phil. Trans.* 1836.

[13] Lib. iv. c. 1. *De Variatione.*

the first place, that vulgar opinion of recent writers concerning magnetic mountains, or a certain magnetic rock, or an imaginary pole at a certain distance from the pole of the earth." For, he adds, "we learn by experience, that there is no such fixed pole or term in the earth for the variation." Gilbert describes the whole earth as a magnetic globe, and attributes the variation to the irregular form of its protuberances, the solid parts only being magnetic. It was not easy to confirm or refute this opinion, but other hypotheses were tried by various writers; for instance, Halley had imagined, from the forms of the lines of equal variation, that there must be four magnetic poles; but Euler[14] showed that the "Halleian lines" would, for the most part, result from the supposition of two magnetic poles, and assigned their position so as to represent pretty well the known state of the variation all over the world in 1744. But the variation was not the only phenomenon which required to be taken into account; the dip at different places, and also the intensity of the force, were to be considered We have already mentioned M. de Humboldt's collection of observations of the dip. These were examined by M. Biot, with the view of reducing them to the action of two poles in the supposed terrestrial magnetic axis. Having, at first, made the distance of these poles from the centre of the earth indefinite, he found that his formulæ agreed more and more nearly with the observations, as the poles were brought nearer; and that fact and theory coincided tolerably well when both poles were at the centre. In 1809,[15] Krafft simplified this result, by showing that, on this supposition, the tangent of the dip was twice the tangent of the latitude of the place as measured from the magnetic equator. But M. Hansteen, who has devoted to the subject of terrestrial magnetism a great amount of labor and skill, has shown that, taking together all the observations which we possess, we are compelled to suppose four magnetic poles; two near the north pole, and two near the south pole, of the terrestrial globe; and that these poles, no two of which are exactly opposite each other, are all in motion, with different velocities, some moving to the east and some to the west. This curious collection of facts awaits the hand of future theorists, when the ripeness of time shall invite them to the task.

[2nd Ed.] [I had thus written in the first edition. The theorist who was needed to reduce this accumulation of facts to their laws,

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[14] *Ac. Berlin*, 1757. [15] *Enc. Met.* p. 742.

had already laid his powerful hand upon them; namely, M. Gauss, a mathematician not inferior to any of the great men who completed the theory of gravitation. And institutions had been established for extending the collection of the facts pertaining to it, on a scale which elevates Magnetism into a companionship with Astronomy. M. Hansteen's *Magnetismus der Erde* was published in 1819. His conclusions respecting the position of the four magnetic "poles" excited so much interest in his own country, that the Norwegian *Storthing*, or parliament, by a unanimous vote, provided funds for a magnetic expedition which he was to conduct along the north of Europe and Asia; and this they did at the very time when they refused to make a grant to the king for building a palace at Christiania. The expedition was made in 1828–30, and verified Hansteen's anticipations as to the existence of a region of magnetic convergence in Siberia, which he considered as indicating a "pole" to the north of that country. M. Erman also travelled round the earth at the same time, making magnetic observations.

About the same time another magnetical phenomenon attracted attention. Besides the general motion of the magnetic poles, and the diurnal movements of the needle, it was found that small and irregular disturbances take place in its position, which M. de Humboldt termed *magnetic storms*. And that which excited a strong interest on this subject was the discovery that these magnetic storms, seen only by philosophers who watch the needle with microscopic exactness, rage simultaneously over large tracts of the surface of our globe. This was detected about 1825 by a comparison of the observations of M. Arago at Paris with simultaneous observations of M. Kupffer at Kasan in Russia, distant more than 47 degrees of longitude.

At the instance of M. de Humboldt, the Imperial Academy of Russia adopted with zeal the prosecution of this inquiry, and formed a chain of magnetic stations across the whole of the Russian empire. Magnetic observations were established at Petersburg and at Kasan, and corresponding observations were made at Moscow, at Nicolaieff in the Crimea, and Barnaoul and Nertchinsk in Siberia, at Sitka in Russian America, and even at Pekin. To these magnetic stations the Russian government afterwards added, Catharineburg in Russia Proper, Helsingfors in Finland, Teflis in Georgia. A comparison of the results obtained at four of these stations made by MM. de Humboldt and Dove, in the year 1830, showed that the magnetic disturbances were simultaneous, and were for the most parallel in their progress.

Important steps in the prosecution of this subject were soon after made by M. Gauss, the great mathematician of Göttingen. He contrived instruments and modes of observation far more perfect than any before employed, and organized a system of comparative observations throughout Europe. In 1835, stations for this purpose were established at Altona, Augsburg, Berlin, Breda, Breslau, Copenhagen, Dublin, Freiberg, Göttingen, Greenwich, Hanover, Leipsic, Marburg, Milan, Munich, Petersburg, Stockholm, and Upsala. At these places, six times in the year, observations were taken simultaneously, at intervals of five minutes for 24 hours. The *Results of the Magnetic Association* (Resultaten des Magnetischen Vereins) were published by MM. Gauss and Weber, beginning in 1836.

British physicists did not at first take any leading part in these plans. But in 1836, Baron Humboldt, who by his long labors and important discoveries in this subject might be considered as peculiarly entitled to urge its claims, addressed a letter to the Duke of Sussex, then President of the Royal Society, asking for the co-operation of this country in so large and hopeful a scheme for the promotion of science. The Royal Society willingly entertained this appeal; and the progress of the cause was still further promoted when it was zealously taken up by the British Association for the Advancement of Science, assembled at Newcastle in 1838. The Association there expressed its strong interest in the German system of magnetic observations; and at the instigation of this body, and of the Royal Society, four complete magnetical observatories were established by the British government, at Toronto, St. Helena, the Cape of Good Hope, and Van Diemen's Land. The munificence of the Directors of the East India Company founded and furnished an equal number at Simla (in the Himalayah), Madras, Bombay, and Sincapore. Sir Thomas Brisbane added another at his own expense at Kelso, in Scotland. Besides this, the government sent out a naval expedition to make discoveries (magnetic among others), in the Antarctic regions, under the command of Sir James Ross. Other states lent their assistance also, and founded or reorganized their magnetic observatories. Besides those already mentioned, one was established by the French government at Algiers; one by the Belgian, at Brussels; two by Austria, at Prague and Milan; one by Prussia, at Breslau; one by Bavaria, at Munich; one by Spain, at Cadiz; there are two in the United States, at Philadelphia and Cambridge; one at Cairo, founded by the Pasha of Egypt; and in India, one at Trevandrum, established by the Rajah of Travancore; and one by the King of Oude, at Lucknow. At all these distant stations the same plan was followed out, by observations strictly simultaneous, made according to the same methods, with the same instrumental means. Such a scheme, combining world-wide extent with the singleness of action of an individual mind, is hitherto without parallel.

At first, the British stations were established for three years only; but it was thought advisable to extend this period three years longer, to end in 1845. And when the termination of that period arrived, a discussion was held among the magneticians themselves, whether it was better to continue the observations still, or to examine and compare the vast mass of observations already collected, so as to see to what results and improvements of methods they pointed. This question was argued at the meeting of the British Association at Cambridge in that year; and the conference ended in the magneticians requesting to have the observations continued, at some of the observatories for an indefinite period, at others, till the year 1848. In the mean time the Antarctic expedition had brought back a rich store of observations, fitted to disclose the magnetic condition of those regions which it had explored. These were *discussed*, and their results exhibited, in the *Philosophical Transactions* for 1843, by Col. Sabine, who had himself, at various periods, made magnetic observations in the Arctic regions, and in several remote parts of the globe, and had always been a zealous laborer in this fruitful field. The general mass of the observations was placed under the management of Professor Lloyd, of Dublin, who has enriched the science of magnetism with several valuable instruments and methods, and who, along with Col. Sabine, made a magnetic survey of the British Isles in 1835 and 1836.

I do not dwell upon magnetic surveys of various countries made by many excellent observers; as MM. Quetelet, Forbes, Fox, Bache and others.

The facts observed at each station were, the *intensity* of the magnetic force; the *declination* of the needle from the meridian, sometimes called *the variation;* and its *inclination* to the horizon, *the dip;*—or at least, some elements equivalent to these. The values of these elements at any given time, if known, can be expressed by charts of the earth's surface, on which are drawn the *isodynamic*, *isogonal*, and *isoclinal* curves. The second of these kinds of charts contain the "Halleian lines" spoken of in a previous page. Moreover the magnetic elements at each place are to be observed in such a manner as to determine both their *periodical* variations (the changes which occur in the period of a day, and of a year), the *secular* changes, as the gradual increase or diminution of the declination at the same place for many years; and the *irregular* fluctuations which, as we have said, are simultaneous over a large part, or the whole, of the earth's surface.

When these Facts have been ascertained over the whole extent of the earth's surface, we shall still have to inquire what is the Cause of the changes in the forces which these phenomena disclose. But as a basis for all speculation on that subject, we must know the law of the phenomena, and of the forces which immediately produce them. I have already said that Euler tried to account for the Halleian lines by means of *two* magnetic "poles," but that M. Hansteen conceived it necessary to assume *four*. But an entirely new light has been thrown upon this subject by the beautiful investigations of Gauss, in his *Theory of Terrestrial Magnetism*, published in 1839. He remarks that the term "poles," as used by his predecessors, involves an assumption arbitrary, and, as it is now found, false; namely, that certain definite points, two, four, or more, acting according to the laws of ordinary magnetical poles, will explain the phenomena. He starts from a more comprehensive assumption, that magnetism is distributed throughout the mass of the earth in an unknown manner. On this assumption he obtains a function $V$, by the differentials of which the elements of the magnetic force at any point will be expressed. This function $V$ is well known in physical astronomy, and is obtained by summing all the elements of magnetic force in each particle, each multiplied by the reciprocal of its distance; or as we may express it, by taking the sum of each element and its proximity jointly. Hence it has been proposed[16] to term this function the "*integral proximity*" of the attracting mass.[17] By using the most refined ma-

[16] *Quart. Rev.* No. 131, p. 283.

[17] The function V is of constant occurrence in investigations respecting attractions. It is introduced by Laplace in his investigations respecting the attractions of spheroids, *Méc. Cel.* Livr. III. Art. 4. Mr. Green and Professor Mac Cullagh have proposed to term this function the *Potential* of the system; but this term (though suggested, I suppose, by analogy with the substantive *Exponential*), does not appear convenient in its form. On the other hand, the term *Integral Proximity* does not indicate that which gives the function its peculiar claim to distinction; namely, that its differentials express the power or attraction of the system. Perhaps *Integral Potentiality*, or *Integral Attractivity*, would be a term combining the recommendations of both the others.

thematical artifices for deducing the values of $V$ and its differentials in converging series, he is able to derive the coefficients of these series from the observed magnetic elements at certain places, and hence, to calculate them for all places. The comparison of the calculation with the observed results is, of course, the test of the truth of the theory.

The degree of convergence of the series depends upon the unknown distribution of magnetism within the earth. "If we could venture to assume," says M. Gauss, "that the members have a sensible influence only as far as the fourth order, complete observations from eight points would be sufficient, theoretically considered, for the determination of the coefficients." And under certain limitations, making this assumption, as the best we can do at present, M. Gauss obtains from eight places, 24 coefficients (each supplying three elements), and hence calculates the magnetic elements (intensity, variation and dip) at 91 places in all parts of the earth. He finds his calculations approach the observed values with a degree of exactness which appears to be quite convincing as to the general truth of his results; especially taking into account how entirely unlimited is his original hypothesis.

It is one of the most curious results of this investigation that according to the most simple meaning which we can give to the term "pole" the earth has only *two* magnetic poles; that is, two points where the direction of the magnetic force is vertical. And thus the *isogonal curves* may be looked upon as *deformations* of the curves deduced by Euler from the supposition of two poles, the deformation arising from this, that the earth does not contain a single definite magnet, but irregularly diffused magnetical elements, which still have collectively a distinct resemblance to a single magnet. And instead of Hansteen's Siberian pole, we have a Siberian region in which the needles converge; but if the apparent convergence be pursued it nowhere comes to a point; and the like is the case in the Antarctic region. When the 24 Gaussian elements at any time are known the magnetic condition of the globe is known, just as the mechanical condition of the solar system is known, when we know the elements of the orbits of the satellites and planets and the mass of each. And the comparison of this magnetic condition of the globe at distant periods of time cannot fail to supply materials for future researches and speculations with regard to the agencies by which the condition of the earth is determined. The condition of which we here speak must necessarily be its *mechanico-chemical* condition, being expressed, as it will be, in terms of the mechanico-chemical sciences. The investigations I have been describing belong to the mechanical side of the subject: but when philosophers have to consider the causes of the secular changes which are found to occur in this mechanical condition, they cannot fail to be driven to electrical, that is, chemical agencies and laws

I can only allude to Gauss's investigations respecting the *Absolute Measure* of the Earth's Magnetic Force. To determine the ratio of the magnetic force of the earth to that of a known magnet, Poisson proposed to observe the time of vibration of a second magnet. The method of Gauss, now universally adopted, consists in observing the position of equilibrium of the second magnet when deflected by the first.

The manner in which the business of magnetic observation has been taken up by the governments of our time makes this by far the greatest scientific undertaking which the world has ever seen. The result will be that we shall obtain in a few years a knowledge of the magnetic constitution of the earth which otherwise it might have required centuries to accumulate. The secular magnetic changes must still require a long time to reduce to their laws of phenomena, except observation be anticipated or assisted by some happy discovery as to the cause of these changes. But besides the special gain to magnetic science by this great plan of joint action among the nations of the earth, there is thereby a beginning made in the recognition and execution of the duty of forwarding science in general by national exertions. For at most of the magnetic observatories, meteorological observations are also carried on; and such observations, being far more extensive, systematic, and permanent than those which have usually been made, can hardly fail to produce important additions to science. But at any rate they do for science that which nations can do, and individuals cannot; and they seek for scientific truths in a manner suitable to the respect now professed for science and to the progress which its methods have made. Nor are we to overlook the effect of such observations as means of training men in the pursuit of science. "There is amongst us," says one of the magnetic observers, "a growing recognition of the importance, both for science and for practical life, of forming exact observers of nature. Hitherto astronomy alone has afforded a very partial opportunity for the formation of fine observers, of which few could avail themselves. Experience has shown that magnetic observations may serve as excellent training schools in this respect." [18]]

[18] *Letter* of W. Weber. *Brit. Assoc. Rep.* 1845, p. 17.

The various other circumstances which terrestrial magnetism exhibits,—the diurnal and annual changes of the position of the compass-needle;—the larger secular change which affects it in the course of years;—the difference of intensity at different places, and other facts, have naturally occupied philosophers with the attempt to determine, both the laws of the phenomena and their causes. But these attempts necessarily depend, not upon laws of statical magnetism, such as they have been explained above; but upon the laws by which the production and intensity of magnetism in different cases are regulated;—laws which belong to a different province, and are related to a different set of principles. Thus, for example, we have not attempted to explain the discovery of the laws by which heat influences magnetism; and therefore we cannot now give an account of those theories of the facts relating to terrestrial magnetism, which depend upon the influence of temperature. The conditions of excitation of magnetism are best studied by comparing this force with other cases where the same effects are produced by very different apparent agencies; such as galvanic and thermo-electricity. To the history of these we shall presently proceed.

*Conclusion.*—The hypothesis of magnetic fluids, as physical realities, was never widely or strongly embraced, as that of electric fluids was. For though the hypothesis accounted, to a remarkable degree of exactness, for large classes of the phenomena, the presence of a material fluid was not indicated by facts of a different kind, such as the spark, the discharge from points, the shock, and its mechanical effects. Thus the belief of a peculiar magnetic fluid or fluids was not forced upon men's minds; and the doctrine above stated was probably entertained by most of its adherents, chiefly as a means of expressing the laws of phenomena in their elementary form.

One other observation occurs here. We have seen that the supposition of a fluid moveable from one part of bodies to another, and capable of accumulation in different parts of the surface, appeared at first to be as distinctly authorized by magnetic as by electric phenomena; and yet that it afterwards appeared, by calculation, that this must be considered as a derivative result; no real transfer of fluid taking place except within the limits of the insensible particles of the body. Without attempting to found a formula of philosophizing on this circumstance, we may observe, that this occurrence, like the disproof of heat as a material fluid, shows the possibility of an hypothesis which shall very exactly satisfy many phenomena, and yet be incomplete: it shows, too, the necessity of bringing facts of all kinds to bear on the hypothesis; thus, in this case it was requisite to take into account the facts of junction and separation of magnetic bodies, as well as their attractions and repulsions.

If we have seen reason to doubt the doctrine of electric fluids as physical realities, we cannot help pronouncing upon the magnetic fluids as having still more insecure claims to a material existence, even on the grounds just stated. But we may add considerations still more decisive; for at a further stage of discovery, as we shall see, magnetic and electric action were found to be connected in the closest manner, so as to lead to the persuasion of their being different effects of one common cause. After those discoveries, no philosopher would dream of assuming electric fluids and magnetic fluids as two distinct material agents. Yet even now the nature of the dependence of magnetism upon any other cause is extremely difficult to conceive. But till we have noticed some of the discoveries to which we have alluded, we cannot even speculate about that dependence. We now, therefore, proceed to sketch the history of these discoveries.

# BOOK XIII.

## *MECHANICO-CHEMICAL SCIENCES.*

(CONTINUED.)

# HISTORY OF GALVANISM,

OR

## VOLTAIC ELECTRICITY.

Percussæ gelido trepidant sub pectore fibræ,
Et nova desuetis subrepens vita medullis
Miscetur morti: tunc omnis palpitat artus
Tenduntur nervi; nec se tellure cadaver
Paullatim per membra levat; terrâque repulsum est
Erectumque simul. LUCAN. vi. 752.

The form which lay before inert and dead,
Sudden a piercing thrill of change o'erspread;
Returning life gleams in the stony face,
The fibres quiver and the sinews brace,
Move the stiff limbs;—nor did the body rise
With tempered strength which genial life supplies,
But upright starting, its full stature held,
As though the earth the supine corse repelled.

## CHAPTER I.

### Discovery of Voltaic Electricity.

WE have given the name of *mechanico-chemical* to the class of sciences now under our consideration; for these sciences are concerned with cases in which mechanical effects, that is, attractions and repulsions, are produced; while the conditions under which these effects occur, depend, as we shall hereafter see, on chemical relations. In that branch of these sciences which we have just treated of, Magnetism, the mechanical phenomena were obvious, but their connexion with chemical causes was by no means apparent, and, indeed, has not yet come under our notice.

The subject to which we now proceed, Galvanism, belongs to the same group, but, at first sight, exhibits only the other, the chemical, portion of the features of the class; for the connexion of galvanic phenomena with chemical action was soon made out, but the mechanical effects which accompany them were not examined till the examination was required by a new train of discovery. It is to be observed, that I do not include in the class of mechanical effects the convulsive motions in the limbs of animals which are occasioned by galvanic action; for these movements are produced, not by attraction and repulsion, but by muscular irritability; and though they indicate the existence of a peculiar agency, cannot be used to measure its intensity and law.

The various examples of the class of agents which we here consider, —magnetism, electricity, galvanism, electro-magnetism, thermo-electricity,—differ from each other principally in the circumstances by which they are called into action; and these differences are in reality of a chemical nature, and will have to be considered when we come to treat of the inductive steps by which the general principles of chemical theory are established. In the present part of our task, therefore, we must take for granted the chemical conditions on which the excitation of these various kinds of action depends, and trace the history of the discovery of their mechanical laws only. This rule will much abridge the account we have here to give of the progress of discovery in the provinces to which I have just referred.

The first step in this career of discovery was that made by Galvani, Professor of Anatomy at Bologna. In 1790, electricity, as an experimental science, was nearly stationary. The impulse given to its progress by the splendid phenomena of the Leyden phial had almost died away; Coulomb was employed in systematizing the theory of the electric fluid, as shown by its statical effects; but in all the other parts of the subject, no great principle or new result had for some time been detected. The first announcement of Galvani's discovery in 1791 excited great notice, for it was given forth as a manifestation of electricity under a new and remarkable character; namely, as residing in the muscles of animals.[1] The limbs of a dissected frog were observed to move, when touched with pieces of two different metals; the agent which produced these motions was conceived to be identified with electricity, and was termed *animal electricity;* and Galvani's experiments were repeated, with various modifications, in all parts of Europe, exciting much curiosity, and giving rise to many speculations.

It is our business to determine the character of each great discovery which appears in the progress of science. Men are fond of repeating that such discoveries are most commonly the result of accident; and we have seen reason to reject this opinion, since that preparation of thought by which the accident produces discovery is the most important of the conditions on which the successful event depends. Such accidents are like a spark which discharges a gun already loaded and pointed. In the case of Galvani, indeed, the discovery may, with more propriety than usual, be said to have been casual; but in the form in which it was first noted, it exhibited no important novelty. His frog was lying on a table near the conductor of an electrical machine, and the convulsions appeared only when a spark was taken from the machine. If Galvani had been as good a physicist as he was an anatomist, he would probably have seen that the movements so occasioned proved only that the muscles or nerves, or the two together, formed a very sensitive indicator of electrical action. It was when he produced such motions by contact of metals alone, that he obtained an important and fundamental fact in science.

The analysis of this fact into its real and essential conditions was the work of Alexander Volta, another Italian professor. Volta, indeed, possessed that knowledge of the subject of electricity which made a hint like that of Galvani the basis of a new science. Galvani appears

[1] *De Viribus Electricis in Motu Musculari* Comm. Bonon. t. vii. 1792.

never to have acquired much general knowledge of electricity: Volta, on the other hand, had labored at this branch of knowledge from the age of eighteen, through a period of nearly thirty years; and had invented an *electrophorus* and an *electrical condenser*, which showed great experimental skill. When he turned his attention to the experiments made by Galvani, he observed that the author of them had been far more surprised than he needed to be, at those results in which an electrical spark was produced; and that it was only in the cases in which no such apparatus was employed, that the observations could justly be considered as indicating a new law, or a new kind of electricity.[2] He soon satisfied himself[3] (about 1794) that the essential conditions of this kind of action depended on the metals; that it is brought into play most decidedly when two different metals touch each other, and are connected by any moist body;—and that the parts of animals which had been used discharged the office both of such moist bodies, and of very sensitive electrometers. The *animal* electricity of Galvani might, he observed, be with more propriety called *metallic* electricity.

The recognition of this agency as a peculiar kind of *electricity*, arose in part perhaps, at first, from the confusion made by Galvani between the cases in which his electrical machine was, and those in which it was not employed. But the identity was confirmed by its being found that the known difference of electrical conductors and non-conductors regulated the conduction of the new influence. The more exact determination of the new facts to those of electricity was a succeeding step of the progress of the subject.

The term "animal electricity" has been superseded by others, of which *galvanism* is perhaps the most familiar. I think it will appear from what has been said, that Volta's office in this discovery is of a much higher and more philosophical kind than that of Galvani; and it would, on this account, be more fitting to employ the term *voltaic electricity;* which, indeed, is very commonly used, especially by our most recent and comprehensive writers.

Volta more fully still established his claim as the main originator of this science by his next step. When some of those who repeated the experiments of Galvani had expressed a wish that there was some method of multiplying the effect of *this* electricity, such as the Leyden phial supplies for common electricity, they probably thought their wishes far from a realization. But the *voltaic pile*, which Volta

[2] *Phil. Trans.* 1793, p. 21. [3] See Fischer, viii. 625.

described in the *Philosophical Transactions* for 1800, completely satisfies this aspiration; and was, in fact, a more important step in the history of electricity than the Leyden jar had been. It has since undergone various modifications, of which the most important was that introduced by Cruikshanks, who[4] substituted a trough for a pile. But in all cases the principle of the instrument was the same;—a continued repetition of the triple combination of two metals and a fluid in contact, so as to form a circuit which returns into itself.

Such an instrument is capable of causing effects of great intensity; as seen both in the production of light and heat, and in chemical changes. But the discovery with which we are here concerned, is not the details and consequences of the effects, (which belong to chemistry,) but the analysis of the conditions under which such effects take place; and this we may consider as completed by Volta at the epoch of which we speak.

## CHAPTER II.

### Reception and Confirmation of the Discovery of Voltaic Electricity.

GALVANI'S experiments excited a great interest all over Europe, in consequence partly of a circumstance which, as we have seen, was unessential, the muscular contractions and various sensations which they occasioned. Galvani himself had not only considered the animal element of the circuit as the origin of the electricity, but had framed a theory,[1] in which he compared the muscles to charged jars, and the nerves to the discharging wires; and a controversy was, for some time, carried on, in Italy, between the adherents of Galvani and those of Volta.[2]

The galvanic experiments, and especially those which appeared to have a physiological bearing, were verified and extended by a number of the most active philosophers of Europe, and especially William von Humboldt. A commission of the Institute of France, appointed in 1797, repeated many of the known experiments, but does not seem to have decided any disputed points. The researches of this commis-

[4] Fischer, viii. p. 683. [1] Ib. viii. 613. [2] Ib. viii. 619.

sion referred rather to the discoveries of Galvani than to those of Volta: the latter were, indeed, hardly known in France till the conquest of Italy by Bonaparte, in 1801. France was, at the period of these discoveries, separated from all other countries by war, and especially from England,[3] where Volta's Memoirs were published.

The political revolutions of Italy affected, in very different manners, the two discoverers of whom we speak. Galvani refused to take an oath of allegiance to the Cisalpine republic, which the French conqueror established; he was consequently stripped of all his offices; and deprived, by the calamities of the times, of most of his relations, he sank into poverty, melancholy, and debility. At last his scientific reputation induced the republican rulers to decree his restoration to his professorial chair; but his claims were recognised too late, and he died without profiting by this intended favor, in 1798.

Volta, on the other hand, was called to Paris by Bonaparte as a man of science, and invested with honors, emoluments, and titles. The conqueror himself, indeed, was strongly interested by this train of research.[4] He himself founded valuable prizes, expressly with a view to promote its prosecution. At this period, there was something in this subject peculiarly attractive to his Italian mind; for the first glimpses of discoveries of great promise have always excited an enthusiastic activity of speculation in the philosophers of Italy, though generally accompanied with a want of precise thought. It is narrated[5] of Bonaparte, that after seeing the decomposition of the salts by means of the voltaic pile, he turned to Corvisart, his physician, and said, "Here, doctor, is the image of life; the vertebral column is the pile, the liver is the negative, the bladder the positive, pole." The importance of voltaic researches is not less than it was estimated by Bonaparte; but the results to which it was to lead were of a kind altogether different from those which thus suggested themselves to his mind. The connexion of mechanical and chemical action was the first great point to be dealt with; and for this purpose the laws of the mechanical action of voltaic electricity were to be studied.

It will readily be supposed that the voltaic researches, thus begun, opened a number of interesting topics of examination and discussion. These, however, it does not belong to our place to dwell upon at present; since they formed parts of the theory of the subject, which

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[3] *Biog. Univ.*, art. *Volta*, (by Biot). [4] Becquerel, *Traité d'Electr.* t. i. p. 107.
[5] Ib. t. i. p. 108.

was not completed till light had been thrown upon it from other quarters. The identity of galvanism with electricity, for instance, was at first, as we have intimated, rather conjectured than proved. It was denied by Dr. Fowler, in 1793; was supposed to be confirmed by Dr. Wells two years later; but was, still later, questioned by Davy. The nature of the operation of the pile was variously conceived. Volta himself had obtained a view of it which succeeding researches confirmed, when he asserted,[6] in 1800, that it resembled an electric battery feebly charged and constantly renewing its charge. In pursuance of this view, the common electrical action was, at a later period (for instance by Ampère, in 1820), called *electrical tension*, while the voltaic action was called the *electrical current*, or *electromotive action.* The different effects produced, by increasing the size and the number of the plates in the voltaic trough, were also very remarkable. The power of producing heat was found to depend on the size of the plates; the power of producing chemical changes, on the other hand, was augmented by the number of plates of which the battery consisted. The former effect was referred to the increased *quantity*, the latter to the *intensity*, of the electric fluid. We mention these distinctions at present, rather for the purpose of explaining the language in which the results of the succeeding investigations are narrated, than with the intention of representing the hypotheses and measures which they imply, as clearly established, at the period of which we speak. For that purpose new discoveries were requisite, which we have soon to relate.

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## CHAPTER III.

### Discovery of the Laws of the Mutual Attraction and Repulsion of Voltaic Currents.—Ampère.

In order to show the place of voltaic electricity among the mechanico-chemical sciences, we must speak of its mechanical laws as separate from the laws of electro-magnetic action; although, in fact, it was only in consequence of the forces which conducting voltaic wires exert upon magnets, that those forces were detected which they exert upon each

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[6] *Phil. Trans.* p. 403.

other. This latter discovery was made by M. Ampère; and the extraordinary rapidity and sagacity with which he caught the suggestion of such forces, from the electro-magnetic experiments of M. Oersted, (of which we shall speak in the next chapter,) well entitle him to be considered as a great and independent discoverer. As he truly says,[1] "it by no means followed, that because a conducting wire exerted a force on a magnet, two conducting wires must exert a force on each other for two pieces of soft iron, both of which affect a magnet, do not affect each other." But immediately on the promulgation of Oersted's experiments, in 1820, Ampère leapt forwards to a general theory of the facts, of which theory the mutual attraction and repulsion of conducting voltaic wires was a fundamental supposition. The supposition was immediately verified by direct trial; and the laws of this attraction and repulsion were soon determined, with great experimental ingenuity, and a very remarkable command of the resources of analysis. But the experimental and analytical investigation of the mutual action of voltaic or electrical currents, was so mixed up with the examination of the laws of electro-magnetism, which had given occasion to the investigation, that we must not treat the two provinces of research as separate. The mention in this place, premature as it might appear, of the labors of Ampère, arises inevitably from his being the author of a beautiful and comprehensive generalization, which not only included the phenomena exhibited by the new combinations of Oersted, but also disclosed forces which existed in arrangements already familiar, although they had never been detected till the theory pointed out how they were to be looked for.

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## CHAPTER IV.

### Discovery of Electro-magnetic Action.—Oersted.

THE impulse which the discovery of galvanism, in 1791, and that of the voltaic pile, in 1800, had given to the study of electricity as a mechanical science, had nearly died away in 1820. It was in that year that M. Oersted, of Copenhagen, announced that the conducting

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[1] *Théorie des Phénom. Electrodynamiques*, p. 113.

wire of a voltaic circuit, acts upon a magnetic needle; and thus recalled into activity that endeavor to connect magnetism with electricity, which, though apparently on many accounts so hopeful, had hitherto been attended with no success. Oersted found that the needle has a tendency to place itself *at right angles* to the wire;—a kind of action altogether different from any which had been suspected.

This observation was of vast importance; and the analysis of its conditions and consequences employed the best philosophers in Europe immediately on its promulgation. It is impossible, without great injustice, to refuse great merit to Oersted as the author of the discovery. We have already said that men appear generally inclined to believe remarkable discoveries to be accidental, and the discovery of Oersted has been spoken of as a casual insulated experiment.[1] Yet Oersted had been looking for such an *accident* probably more carefully and perseveringly than any other person in Europe. In 1807, he had published[2] a work, in which he professed that his purpose was "to ascertain whether electricity, in its most latent state, had any effect on the magnet." And he, as I know from his own declaration, considered his discovery as the natural sequel and confirmation of his early researches; as, indeed, it fell in readily and immediately with speculations on these subjects then very prevalent in Germany. It was an accident like that by which a man guesses a riddle on which his mind has long been employed.

Besides the confirmation of Oersted's observations by many experimenters, great additions were made to his facts: of these, one of the most important was due to Ampère. Since the earth is in fact magnetic, the voltaic wire ought to be affected by terrestrial magnetism alone, and ought to tend to assume a position depending on the position of the compass-needle. At first, the attempts to produce this effect failed, but soon, with a more delicate apparatus, the result was found to agree with the anticipation.

It is impossible here to dwell on any of the subsequent researches, except so far as they are essential to our great object, the progress towards a general theory of the subject. I proceed, therefore, immediately to the attempts made towards this object.

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[1] See *Schelling ueber Faraday's Entdeckung*, p. 27.

[2] Ampère, p. 69.

## CHAPTER V.

### Discovery of the Laws of Electro-magnetic Action.

ON attempting to analyse the electro-magnetic phenomena observed by Oersted and others into their simplest forms, they appeared, at least at first sight, to be different from any mechanical actions which had yet been observed. It seemed as if the conducting wire exerted on the pole of the magnet a force which was not attractive or repulsive, but *transverse*;—not tending to draw the point acted on nearer, or to push it further off, in the line which reached from the acting point, but urging it to move at right angles to this line. The forces appeared to be such as Kepler had dreamt of in the infancy of mechanical conceptions; rather than such as those of which Newton had established the existence in the solar system, and such as he, and all his successors, had supposed to be the only kinds of force which exist in nature. The north pole of the needle moved as if it were impelled by a vortex revolving round the wire in one direction, while the south pole seemed to be driven by an opposite vortex. The case seemed novel, and almost paradoxical.

It was soon established by experiments, made in a great variety of forms, that the mechanical action was really of this transverse kind. And a curious result was obtained, which a little while before would have been considered as altogether incredible;—that this force would cause a constant and rapid revolution of either of the bodies about the other;—of the conducting wire about the magnet, or of the magnet about the conducting wire. This was effected by Mr. Faraday in 1821.

The laws which regulated the intensity of this force, with reference to the distance and postiion of the bodies, now naturally came to be examined. MM. Biot and Savart in France, and Mr. Barlow in England, instituted such measures; and satisfied themselves that the elementary force followed the law of magnitude of all known elementary forces, in being inversely as the square of the distance; although, in its direction, it was so entirely different from other forces. But the investigation of the *laws of phenomena* of the subject was too closely connected with the choice of a mechanical theory, to be established previously and independently, as had been done in astronomy. The experiments gave complex results, and the analysis of these into their elementary actions was almost an indispensable step in order to disentangle their laws. We must, therefore, state the progress of this analysis.

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## CHAPTER VI.

### Theory of Electrodynamical Action.

AMPÈRE'S Theory.—Nothing can show in a more striking manner the advanced condition of physical speculation in 1820, than the reduction of the strange and complex phenomena of electromagnetism to a simple and general theory as soon as they were published. Instead of a gradual establishment of laws of phenomena, and of theories more and more perfect, occupying ages, as in the case of astronomy, or generations, as in the instances of magnetism and electricity, a few months sufficed for the whole process of generalization; and the experiments made at Copenhagen were announced at Paris and London, almost at the same time with the skilful analysis and comprehensive inductions of Ampère.

Yet we should err if we should suppose, from the celerity with which the task was executed, that it was an easy one. There were required in the author of such a theory, not only those clear conceptions of the relations of space and force, which are the first conditions of all sound theory, and a full possession of the experiments; but also a masterly command of the mathematical arms by which alone the victory could be gained, and a sagacious selection of proper experiments which might decide the fate of the proposed hypothesis.

It is true, that the nature of the requisite hypothesis was not difficult to see in a certain vague and limited way. The conducting-wire and the magnetic needle had a tendency to arrange themselves at right angles to one another. This might be represented by supposing the wire to be made up of transverse magnetic needles, or by supposing the needle to be made up of transverse conducting-wires; for it was easy to conceive forces which should bring corresponding elements, either magnetic or voltaic, into parallel positions; and then the gene ral phenomena above stated would be accounted for. And the choice between the two modes of conception, appeared at first sight a matter of indifference. The majority of philosophers at first adopted, or at least employed, the former method, as Oersted in Germany, Berzelius in Sweden, Wollaston in England.

Ampère adopted the other view, according to which the magnet is made up of conducting-wires in a transverse position. But he did for his hypothesis what no one did or could do for the other: he showed that it was the only one which would account, without additional and arbitrary suppositions, for the facts of *continued* motion in electro-magnetic cases. And he further elevated his theory to a higher rank of generality, by showing that it explained,—not only the action of a conducting-wire upon a magnet, but also two other classes of facts, already spoken of in this history,—the action of magnets upon each other,—and the action of conducting-wires upon each other.

The deduction of such particular cases from the theory, required, as may easily be imagined, some complex calculations: but the deduction being satisfactory, it will be seen that Ampère's theory conformed to that description which we have repeatedly had to point out as the usual character of a true and stable theory; namely, that besides accounting for the class of phenomena which suggested it, it supplies an unforeseen explanation of other known facts. For the mutual action of magnets, which was supposed to be already reduced to a satisfactory theoretical form by Coulomb, was not contemplated by Ampère in the formation of his hypothesis; and the mutual action of voltaic currents, though tried only in consequence of the suggestion of the theory, was clearly a fact distinct from electromagnetic action; yet all these facts flowed alike from the theory. And thus Ampère brought into view a class of forces for which the term "electromagnetic" was too limited, and which he designated[1] by the appropriate term *electrodynamic*; distinguishing them by this expression, as the forces of an electric *current*, from the *statical* effects of electricity which we had formerly to treat of. This term has passed into common use among scientific writers, and remains the record and stamp of the success of the Amperian induction.

The first promulgation of Ampère's views was by a communication to the French Academy of Sciences, September the 18th, 1820; Oersted's discoveries having reached Paris only in the preceding July

[1] *Ann. de Chim.*, tom. xx. p. 60 (1822).

At almost every meeting of the Academy during the remainder of that year and the beginning of the following one, he had new developements or new confirmations of his theory to announce. The most hypothetical part of his theory,—the proposition that magnets might be considered in their effects as identical with spiral voltaic wires,—he asserted from the very first. The mutual attraction and repulsion of voltaic wires,—the laws of this action,—the deduction of the observed facts from it by calculation,—the determination, by new experiments, of the constant quantities which entered into his formulæ,—followed in rapid succession. The theory must be briefly stated. It had already been seen that parallel voltaic currents attracted each other; when, instead of being parallel, they were situate in any directions, they still exerted attractive and repulsive forces depending on the distance, and on the directions of each element of both currents. Add to this doctrine the hypothetical constitution of magnets, namely, that a voltaic current runs round the axis of each particle, and we have the means of calculating a vast variety of results which may be compared with experiment. But the laws of the elementary forces required further fixation. What *functions* are the forces of the distance and the directions of the elements?

To extract from experiment an answer to this inquiry was far from easy, for the elementary forces were mathematically connected with the observed facts, by a double mathematical integration;—a long, and, while the constant coefficients remained undefined, hardly a possible operation. Ampère made some trials in this way, but his happier genius suggested to him a better path. It occurred to him, that if his integrals, without being specially found, could be shown to vanish upon the whole, under certain conditions of the problem, this circumstance would correspond to arrangements of his apparatus in which a state of equilibrium was preserved, however the form of some of the parts might be changed. He found two such cases, which were of great importance to the theory. The first of these cases proved that the force exerted by any element of the voltaic wire might be resolved into other forces by a theorem resembling the well-known proposition of the parallelogram of forces. This was proved by showing that the action of a straight wire is the same with that of another wire which joins the same extremities, but is bent and contorted in any way whatever. But it still remained necessary to determine two fundamental quantities; one which expressed the *power* of the distance according to which the force varied; the other, the degree in which the force is affected by the *obliquity* of the elements. One of the general causes of equilibrium, of which we have spoken, gave a relation between these two quantities;[2] and as the power was naturally, and, as it afterwards appeared, rightly conjectured to be the inverse square, the other quantity also was determined; and the general problem of electrodynamical action was fully solved.

If Ampère had not been an accomplished analyst, he would not have been able to discover the condition on which the nullity of the integral in this case depended.[3] And throughout his labors, we find reason to admire, both his mathematical skill, and his steadiness of thought; although these excellences are by no means accompanied throughout with corresponding clearness and elegance of exposition in his writings.

*Reception of Ampère's Theory.*—Clear mathematical conceptions, and some familiarity with mathematical operations, were needed by readers also, in order to appreciate the evidence of the theory; and, therefore, we need not feel any surprise if it was, on its publication and establishment, hailed with far less enthusiasm than so remarkable a triumph of generalizing power might appear to deserve. For some time, indeed, the greater portion of the public were naturally held in suspense by the opposing weight of rival names. The Amperian theory did not make its way without contention and competition. The electro-magnetic experiments, from their first appearance, gave a clear promise of some new and wide generalization; and held out a prize of honor and fame to him who should be first in giving the right interpretation of the riddle. In France, the emulation for such reputation is perhaps more vigilant and anxious than it is elsewhere; and we see, on this as on other occasions, the scientific host of Paris springing upon a new subject with an impetuosity which, in a short time, runs into controversies for priority or for victory. In this case, M. Biot, as well as Ampère, endeavored to reduce the electro-magnetic phenomena to general laws. The discussion between him and Ampère turned on some points which are curious. M. Biot was disposed to consider as an elementary action, the force which an element of a voltaic wire exerts upon a magnetic particle, and which is, as we have seen, at right angles to their mutual distance; and he conceived that

[2] Communication to the Acad. Sc., June 10, 1822. See Ampère, *Recueil*, p 292.

[3] *Recueil*, p. 314.

the equal reaction which necessarily accompanies this action acts oppositely to the action, not in the same line, but in a parallel line, at the other extremity of the distance; thus forming a primitive *couple* to use a technical expression borrowed from mechanics. To this Ampère objected,[4] that the *direct* opposition of all elementary action and reaction was a universal and necessary mechanical law. He showed too that such a couple as had been assumed, would follow as a *derivative* result from his theory. And in comparing his own theory with that in which the voltaic wire is assimilated to a collection of transverse magnets, he was also able to prove that no such assemblage of forces acting to and from fixed points, as the forces of magnets do act, could produce a continued motion like that discovered by Faraday. This, indeed, was only the well-known demonstration of the impossibility of a perpetual motion. If, instead of a collection of magnets, the adverse theorists had spoken of a magnetic *current*, they might probably interpret their expressions so as to explain the facts; that is, if they considered every element of such a current as a magnet, and consequently, every point of it as being a north and a south point at the same instant. But to introduce such a conception of a magnetic current was to abandon all the laws of magnetic action hitherto established; and consequently to lose all that gave the hypothesis its value. The idea of an electric current, on the other hand, was so far from being a new and hazardous assumption, that it had already been forced upon philosophers from the time of Volta; and in this current, the relation of *preceding* and *succeeding*, which necessarily existed between the extremities of any element, introduced that relative polarity on which the success of the explanations of the facts depended. And thus in this controversy, the theory of Ampère has a great and undeniable superiority over the rival hypotheses.

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## CHAPTER VII.

### Consequences of the Electrodynamic Theory.

IT is not necessary to state the various applications which were soon made of the electro-magnetic discoveries. But we may notice one

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[4] Ampère, *Théorie*, p. 154.

of the most important,—the *Galvanometer*, an instrument which, by enabling the philosopher to detect and to measure extremely minute electrodynamic actions, gave an impulse to the subject similar to that which it received from the invention of the Leyden Phial, or the Voltaic Pile. The strength of the voltaic current was measured, in this instrument, by the deflection produced in a compass-needle; and its sensibility was multiplied by making the wire pass repeatedly above and below the needle. Schweigger, of Halle, was one of the first devisers of this apparatus.

The substitution of electro-magnets, that is, of spiral tubes composed of voltaic wires, for common magnets, gave rise to a variety of curious apparatus and speculations, some of which I shall hereafter mention.

[2nd Ed.] [When a voltaic apparatus is in action, there may be conceived to be a current of electricity running through its various elements, as stated in the text. The force of this current in various parts of the circuit has been made the subject of mathematical investigation by M. Ohm.[1] The problem is in every respect similar to that of the flow of heat through a body, and taken generally, leads to complex calculations of the same kind. But Dr. Ohm, by limiting the problem in the first place by conditions which the usual nature and form of voltaic apparatus suggest, has been able to give great simplicity to his reasonings. These conditions are, the linear form of the conductors (wires) and the steadiness of the electric state. For this part of the problem Dr. Ohm's reasonings are as simple and as demonstrative as the elementary propositions of Mechanics. The formulæ for the electric force of a voltaic current to which he is led have been experimentally verified by others, especially Fechner,[2] Gauss,[3] Lenz, Jacobi, Poggendorf, and Pouillet.

Among ourselves, Mr. Wheatstone has confirmed and applied the views of M. Ohm, in a Memoir[4] *On New Instruments and Processes for determining the Constants of a Voltaic Circuit.* He there remarks that the clear ideas of electromotive forces and resistances, substituted by Ohm for the vague notions of quantity and intensity which have long been prevalent, give satisfactory explanations of the most important difficulties, and express the laws of a vast number of phenomena

[1] *Die Galvanische Kette Mathematisch bearbeitet von Dr. G. S. Ohm,* Berlin, 1827.

[2] *Mass-bestimmengen über die Galvanische Kette.* Leipzig, 1831.

[3] *Results of the Magnetic Association.* [4] *Phil. Trans.* 1843. Pt. 11.

in formulæ of remarkable simplicity and generality. In this Memoir, Professor Wheatstone describes an instrument which he terms *Rheostat*, because it brings to a common standard the voltaic currents which are compared by it. He generalizes the language of the subject by employing the term *rheomotor* for any apparatus which originates an electric current (whether voltaic or thermoelectric, &c.) and *rheometer* for any instrument to measure the force of such a current. It appears that the idea of constructing an instrument of the nature of the Rheostat had occurred also to Prof. Jacobi, of St. Petersburg.]

The galvanometer led to the discovery of another class of cases in which the electrodynamical action was called into play, namely, those in which a circuit, composed of two metals only, became electro-magnetic by *heating* one part of it. This discovery of *thermo-electricity* was made by Professor Seebeck of Berlin, in 1822, and prosecuted by various persons; especially by Prof. Cumming[6] of Cambridge, who, early in 1823, extended the examination of this property to most of the metals, and determined their thermo-electric order. But as these investigations exhibited no new mechanical effects of electromotive forces, they do not now further concern us; and we pass on, at present, to a case in which such forces act in a manner different from any of those already described.

### Discovery of Diamagnetism.

[2nd Ed.] [By the discoveries just related, a cylindrical spiral of wire through which an electric current is passing is identified with a magnet; and the effect of such a spiral is increased by placing in it a core of soft iron. By the use of such a combination under the influence of a voltaic battery, magnets are constructed far more powerful than those which depend upon the permanent magnetism of iron. The electro-magnet employed by Dr. Faraday in some of his experiments would sustain a hundred-weight at either end.

By the use of such magnets Dr. Faraday discovered that, besides iron, nickel and cobalt, which possess magnetism in a high degree, many bodies are magnetic in a slight degree. And he made the further very important discovery, that of those substances which are not magnetic, many, perhaps all, possess an opposite property, in virtue of which he terms them *diamagnetic*. The opposition is of this kind;—

[6] *Camb. Trans.* vol. ii. p. 62. *On the Development of Electro-Magnetism by Heat*

that magnetic bodies in the form of bars or needles, if free to move, arrange themselves in the *axial* line joining the poles; diamagnetic bodies under the same circumstances arrange themselves in an *equatorial* position, perpendicular to the axial line. And this tendency he conceives to be the result of one more general; that whereas magnetic bodies are attracted to the poles of a magnet, diamagnetic bodies are repelled from the poles. The list of diamagnetic bodies includes all kinds of substances; not only metals, as antimony, bismuth, gold, silver, lead, tin, zinc, but many crystals, glass, phosphorus, sulphur, sugar, gum, wood, ivory; and even flesh and fruit.

It appears that M. le Bailli had shown, in 1829, that both bismuth and antimony and bismuth repelled the magnetic needle; and as Dr. Faraday remarks, it is astonishing that such an experiment should have remained so long without further results. M. Becquerel in 1827 observed, and quoted Coulomb as having also observed, that a needle of wood under certain conditions pointed across the magnetic curves; and also stated that he had found a needle of wood place itself parallel to the wires of a galvanometer. This he referred to a magnetism transverse to the length. But he does not refer the phenomena to elementary repulsive action, nor show that they are common to an immense class of bodies, nor distinguish this diamagnetic from the magnetic class, as Faraday has taught us to do.

I do not dwell upon the peculiar phenomena of copper which, in the same series of researches, are traced by Dr. Faraday to the combined effect of its diamagnetic character, and the electric currents excited in it by the electro-magnet; nor to the optical phenomena manifested by certain transparent diamagnetic substances under electric action; as already stated in Book ix.[6]]

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## CHAPTER VIII.

### Discovery of the Laws of Magneto-Electric Induction.—Faraday.

It was clearly established by Ampère, as we have seen that magnetic action is a peculiar form of electromotive actions, and that, in

[6] See the *Twentieth Series of Experimental Researches in Electricity*, read to the Royal Society, Dec. 18, 1845.

this kind of agency, action and reaction are equal and opposite. It appeared to follow almost irresistibly from these considerations, that magnetism might be made to produce electricity, as electricity could be made to imitate all the effects of magnetism. Yet for a long time the attempts to obtain such a result were fruitless. Faraday, in 1825, endeavored to make the conducting-wire of the voltaic circuit excite electricity in a neighboring wire by induction, as the conductor charged with common electricity would have done, but he obtained no such effect. If this attempt had succeeded, the magnet, which, for all such purposes, is an assemblage of voltaic circuits, might also have been made to excite electricity. About the same time, an experiment was made in France by M. Arago, which really involved the effect thus sought; though this effect was not extricated from the complex phenomenon, till Faraday began his splendid career of discovery on this subject in 1832. Arago's observation was, that the rapid revolution of a conducting-plate in the neighborhood of a magnet, gave rise to a force acting on the magnet. In England, Messrs. Barlow and Christie, Herschel and Babbage, repeated and tried to analyse this experiment; but referring the forces only to conditions of space and time, and overlooking the real cause, the electrical currents produced by the motion, these philosophers were altogether unsuccessful in their labors. In 1831, Faraday again sought for electro-dynamical induction, and after some futile trials, at last found it in a form different from that in which he had looked for it. It was then seen, that at the precise time of making or breaking the contact which closed the galvanic circuit, a momentary effect was induced in a neighboring wire, but disappeared instantly.[1] Once in possession of this fact, Mr. Faraday ran rapidly up the ladder of discovery, to the general point of view.—Instead of suddenly making or breaking the contact of the inducing circuit, a similar effect was produced by removing the inducible wire nearer to or further from the circuit;[2]—the effects were increased by the proximity of soft iron;[3]—when the soft iron was affected by an ordinary magnet instead of the voltaic wire, the same effect still recurred;[4]—and thus it appeared, that by making and breaking magnetic contact, a momentary electric current was produced. It was produced also by moving the magnet;[5]—or by moving the wire with reference to the magnet.[6] Finally, it was found that the earth might supply the place of a magnet

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[1] *Phil. Trans* 1832, p. 127, First Series, Art. 10. [2] Art. 18. [3] Art. 28.
[4] Art. 37. [5] Art. 39. [6] Art. 53

in this as in other experiments;[7] and the mere motion of a wire, under proper circumstances, produced in it, it appeared, a momentary electric current.[8] These facts were curiously confirmed by the results in special cases. They explained Arago's experiments: for the momentary effect became permanent by the revolution of the plate. And without using the magnet, a revolving plate became an electrical machine;[9]—a revolving globe exhibited electro-magnetic action,[10] the circuit being complete in the globe itself without the addition of any wire;—and a mere motion of the wire of a galvanometer produced an electro-dyna mic effect upon its needle.[11]

But the question occurs, What is the general law which determines the direction of electric currents thus produced by the joint effects of motion and magnetism? Nothing but a peculiar steadiness and clearness in his conceptions of space, could have enabled Mr. Faraday to detect the law of this phenomenon. For the question required that he should determine the mutual relations in space which connect the magnetic poles, the position of the wire, the direction of the wire's motion, and the electrical current produced in it. This was no easy problem; indeed, the mere relation of the magnetic to the electric forces, the one set being perpendicular to the other, is of itself sufficient to perplex the mind; as we have seen in the history of the electrodynamical discoveries. But Mr. Faraday appears to have seized at once the law of the phenomena. "The relation," he says,[12] "which holds between the magnetic pole, the moving wire or metal, and the direction of the current evolved, is very simple (so it seemed to him) although rather difficult to express." He represents it by referring position and motion to the "magnetic curves," which go from a magnetic pole to the opposite pole. The current in the wire sets one way or the other, according to the direction in which the motion of the wire cuts these curves. And thus he was enabled, at the end of his Second Series of *Researches* (December, 1831), to give, in general terms, the law of nature to which may be referred the extraordinary number of new and curious experiments which he has stated;[13]—namely, that if a wire move so as to cut a magnetic curve, a power is called into action which tends to urge a magnetic current through the wire; and that if a mass move so that its parts do not move in the same direction across the magnetic curves

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[7] Second Series, *Phil. Trans.* p. 163. [8] Art. 141. [9] Art. 150 [10] Art. 164.
[11] Art. 171. [12] First Series, Art. 114. [13] Art. 256—264.

and with the same angular velocity, electrical currents are called into play in the mass.

This rule, thus simple from its generality, though inevitably complex in every special case, may be looked upon as supplying the first demand of philosophy, *the law of the phenomena;* and accordingly Dr. Faraday has, in all his subsequent researches on magneto-electric induction, applied this law to his experiments; and has thereby unravelled an immense amount of apparent inconsistency and confusion, for those who have followed him in his mode of conceiving the subject.

But yet other philosophers have regarded these phenomena in other points of view, and have stated the laws of the phenomena in a manner different from Faraday's, although for the most part equivalent to his. And these attempts to express, in the most simple and general form, the law of the phenomena of magneto-electrical induction, have naturally been combined with the expression of other laws of electrical and magnetical phenomena. Further, these endeavors to connect and generalize the Facts have naturally been clothed in the garb of various Theories:—the *laws of phenomena* have been expressed in terms of the supposed *causes of the phenomena;* as fluids, attractions and repulsions, particles with currents running through them or round them, physical lines of force, and the like. Such views, and the conflict of them, are the natural and hopeful prognostics of a theory which shall harmonize their discords and include all that each contains of Truth. The fermentation at present is perhaps too great to allow us to see clearly the truth which lies at the bottom. But a few of the leading points of recent discussions on these subjects will be noticed in the Additions to this volume.

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## CHAPTER IX.

### Transition to Chemical Science.

THE preceding train of generalization may justly appear extensive, and of itself well worthy of admiration. Yet we are to consider all that has there been established as only one-half of the science to which it belongs,—one limb of the colossal form of Chemistry. We have ascertained, we will suppose, the laws of Electric Polarity; but we have then to ask, What is the relation of this Polarity to Chemical Composition? This was the great problem which, constantly present to the minds of electro-chemical inquirers, drew them on, with the promise of some deep and comprehensive insight into the mechanism of nature. Long tasks of research, though only subsidiary to this, were cheerfully undertaken. Thus Faraday[1] describes himself as compelled to set about satisfying himself of the identity of common, animal, and voltaic electricity, as "the decision of a doubtful point which interfered with the extension of his views, and destroyed the strictness of reasoning." Having established this identity, he proceeded with his grand undertaking of electro-chemical research.

The connexion of electrical currents with chemical action, though kept out of sight in the account we have hitherto given, was never forgotten by the experimenters; for, in fact, the modes in which electrical currents were excited, were chemical actions;—the action of acids and metals on each other in the voltaic trough, or in some other form. The dependence of the electrical effect on these chemical actions, and still more, the chemical actions produced by the agency of the poles of the circuit, had been carefully studied; and we must now relate with what success.

But in what terms shall we present this narration? We have spoken of chemical actions,—but what kind of actions are these? *Decomposition;* the *resolution* of compounds into their ingredients; the separation of *acids* from *bases;* the reduction of bodies to *simple elements.* These names open to us a new drama; they are words which belong to a different set of relations of things, a different train of scientific inductions, a different system of generalizations, from any with which we have hitherto been concerned. We must learn to understand these phrases, before we can advance in our history of human knowledge.

And how are we to learn the meaning of this collection of words? In what other language shall it be explained? In what terms shall we define these new expressions? To this we are compelled to reply, that we cannot translate these terms into any ordinary language;—that we cannot define them in any terms already familiar to us. Here, as in all other branches of knowledge, the meaning of words is to be sought in the progress of thought; the history of science is our dic-

[1] Dec. 1832. *Researches,* 266.

tionary; the steps of scientific induction are our definitions. It is only by going back through the successful researches of men respecting the composition and elements of bodies, that we can learn in what sense such terms must be understood, so as to convey real knowledge. In order that they may have a meaning for us, we must inquire what meaning they had in the minds of the authors of our discoveries.

And thus we cannot advance a step, till we have brought up our history of Chemistry to the level of our history of Electricity;—till we have studied the progress of the analytical, as well as the mechanical sciences. We are compelled to pause and look backwards here; just as happened in the history of astronomy, when we arrived at the brink of the great mechanical inductions of Newton, and found that we must trace the history of Mechanics, before we could proceed to mechanical Astronomy. The terms "force, attraction, inertia, momentum," sent us back into preceding centuries then, just as the terms "composition" and "element" send us back now.

Nor is it to a small extent that we have thus to double back upon our past advance. Next to Astronomy, Chemistry is one of the most ancient of sciences;—the field of the earliest attempts of man to command and understand nature. It has held men for centuries by a kind of fascination; and innumerable and endless are the various labors, the failures and successes, the speculations and conclusions, the strange pretences and fantastical dreams, of those who have pursued it. To exhibit all these, or give any account of them, would be impossible; and for our design, it would not be pertinent. To extract from the mass that which is to our purpose, is difficult; but the attempt must be made. We must endeavor to analyse the history of Chemistry, so far as it has tended towards the establishment of general principles. We shall thus obtain a sight of generalizations of a new kind, and shall prepare ourselves for others of a higher order.

# BOOK XIV.

*THE ANALYTICAL SCIENCE*

# HISTORY OF CHEMISTRY.

. . . . . . . Soon had his crew
Opened into the hill a spacious wound,
And digged out ribs of gold . . . . .
Anon out of the earth a fabric huge
Rose like an exhalation, with the sound
Of dulcet symphonies and voices sweet,
Built like a temple.

MILTON. *Paradise Lost*, I.

# CHAPTER I.

## Improvement of the Notion of Chemical Analysis, and Recognition of it as the Spagiric Art.

THE doctrine of "the four elements" is one of the oldest monuments of man's speculative nature; goes back, perhaps, to times anterior to Greek philosophy; and as the doctrine of Aristotle and Galen, reigned for fifteen hundred years over the Gentile, Christian, and Mohammedan world. In medicine, taught as the doctrine of the four "elementary qualities," of which the human body and all other substances are compounded, it had a very powerful and extensive influence upon medical practice. But this doctrine never led to any attempt actually to analyse bodies into their supposed elements: for composition was inferred from the resemblance of the qualities, not from the separate exhibition of the ingredients; the supposed analysis was, in short, a decomposition of the body into adjectives, not into substances.

This doctrine, therefore, may be considered as a negative state, antecedent to the very beginning of chemistry; and some progress beyond this mere negation was made, as soon as men began to endeavor to compound and decompound substances by the use of fire or mixture, however erroneous might be the opinions and expectations which they combined with their attempts. Alchemy is a step in chemistry, so far as it implies the recognition of the work of the cupel and the retort, as the produce of analysis and synthesis. How perplexed and perverted were the forms in which this recognition was clothed,—how mixed up with mythical follies and extravagancies, we have already seen; and the share which Alchemy had in the formation of any sounder knowledge, is not such as to justify any further notice of that pursuit.

The result of the attempts to analyse bodies by heat, mixture, and the like processes, was the doctrine that the first principles of things are *three*, not four; namely, *salt*, *sulphur*, and *mercury*; and that, of these three, all things are compounded. In reality, the doctrine, as thus stated, contained no truth which was of any value; for, though the chemist could extract from most bodies portions which he called salt.

and sulphur, and mercury, these names were given, rather to save the hypothesis, than because the substances were really those usually so called: and thus the supposed analyses proved nothing, as Boyle justly urged against them.[1]

The only real advance in chemical theory, therefore, which we can ascribe to the school of *the three principles*, as compared with those who held the ancient dogma of the four elements, is, the acknowledgment of the changes produced by the chemist's operations, as being changes which were to be accounted for by the union and separation of substantial elements, or, as they were sometimes called, of *hypostatical principles.* The workmen of this school acquired, no doubt, a considerable acquaintance with the results of the kinds of processes which they pursued; they applied their knowledge to the preparation of new medicines; and some of them, as Paracelsus and Van Helmont, attained, in this way, to great fame and distinction: but their merits, as regards theoretical chemistry, consist only in a truer conception of the problem, and of the mode of attempting its solution, than their predecessors had entertained.

This step is well marked by a word which, about the time of which we speak, was introduced to denote the chemist's employment. It was called the *Spagiric art*, (often misspelt *Spagyric*,) from two Greek words, (σπάω, ἀγείρω,) which mean to *separate* parts, and to *unite* them. These two processes, or in more modern language, *analysis* and *synthesis*, constitute the whole business of the chemist. We are not making a fanciful arrangement, therefore, when we mark the recognition of this object as a step in the progress of chemistry. I now proceed to consider the manner in which the conditions of this analysis and synthesis were further developed.

## CHAPTER II.

### Doctrine of Acid and Alkali.—Sylvius.

AMONG the results of mixture observed by chemists, were many instances in which two ingredients, each in itself pungent or destructive, being put together, became mild and inoperative; each

[1] Shaw's Boyle. *Skeptical Chymi t*, pp. 312, 313 &c.

counteracting and neutralizing the activity of the other. The notion of such opposition and neutrality is applicable to a very wide range of chemical processes. The person who appears first to have steadily seized and generally applied this notion is Francis de la Boé Sylvius; who was born in 1614, and practised medicine at Amsterdam, with a success and reputation which gave great currency to his opinions on that art.[1] His chemical theories were propounded as subordinate to his medical doctrines; and from being thus presented under a most important practical aspect, excited far more attention than mere theoretical opinions on the composition of bodies could have done. Sylvius is spoken of by historians of science, as the founder of the *iatro-chemical* sect among physicians; that is, the sect which considers the disorders in the human frame as the effects of chemical relations of the fluids, and applies to them modes of cure founded upon this doctrine. We have here to speak, not of his physiological, but of his chemical views.

The distinction of *acid* and *alkaline* bodies (*acidum*, *lixivum*) was familiar before the time of Sylvius; but he framed a system, by considering them both as eminently acrid and yet opposite, and by applying this notion to the human frame. Thus[2] the lymph contains an acid, the bile an alkaline salt. These two opposite acrid substances, when they are brought together, *neutralize* each other (*infringunt*), and are changed into an intermediate and milder substance.

The progress of this doctrine, as a physiological one, is an important part of the history of medical science in the seventeenth century; but with that we are not here concerned. But as a chemical doctrine, this notion of the opposition of acid and alkali, and of its very general applicability, struck deep root, and has not been eradicated up to our own time. Boyle, indeed, whose disposition led him to suspect all generalities, expressed doubts with regard to this view;[3] and argued that the supposition of acid and alkaline parts in all bodies was precarious, their offices arbitrary, and the notion of them unsettled. Indeed it was not difficult to show, that there was no one certain criterion to which all supposed acids conformed. Yet the general conception of such a combination as that of acid and alkali was supposed to

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[1] Sprengel. *Geschichte der Arzneykunde*, vol. iv. Thomson's *History of Chemistry* in the corresponding part is translated from Sprengel.

[2] *De Methodo Medendi*, Amst. 1679. Lib. ii. cap. 28, sects. 8. and 53.

[3] Shaw's *Boyle*, iii. p. 432.

be, served so well to express many chemical facts, that it kept its ground. It is found, for instance, in Lemery's *Chemistry*, which was one of those in most general use before the introduction of the phlogistic theory. In this work (which was translated into English by Keill, in 1698) we find alkalies defined by their effervescing with acids.[4] They were distinguished as the *mineral* alkali (soda), the *vegetable* alkali (potassa), and the *volatile* alkali (ammonia). Again, in Macquer's *Chemistry*, which was long the text-book in Europe during the reign of phlogiston, we find acids and alkalies, and their union, in which they rob each other of their characteristic properties, and form neutral salts, stated among the leading principles of the science.[5]

In truth, the mutual relation of acids to alkalies was the most essential part of the knowledge which chemists possessed concerning them. The importance of this relation arose from its being the first distinct form in which the notion of chemical attraction or affinity appeared. For the acrid or caustic character of acids and alkalies is, in fact, a tendency to alter the bodies they touch, and thus to alter themselves; and the neutral character of the compounds in the absence of any such proclivity to change. Acids and alkalies have a strong disposition to unite. They combine, often with vehemence, and produce neutral salts; they exhibit, in short, a prominent example of the chemical attraction, or affinity, by which two ingredients are formed into a compound. The relation of *acid* and *base* in a salt is, to this day, one of the main grounds of all theoretical reasonings.

The more distinct development of the notion of such chemical attraction, gradually made its way among the chemists of the latter part of the seventeenth and the beginning of the eighteenth century, as we may see in the writings of Boyle, Newton, and their followers. Beecher speaks of this attraction as a *magnetism;* but I do not know that any writer in particular, can be pointed out as the person who firmly established the general notion of *chemical attraction.*

But this idea of chemical attraction became both more clear and more extensively applicable, when it assumed the form of the doctrine of *elective* attractions, in which shape we must now speak of it.

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[4] Lemery, p. 25.

[5] Macquer, p. 19.

## CHAPTER III.

### Doctrine of Elective Attractions. Geoffroy. Bergman

THOUGH the chemical combinations of bodies had already been referred to attraction, in a vague and general manner, it was impossible to explain the changes that take place, without supposing the attraction to be greater or less, according to the nature of the body. Yet it was some time before the necessity of such a supposition was clearly seen. In the history of the French Academy for 1718 (published 1719), the writer of the introductory notice (probably Fontenelle) says, "That a body which is united to another, for example, a solvent which has penetrated a metal, should quit it to go and unite itself with another which we present to it, is a thing of which the possibility had never been guessed by the most subtle philosophers, and of which the explanation even now is not easy." The doctrine had, in fact, been stated by Stahl, but the assertion just quoted shows, at least, that it was not familiar. The principle, however, is very clearly stated[1] in a memoir in the same volume, by Geoffroy, a French physician of great talents and varied knowledge. "We observe in chemistry," he says, "certain relations amongst different bodies, which cause them to unite. These relations have their *degrees* and their *laws*. We observe their different degrees in this;—that among different matters jumbled together, which have a certain disposition to unite, we find that one of these substances always unites constantly with a certain other, preferably to all the rest." He then states that those which unite by preference, have "plus de rapport," or, according to a phrase afterwards used, more *affinity*. "And I have satisfied myself," he adds, "that we may deduce, from these observations, the following proposition, which is very extensively true, though I cannot enunciate it as universal, not having been able to examine all the possible combinations, to assure myself that I should find no exception." The proposition which he states in this admirable spirit of philosophical caution, is this: "In all cases where two substances

[1] *Mém. Acad. Par.* 1718, p. 202.

which have any disposition to combine, are united; if there approaches them a third, which has more affinity with one of the two, this one unites with the third and lets go the other." He then states these affinities in the form of a Table; placing a substance at the head of each column, and other substances in succession below it, according to the order of their affinities for the substance which stands at the head. He allows that the separation is not always complete (an imperfection which he ascribes to the glutinosity of fluids and other causes), but, with such exceptions, he defends very resolutely and successfully his Table, and the notions which it implies.

The value of such a tabulation was immense at the time, and is even still very great; it enabled the chemist to trace beforehand the results of any operation; since, when the ingredients were given, he could see which were the strongest of the affinities brought into play, and, consequently, what compounds would be formed. Geoffroy himself gave several good examples of this use of his table. It was speedily adopted into works on chemistry. For instance, Macquer[2] places it at the end of his book; "taking it," as he says, "to be of great use at the end of an elementary tract, as it collects into one point of view, the most essential and fundamental doctrines which are dispersed through the work."

The doctrine of *Elective Attractions*, as thus promulgated, contained so large a mass of truth, that it was never seriously shaken, though it required further development and correction. In particular the celebrated work of Torbern Bergman, professor at Upsala, *On Elective Attractions*, published in 1775, introduced into it material improvements. Bergman observed, that not only the order of attractions, but the *sum* of those attractions which had to form the new compounds, must be taken account of, in order to judge of the result. Thus,[3] if we have a combination of two elements, $P$, $s$, (potassa and vitriolic acid), and another combination, $L$, $m$, (lime and muriatic acid,) though $s$ has a greater affinity for $P$ than for $L$, yet the sum of the attractions of $P$ to $m$, and of $L$ to $s$, is greater than that of the original compounds, and therefore if the two combinations are brought together, the new compounds, $P$, $m$, and $L$, $s$, are formed.

The Table of Elective Attractions, modified by Bergman in pursuance of these views, and corrected according to the advanced knowledge of the time, became still more important than before. The next step

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[2] Pref., p. 13. [3] *Elect. Attract.*, p. 19.

was to take into account the quantities of the elements which combined; but this leads us into a new train of investigation, which was, indeed, a natural sequel to the researches of Geoffroy and Bergman.

In 1803, however, a chemist of great eminence, Berthollet, published a work (*Essai de Statique Chimique*), the tendency of which appeared to be to throw the subject back into the condition in which it had been before Geoffroy. For Berthollet maintained that the rules of chemical combination were not definite, and dependent on the nature of the substances alone, but indefinite, depending on the quantity present, and other circumstances. Proust answered him, and as Berzelius says,[4] "Berthollet defended himself with an acuteness which makes the reader hesitate in his judgment; but the great mass of facts finally decided the point in favor of Proust." Before, however, we trace the result of these researches, we must consider Chemistry as extending her inquiries to combustion as well as mixture, to airs as well as fluids and solids, and to weight as well as quality. These three steps we shall now briefly treat of.

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## CHAPTER IV.

### Doctrine of Acidification and Combustion.—Phlogistic Theory.

PUBLICATION *of the Theory by Beccher and Stahl.*—It will be recollected that we are tracing the history of the *progress* only of Chemistry, not of its errors;—that we are concerned with doctrines only so far as they are true, and have remained part of the received system of chemical truths. The Phlogistic Theory was deposed and succeeded by the Theory of Oxygen. But this circumstance must not lead us to overlook the really sound and permanent part of the opinions which the founders of the phlogistic theory taught. They brought together, as processes of the same kind, a number of changes which at first appeared to have nothing in common; as acidification, combustion, respiration. Now this classification is true; and its importance remains undiminished, whatever are the explanations which we adopt of the processes themselves.

The two chemists to whom are to be ascribed the merit of this step, and the establishment of the *phlogistic theory* which they connected

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[4] *Chem.* t iii. p. 23.

with it, are John Joachim Beccher and George Ernest Stahl; the former of whom was professor at Mentz, and physician to the Elector of Bavaria (born 1625, died 1682); the latter was professor at Halle, and afterwards royal physician at Berlin (born 1660, died 1734). These two men, who thus contributed to a common purpose, were very different from each other. The first was a frank and ardent enthusiast in the pursuit of chemistry, who speaks of himself and his employments with a communicativeness and affection both amusing and engaging. The other was a teacher of great talents and influence, but accused of haughtiness and moroseness; a character which is well borne out by the manner in which, in his writings, he anticipates an unfavorable reception, and defies it. But it is right to add to this that he speaks of Beccher, his predecessor, with an ungrudging acknowledgment of obligations to him, and a vehement assertion of his merit as the founder of the true system, which give a strong impression of Stahl's justice and magnanimity.

Beccher's opinions were at first promulgated rather as a correction than a refutation of the doctrine of the three principles, salt, sulphur, and mercury. The main peculiarity of his views consists in the offices which he ascribes to his *sulphur*, these being such as afterwards induced Stahl to give the name of *Phlogiston* to this element. Beccher had the sagacity to see that the reduction of metals to an earthy form (*calx*), and the formation of sulphuric acid from sulphur, are operations connected by a general analogy, as being alike processes of combustion. Hence the metal was supposed to consist of an earth, and of something which, in the process of combustion, was separated from it; and, in like manner, sulphur was supposed to consist of the sulphuric acid, which remained after its combustion, and of the combustible part or true sulphur, which flew off in the burning. Beccher insists very distinctly upon this difference between his element sulphur and the "sulphur" of his Paracelsian predecessors.

It must be considered as indicating great knowledge and talent in Stahl, that he perceived so clearly what part of the views of Beccher was of general truth and permanent value. Though he[1] everywhere gives to Beccher the credit of the theoretical opinions which he promulgates, ("Beccheriana sunt quæ profero,") it seems certain that he had the merit, not only of proving them more completely, and applying them more widely than his forerunner, but also of conceiving them

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[1] *Stahl, Præf. ad Specim. Becch.* 1703.

with a distinctness which Becher did not attain. In 1697, appeared Stahl's *Zymotechnia Fundamentalis* (the Doctrine of Fermentation), "simulque *experimentum novum* sulphur verum arte producendi." In this work (besides other tenets which the author considered as very important), the opinion published by Becher was now maintained in a very distinct form;—namely, that the process of forming sulphur from sulphuric acid, and of restoring the metals from their calces, are analogous, and consist alike in the addition of some combustible element, which Stahl termed *phlogiston* (φλογίστον, *combustible*). The experiment most insisted on in the work now spoken of,[2] was the formation of sulphur from sulphate of potass (or of soda) by fusing the salt with an alkali, and throwing in coals to supply phlogiston. This is the "experimentum novum." Though Stahl published an account of this process, he seems to have regretted his openness. "He denies not," he says, "that he should peradventure have dissembled this experiment as the true foundation of the Becherian assertion concerning the nature of sulphur, if he had not been provoked by the pretending arrogance of some of his contemporaries."

From this time, Stahl's confidence in his theory may be traced becoming more and more settled in his succeeding publications. It is hardly necessary to observe here, that the explanations which his theory gives are easily transformed into those which the more recent theory supplies. According to modern views, the addition of oxygen takes place in the formation of acids and of calces, and in combustion, instead of the subtraction of phlogiston. The coal which Stahl supposed to supply the combustible in his experiment, does in fact absorb the liberated oxygen. In like manner, when an acid corrodes a metal, and, according to existing theory, combines with and oxidates it, Stahl supposed that the phlogiston separated from the metal and combined with the acid. That the explanations of the phlogistic theory are so generally capable of being translated into the oxygen theory, merely by inverting the supposed transfer of the combustible element, shows us how important a step towards the modern doctrines the phlogistic theory really was.

The question, whether these processes were in fact addition or subtraction, was decided by the balance, and belongs to a succeeding period of the science. But we may observe, that both Becher and Stahl were aware of the increase of weight which metals undergo in calcina

[2] P. 117.

tion; although the time had not yet arrived in which this fact was to be made one of the bases of the theory.

It has been said,[3] that in the adoption of the phlogistic theory, that is, in supposing the above-mentioned processes to be addition rather than subtraction, "of two possible roads the wrong was chosen, as if to prove the perversity of the human mind." But we must not forget how natural it was to suppose that some part of a body was *destroyed* or *removed* by combustion; and we may observe, that the merit of Becher and Stahl did not consist in the selection of one road or two, but in advancing so far as to reach this point of separation. That, having done this, they went a little further on the wrong line, was an error which detracted little from the merit or value of the progress really made. It would be easy to show, from the writings of phlogistic chemists, what important and extensive truths their theory enabled them to express simply and clearly.

That an enthusiastic temper is favorable to the production of great discoveries in science, is a rule which suffers no exception in the character of Becher. In his preface[4] addressed "to the benevolent reader" of his *Physica Subterranea*, he speaks of the chemists as a strange class of mortals, impelled by an almost insane impulse to seek their pleasure among smoke and vapor, soot and flame, poisons and poverty. "Yet among all these evils," he says, "I seem to myself to live so sweetly, that, may I die if I would change places with the Persian king." He is, indeed, well worthy of admiration, as one of the first who pursued the labors of the furnace and the laboratory, without the bribe of golden hopes. "My kingdom," he says, "is not of this world. I trust that I have got hold of my pitcher by the right handle,—the true method of treating this study. For the *Pseudochymists* seek gold; but the *true philosophers*, science, which is more precious than any gold."

The *Physica Subterranea* made no converts. Stahl, in his indignant manner, says,[5] "No one will wonder that it never yet obtained a physician or a chemist as a disciple, still less as an advocate." And again, "This work obtained very little reputation or estimation, or, to speak ingenuously, as far as I know, none whatever." In 1671, Becher published a supplement to his work, in which he showed how metal might be extracted from mud and sand. He offered to execute

[3] Herschel's *Introd. to Nat. Phil.* p. 300.

[4] Frankfort, 1681.

[5] Præf. *Phys. Sub.* 1703.

this at Vienna; but found that people there cared nothing about such novelties. He was then induced, by Baron D'Isola, to go to Holland for similar purposes. After various delays and quarrels, he was obliged to leave Holland for fear of his creditors; and then, I suppose, came to Great Britain, where he examined the Scottish and Cornish mines. He is said to have died in London in 1682.

Stahl's publications appear to have excited more notice, and led to controversy on the "so-called sulphur." The success of the experiment had been doubted, which, as he remarks, it was foolish to make a matter of discussion, when any one might decide the point by experiment; and finally, it had been questioned whether the substance obtained by this process were pure sulphur. The originality of his doctrine was also questioned, which, as he says, could not with any justice be impugned. He published in defence and development of his opinion at various intervals, as the *Specimen Beccherianum* in 1703, the *Documentum Theoriæ Beccherianæ*, a Dissertation *De Anatomia Sulphuris Artificialis*; and finally, *Casual Thoughts on the so-called Sulphur*, in 1718, in which he gave (in German) both a historical and a systematic view of his opinions on the nature of salts and of his Phlogiston.

*Reception and Application of the Theory.*—The theory that the formation of sulphuric acid, and the restoration of metals from their calces, are analogous processes, and consist in the addition of *phlogiston*, was soon widely received; and the Phlogistic School was thus established. From Berlin, its original seat, it was diffused into all parts of Europe. The general reception of the theory may be traced, not only in the use of the term "phlogiston," and of the explanations which it implies; but in the adoption of a nomenclature founded on those explanations, which, though not very extensive, is sufficient evidence of the prevalence of the theory. Thus when Priestley, in 1774, discovered oxygen, and when Scheele, a little later, discovered chlorine, these gases were termed *dephlogisticated air*, and *dephlogisticated marine acid*; while azotic acid gas, having no disposition to combustion, was supposed to be saturated with phlogiston, and was called *phlogisticated air*.

This phraseology kept its ground, till it was expelled by the antiphlogistic, or oxygen theory. For instance, Cavendish's papers on the chemistry of the airs are expressed in terms of it, although his researches led him to the confines of the new theory. We must now give an account of such researches, and of the consequent revolution in the science.

## CHAPTER V.

### Chemistry of Gases.—Black. Cavendish.

THE study of the properties of aëriform substances, or Pneumatic Chemistry, as it was called, occupied the chemists of the eighteenth century, and was the main occasion of the great advances which the science made at that period. The most material general truths which came into view in the course of these researches, were, that gases were to be numbered among the constituent elements of solid and fluid bodies; and that, in these, as in all other cases of composition, the compound was equal to the sum of its elements. The latter proposition, indeed, cannot be looked upon as a discovery, for it had been frequently acknowledged, though little applied; in fact, it could not be referred to with any advantage, till the aëriform elements, as well as others, were taken into the account. As soon as this was done, it produced a revolution in chemistry.

[2nd Ed.] [Though the view of the mode in which gaseous elements become fixed in bodies and determine their properties, had great additional light thrown upon it by Dr. Black's discoveries, as we shall see, the notion that solid bodies involve such gaseous elements was not new at that period. Mr. Vernon Harcourt has shown[1] that Newton and Boyle admitted into their speculations airs of various kinds, capable of fixation in bodies. I have, in the succeeding chapter (chap. vi.), spoken of the views of Rey, Hooke, and Mayow, connected with the function of airs in chemistry, and forming a prelude to the Oxygen Theory.]

Notwithstanding these preludes, the credit of the first great step in pneumatic chemistry is, with justice, assigned to Dr. Black, afterwards professor at Edinburgh, but a young man of the age of twenty-four at the time when he made his discovery.[2] He found that the difference between caustic lime and common limestone arose from this, that the latter substance consists of the former, combined with a certain air, which, being thus fixed in the solid body, he called *fixed air* (carbonic

[1] *Phil. Mag.* 1846.

[2] Thomson's *Hist. Chem.* i. 317.

acid gas). He found, too, that magnesia, caustic potash, and caustic soda, would combine with the same air, with similar results. This discovery consisted, of course, in a new interpretation of observed changes. Alkalies appeared to be made caustic by contact with quicklime: at first Black imagined that they underwent this change by acquiring igneous matter from the quicklime; but when he perceived that the lime gained, not lost, in magnitude as it became mild, he rightly supposed that the alkalies were rendered caustic by imparting their air to the lime. This discovery was announced in Black's inaugural dissertation, pronounced in 1755, on the occasion of his taking his degree of Doctor in the University of Edinburgh.

The chemistry of airs was pursued by other experimenters. The Honorable Henry Cavendish, about 1765, invented an apparatus, in which aërial fluids are confined by water, so that they can be managed and examined. This hydro-pneumatic apparatus, or as it is sometimes called, *the pneumatic trough*, from that time was one of the most indispensable parts of the chemist's apparatus. Cavendish,[3] in 1766, showed the identity of the properties of fixed air derived from various sources; and pointed out the peculiar qualities of *inflammable air* (afterwards called hydrogen gas), which, being nine times lighter than common air, soon attracted general notice by its employment for raising balloons. The promise of discovery which this subject now offered, attracted the confident and busy mind of Priestley, whose *Experiments and Observations on different kinds of Air* appeared in 1744–79. In these volumes, he describes an extraordinary number of trials of various kinds; the results of which were, the discovery of new kinds of air, namely, *phlogisticated air* (azotic gas), *nitrous air* (nitrous gas), and *dephlogisticated air* (oxygen gas).

But the discovery of new substances, though valuable in supplying chemistry with materials, was not so important as discoveries respecting their modes of composition. Among such discoveries, that of Cavendish, published in the *Philosophical Transactions* for 1784, and disclosing the composition of water by the union of two gases, oxygen and hydrogen, must be considered as holding a most distinguished place. He states,[4] that his "experiments were made principally with a view to find out the cause of the diminution which common air is well known to suffer, by all the various ways in which it is phlogisticated." And, after describing various unsuccessful attempts, he finds

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[3] *Phil. Trans.* 1766. [4] *Phil. Trans.* 1784, p. 119.

that when inflammable air is used in this phlogistication (or burning), the diminution of the common air is accompanied by the formation of a dew in the apparatus.[5] And thus he infers[6] that "almost all the inflammable air, and one-fifth of the common air, are turned into pure water."

Lavoisier, to whose researches this result was, as we shall soon see, very important, was employed in a similar attempt at the same time (1783), and had already succeeded,[7] when he learned from Dr. Blagden, who was present at the experiment, that Cavendish had made the discovery a few months sooner. Monge had, about the same time, made the same experiments, and communicated the result to Lavoisier and Laplace immediately afterwards. The synthesis was soon confirmed by a corresponding analysis. Indeed the discovery undoubtedly lay in the direct path of chemical research at the time. It was of great consequence in the view it gave of experiments in composition; for the small quantity of water produced in many such processes, had been quite overlooked; though, as it now appeared, this water offered the key to the whole interpretation of the change.

Though some objections to Mr. Cavendish's view were offered by Kirwan,[8] on the whole they were generally received with assent and admiration. But the bearing of these discoveries upon the new theory of Lavoisier, who rejected phlogiston, was so close, that we cannot further trace the history of the subject without proceeding immediately to that theory.

[2nd Ed.] [I have elsewhere stated,[9]—with reference to recent attempts to deprive Cavendish of the credit of his discovery of the composition of water, and to transfer it to Watt,—that Watt not only did not anticipate, but did not fully appreciate the discovery of Cavendish and Lavoisier; and I have expressed my concurrence with Mr. Vernon Harcourt's views, when he says,[10] that "Cavendish pared off from the current hypotheses their theory of combustion, and their affinities of imponderable for ponderable matter, as complicating chemical with physical considerations; and he then corrected and adjusted them with admirable skill to the actual phenomena, not binding the facts to the theory, but adapting the theory to the facts."

I conceive that the discussion which the subject has recently received, has left no doubt on the mind of any one who has perused the docu-

[5] *Phil. Trans.* 1784, p. 128. [6] Ib. p. 129. [7] *A. P.* 1781, p. 472. [8] *P. T.* 1784, p. 154.
[9] *Philosophy*, b. vi. c. 4. [10] *Address to the British Association*, 1839.

ments, that Cavendish is justly entitled to the honor of this discovery, which in his own time was never contested. The publication of his Journals of Experiments[11] shows that he succeeded in establishing the point in question in July, 1781. His experiments are referred to in an abstract of a paper of Priestley's, made by Dr. Maty, the secretary of the Royal Society, in June, 1783. In June, 1783, also, Dr. Blagden communicated the result of Cavendish's experiments to Lavoisier, at Paris. Watt's letter, containing his hypothesis that "water is composed of dephlogisticated air and phlogiston deprived of part of their latent or elementary heat; and that phlogisticated or pure air is composed of water deprived of its phlogiston and united to elementary heat and light," was not read till Nov. 1783; and even if it could have suggested such an experiment as Cavendish's (which does not appear likely), is proved, by the dates, to have had no share in doing so.

Mr. Cavendish's experiment was suggested by an experiment in which Warltire, a lecturer on chemistry at Birmingham, exploded a mixture of hydrogen and common air in a close vessel, in order to determine whether heat were ponderable.]

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## CHAPTER VI.

### Epoch of the Theory of Oxygen.—Lavoisier.

#### *Sect.* I.—*Prelude to the Theory.—Its Publication.*

We arrive now at a great epoch in the history of Chemistry. Few revolutions in science have immediately excited so much general notice as the introduction of the theory of oxygen. The simplicity and symmetry of the modes of combination which it assumed; and, above all, the construction and universal adoption of a nomenclature which applied to all substances, and which seemed to reveal their inmost constitution by their name, naturally gave it an almost irresistible sway over men's minds. We must, however, dispassionately trace the course of its introduction.

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[11] *Appendix* to Mr. V. Harcourt's *Address.*

Antoine Laurent Lavoisier, an accomplished French chemist, had pursued, with zeal and skill, researches such as those of Black, Cavendish, and Priestley, which we have described above. In 1774, he showed that, in the calcination of metals in air, the metal acquires as much weight as the air loses. It might appear that this discovery at once overturned the view which supposed the metal to be phlogiston *added* to the calx. Lavoisier's contemporaries were, however, far from allowing this; a greater mass of argument was needed to bring them to this conclusion. Convincing proofs of the new opinion were, however, rapidly supplied. Thus, when Priestley had discovered dephlogisticated air, in 1774, Lavoisier showed, in 1775, that fixed air consisted of charcoal and the dephlogisticated or pure air; for the mercurial calx which, heated by itself, gives out pure air, gives out, when heated with charcoal, fixed air,[1] which has, therefore, since been called *carbonic acid gas.*

Again, Lavoisier showed that the atmospheric air consists of pure or vital air, and of an *unvital* air, which he thence called *azot.* The vital air he found to be the agent in combustion, acidification, calcination, respiration; all of these processes were analogous: all consisted in a decomposition of the atmospheric air, and a fixation of the pure or vital portion of it.

But he thus arrived at the conclusion, that this pure air was added, in all the cases in which, according to the received theory, *phlogiston* was subtracted, and *vice versâ.* He gave the name[2] of *oxygen* (*principe oxygène*) to "the substance which thus unites itself with metals to form their calces, and with combustible substances to form acids."

A new theory was thus produced, which would account for all the facts which the old one would explain, and had besides the evidence of the balance in its favor. But there still remained some apparent objections to be removed. In the action of dilute acids on metals, inflammable air was produced. Whence came this element? The discovery of the decomposition of water sufficiently answered this question, and converted the objection into an argument on the side of the theory: and thus the decomposition of water was, in fact, one of the most critical events for the fortune of the Lavoisierian doctrine, and one which, more than any other, decided chemists in its favor. In succeeding years, Lavoisier showed the consistency of his theory with

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[1] *Mém. Ac. Par* 1775. [2] *Mem. Ac. Par.* 1781. p. 448.

all that was discovered concerning the composition of alcohol, oil, animal and vegetable substances, and many other bodies.

It is not necessary for us to consider any further the evidence for this theory, but we must record a few circumstances respecting its earlier history. Rey, a French physician, had in 1630, published a book, in which he inquires into the grounds of the increase of the weight of metals by calcination.[3] He says, "To this question, then, supported on the grounds already mentioned, I answer, and maintain with confidence, that the increase of weight arises from the air, which is condensed, rendered heavy and adhesive, by the heat of the furnace." Hooke and Mayow had entertained the opinion that the air contains a "nitrous spirit," which is the supporter of combustion. But Lavoisier disclaimed the charge of having derived anything from these sources; nor is it difficult to understand how the received generalizations of the phlogistic theory had thrown all such narrower explanations into obscurity. The merit of Lavoisier consisted in his combining the generality of Stahl with the verified conjectures of Rey and Mayow.

No one could have a better claim, by his early enthusiasm for science, his extensive knowledge, and his zealous labors, to hope that a great discovery might fall to his share, than Lavoisier. His father,[4] a man of considerable fortune, had allowed him to make science his only profession; and the zealous philosopher collected about him a number of the most active physical inquirers of his time, who met and experimented at his house one day in the week. In this school, the new chemistry was gradually formed. A few years after the publication of Priestley's first experiments, Lavoisier was struck with the presentiment of the theory which he was afterwards to produce. In 1772, he deposited[5] with the secretary of the Academy, a note which contained the germ of his future doctrines. "At that time," he says, in explaining this step, "there was a kind of rivalry between France and England in science, which gave importance to new experiments, and which sometimes was the cause that the writers of the one or other of the nations disputed the discovery with the real author." In 1777, the editor of the Memoirs of the Academy speaks of his theory as overturning that of Stahl; but the general acceptance of the new opinion did not take place till later.

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[3] Thomson, *Hist. Chem.* ii. 95.
[4] *Biogr. Univ.* (Cuvier.)
[5] Thomson, ii. 99.

*Sect. 2.—Reception and Confirmation of the Theory of Oxygen.*

The Oxygen Theory made its way with extraordinary rapidity among the best philosophers.[6] In 1785, that is, soon after Cavendish's synthesis of water had removed some of the most formidable objections to it, Berthollet, already an eminent chemist, declared himself a convert. Indeed it was so soon generally adopted in France, that Fourcroy promulgated its doctrines under the name of "La Chimie Française," a title which Lavoisier did not altogether relish. The extraordinary eloquence and success of Fourcroy as a lecturer at the Jardin des Plantes, had no small share in the diffusion of the oxygen theory; and the name of "the apostle of the new chemistry" which was at first given him in ridicule, was justly held by him to be a glorious distinction.[7]

Guyton de Morveau, who had at first been a strenuous advocate of the phlogistic theory, was invited to Paris, and brought over to the opinions of Lavoisier; and soon joined in the formation of the nomenclature founded upon the theory. This step, of which we shall shortly speak, fixed the new doctrine, and diffused it further. Delametherie alone defended the phlogistic theory with vigor, and indeed with violence. He was the editor of the *Journal de Physique*, and to evade the influence which this gave him, the antiphlogistians[8] established, as the vehicle of their opinions, another periodical, the *Annales de Chimie.*

In England, indeed, their success was not so immediate. Cavendish,[9] in his Memoir of 1784, speaks of the question between the two opinions as doubtful. "There are," he says, "several Memoirs of M. Lavoisier, in which he entirely discards phlogiston; and as not only the foregoing experiments, but most other phenomena of nature, seem explicable as well, or nearly as well, upon this as upon the commonly believed principle of phlogiston," Cavendish proceeds to explain his experiments according to the new views, expressing no decided preference, however, for either system. But Kirwan, another English chemist, contested the point much more resolutely. His theory identified inflammable air, or hydrogen, with phlogiston; and in this view, he wrote a work which was intended as a confutation of

[6] Thomson, ii. 130.

[7] Cuvier, *Eloges,* i. p. 20.

[8] Thomson, ii. 133.

[9] *Phil. Trans.* 1784, p. 150.

the essential part of the oxygen theory. It is a strong proof of the steadiness and clearness with which the advocates of the new system possessed their principles, that they immediately translated this work, adding, at the end of each chapter, a refutation of the phlogistic doctrines which it contained. Lavoisier, Berthollet, De Morveau, Fourcroy, and Monge, were the authors of this curious specimen of scientific polemics. It is also remarkable evidence of the candor of Kirwan, that notwithstanding the prominent part he had taken in the controversy, he allowed himself at last to be convinced. After a struggle of ten years, he wrote[10] to Berthollet in 1796, "I lay down my arms, and abandon the cause of phlogiston." Black followed the same course. Priestley alone, of all the chemists of great name, would never assent to the new doctrines, though his own discoveries had contributed so much to their establishment. "He saw," says Cuvier,[11] "without flinching, the most skilful defenders of the ancient theory go over to the enemy in succession; and when Kirwan had, almost the last of all, abjured phlogiston, Priestley remained alone on the field of battle, and threw out a new challenge, in a memoir addressed to the principal French chemists." It happened, curiously enough, that the challenge was accepted, and the arguments answered by M. Adet, who was at that time (1798,) the French ambassador to the United States, in which country Priestley's work was published. Even in Germany, the birth-place and home of the phlogistic theory, the struggle was not long protracted. There was, indeed, a controversy, the older philosophers being, as usual, the defenders of the established doctrines; but in 1792, Klaproth repeated, before the Academy of Berlin, all the fundamental experiments; and "the result was a full conviction on the part of Klaproth and the Academy, that the Lavoisierian theory was the true one."[12] Upon the whole, the introduction of the Lavoisierian theory in the scientific world, when compared with the great revolution of opinion to which it comes nearest in importance, the introduction of the Newtonian theory, shows, by the rapidity and temper with which it took place, a great improvement, both in the means of arriving at truth, and in the spirit with which they were used.

Some English writers[13] have expressed an opinion that there was

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[10] Pref. to Fourcroy's *Chemistry*, xiv. [11] Cuvier, *Eloge de Priestley*, p. 208.

[12] Thomson, vol. ii. p. 136.

[13] Brande, *Hist. Diss.* in *Enc. Brit.* p. 182. Lunn, *Chem.* in *Enc. Met.* p. 596

little that was original in the new doctrines. But if they were so obvious, what are we to say of eminent chemists, as Black and Cavendish, who hesitated when they were presented, or Kirwan and Priestley, who rejected them? This at least shows that it required some peculiar insight to see the evidence of these truths. To say that most of the materials of Lavoisier's theory existed before him, is only to say that his great merit was, that which must always be the great merit of a new theory, his generalization. The effect which the publication of his doctrines produced, shows us that he was the first person who, possessing clearly the idea of quantitative composition, applied it steadily to a great range of well-ascertained facts. This is, as we have often had to observe, precisely the universal description of an inductive discoverer. It has been objected, in like manner, to the originality of Newton's discoveries, that they were contained in those of Kepler. They were so, but they needed a Newton to find them there. The originality of the theory of oxygen is proved by the conflict, short as it was, which accompanied its promulgation; its importance is shown by the changes which it soon occasioned in every part of the science.

Thus Lavoisier, far more fortunate than most of those who had, in earlier ages, produced revolutions in science, saw his theory accepted by all the most eminent men of his time, and established over a great part of Europe within a few years from its first promulgation. In the common course of events, it might have been expected that the later years of his life would have been spent amid the admiration and reverence which naturally wait upon the patriarch of a new system of acknowledged truths. But the times in which he lived allowed no such euthanasia to eminence of any kind. The democracy which overthrew the ancient political institutions of France, and swept away the nobles of the land, was not, as might have been expected, enthusiastic in its admiration of a great revolution in science, and forward to offer its homage to the genuine nobility of a great discoverer. Lavoisier was thrown into prison on some wretched charge of having, in the discharge of a public office which he had held, adulterated certain tobacco; but in reality, for the purpose of confiscating his property.[14] In his imprisonment, his philosophy was his resource; and he employed himself in the preparation of his papers for printing. When he was brought before the revolutionary tribunal, he begged for a respite of a few days, in order to complete some researches, the results of which

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[14] *Biog. Univ* (Cuvier.)

were, he said, important to the good of humanity. The brutish idiot, whom the state of the country at that time had placed in the judgment-seat, told him that the republic wanted no sçavans. He was dragged to the guillotine, May the 8th, 1794, and beheaded, in the fifty-second year of his age; a melancholy proof that, in periods of political ferocity, innocence and merit, private virtues and public services, amiable manners and the love of friends, literary fame and exalted genius, are all as nothing to protect their possessor from the last extremes of violence and wrong, inflicted under judicial forms.

### *Sect. 3.—Nomenclature of the Oxygen Theory.*

As we have already said, a powerful instrument in establishing and diffusing the new chemical theory, was a Systematic Nomenclature founded upon it, and applicable to all chemical compounds, which was soon constructed and published by the authors of the theory. Such a nomenclature made its way into general use the more easily, in that the want of such a system had already been severely felt; the names in common use being fantastical, arbitrary, and multiplied beyond measure. The number of known substances had become so great, that a list of names with no regulative principle, founded on accident, caprice, and error, was too cumbrous and inconvenient to be tolerated. Even before the currency which Lavoisier's theory obtained, these evils had led to attempts towards a more convenient set of names. Bergman and Black had constructed such lists; and Guyton de Morveau, a clever and accomplished lawyer of Dijon, had formed a system of nomenclature in 1782, before he had become a convert to Lavoisier's theory, in which task he had been exhorted and encouraged by Bergman and Macquer. In this system,[15] we do not find most of the characters of the method which was afterwards adopted. But a few years later, Lavoisier, De Morveau, Berthollet and Fourcroy, associated themselves for the purpose of producing a nomenclature which should correspond to the new theoretical views. This appeared in 1787, and soon made its way into general use. The main features of this system are, a selection of the simplest radical words, by which substances are designated, and a systematic distribution of terminations, to express their relations. Thus, sulphur, combined with oxygen in two different proportions, forms two acids, the

[15] *Journal de Physique*, 1782, p. 370.

sulphur*ous* and the sulphur*ic;* and these acids forn, with earthy or alkaline bases, sulph*ites* and sulph*ates;* while sulphur directly combined with another element, forms a sulph*uret.* The term *oxyd* (now usually written *oxide*) expressed a lower degree of combination with oxygen than the acids. The *Méthode de Nomenclature Chimique* was published in 1787; and in 1789, Lavoisier published a treatise on chemistry in order further to explain this method. In the preface to this volume, he apologizes for the great amount of the changes, and pleads the authority of Bergman, who had exhorted De Morveau "to spare no improper names; those who are learned will always be learned, and those who are ignorant will thus learn sooner." To this maxim they so far conformed, that their system offers few anomalies; and though the progress of discovery, and the consequent changes of theoretical opinion, which have since gone on, appear now to require a further change of nomenclature, it is no small evidence of the skill with which this scheme was arranged, that for half a century it was universally used, and felt to be far more useful and effective than any nomenclature in any science had ever been before.

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## CHAPTER VII.

### Application and Correction of the Oxygen Theory.

SINCE a chemical theory, as far as it is true, must enable us to obtain a true view of the intimate composition of all bodies whatever, it will readily be supposed that the new chemistry led to an immense number of analyses and researches of various kinds. These it is not necessary to dwell upon; nor will I even mention the names of any of the intelligent and diligent men who have labored in this field. Perhaps one of the most striking of such analyses was Davy's decomposition of the earths and alkalies into metallic bases and oxygen, in 1807 and 1808; thus extending still further that analogy between the earths and the calces of the metals, which had had so large a share in the formation of chemical theories. This discovery, however, both in the means by which it was made, and in the views to which it led, bears upon subjects hereafter to be treated of.

The Lavoisierian theory also, wide as was the range of truth which it embraced, required some limitation and correction. I do not now peak of some erroneous opinions entertained by the author of the theory; as, for instance, that the heat produced in combustion, and even in respiration, arose from the conversion of oxygen gas to a solid consistence, according to the doctrine of latent heat. Such opinions not being necessarily connected with the general idea of the theory, need not here be considered. But the leading generalization of Lavoisier, that acidification was *always* combination with oxygen, was found untenable. The point on which the contest on this subject took place was the constitution of the *oxymuriatic* and *muriatic* acids;—as they had been termed by Berthollet, from the belief that muriatic acid contained oxygen, and oxymuriatic a still larger dose of oxygen. In opposition to this, a new doctrine was put forward in 1809 by Gay-Lussac and Thenard in France, and by Davy in England;—namely, that oxymuriatic acid was a simple substance, which they termed *chlorine*, and that muriatic acid was a combination of chlorine with hydrogen, which therefore was called *hydrochloric acid.* It may be observed, that the point in dispute in the controversy on this subject was nearly the same which had been debated in the course of the establishment of the oxygen theory; namely, whether in the formation of muriatic acid from chlorine, oxygen is subtracted, or hydrogen added, and the water concealed.

In the course of this dispute, it was allowed on both sides, that the combination of dry muriatic acid and ammonia afforded an *experimentum crucis;* since, if water was produced from these elements, oxygen must have existed in the acid. Davy being at Edinburgh in 1812, this experiment was made in the presence of several eminent philosophers; and the result was found to be, that though a slight dew appeared in the vessel, there was not more than might be ascribed to unavoidable imperfection in the process, and certainly not so much as the old theory of muriatic acid required. The new theory, after this period, obtained a clear superiority in the minds of philosophical chemists, and was further supported by new analogies.[1]

For, the existence of one *hydracid* being thus established, it was found that other substances gave similar combinations; and thus chemists obtained the *hydriodic*, *hydrofluoric*, and *hydrobromic* acids. These acids, it is to be observed, form salts with bases, in the same manner as the oxygen acids do. The analogy of the muriatic and fluoric compounds was first clearly urged by a philosopher who was

[1] Paris, *Life of Davy*, i. 337.

not peculiarly engaged in chemical research, but who was often distinguished by his rapid and happy generalizations, M. Ampère. He supported this analogy by many ingenious and original arguments, in letters written to Davy, while that chemist was engaged in his researches on fluor spar, as Davy himself declares.[2]

Still further changes have been proposed, in that classification of elementary substances to which the oxygen theory led. It has been held by Berzelius and others, that other elements, as, for example, sulphur, form *salts* with the alkaline and earthy metals, rather than sulphurets. The character of these *sulpho-salts*, however, is still questioned among chemists; and therefore it does not become us to speak as if their place in history were settled. Of course, it will easily be understood that, in the same manner in which the oxygen theory introduced its own proper nomenclature, the overthrow or material transformation of the theory would require a change in the nomenclature, or rather, the anomalies which tended to disturb the theory, would, as they were detected, make the theoretical terms be felt as inappropriate, and would suggest the necessity of a reformation in that respect. But the discussion of this point belongs to a step of the science which is to come before us hereafter.

It may be observed, that in approaching the limits of this part of our subject, as we are now doing, the doctrine of the combination of *acids* and *bases*, of which we formerly traced the rise and progress, is still assumed as a fundamental relation by which other relations are tested. This remark connects the stage of chemistry now under our notice with its earliest steps. But in order to point out the chemical bearing of the next subjects of our narrative, we may further observe, that *metals*, *earths*, *salts*, are spoken of as known *classes* of substances; and in like manner the newly-discovered elements, which form the last trophies of chemistry, have been distributed into such classes according to their analogies; thus *potassium*, *sodium*, *barium*, have been asserted to be metals; *iodine*, *bromine*, *fluorine*, have been arranged as analogical to *chlorine*. Yet there is something vague and indefinite in the boundaries of such classifications and analogies; and it is precisely where this vagueness falls, that the science is still obscure or doubtful. We are led, therefore, to see the dependence of Chemistry upon Classification; and it is to Sciences of Classification which we shall next proceed; as soon as we have noticed the most general views

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[2] Paris, *Life of Davy*, i. 370.

which have been given of chemical relations, namely, the views of the electro-chemists.

But before we do this, we must look back upon a law which obtains in the combination of elements, and which we have hitherto not stated; although it appears, more than any other, to reveal to us the intimate constitution of bodies, and to offer a basis for future generalizations. I speak of the *Atomic Theory*, as it is usually termed; or, as we might rather call it, the Doctrine of Definite, Reciprocal, and Multiple Proportions.

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## CHAPTER VIII.

### Theory of Definite, Reciprocal, and Multiple Proportions.

#### *Sect.* 1.—*Prelude to the Atomic Theory, and its Publication by Dalton.*

THE general laws of chemical combination announced by Mr. Dalton are truths of the highest importance in the science, and are now nowhere contested; but the view of matter as constituted of *atoms*, which he has employed in conveying those laws, and in expressing his opinion of their cause, is neither so important nor so certain. In the place which I here assign to his discovery, as one of the great events of the history of chemistry, I speak only of the *law of phenomena*, the rules which govern the quantities in which elements combine.

This law may be considered as consisting of three parts, according to the above description of it;—that elements combine in *definite* proportions;—that these determining proportions operate *reciprocally*,—and that when, between the same elements, several combining proportions occur, they are related as *multiples*.

That elements combine in certain definite proportions of quantity, and in no other, was implied, as soon as it was supposed that chemical compounds had any definite properties. Those who first attempted to establish regular formulæ[1] for the constitution of salts, minerals, and

[1] Thomson, *Hist. Chem.* vol. ii. p. 279

other compounds, assumed, as the basis of this process, that the elements in different specimens had the same proportion. Wenzel, in 1777, published his *Lehre von der Verwandschaft der Körper;* or, *Doctrine of the Affinities of Bodies;* in which he gave many good and accurate analyses. His work, it is said, never grew into general notice. Berthollet, as we have already stated, maintained that chemical compounds were not definite; but this controversy took place at a later period. It ended in the establishment of the doctrine, that there is, for each combination, only one proportion of the elements, or at most only two or three.

Not only did Wenzel, by his very attempt, presume the first law of chemical composition, the definiteness of the proportions, but he was also led, by his results, to the second rule, that they are reciprocal. For he found that when two *neutral* salts decompose each other, the resulting salts are also neutral. The neutral character of the salts shows that they are definite compounds; and when the two elements of the one salt, *P* and *s*, are presented to those of the other, *B* and *n*, if *P* be in such quantity as to combine definitely with *n*, *B* will also combine definitely with *s*.[2]

Views similar to those of Wenzel were also published by Jeremiah Benjamin Richter[3] in 1792, in his *Anfangsgründe der Stöchyometrie, oder Messkunst Chymischer Elemente*, (*Principles of the Measure of Chemical Elements*,) in which he took the law, just stated, of reciprocal proportions, as the basis of his researches, and determined the numerical quantities of the common bases and acids which would saturate each other. It is clear that, by these steps, the two first of our three rules may be considered as fully developed. The change of general views which was at this time going on, probably prevented chemists from feeling so much interest as they might have done otherwise, in these details; the French and English chemists, in particular, were fully employed with their own researches and controversies.

Thus the rules which had already been published by Wenzel and Richter had attracted so little notice, that we can hardly consider Mr. Dalton as having been anticipated by those writers, when, in 1803, he began to communicate his views on the chemical constitution of

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[2] I am told that Wenzel (whose book I have not seen), though he adduces many cases in which double decomposition gives neutral salts, does not express the proposition in a general form, nor use letters in expressing it.

[3] Thomson, *Hist. Chem.* vol. ii. p. 283.

bodies; these views being such as to include both these two rules in their most general form, and further, the rule, at that time still more new to chemists, of *multiple* proportions. He conceived bodies as composed of atoms of their constituent elements, grouped, either one and one, or one and two, or one and three, and so on. Thus, if *C* represent an atom of carbon and *O* one of oxygen, *O C* will be an atom of *carbonic oxide*, and *O C O* an atom of *carbonic acid;* and hence it follows, that while both these bodies have a definite quantity of oxygen to a given quantity of carbon, in the latter substance this quantity is *double* of what it is in the former.

The consideration of bodies as consisting of compound atoms, each of these being composed of elementary atoms, naturally led to this law of multiple proportions. In this mode of viewing bodies, Mr. Dalton had been preceded (unknown to himself) by Mr. Higgins, who, in 1789, published[4] his *Comparative View of the Phlogistic and Antiphlogistic Theories.* He there says,[5] "That in volatile vitriolic acid, a single ultimate particle of sulphur is united only to a single particle of dephlogisticated air; and that in perfect vitriolic acid, every single particle of sulphur is united to two of dephlogisticated air, being the quantity necessary to saturation;" and he reasons in the same manner concerning the constitution of water, and the compounds of nitrogen and oxygen. These observations of Higgins were, however, made casually, and not followed out, and cannot affect Dalton's claim to original merit.

Mr. Dalton's generalization was first suggested[6] during his examination of olefiant gas and carburetted hydrogen gas; and was asserted generally, on the strength of a few facts, being, as it were, irresistibly recommended by the clearness and simplicity which the notion possessed. Mr. Dalton himself represented the compound atoms of bodies by symbols, which professed to exhibit the arrangement of the elementary atoms in space as well as their numerical proportion; and he attached great importance to this part of his scheme. It is clear, however, that this part of his doctrine is not essential to that numerical comparison of the law with facts, on which its establishment rests. These hypothetical configurations of atoms have no value till they are confirmed by corresponding facts, such as the optical or crystalline properties of bodies may perhaps one day furnish.

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Turner's *Chem.* p. 217.

P. 36 and 37.

Thomson, vol. ii. p. 291.

### *Sect. 2.—Reception and Confirmation of the Atomic Theory.*

In order to give a sketch of the progress of the Atomic Theory into general reception, we cannot do better than borrow our information mainly from Dr. Thomson, who was one of the earliest converts and most effective promulgators of the doctrine. Mr. Dalton, at the time when he conceived his theory, was a teacher of mathematics at Manchester, in circumstances which might have been considered narrow, if he himself had been less simple in his manner of life, and less moderate in his worldly views. His experiments were generally made with apparatus of which the simplicity and cheapness corresponded to the rest of his habits. In 1804, he was already in possession of his atomic theory, and explained it to Dr. Thomson, who visited him at that time. It was made known to the chemical world in Dr. Thomson's *Chemistry*, in 1807; and in Dalton's own *System of Chemistry* (1808) the leading ideas of it were very briefly stated. Dr. Wollaston's memoir, "on superacid and subacid salts," which appeared in the *Philosophical Transactions* for 1808, did much to secure this theory a place in the estimation of chemists. Here the author states, that he had observed, in various salts, the quantities of acid combined with the base in the neutral and in the superacid salts to be as one to two: and he says that, thinking it likely this law might obtain generally in such compounds, it was his design to have pursued this subject, with the hope of discovering the cause to which so regular a relation may be ascribed. But he adds, that this appears to be superfluous after the publication of Dalton's theory by Dr. Thomson, since all such facts are but special cases of the general law. We cannot but remark here, that the scrupulous timidity of Wollaston was probably the only impediment to his anticipating Dalton in the publication of the rule of multiple proportions; and the forwardness to generalize, which belongs to the character of the latter, justly secured him, in this instance, the name of the discoverer of this law. The rest of the English chemists soon followed Wollaston and Thomson, though Davy for some time resisted. They objected, indeed, to Dalton's assumption of atoms, and, to avoid this hypothetical step, Wollaston used the phrase *chemical equivalents*, and Davy the word *proportions*, for the numbers which expressed Dalton's atomic weights. We may, however, venture to say that the term "atom" is the most convenient, and it need not be understood as claiming our assent to the hypothesis of indivisible molecules.

As Wollaston and Dalton were thus arriving independently at the same result in England, other chemists, in other countries, were, unknown to each other, travelling towards the same point.

In 1807, Berzelius,[7] intending to publish a system of chemistry, went through several works little read, and among others the treatises of Richter. He was astonished, he tells us, at the light which was there thrown upon composition and decomposition, and which had never been turned to profit. He was led to a long train of experimental research, and, when he received information of Dalton's ideas concerning multiple proportions, he found, in his own collection of analyses, a full confirmation of this theory.

Some of the Germans, indeed, appear discontented with the partition of reputation which has taken place with respect to the Theory of Definite Proportions. One[8] of them says, "Dalton has only done this;—he has wrapt up the good Richter (whom he knew; compare Schweigger, T, older series, vol. x., p. 381;) in a ragged suit, patched together of atoms; and now poor Richter comes back to his own country in such a garb, like Ulysses, and is not recognized." It is to be recollected, however, that Richter says nothing of multiple proportions.

The general doctrine of the atomic theory is now firmly established over the whole of the chemical world. There remain still several controverted points, as, for instance, whether the atomic weights of all elements are exact multiples of the atomic weight of hydrogen. Dr. Prout advanced several instances in which this appeared to be true, and Dr. Thomson has asserted the law to be of universal application. But, on the other hand, Berzelius and Dr. Turner declare that this hypothesis is at variance with the results of the best analyses. Such controverted points do not belong to our history, which treats only of the progress of scientific truths already recognized by all competent judges.

Though Dalton's discovery was soon generally employed, and universally spoken of with admiration, it did not bring to him anything but barren praise, and he continued in the humble employment of which we have spoken, when his fame had filled Europe, and his name become a household word in the laboratory. After some years he was appointed a corresponding member of the Institute of France; which may be considered as a European recognition of the importance

[7] Berz. *Chem.* B. iii. p. 27. [8] Marx. *Gesch. der Cryst.* p. 202

of what he had done; and, in 1826, two medals for the encouragement of science having been placed at the disposal of the Royal Society by the King of England, one of them was assigned to Dalton, "for his development of the atomic theory." In 1833, at the meeting of the British Association for the Advancement of Science, which was held in Cambridge, it was announced that the King had bestowed upon him a pension of 150*l.*; at the preceding meeting at Oxford, that university had conferred upon him the degree of Doctor of Laws, a step the more remarkable, since he belonged to the sect of Quakers. At all the meetings of the British Association he has been present, and has always been surrounded by the reverence and admiration of all who feel any sympathy with the progress of science. May he long remain among us thus to remind us of the vast advance which Chemistry owes to him!

[2nd Ed.] [Soon after I wrote these expressions of hope, the period of Dalton's sojourn among us terminated. He died on the 27th of July, 1844, aged 78.

His fellow-townsmen, the inhabitants of Manchester, who had so long taken a pride in his residence among them, soon after his death came to a determination to perpetuate his memory by establishing in his honor a Professor of Chemistry at Manchester.]

### *Sect. 3.—The Theory of Volumes.—Gay-Lussac.*

The atomic theory, at the very epoch of its introduction into France, received a modification in virtue of a curious discovery then made. Soon after the publication of Dalton's system, Gay-Lussac and Humboldt found a rule for the combination of substances, which includes that of Dalton as far as it goes, but extends to combinations of gases only. This law is the *theory of volumes;* namely, that gases unite together *by volume* in very simple and definite proportions. Thus water is composed exactly of 100 measures of oxygen and 200 measures of hydrogen. And since these simple ratios 1 and 1, 1 and 2, 1 and 3, alone prevail in such combinations, it may easily be shown that laws like Dalton's law of multiple proportions, must obtain in such cases as he considered.

[2nd Ed.] [M. Schröder, of Mannheim, has endeavored to extend to solids a law in some degree resembling Gay-Lussac's law of the volumes of gases. According to him, the volumes of the chemical equivalents of simple substances and their compounds are as whole numbers.[9] MM. Kopp, Playfair, and Joule have labored in the same field.]

I cannot now attempt to trace other bearings and developments of this remarkable discovery. I hasten on to the last generalization of chemistry; which presents to us chemical forces under a new aspect, and brings us back to the point from which we departed in commencing the history of this science.

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## CHAPTER IX.

### Epoch of Davy and Faraday.

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*Sect.* 1.—*Promulgation of the Electro-chemical Theory by Davy.*

THE reader will recollect that the History of Chemistry, though highly important and instructive in itself, has been an interruption of the History of Electro-dynamic Research:—a necessary interruption, however; for till we became acquainted with Chemistry in general, we could not follow the course of Electro-chemistry: we could not estimate its vast yet philosophical theories, nor even express its simplest facts. We have now to endeavor to show what has thus been done, and by what steps;—to give a fitting view of the Epoch of Davy and Faraday.

This is, doubtless, a task of difficulty and delicacy. We cannot execute it at all, except we suppose that the great truths, of which the discovery marks this epoch, have already assumed their definite and permanent form. For we do not learn the just value and right place of imperfect attempts and partial advances in science, except by seeing to what they lead. We judge properly of our trials and guesses only when we have gained our point and guessed rightly. We might personify philosophical theories, and might represent them to ourselves as figures, all pressing eagerly onwards in the same direc-

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[9] *Die molecular-volume der Chemischen Verbindungen in festen und flüssingen Zustande*, 1843.

tion, whom we have to pursue: and it is only in proportion as we ourselves overtake those figures in the race, and pass beyond them, that we are enabled to look back upon their faces; to discern their real aspects, and to catch the true character of their countenances. Except, therefore, I were of opinion that the great truths which Davy brought into sight have been firmly established and clearly developed by Faraday, I could not pretend to give the history of this striking portion of science. But I trust, by the view I have to offer of these beautiful trains of research and their result, to justify the assumption on which I thus proceed.

I must, however, state, as a further appeal to the reader's indulgence, that, even if the great principles of electro-chemistry have now been brought out in their due form and extent, the discovery is but a very few years, I might rather say a few months, old, and that this novelty adds materially to the difficulty of estimating previous attempts from the point of view to which we are thus led. It is only slowly and by degrees that the mind becomes sufficiently imbued with those new truths, of which the office is, to change the face of a science. We have to consider familiar appearances under a new aspect; to refer old facts to new principles; and it is not till after some time, that the struggle and hesitation which this employment occasions, subsides into a tranquil equilibrium. In the newly acquired provinces of man's intellectual empire, the din and confusion of conquest pass only gradually into quiet and security. We have seen, in the history of all capital discoveries, how hardly they have made their way, even among the most intelligent and candid philosophers of the antecedent schools: we must, therefore, not expect that the metamorphosis of the theoretical views of chemistry which is now going on, will be effected without some trouble and delay.

I shall endeavor to diminish the difficulties of my undertaking, by presenting the earlier investigations in the department of which I have now to speak, as much as possible according to the most deliberate view taken of them by the great discoverers themselves, Davy and Faraday; since these philosophers are they who have taught us the true import of such investigations.

There is a further difficulty in my task, to which I might refer;—the difficulty of speaking, without error and without offence, of men now alive, or who were lately members of social circles which exist still around us. But the scientific history in which such persons play a part, is so important to my purpose, that I do not hesitate to incur the responsibility which the narration involves and I have endeavored earnestly, and I hope not in vain, to speak as if I were removed by centuries from the personages of my story.

The phenomena observed in the Voltaic apparatus were naturally the subject of many speculations as to their cause, and thus gave rise to "Theories of the Pile." Among these phenomena there was one class which led to most important results: it was discovered by Nichol son and Carlisle, in 1800, that water was *decomposed* by the pile of Volta; that is, it was found that when the wires of the pile were placed with their ends near each other in the fluid, a stream of bubbles of air arose from each wire, and these airs were found on examination to be oxygen and hydrogen; which, as we have had to narrate, had already been found to be the constituents of water. This was, as Davy says,[1] the true origin of all that has been done in electro-chemical science. It was found that other substances also suffered a like decomposition under the same circumstances. Certain metallic solutions were decomposed, and an alkali was separated on the negative plates of the apparatus. Cruickshank, in pursuing these experiments, added to them many important new results; such as the decomposition of muriates of magnesia, soda, and ammonia by the pile; and the general observation that the alkaline matter always appeared at the *negative*, and the acid at the *positive*, pole.

Such was the state of the subject when one who was destined to do so much for its advance, first contributed his labors to it. Humphry Davy was a young man who had been apprenticed to a surgeon at Penzance, and having shown an ardent love and a strong aptitude for chemical research, was, in 1798, made the superintendent of a "Pneumatic Institution," established at Bristol by Dr. Beddoes, for the purpose of discovering medical powers of factitious airs.[2] But his main attention was soon drawn to galvanism; and when, in consequence of the reputation he had acquired, he was, in 1801, appointed lecturer at the Royal Institution in London (then recently established), he was soon put in possession of a galvanic apparatus of great power; and with this he was not long in obtaining the most striking results.

His first paper on the subject[3] is sent from Bristol, in September 1800; and describes experiments, in which he had found that the decompositions observed by Nicholson and Carlisle go on, although the

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[1] *Phil. Trans.* 1826, p. 386.

[2] Paris, *Life of Davy*, i. 58.

[3] Nicholson's *Journal*, 4to. iv. 275.

water, or other substance in which the two wires are plunged, be separated into two portions, provided these portions are connected by muscular or other fibres. This use of muscular fibres was, probably, a remnant of the original disposition, or accident, by which galvanism had been connected with physiology, as much as with chemistry. Davy, however, soon went on towards the conclusion, that the phenomena were altogether chemical in their nature. He had already conjectured,[4] in 1802, that all decompositions might be *polar;* that is, that in all cases of chemical decomposition, the elements might be related to each other as electrically *positive* and *negative;* a thought which it was the peculiar glory of his school to confirm and place in a distinct light. At this period such a view was far from obvious; and it was contended by many, on the contrary, that the elements which the voltaic apparatus brought to view, were not liberated from combinations, but generated. In 1806, Davy attempted the solution of this question; he showed that the ingredients which had been supposed to be produced by electricity, were due to impurities in the water, or to the decomposition of the vessel; and thus removed all preliminary difficulties. And then he says,[5] "referring to my experiments of 1800, 1801, and 1802, and to a number of new facts, which showed that inflammable substances and oxygen, alkalies and acids, and oxidable and noble metals, were in electrical relations of positive and negative, I drew the conclusion, *that the combinations and decompositions by electricity were referrible to the law of electrical attractions and repulsions,*" and advanced the hypothesis, "*that chemical and electrical attractions were produced by the same cause, acting in the one case on particles, in the other on masses; . . . and that the same property, under different modifications, was the cause of all the phenomena exhibited by different voltaic combinations.*"

Although this is the enunciation, in tolerably precise terms, of the great discovery of his epoch, it was, at the period of which we speak, conjectured rather than proved; and we shall find that neither Davy nor his followers, for a considerable period, apprehended it with that distinctness which makes a discovery complete. But in a very short time afterwards, Davy drew great additional notice to his researches by effecting, in pursuance, as it appeared, of his theoretical views, the decomposition of potassa into a metallic base and oxygen. This was, as he truly said, in the memorandum written in his journal at the

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[4] *Phil. Trans.* 182[illegible].

[5] Ib. 1826, p. 389

instant, "a capital experiment." This discovery was soon followed by that of the decomposition of soda; and shortly after, of other bodies of the same kind; and the interest and activity of the whole chemical world were turned to the subject in an intense degree.

At this period, there might be noticed three great branches of speculation on this subject; *the theory of the pile*, *the theory of electrical decomposition*, and the *theory of the identity of chemical and electrical forces;* which last doctrine, however, was found to include the other two, as might have been anticipated from the time of its first suggestion.

It will not be necessary to say much on the theories of the voltaic pile, as separate from other parts of the subject. The *contact-theory*, which ascribed the action to the contact of different metals, was maintained by Volta himself; but gradually disappeared, as it was proved (by Wollaston[6] especially,) that the effect of the pile was inseparably connected with oxidation or other chemical changes. The theories of electro-chemical decomposition were numerous, and especially after the promulgation of Davy's *Memoir* in 1806; and, whatever might be the defects under which these speculations for a long time labored, the subject was powerfully urged on in the direction in which truth lay, by Davy's discoveries and views. That there remained something still to be done, in order to give full evidence and consistency to the theory, appears from this;—that some of the most important parts of Davy's results struck his followers as extraordinary paradoxes; —for instance, the fact that the decomposed elements are transferred from one part of the circuit to another, in a form which escapes the cognizance of our senses, through intervening substances for which they have a strong affinity. It was found afterwards that the circumstance which appeared to make the process so wonderful, was, in fact, the condition of its going on at all. Davy's expressions often seem to indicate the most exact notions: for instance, he says, "It is very natural to suppose that the repellent and attractive energies are communicated from one particle to another of the same kind, so as to establish a conducting *chain* in the fluid; and that the locomotion takes place in consequence;"[7] and yet at other times he speaks of the element as *attracted* and *repelled* by the metallic surfaces which form the *poles;*— a different, and, as it appeared afterwards, an untenable view. Mr. Faraday, who supplied what was wanting, justly notices this vagueness.

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[6] *Phil. Trans.* 1801, p. 427. Paris, i. 154.

He says,[8] that though, in Davy's celebrated Memoir of 1806, the points established are of the utmost value, the mode of action by which the effects take place is stated very generally; so generally, indeed, that probably a dozen precise schemes of electro-chemical action might be drawn up, differing essentially from each other, yet all agreeing with the statement there given." And at a period a little later, being reproached by Davy's brother with injustice in this expression, he substantiated his assertion by an enumeration of twelve such schemes which had been published.

But yet we cannot look upon this Memoir of 1806, otherwise than as a great event, perhaps the most important event of the epoch now under review. And as such it was recognized at once all over Europe. In particular, it received the distinguished honor of being crowned by the Institute of France, although that country and England were then engaged in fierce hostility. Buonaparte had proposed a prize of sixty thousand francs "to the person who by his experiments and discoveries should advance the knowledge of electricity and galvanism, as much as Franklin and Volta did;" and "of three thousand francs for the best experiment which should be made in the course of each year on the galvanic fluid;" the latter prize was, by the First Class of the Institute, awarded to Davy.

From this period he rose rapidly to honors and distinctions, and reached a height of scientific fame as great as has ever fallen to the lot of a discoverer in so short a time. I shall not, however, dwell on such circumstances, but confine myself to the progress of my subject.

### *Sect. 2.—Establishment of the Electro-chemical Theory by Faraday.*

The defects of Davy's theoretical views will be seen most clearly by explaining what Faraday added to them. Michael Faraday was in every way fitted and led to become Davy's successor in his great career of discovery. In 1812, being then a bookseller's apprentice, he attended the lectures of Davy, which at that period excited the highest admiration.[9] "My desire to escape from trade," Mr. Faraday says, "which I thought vicious and selfish, and to enter into the service of science, which I imagined made its pursuers amiable and liberal, induced me at last to take the bold and simple step of writing to Sir H. Davy." He was favorably received, and, in the next year, became

[8] *Researches*, 482.

[9] Paris, ii. 3.

Davy's assistant at the Institution; and afterwards his successor. The Institution which produced such researches as those of these two men, may well be considered as a great school of exact and philosophical chemistry. Mr. Faraday, from the beginning of his course of inquiry, appears to have had the consciousness that he was engaged on a great connected work. His *Experimental Researches*, which appeared in a series of Memoirs in the *Philosophical Transactions*, are divided into short paragraphs, numbered into a continued order from 1 up to 1160, at the time at which I write;[10] and destined, probably, to extend much further. These paragraphs are connected by a very rigorous method of investigation and reasoning which runs through the whole body of them. Yet this unity of purpose was not at first obvious. His first two Memoirs were upon subjects which we have already treated of (B. xiii. c. 5 and c. 8), Voltaic Induction, and the evolution of Electricity from Magnetism. His "Third Series" has also been already referred to. Its object was, as a preparatory step towards further investigation, to show the identity of voltaic and animal electricity with that of the electrical machine; and as machine electricity differs from other kinds in being successively in a state of tension and explosion, instead of a continued current, Mr. Faraday succeeded in identifying it with them, by causing the electrical discharge to pass through a bad conductor into a discharging-train of vast extent; nothing less, indeed, than the whole fabric of the metallic gas-pipes and water-pipes of London. In this Memoir[11] it is easy to see already traces of the general theoretical views at which he had arrived; but these are not expressly stated till his "Fifth Series;" his intermediate Fourth Series being occupied by another subsidiary labor on the conditions of conduction. At length, however, in the Fifth Series, which was read to the Royal Society in June, 1833, he approaches the theory of electro-chemical decomposition. Most preceding theorists, and Davy amongst the number, had referred this result to *attractive powers* residing in the *poles* of the apparatus; and had even pretended to compare the intensity of this attraction at different distances from the poles. By a number of singularly beautiful and skilful experiments, Mr. Faraday shows that the phenomena can with no propriety be

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[10] December, 1835. (At present, when I am revising the second edition, September, 1846, Dr. Faraday has recently published the "Twenty-first Series" of his *Researches* ending with paragraph 2453.)

[11] *Phil. Trans.* 1833.

ascribed to the attraction of the poles.[12] "As the substances evolved in cases of electro-chemical decomposition may be made to appear against air,[13] which, according to common language, is not a conductor, nor is decomposed; or against water,[14] which is a conductor, and can be decomposed; as well as against the metal poles, which are excellent conductors, but undecomposable; there appears but little reason to consider this phenomenon generally as due to the attraction or attractive powers of the latter, when used in the ordinary way, since similar attractions can hardly be imagined in the former instances."

Faraday's opinion, and, indeed, the only way of expressing the results of his experiments, was, that the chemical elements, in obedience to the direction of the voltaic currents established in the decomposing substance, were evolved, or, as he prefers to say, *ejected* at its extremities.[15] He afterwards states that the influence which is present in the electric current may be described[16] as *an axis of power, having* [at each point] *contrary forces exactly equal in amount in contrary directions.*

Having arrived at this point, Faraday rightly wished to reject the term *poles*, and other words which could hardly be used without suggesting doctrines now proved to be erroneous. He considered, in the case of bodies electrically decomposed, or, as he termed them, *electrolytes*, the elements as travelling in two opposite directions; which, with reference to the direction of terrestrial magnetism, might be considered as naturally east and west; and he conceived elements as, in this way, arriving at the doors or outlets at which they finally made their separate appearance. The doors he called *electrodes*, and, separately, the *anode* and the *cathode;*[17] and the elements which thus travel he termed the *anïon* and the *catïon* (or *cathïon*).[18] By means of this nomenclature he was able to express his general results with much more distinctness and facility.

But this general view of the electrolytical process required to be pursued further, in order to explain the nature of the action. The identity of electrical and chemical forces, which had been hazarded as

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[12] *Researches*, Art. 497. [13] *Researches*, Arts. 465, 469.
[14] 495. [15] 493. [16] 517. [17] 663.
[18] The analogy of the Greek derivation requires *catïon;* but to make the relation to *cathode* obvious to the English reader, and to avoid a violation of the habits of English pronunciation, I should prefer *cathïon.*

conjecture by Davy, and adopted as the basis of chemistry by Berzelius, could only be established by exact measures and rigorous proofs. Faraday had, in his proof of the identity of voltaic and electric agency, attempted also to devise such a measure as should give him a comparison of their quantity; and in this way he proved that[19] a voltaic group of two small wires of platinum and zinc, placed near each other, and immersed in dilute acid for three seconds, yields as much electricity as the electrical battery, charged by ten turns of a large machine; and this was established both by its momentary electro-magnetic effect, and by the amount of its chemical action.[20]

It was in his "Seventh Series," that he finally established a principle of definite measurement of the amount of electrolytical action, and described an instrument which he termed[21] a *volta-electrometer*. In this instrument the amount of action was measured by the quantity of water decomposed: and it was necessary, in order to give validity to the mensuration, to show (as Faraday did show) that neither the size of the electrodes, nor the intensity of the current, nor the strength of the acid solution which acted on the plates of the pile, disturbed the accuracy of this measure. He proved, by experiments upon a great variety of substances, of the most different kinds, that the electro-chemical action is definite in amount according to the measurement of the new instrument.[22] He had already, at an earlier period,[23] asserted, that *the chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes;* but the volta-electrometer enabled him to fix with more precision the meaning of this general proposition, as well as to place it beyond doubt.

The vast importance of this step in chemistry soon came into view. By the use of the volta-electrometer, Faraday obtained, for each elementary substance, a number which represented the relative amount of its decomposition, and which might properly[24] be called its "electro-chemical equivalent." And the question naturally occurs, whether these numbers bore any relation to any previously established chemical measures. The answer is remarkable. *They were no other than the atomic weights of the Daltonian theory*, which formed the climax of the previous ascent of chemistry; and thus here, as everywhere in

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[19] *Researches*, Art. 371. [20] 537. [21] 739.
[22] Arts. 758, 814. [23] 377. [24] 792.

the progress of science, the generalizations of one generation are absorbed in the wider generalizations of the next.

But in order to reach securely this wider generalization, Faraday combined the two branches of the subject which we have already noticed;—the *theory of electrical decomposition* with the *theory of the pile.* For his researches on the origin of activity of the voltaic circuit (his Eighth Series), led him to see more clearly than any one before him, what, as we have said, the most sagacious of preceding philosophers had maintained, that the current in the pile was due to the mutual chemical action of its elements. He was led to consider the processes which go on in the *exciting-cell* and in the decomposing place as of the same kind, but opposite in direction. The chemical *composition* of the fluid with the zinc, in the common apparatus, produces, when the circuit is completed, a current of electric influence in the wire; and this current, if it pass through an electrolyte, manifests itself by *decomposition,* overcoming the chemical affinity which there resists it. An electrolyte cannot conduct without being decomposed. The forces at the point of composition and the point of decomposition are of the same kind, and are opposed to each other by means of the conducting-wire; the wire may properly be spoken of [25] as *conducting chemical affinity :* it allows two forces of the same kind to oppose one another;[26] electricity is only another mode of the exertion of chemical forces;[27] and we might express all the circumstances of the voltaic pile without using any other term than chemical affinity, though that of electricity may be very convenient.[28] Bodies are held together by a definite power, which, when it ceases to discharge that office, may be thrown into the condition of an electric current.[29]

Thus the great principle of the identity of electrical and chemical action was completely established. It was, as Faraday with great candor says,[30] a confirmation of the general views put forth by Davy, in 1806, and might be expressed in his terms, that "chemical and electrical attractions are produced by the same cause;" but it is easy to see that neither was the full import of these expressions understood nor were the quantities to which they refer conceived as measurable quantities, nor was the assertion anything but a sagacious conjecture, till Faraday gave the interpretation, measure, and proof, of which we have spoken. The evidence of the incompleteness of the views of his predecessor we have already adduced, in speaking of his vague and incon-

[25] Researches Art. 918. [27] 915. [28] 917. [29] 855. [30] 965.

sistent theoretical account of decomposition. The confirmation of Davy's discoveries by Faraday is of the nature of Newton's confirmation of the views of Borelli and Hooke respecting gravity, or like Young's confirmation of the undulatory theory of Huyghens.

We must not omit to repeat here the moral which we wish to draw from all great discoveries, that they depend upon the combination of *exact facts* with *clear ideas*. The former of these conditions is easily illustrated in the case of Davy and Faraday, both admirable and delicate experimenters. Davy's rapidity and resource in experimenting were extraordinary,[31] and extreme elegance and ingenuity distinguish almost every process of Faraday. He had published, in 1829, a work on *Chemical Manipulation*, in which directions are given for performing in the neatest manner all chemical processes. Manipulation, as he there truly says, is to the chemist like the external senses to the mind;[32] and without the supply of fit materials which such senses only can give, the mind can acquire no real knowledge.

But still the operations of the mind as well as the information of the senses, ideas as well as facts, are requisite for the attainment of any knowledge; and all great steps in science require a peculiar distinctness and vividness of thought in the discoverer. This it is difficult to exemplify in any better way than by the discoveries themselves. Both Davy and Faraday possessed this vividness of mind; and it was a consequence of this endowment, that Davy's lecture upon chemistry, and Faraday's upon almost any subject of physical philosophy, were of the most brilliant and captivating character. In discovering the nature of voltaic action, the essential intellectual requisite was to have a distinct conception of that which Faraday expressed by the remarkable phrase,[33] "*an axis of power having equal and opposite forces*:" and the distinctness of this idea in Faraday's mind shines forth in every part of his writings. Thus he says, the force which determines the decomposition of a body is *in* the body, not in the poles.[34] But for the most part he can of course only convey this fundamental idea by illustrations. Thus[35] he represents the voltaic circuit by a double circle, studded with the elements of the circuit, and shows how the *anïons* travel round it in one direction, and the *cathïons* in the opposite. He considers[36] the powers at the two places of action as balancing against each other through the medium of the conductors, in a manner analo-

[31] Paris, i. 145. [32] *Pref.* p. ii. [33] Art. 517.
[34] Art. 661. [35] 96 [36] 917.

gous to that in which mechanical forces are balanced against each other by the intervention of the lever. It is impossible to him[37] to resist the idea, that the voltaic current must be preceded by a state of tension in its interrupted condition, which is relieved when the circuit is completed. He appears to possess the idea of this kind of force with the same eminent distinctness with which Archimedes in the ancient, and Stevinus in the modern history of science, possessed the idea of pressure, and were thus able to found the science of mechanics.[38] And when he cannot obtain these distinct modes of conception, he is dissatisfied, and conscious of defect. Thus in the relation between magnetism and electricity,[39] "there appears to be a link in the chain of effects, a wheel in the physical mechanism of the action, as yet unrecognized." All this variety of expression shows how deeply seated is the thought. This conception of Chemical Affinity as a peculiar influence of force, which, acting in opposite directions, combines and resolves bodies;—which may be liberated and thrown into the form of a voltaic current, and thus be transferred to remote points, and applied in various ways; is essential to the understanding, as it was to the making, of these discoveries.

By those to whom this conception has been conveyed, I venture to trust that I shall be held to have given a faithful account of this important event in the history of science. We may, before we quit the subject, notice one or two of the remarkable subordinate features of Faraday's discoveries.

### *Sect. 3.—Consequences of Faraday's Discoveries.*

Faraday's volta-electrometer, in conjunction with the method he had already employed, as we have seen, for the comparison of voltaic and common electricity, enabled him to measure the actual quantity of electricity which is exhibited, in given cases, in the form of chemical affinity. His results appeared in numbers of that enormous amount which so often comes before us in the expression of natural laws. One grain of water[40] will require for its decomposition as much electricity as would make a powerful flash of lightning. By further calculation, he finds this quantity to be not less than 800,000 charges of his Leyden battery;[41] and this is, by his theory of the identity of the combining with the decomposing force, the quantity of electricity

[37] Art. 950. [38] 990. [39] 1114. [40] 153. [41] 861

which is naturally associated with the elements of the grain of water, endowing them with their mutual affinity.

Many of the subordinate facts and laws which were brought to light by these researches, clearly point to generalizations, not included in that which we have had to consider, and not yet discovered: such laws do not properly belong to our main plan, which is to make our way *up to* the generalizations. But there is one which so evidently promises to have an important bearing on future chemical theories, that I will briefly mention it. The class of bodies which are capable of electrical decomposition is limited by a very remarkable law: they are such binary compounds only as consist of *single* proportionals of their elementary principles. It does not belong to us here to speculate on the possible import of this curious law; which, if not fully established, Faraday has rendered, at least, highly probable:[42] but it is impossible not to see how closely it connects the Atomic with the Electro-chemical Theory; and in the connexion of these two great members of Chemistry, is involved the prospect of its reaching wider generalizations, and principles more profound than we have yet caught sight of.

As another example of this connexion, I will, finally, notice that Faraday has employed his discoveries in order to decide, in some doubtful cases, what is the true chemical equivalent;[43] "I have such conviction," he says, "that the power which governs electro-decomposition and ordinary chemical attractions is the same; and such confidence in the overruling influence of those natural laws which render the former definite, as to feel no hesitation in believing that the latter must submit to them too. Such being the case, I can have no doubt that, assuming hydrogen as 1, and dismissing small fractions for the simplicity of expression, the equivalent number or atomic weight of oxygen is 8, of chlorine 36, of bromine 78·4, of lead 103·5, of tin 59, &c.; notwithstanding that a very high authority doubles several of these numbers."

### *Sect. 4.—Reception of the Electro-chemical Theory.*

The epoch of establishment of the electro-chemical theory, like other great scientific epochs, must have its sequel, the period of its reception and confirmation, application and extension. In that period we

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[42] Art. 697. [43] 851.

are living, and it must be the task of future historians to trace its course.

We may, however, say a word on the reception which the theory met with, in the forms which it assumed, anterior to the labors of Faraday. Even before the great discovery of Davy, Grotthuss, in 1805, had written upon the theory of electro-chemical decomposition; but he and, as we have seen, Davy, and afterwards other writers, as Riffault and Chompré, in 1807, referred the effects to the poles.[44] But the most important attempt to appropriate and employ the generalization which these discoveries suggested, was that of Berzelius; who adopted at once the view of the identity, or at least the universal connexion, of electrical relations with chemical affinity. He considered,[45] that in all chemical combinations the elements may be considered as electro-positive and electro-negative; and made this opposition the basis of his chemical doctrines; in which he was followed by a large body of the chemists of Germany. He held too that the heat and light, evolved during cases of powerful combination, are the consequence of the electric discharge which is at that moment taking place: a conjecture which Faraday at first spoke of with praise.[46] But at a later period he more sagely says,[47] that the flame which is produced in such cases exhibits but a small portion of the electric power which really acts. "These therefore may not, cannot, be taken as evidences of the nature of the action; but are merely incidental results, incomparably small in relation to the forces concerned, and supplying no information of the way in which the particles are active on each other, or in which their forces are finally arranged." And comparing the evidence which he himself had given of the principle on which Berzelius's speculations rested, with the speculations themselves, Faraday justly conceived, that he had transferred the doctrine from the domain of what he calls *doubtful knowledge*, to that of inductive certainty.

Now that we are arrived at the starting-place, from which this well-proved truth, the identity of electric and chemical forces, must make its future advances, it would be trifling to dwell longer on the details of the diffusion of that doubtful knowledge which preceded this more certain science. Our history of chemistry is, therefore, here at an end. I have, as far as I could, executed my task; which was, to mark all the

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[44] Faraday (*Researches*, Art. 4?1 492). [45] *Ann. Chim.* lxxxvi. 146, for 1813.

[46] *Researches*, Art. 870. [47] 960.

great steps of its advance, from the most unconnected facts and the most imperfect speculations, to the highest generalization at which chemical philosophers have yet arrived.

Yet it will appear to our purpose to say a few words on the connexion of this science with those of which we are next to treat; and that I now proceed to do.

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## CHAPTER X.

### Transition from the Chemical to the Classificatory Sciences.

IT is the object and the boast of chemistry to acquire a knowledge of bodies which is more exact and constant than any knowledge borrowed from their sensible qualities can be; since it penetrates into their intimate constitution, and discloses to us the invariable laws of their composition. But yet it will be seen, on a little reflection, that such knowledge could not have any existence, if we were not also attentive to their sensible qualities.

The whole fabric of chemistry rests, even at the present day, upon the opposition of acids and bases: an acid was certainly at first known by its sensible qualities, and how otherwise, even now, do we perceive its quality? It was a great discovery of modern times that earths and alkalies have for their bases metals: but what are *metals?* or how, except from lustre, hardness, weight, and the like, do we recognize a body as a metal? And how, except by such characters, even before its analysis, was it known to be an earth or an alkali? We must suppose some classification established, before we can make any advance by experiment or observation.

It is easy to see that all attempts to avoid this difficulty by referring to processes and analogies, as well as to substances, bring us back to the same point in a circle of fallacies. If we say that an acid and alkali are known by combining with each other, we still must ask, What is the criterion that they have *combined?* If we say that the distinctive qualities of metals and earths are, that metals become earths by oxidation, we must still inquire how we recognize the process of *oxidation?* We have seen how important a part combustion plays in the history of chemical speculation; and we may usefully form such classes of bodies as *combustibles* and *supporters of combustion.* But even *combustion* is not capable of being infallibly known, for it passes by insensible shades into oxidation. We can find no basis for our reasonings, which does not assume a classification of obvious facts and qualities.

But any classification of substances on such grounds, appears, at first sight, to involve us in vagueness, ambiguity, and contradiction. Do we really take the sensible qualities of an acid as the criterion of its being an acid?—for instance, its sourness? Prussic acid, arsenious acid, are not sour. "I remember," says Dr. Paris,[1] "a chemist having been exposed to much ridicule from speaking of a *sweet* acid,—why not?" When Davy had discovered potassium, it was disputed whether it was a metal; for though its lustre and texture are metallic, it is so light as to swim on water. And if potassium be allowed to be a metal, is silicium one, a body which wants the metallic lustre, and is a non-conductor of electricity? It is clear that, at least, the *obvious* application of a classification by physical characters, is attended with endless perplexity.

But since we cannot even begin our researches without assuming a classification, and since the forms of such a classification which first occur, end in apparent confusion, it is clear that we must look to our philosophy for a solution of this difficulty; and must avoid the embarrassments and contradictions of casual and unreflective classification, by obtaining a consistent and philosophical arrangement. We must employ external characters and analogies in a connected and systematic manner; we must have *Classificatory Sciences*, and these must have a bearing even on Chemistry.

Accordingly, the most philosophical chemists now proceed upon this principle. "The method which I have followed," says M. Thenard, in his *Traité de Chimie*, published in 1824, "is, to unite in one group all analogous bodies; and the advantage of this method, which is that employed by naturalists, is very great, especially in the study of the metals and their compounds."[2] In this, as in all good systems of chemistry, which have appeared since the establishment of the phlogistic theory, combustion, and the analogous processes, are one great element in the arrangement, while the difference of metallic and non-metallic, is another element. Thus Thenard, in the first place, speaks of Oxygen; in the next place, of the Non-metallic Combustibles, as Hydrogen, Carbon, Sulphur, Chlorine; and in the next place, of Metals. But the Metals are again divided into six Sections, with reference, princi-

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[1] *Life of Davy*, i. 263. [2] Pref., p. viii.

pally, to their facility of combination with oxygen. Thus, the First Section is the Metals of the Earths; the Second, the Metals of the Alkalies; the Third, the Easily Oxidable Metals, as Iron; the Fourth, Metals Less Oxidable, as Copper and Lead; the Fifth Section contains only Mercury and Osmium; and the Sixth, what were at an earlier period termed the *Noble* Metals, Gold, Silver, Platinum, and others.

How such principles are to be applied, so as to produce a definite and consistent arrangement, will be explained in speaking of the philosophy of the Classificatory Sciences; but there are one or two peculiarities in the classes of bodies thus recognized by modern chemistry, which it may be useful to notice.

1. The distinction of Metallic and Non-metallic is still employed, as of fundamental importance. The discovery of new metals is so much connected with the inquiries concerning chemical elements, that we may notice the general progress of such discoveries. *Gold*, *Silver*, *Iron*, *Copper*, *Quicksilver*, *Lead*, *Tin*, were known from the earliest antiquity. In the beginning of the sixteenth century, mine-directors, like George Agricola, had advanced so far in practical metallurgy, that they had discovered the means of extracting three additional metals, *Zinc*, *Bismuth*, *Antimony*. After this, there was no new metal discovered for a century, and then such discoveries were made by the theoretical chemists, a race of men who had not existed before Becher and Stahl. Thus *Arsenic* and *Cobalt* were made known by Brandt, in the middle of the eighteenth century, and we have a long list of similar discoveries belonging to the same period; *Nickel*, *Manganese*, and *Tungsten*, which were detected by Cronstedt, Gahn, and Scheele, and Delhuyart, respectively; metals of a very different kind, *Tellurium* and *Molybdenum*, which were brought to light by Müller, Scheele, Bergman, and Hielm; *Platinum*, which was known as early as 1741, but with the ore of which, in 1802 and 1803, the English chemists, Wollaston and Tennant, found that no less than four other new metals (*Palladium*, *Rhodium*, *Iridium* and *Osmium*) were associated. Finally, (omitting some other new metals,) we have another period of discovery, opened in 1807, by Davy's discovery of *Potassium*, and including the resolution of all, or almost all, the alkalies and earths into metallic bases.

[2nd Ed.] [The next few years made some, at least some conjectural, additions to the list of simple substances, detected by a more minute scrutiny of known substances. *Thorium* was discovered by Berzelius in 1828; and *Vanadium* by Professor Sefström in 1830. A metal named *Cerium*, was discovered in 1803, by Hisinger and Berzelius, in a rare Swedish mineral known by the name of Cerit. Mosander more recently has found combined with Cerium, other new metals, which he has called *Lanthanium*, *Didymium*, *Erbium*, and *Terbium*: M. Klaus has found a new metal, *Ruthenium*, in the ore of Platinum; and Rose has discovered in Tantalite two other new metals, which he has announced under the names of *Pelopium* and *Niobium*. Svanberg is said to have discovered a new earth in Eudialyt, which is supposed to have, like the rest, a new radical. If these last discoveries be confirmed, the number of simple substances will be raised to *sixty-two*.]

2. Attempts have been made to indicate the classification of chemical substances by some peculiarity in the Name; and the Metals, for example, have been designated generally by names in *um*, like the Latin names of the ancient metals, *aurum*, *ferrum*. This artifice is a convenient nomenclature for the purpose of marking a recognized difference; and it would be worth the while of chemists to agree to make it universal, by writing molybden*um* and platin*um;* which is sometimes done, but not always.

3. I am not now to attempt to determine how far this class,—Metals,—extends; but where the analogies of the class cease to hold, there the nomenclature must also change. Thus, some chemists, as Dr. Thomson, have conceived that the base of Silica is more analogous to Carbon and Boron, which form acids with oxygen, than it is to the metals: and he has accordingly associated this base with these substances, and has given it the same termination, *Silicon*. But on the validity of this analogy chemists appear not to be generally agreed.

4. There is another class of bodies which have attracted much notice among modern chemists, and which have also been assimilated to each other in the form of their names; the English writers calling them *Chlorine*, *Fluorine*, *Iodine*, *Bromine*, while the French use the terms *Chlore*, *Phtore*, *Iode*, *Brome*. We have already noticed the establishment of the doctrine—that muriatic acid is formed of a base, chlorine, and of hydrogen,—as a great reform in the oxygen theory; with regard to which rival claims were advanced by Davy, and by MM. Gay-Lussac and Thenard in 1809. Iodine, a remarkable body which, from a dark powder, is converted into a violet-colored gas by the application of heat, was also, in 1813, the subject of a similar rivalry between the same English and French chemists. Bromine

was only discovered as late as 1826; and Fluorine, or *Phtore*, as, from its destructive nature, it has been proposed to term it, has not been obtained as a separate substance, and is inferred to exist by analogy only. The analogies of these bodies (Chlore, Phtore, &c.) are very peculiar; for instance, by combination with metals they form *salts;* by combination with hydrogen they form very strong acids; and all, at the common temperature of the atmosphere, operate on other bodies in the most energetic manner. Berzelius[3] proposes to call them *halogenous* bodies, or *halogenes.*

5. The number of Elementary Substances which are at present presented in our treatises of chemistry[4] is *fifty-three*, [or rather, as we have said above, *sixty-two.*] It is naturally often asked what evidence we have, that all these are *elementary*, and what evidence that they are *all* the elementary bodies;—how we know that new elements may not hereafter be discovered, or these supposed simple bodies resolved into simpler still? To these questions we can only answer, by referring to the history of chemistry;—by pointing out what chemists have understood by analysis, according to the preceding narrative. They have considered, as the analysis of a substance, that elementary constitution of it which gives the only intelligible explanation of the results of chemical manipulation, and which is proved to be complete as to quantity, by the balance, since the whole can only be equal to all its parts. It is impossible to maintain that new substances may not hereafter be discovered; for they may lurk, even in familiar substances, in doses so minute that they have not yet been missed amid the inevitable slight inaccuracies of all analysis; in the way in which iodine and bromine remained so long undetected in sea-water; and new minerals, or old ones not yet sufficiently examined, can hardly fail to add something to our list. As to the possibility of a further analysis of our supposed simple bodies, we may venture to say that, in regard to such supposed simple bodies as compose a numerous and well-characterized class, no such step can be made, except through some great change in chemical theory, which gives us a new view of all the general relations which chemistry has yet discovered. The proper evidence of the reality of any supposed new analysis is, that it is more consistent with the known analogies of chemistry, to suppose the process analytical than synthetical. Thus, as has already been said, chemists admit the existence of fluorine, from the analogy of chlorine; and Davy, when it was found

[3] *Chem.* i. 262. [4] Turner, p. 971.

that ammonia formed an amalgam with mercury, was tempted to assign to it a metallic basis. But then he again hesitates,[6] and doubts whether the analogies of our knowledge are not better preserved by supposing that ammonia, as a compound of hydrogen and another principle, is "a type of the composition of the metals."

Our history, which is the history of what we know, has little to do with such conjectures. There are, however, some not unimportant principles which bear upon them, and which, as they are usually employed, belong to the science which next comes under our review, Mineralogy.

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[6] *Elem. Chem. Phil.* 1812, p. 481.

# BOOK XV.

*THE ANALYTICO-CLASSIFICATORY SCIENCE.*

# HISTORY OF MINERALOGY.

Κρύσταλλον φαέθοντα διαυγέα λάζεο χερσὶ,
Λᾶαν ἀπόῤῥοιαν περιφεγγέος ἀμβρότου αἴγλης,
Αἰθέρι δ' ἀθανάτων μέγα τέρπεται ἄφθιτον ἦτορ.
Τόν κ' εἴπερ μετὰ χειρὰς ἔχων, περὶ νηὸν ἵκηαι,
Οὔτις τοι μακάρων ἀρνήσεται εὐχωλῆσι.

ORPHEUS. *Lithica.*

Now, if the bold but pious thought be thine,
To reach our spacious temple's inner shrine,
Take in thy reverent hands the crystal stone.
Where heavenly light in earthy shroud is shown :—
Where, moulded into measured form, with rays
Complex yet clear, the eternal Ether plays ;
This if thou firmly hold and rightly use,
Not long the gods thy ardent wish refuse.

# INTRODUCTION.

### *Sect. 1.—Of the Classificatory Sciences.*

THE horizon of the sciences spreads wider and wider before us, as we advance in our task of taking a survey of the vast domain. We have seen that the existence of Chemistry as a science which declares the ingredients and essential constitution of all kinds of bodies, implies the existence of another corresponding science, which shall divide bodies into kinds, and point out steadily and precisely what bodies they are which we have analysed. But a science thus dividing and defining bodies, is but one member of an order of sciences, different from those which we have hitherto described; namely, of the *classificatory sciences.* Such sciences there must be, not only having reference to the bodies with which chemistry deals, but also to all things respecting which we aspire to obtain any general knowledge, as, for instance, plants and animals. Indeed it will be found, that it is with regard to these latter objects, to organized beings, that the process of scientific classification has been most successfully exercised; while with regard to inorganic substances, the formation of a satisfactory system of arrangement has been found extremely difficult; nor has the necessity of such a system been recognised by chemists so distinctly and constantly as it ought to be. The best exemplification of these branches of knowledge, of which we now have to speak, will, therefore, be found in the organic world, in Botany and Zoology; but we will, in the first place, take a brief view of the science which classifies inorganic bodies, and of which Mineralogy is hitherto the very imperfect representative.

The principles and rules of the Classificatory Sciences, as well as of those of the other orders of sciences, must be fully explained when we come to treat of the Philosophy of the Sciences; and cannot be introduced here, where we have to do with history only. But I may observe very briefly, that with the process of *classing*, is joined the process of *naming;*—that names imply classification;—and that even the rudest and earliest application of language presupposes a distribution of objects according to their kinds;—but that such a spontaneous and unsystematic distribution cannot, in the cases we now have to consider, answer the purposes of exact and general knowledge. Our classification of objects must be made consistent and systematic, in order to be scientific; we must discover marks and characters, properties and conditions, which are constant in their occurrence and relations; we must form our classes, we must impose our names, according to such marks. We can thus, and thus alone, arrive at that precise, certain, and systematic knowledge, which we seek; that is, at science. The object, then, of the classificatory sciences is to obtain FIXED CHARACTERS of the kinds of things; and the criterion of the fitness of names is, that THEY MAKE GENERAL PROPOSITIONS POSSIBLE.

I proceed to review the progress of certain sciences on these principles, and first, though briefly, the science of Mineralogy.

### *Sect. 2.—Of Mineralogy as the Analytico-classificatory Science.*

MINERALOGY, as it has hitherto been cultivated, is, as I have already said, an imperfect representative of the department of human knowledge to which it belongs. The attempts at the science have generally been made by collecting various kinds of information respecting mineral bodies; but the science which we require is a complete and consistent classified system of all inorganic bodies. For chemistry proceeds upon the principle that the constitution of a body invariably determines its properties; and, consequently, its kind: but we cannot apply this principle, except we can speak with precision of the *kind* of a body, as well as of its composition. We cannot attach any sense to the assertion, that "soda or baryta has a metal for its base," except we know what *a metal* is, or at least what properties it implies. It may not be, indeed it is not, possible, to define the kinds of bodies by words only; but the classification must proceed by some constant and generally applicable process; and the knowledge which has reference to the classification will be precise as far as this process is precise, and vague as far as this is vague.

There must be, then, as a necessary supplement to Chemistry, a Science of those properties of bodies by which we divide them into *kinds*. Mineralogy is the branch of knowledge which has discharged the office of such a science, so far as it has been discharged; and, indeed, Mineralogy has been gradually approaching to a clear consciousness of her real place, and of her whole task; I shall give the history of some of the advances which have thus been made. They are, principally, the establishment and use of External Characters, especially of *Crystalline Form*, as a fixed character of definite substances; and the attempts to bring into view the connexion of Chemical Constitution and External Properties, made in the shape of mineralogical *Systems;* both those in which *chemical methods of arrangement* are adopted, and those which profess to classify by the *natural-history method.*

# CRYSTALLOGRAPHY

## CHAPTER I.

### PRELUDE TO THE EPOCH OF DE LISLE AND HAÜY.

OF all the physical properties of bodies, there is none so fixed, and in every way so remarkable, as this;—that the same chemical compound always assumes, with the utmost precision, the same geometrical form. This identity, however, is not immediately obvious; it is often obscured by various mixtures and imperfections in the substance; and even when it is complete, it is not immediately recognized by a common eye, since it consists, not in the equality of the sides or faces of the figures, but in the equality of their angles. Hence it is not surprising that the constancy of form was not detected by the early observers. Pliny says,[1] " Why crystal is generated in a hexagonal form, it is difficult to assign a reason; and the more so, since, while its faces are smoother than any art can make them, the pyramidal points are *not all of the same kind*." The quartz crystals of the Alps, to which he refers, are, in some specimens, very regular, while in others, one side of the pyramid becomes much the largest; yet the angles remain constantly the same. But when the whole shape varied so much, the angles also seemed to vary. Thus Conrad Gessner, a very learned naturalist, who, in 1564, published at Zurich his work, *De rerum Fossilium, Lapidum et Gemmarum maxime, Figuris*, says,[2] " One crystal differs from another in its angles, and consequently in its figure." And Cæsalpinus, who, as we shall find, did so much in establishing fixed characters in botany, was led by some of his general views to disbelieve the fixity of the form of crystals. In his work *De Metallicis*, published at Nuremberg in 1602, he says,[3] "To ascribe to inanimate bodies a definite form, does not appear consentaneous to reason; for it is the office of organization to produce a definite form;'

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[1] *Nat. Hist.* xxvii. 2. [2] p. 25. [3] p. 97

an opinion very natural in one who had been immersed in the study of the general analogies of the forms of plants. But though this is excusable in Cæsalpinus, the rejection of this definiteness of form a hundred years later, when its existence had been proved, and its laws developed by numerous observers, cannot be ascribed to anything but strong prejudice; yet this was the course taken by no less a person than Buffon. "The form of crystallization," says he,[4] "is *not a constant character*, but is more equivocal and more variable than any other of the characters by which minerals are to be distinguished." And accordingly, he makes no use of this most important feature in his history of minerals. This strange perverseness may perhaps be ascribed to the dislike which Buffon is said to have entertained for Linnæus, who had made crystalline form a leading character of minerals.

It is not necessary to mark all the minute steps by which mineralogists were gradually led to see clearly the nature and laws of the fixity of crystalline forms. These forms were at first noticed in that substance which is peculiarly called rock-crystal or quartz; and afterwards in various stones and gems, in salts obtained from various solutions, and in snow. But those who observed the remarkable regular figures which these substances assume, were at first impelled onwards in their speculations by the natural tendency of the human mind to generalize and guess, rather than to examine and measure. They attempted to snatch at once the general laws of geometrical regularity of these occurrences, or to connect them with some doctrine concerning formative causes. Thus Kepler,[5] in his *Harmonics of the World*, asserts a "*formatrix facultas*, which has its seat in the entrails of the earth, and, after the manner of a pregnant woman, expresses the five regular geometrical solids in the forms of gems." But Philosophers, in the course of time, came to build more upon observation, and less upon abstract reasonings. Nicolas Steno, a Dane, published, in 1669, a dissertation *De Solido intra Solidum Naturaliter contento*, in which he says,[6] that though the sides of the hexagonal crystal may vary, *the angles are not changed*. And Dominic Gulielmini, in a *Dissertation on Salts*, published in 1707, says,[7] in a true inductive spirit, "Nature does not employ all figures, but only certain ones of those which are possible; and of these, the determination is not to be fetched from the brain, or proved *à priori*, but obtained by experiments and observations." And

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[4] *Hist. des Min.* p. 343. [5] Linz 1619, p. 161. [6] p. 69. [7] p. 19.

he speaks[8] with entire decision on this subject: "Nevertheless since there is here a principle of crystallization, the inclination of the planes and of the angles is always constant." He even anticipates, very nearly, the views of later crystallographers as to the mode in which crystals are formed from elementary molecules. From this time, many persons labored and speculated on this subject; as Cappeller, whose *Prodromus Crystallographiæ* appeared at Lucern in 1723; Bourguet, who published *Lettres Philosophiques sur la Formation de Sels et de Cristaux*, at Amsterdam, in 1792; and Henckel, the "Physicus" of the Elector of Saxony, whose *Pyritologia* came forth in 1725. In this last work we have an example of the description of the various forms of special classes of minerals, (iron pyrites, copper pyrites, and arsenic pyrites;) and an example of the enthusiasm which this apparently dry and laborious study can excite: "Neither tongue nor stone," he exclaims,[9] "can express the satisfaction which I received on setting eyes upon this sinter covered with galena; and thus it constantly happens, that one must have more pleasure in what seems worthless rubbish, than in the purest and most precious ores, if we know aught of minerals."

Still, however, Henckel[10] disclaims the intention of arranging minerals according to their mathematical forms; and this, which may be considered as the first decided step in the formation of crystallographic mineralogy, appears to have been first attempted by Linnæus. In this attempt, however, he was by no means happy; nor does he himself appear to have been satisfied. He begins his preface by saying, "Lithology is not what I plume myself upon." (*Lithologia mihi cristas non eriget.*) Though his sagacity, as a natural historian, led him to see that crystalline form was one of the most definite, and therefore most important, characters of minerals, he failed in profiting by this thought, because, in applying it, he did not employ the light of geometry, but was regulated by what appeared to him resemblances, arbitrarily selected, and often delusive.[11] Thus he derived the form of pyrites from that of vitriol;[12] and brought together alum and diamond on account of their common octohedral form. But he had the great merit of animating to this study one to whom, more perhaps than to any other person, it owes its subsequent progress; I mean Romé de Lisle. "Instructed," this writer says, in his preface to his *Essais de Crystallographie*, "by the works of the celebrated Von Linnée, how

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[8] p. 83. [9] p. 343. [10] p. 167. [11] Marx. *Gesch.* p. 97

[12] *Syst. Nat.* vi. p. 220.

greatly the study of the angular form of crystals might become interesting, and fitted to extend the sphere of our mineralogical knowledge, I have followed them in all their metamorphoses with the most scrupulous attention." The views of Linnæus, as to the importance of this character, had indeed been adopted by several others; as John Hill, the King's gardener at Kew, who, in 1777, published his *Spatho genesia;* and Grignon, who, in 1775, says, "These crystallizations may give the means of finding a new theory of the generation of crystalline gems."

The circumstance which threw so much difficulty in the way of those who tried to follow out his thought was, that in consequence of the apparent irregularity of crystals, arising from the extension or contraction of particular sides of the figure, each kind of substance may really appear under many different forms, connected with each other by certain geometrical relations. These may be conceived by considering a certain fundamental form to be cut into new forms in particular ways. Thus if we take a cube, and cut off all the eight corners, till the original faces disappear, we make it an octohedron; and if we stop short of this, we have a figure of fourteen faces, which has been called a *cubo-octohedron.* The first person who appears distinctly to have conceived this *truncation* of angles and edges, and to have introduced the word, is Démeste;[13] although Wallerius[14] had already said, in speaking of the various crystalline forms of calcspar, "I conceive it would be better not to attend to all differences, lest we be overwhelmed by the number." And Werner, in his celebrated work *On the External Characters of Minerals*,[15] had formally spoken of *truncation*, *acuation*, and *acumination*, or replacement by a plane, an edge, a point respectively, (*abstumpfung*, *zuschärfung*, *zuspitzung*,) as ways in which the forms of crystals are modified and often disguised. He applied this process in particular to show the connexion of the various forms which are related to the cube. But still the extension of the process to the whole range of minerals and other crystalline bodies, was due to Romé de Lisle.

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[13] *Lettres*, 1779, i. 48. [14] *Systema Mineralogicum*, 1772–5, i. 143

[15] Leipzig, 1774.

## CHAPTER II.

EPOCH OF ROMÉ DE LISLE AND HAÜY.—ESTABLISHMENT OF THE FIXITY OF CRYSTALLINE ANGLES, AND THE SIMPLICITY OF THE LAWS OF DERIVATION.

WE have already seen that, before 1780, several mineralogists had recognized the constancy of the angles of crystals, and had seen (as Démeste and Werner,) that the forms were subject to modifications of a definite kind. But neither of these two thoughts was so apprehended and so developed, as to supersede the occasion for a discoverer who should put forward these principles as what they really were, the materials of a new and complete science. The merit of this step belongs jointly to Romé de Lisle and to Haüy. The former of these two men had already, in 1772, published an *Essai de Crystallographie*, in which he had described a number of crystals. But in this work his views are still rude and vague; he does not establish any connected sequence of transitions in each kind of substance, and lays little or no stress on the angles. But in 1783, his ideas[1] had reached a maturity which, by comparison, excites our admiration. In this he asserts, in the most distinct manner, the *invariability* of the angles of crystals of each kind, under all the changes of relative dimension which the faces may undergo;[2] and he points out that this invariability applies only to the *primitive forms*, from each of which many secondary forms are derived by various changes.[3] Thus we cannot deny him the merit of having taken steady hold on both the handles of this discovery, though something still remained for another to do. Romé pursues his general ideas into detail with great labor and skill. He gives drawings of more than five hundred regular forms (in his first work he had inserted only one hundred and ten; Linnæus only knew forty); and assigns them to their proper substances; for instance, thirty to calcspar, and sixteen to felspar. He also invented and used a goniometer. We cannot doubt that he would have been

[1] *Cristallographie, ou Description de Formes propres à tous les Corps du Règne Minéral.* 3 vols. and 1 vol. of plates.

[2] p. 68.

[3] p. 73.

looked upon as a great discoverer, if his fame had not been dimmed by the more brilliant success of his contemporary Haüy.

René-Just Haüy is rightly looked upon as the founder of the modern school of crystallography; for all those who have, since him, pursued the study with success, have taken his views for their basis. Besides publishing a system of crystallography and of mineralogy, far more complete than any which had yet appeared, the peculiar steps in the advance which belong to him are, the discovery of the importance of *cleavage*, and the consequent expression of the laws of derivation of secondary from primary forms, by means of the *decrements* of the successive layers of *integrant molecules.*

The latter of these discoveries had already been, in some measure, anticipated by Bergman, who had, in 1773, conceived a hexagonal prism to be built up by the juxtaposition of solid rhombs on the planes of a rhombic nucleus.[4] It is not clear[5] whether Haüy was acquainted with Bergman's Memoir, at the time when the cleavage of a hexagonal prism of calcspar, accidentally obtained, led him to the same conception of its structure. But however this might be, he had the indisputable credit of following out this conception with all the vigor of originality, and with the most laborious and persevering earnestness; indeed he made it the business of his life. The hypothesis of a solid, built up of small solids, had this peculiar advantage in reference to crystallography; it rendered a reason of this curious fact;—that a certain series of forms occur in crystals of the same kind, while other forms, apparently intermediate between those which actually occur, are rigorously excluded. The doctrine of decrements explained this; for by placing a number of regularly-decreasing rows of equal solids, as, for instance, of bricks, upon one another, we might form a regular equal-sided triangle, as the gable of a house; and if the breadth of the gable were one hundred bricks, the height of the triangle might be one hundred, or fifty, or twenty-five; but it would be found that if the height were an intermediate number, as fifty-seven, or forty-three, the edge of the wall would become irregular; and such irregularity is assumed to be inadmissible in the regular structure of crystals. Thus this mode of conceiving crystals allows of certain definite secondary forms, and no others.

The mathematical deduction of the dimensions and proportions

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[4] *De Formis Crystallorum.* Nov. Act. Reg. Soc. Sc. Ups. 1773.

[5] *Traité de Minér* 1822, i. 15.

of these secondary forms;—the invention of a notation to express them;—the examination of the whole mineral kingdom in accordance with these views;—the production of a work[6] in which they are explained with singular clearness and vivacity;—are services by which Haüy richly earned the admiration which has been bestowed upon him. The wonderful copiousness and variety of the forms and laws to which he was led, thoroughly exercised and nourished the spirit of deduction and calculation which his discoveries excited in him. The reader may form some conception of the extent of his labors, by being told—that the mere geometrical propositions which he found it necessary to premise to his special descriptions, occupy a volume and a half of his work;—that his diagrams are nearly a thousand in number;—that in one single substance (calcspar) he has described forty-seven varieties of form;—and that he has described one kind of crystal (called by him *fer sulfuré parallélique*) which has one hundred and thirty-four faces.

In the course of a long life, he examined, with considerable care, all the forms he could procure of all kinds of mineral; and the interpretation which he gave of the laws of those forms was, in many cases, fixed, by means of a name applied to the mineral in which the form occurred; thus, he introduced such names as *équiaxe*, *métastatique*, *unibinaire*, *perihexahèdre*, *bisalterne*, and others. It is not now desirable to apply separate names to the different forms of the same mineral species, but these terms answered the purpose, at the time, of making the subjects of study more definite. A symbolical notation is the more convenient mode of designating such forms, and such a notation Haüy invented; but the symbols devised by him had many inconveniences, and have since been superseded by the systems of other crystallographers.

Another of Haüy's leading merits was, as we have already intimated, to have shown, more clearly than his predecessors had done, that the crystalline angles of substances are a criterion of the substances; and that this is peculiarly true of the *angles of cleavage*;—that is, the angles of those edges which are obtained by cleaving a crystal in two different directions;—a mode of division which the structure of many kinds of crystals allowed him to execute in the most complete manner. As an instance of the employment of this criterion, I may mention his separation of the sulphates of baryta and strontia, which had

[6] *Traité de Minéralogie*, 1801, 5 vols.

previously been confounded. Among crystals which in the collections were ranked together as "heavy spar," and which were so perfect as to admit of accurate measurement, he found that those which were brought from Sicily, and those of Derbyshire, differed in their cleavage angle by three degrees and a half. "I could not suppose," he says,[7] "that this difference was the effect of any law of decrement; for it would have been necessary to suppose so rapid and complex a law, that such an hypothesis might have been justly regarded as an abuse of the theory." He was, therefore, in great perplexity. But a little while previous to this, Klaproth had discovered that there is an earth which, though in many respects it resembles baryta, is different from it in other respects; and this earth, from the place where it was found (in Scotland), had been named *Strontia*. The French chemists had ascertained that the two earths had, in some cases, been mixed or confounded; and Vauquelin, on examining the Sicilian crystals, found that their base was strontia, and not, as in the Derbyshire ones, baryta. The riddle was now read; all the crystals with the larger angle belong to the one, all those with the smaller, to the other, of these two sulphates; and crystallometry was clearly recognized as an authorized test of the difference of substances which nearly resemble each other.

Enough has been said, probably, to enable the reader to judge how much each of the two persons, now under review, contributed to crystallography. It would be unwise to compare such contributions to science with the great discoveries of astronomy and chemistry; and we have seen how nearly the predecessors of Romé and Haüy had reached the point of knowledge on which these two crystallographers took their stand. But yet it is impossible not to allow, that in these discoveries, which thus gave form and substance to the science of crystallography, we have a manifestation of no common sagacity and skill. Here, as in other discoveries, were required ideas and facts;—clearness of geometrical conception which could deal with most complex relations of form; a minute and extensive acquaintance with actual crystals; and the talent and habit of referring these facts to the general ideas. Haüy, in particular, was happily endowed for his task. Without being a great mathematician, he was sufficiently a geometer to solve all the problems which his undertaking demanded; and though the mathematical reasoning might have been made more compendious

[7] *Traité*, ii. 320.

by one who was more at home in mathematical generalization, probably this could hardly have been done without making the subject less accessible and less attractive to persons moderately disciplined in mathematics. In all his reasonings upon particular cases, Haüy is acute and clear; while his general views appear to be suggested rather by a lively fancy than by a sage inductive spirit: and though he thus misses the character of a great philosopher, the vivacity of style, and felicity and happiness of illustration, which grace his book, and which agree well with the character of an Abbé of the old French monarchy, had a great and useful influence on the progress of the subject.

Unfortunately Romé de Lisle and Haüy were not only rivals, but in some measure enemies. The former might naturally feel some vexation at finding himself, in his later years (he died in 1790), thrown into shade by his more brilliant successor. In reference to Haüy's use of cleavage, he speaks[8] of "innovators in crystallography, who may properly be called *crystalloclasts*." Yet he adopted, in great measure, the same views of the formation of crystals by laminæ,[9] which Haüy illustrated by the destructive process at which he thus sneers. His sensitiveness was kept alive by the conduct of the Academy of Sciences, which took no notice of him and his labors;[10] probably because it was led by Buffon, who disliked Linnæus, and might dislike Romé as his follower; and who, as we have seen, despised crystallography. Haüy revenged himself by rarely mentioning Romé in his works, though it was manifest that his obligations to him were immense; and by recording his errors while he corrected them. More fortunate than his rival, Haüy was, from the first, received with favor and applause. His lectures at Paris were eagerly listened to by persons from all quarters of the world. His views were, in this manner, speedily diffused; and the subject was soon pursued, in various ways, by mathematicians and mineralogists in every country of Europe.

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## CHAPTER III.

### Reception and Corrections of the Haüian Crystallography.

I HAVE not hitherto noticed the imperfections of the crystallographic views and methods of Haüy, because my business in the last section

[8] Pref. p. xxvii. [9] T. ii. p. 21. [10] Marx *Gesch. d. Cryst.* 130.

was to mark the permanent additions he made to the science. His system did, however, require completion and rectification in various points; and in speaking of the crystallographers of the subsequent time, who may all be considered as the cultivators of the Hauïan doctrines, we must also consider what they did in correcting them.

The three main points in which this improvement was needed were;—a better determination of the crystalline forms of the special substances;—a more general and less arbitrary method of considering crystalline forms according to their symmetry; and a detection of more general conditions by which the crystalline angle is regulated. The first of these processes may be considered as the natural sequel of the Hauïan epoch: the other two must be treated as separate steps of discovery.

When it appeared that the angle of natural or of cleavage faces could be used to determine the differences of minerals, it became important to measure this angle with accuracy. Haüy's measurements were found very inaccurate by many succeeding crystallographers: Mohs says[1] that they are so generally inaccurate, that no confidence can be placed in them. This was said, of course, according to the more rigorous notions of accuracy to which the establishment of Haüy's system led. Among the persons who principally labored in ascertaining, with precision, the crystalline angles of minerals, were several Englishmen, especially Wollaston, Phillips, and Brooke. Wollaston, by the invention of his Reflecting Goniometer, placed an entirely new degree of accuracy within the reach of the crystallographer; the angle of two faces being, in this instrument, measured by means of the reflected images of bright objects seen in them, so that the measure is the more accurate the more minute the faces are. In the use of this instrument, no one was more laborious and successful than William Phillips, whose power of apprehending the most complex forms with steadiness and clearness, led Wollaston to say that he had "a geometrical sense." Phillips published a Treatise on Mineralogy, containing a great collection of such determinations; and Mr. Brooke, a crystallographer of the same exact and careful school, has also published several works of the same kind. The precise measurement of crystalline angles must be the familiar employment of all who study crystallography; and, therefore, any further enumeration of those

[1] Marx. p. 153

who have added in this way to the stock of knowledge, would be superfluous.

Nor need I dwell long on those who added to the knowledge which Haüy left, of derived forms. The most remarkable work of this kind was that of Count Bournon, who published a work on a single mineral (calcspar) in three quarto volumes.[2] He has here given representations of seven hundred forms of crystals, of which, however, only fifty-six are essentially different. From this example the reader may judge what a length of time, and what a number of observers and calculators, were requisite to exhaust the subject.

If the calculations, thus occasioned, had been conducted upon the basis of Haüy's system, without any further generalization, they would have belonged to that process, the natural sequel of inductive discoveries, which we call *deduction;* and would have needed only a very brief notice here. But some additional steps were made in the upward road to scientific truth, and of these we must now give an account.

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## CHAPTER IV.

### Establishment of the Distinction of Systems of Crystallization.—Weiss and Mohs.

IN Haüy's views, as generally happens in new systems, however true, there was involved something that was arbitrary, something that was false or doubtful, something that was unnecessarily limited. The principal points of this kind were;—his having made the laws of crystalline derivation depend so much upon cleavage;—his having assumed an atomic constitution of bodies as an essential part of his system; and his having taken a set of primary forms, which, being selected by no general view, were partly superfluous, and partly defective.

How far evidence, such as has been referred to by various philosophers, has proved, or can prove, that bodies are constituted of indivisible atoms, will be more fully examined in the work which treats of the Philosophy of this subject. There can be little doubt that the

[2] *Traité complet de la Chaux Carbonatée et d'Aragonite,* par M. le Comte de Bournon. London, 1808.

portion of Haüy's doctrine which most riveted popular attention and applause, was his dissection of crystals, in a manner which was supposed to lead actually to their ultimate material elements. Yet it is clear, that since the solids given by cleavage are, in many cases, such as cannot make up a solid space, the primary conception of a necessary geometrical identity between the results of division and the elements of composition, which is the sole foundation of the supposition that crystallography points out the actual elements, disappears on being scrutinized: and when Haüy, pressed by this difficulty, as in the case of fluor-spar, put his integrant octohedral molecules together, touching by the edges only, his method became an empty geometrical diagram, with no physical meaning.

The real fact, divested of the hypothesis which was contained in the fiction of decrements, was, that when the relation of the derivative to the primary faces is expressed by means of numerical indices, these numbers are integers, and generally very small ones; and this was the form which the law gradually assumed, as the method of derivation was made more general and simple by Weiss and others.

"When, in 1809, I published my Dissertation," says Weiss,[1] "I shared the common opinion as to the necessity of the assumption and the reality of the existence of a primitive form, at least in a sense not very different from the usual sense of the expression. While I sought," he adds, referring to certain doctrines of general philosophy which he and others entertained, "a *dynamical* ground for this, instead of the untenable atomistic view, I found that, out of my primitive forms, there was gradually unfolded to my hands, that which really governs them, and is not affected by their casual fluctuations, the fundamental relations of those Dimensions according to which a multiplicity of internal oppositions, necessarily and mutually interdependent, are developed in the mass, each having its own polarity; so that the crystalline character is co-extensive with these polarities."

The "Dimensions" of which Weiss here speaks, are the *Axes of Symmetry* of the crystal; that is, those lines in reference to which, every face is accompanied by other faces, having like positions and properties, Thus a rhomb, or more properly a *rhombohedron*,[2] of

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[1] *Mem. Acad. Berl.* 1816, p. 307.

[2] I use this name for the solid figure, since *rhomb* has always been used for a plane figure.

calcspar may be placed with one of its obtuse corners uppermost, so that all the three faces which meet there are equally inclined to the vertical line. In this position, every derivative face, which is obtained by any modification of the faces or edges of the rhombohedron, implies either three or six such derivative faces; for no one of the three upper faces of the rhombohedron has any character or property different from the other two; and, therefore, there is no reason for the existence of a derivative from one of these primitive faces, which does not equally hold for the other primitive faces. Hence the derivative forms will, in all cases, contain none but faces connected by this kind of correspondence. The axis thus made vertical will be an Axis of Symmetry, and the crystal will consist of three divisions, ranged round this axis, and exactly resembling each other. According to Weiss's nomenclature, such a crystal is "three-and-three-membered."

But this is only one of the kinds of symmetry which crystalline forms may exhibit. They may have *three axes* of complete and *equal* symmetry at right angles to each other, as the cube and the regular octohedron;—or, *two axes* of equal symmetry, perpendicular to each other and to a *third axis*, which is not affected with the same symmetry with which they are; such a figure is a square pyramid;—or they may have *three* rectangular *axes*, all of *unequal* symmetry, the modifications referring to each axis separately from the other two.

These are essential and necessary distinctions of crystalline form; and the introduction of a classification of forms founded on such relations, or, as they were called, *Systems of Crystallization*, was a great improvement upon the divisions of the earlier crystallographers, for those divisions were separated according to certain arbitrarily-assumed primary forms. Thus Romé de Lisle's fundamental forms were, the tetrahedron, the cube, the octohedron, the rhombic prism, the rhombic octohedron, the dodecahedron with triangular faces: Haüy's primary forms are the cube, the rhombohedron, the oblique rhombic prism, the right rhombic prism, the rhombic dodecahedron, the regular octohedron, tetrahedron, and six-sided prism, and the bipyramidal dodecahedron. This division, as I have already said, errs both by excess and defect, for some of these primary forms might be made derivatives from others; and no solid reason could be assigned why they were not. Thus the cube may be derived from the tetrahedron, by truncating the edges; and the rhombic dodecahedron again from the cube, by truncating its edges; while the square pyramid could not be legitimately identified with the derivative of any of these forms; for if we were to derive it from the rhombic prism, why should the acute angles always suffer decrements corresponding in a certain way to those of the obtuse angles, as they must do in order to give rise to a square pyramid?

The introduction of the method of reference to Systems of Crystallization has been a subject of controversy, some ascribing this valuable step to Weiss, and some to Mohs.[3] It appears, I think, on the whole, that Weiss first published works in which the method is employed; but that Mohs, by applying it to all the known species of minerals, has had the merit of making it the basis of real crystallography. Weiss, in 1809, published a Dissertation *On the mode of investigating the principal geometrical character of crystalline forms*, in which he says,[4] "No part, line, or quantity, is so important as the axis; no consideration is more essential or of a higher order than the relation of a crystalline plane to the axis;" and again, "An axis is any line governing the figure, about which all parts are similarly disposed, and with reference to which they correspond mutually." This he soon followed out by examination of some difficult cases, as Felspar and Epidote. In the Memoirs of the Berlin Academy,[5] for 1814–15, he published *An Exhibition of the natural Divisions of Systems of Crystallization*. In this Memoir, his divisions are as follows:—The *regular* system, the *four-membered*, the *two-and-two-membered*, the *three-and-three-membered*, and some others of inferior degrees of symmetry. These divisions are by Mohs (*Outlines of Mineralogy*, 1822), termed the *tessular*, *pyramidal*, *prismatic*, and *rhombohedral* systems respectively. Hausmann, in his *Investigations concerning the Forms of Inanimate Nature*,[6] makes a nearly corresponding arrangement;—the *isometric*, *monodimetric*, *trimetric*, and *monotrimetic*; and one or other of these sets of terms have been adopted by most succeeding writers.

In order to make the distinctions more apparent, I have purposely omitted to speak of the systems which arise when the *prismatic* system loses some part of its symmetry;—when it has only half or a quarter its complete number of faces;—or, according to Mohs's phraseology, when it is *hemihedral* or *tetartohedral*. Such systems are represented by the singly-oblique or doubly-oblique prism; they are termed by Weiss *two-and-one-membered*, and *one-and-one-membered*; by other writers, *Monoklinometric*, and *Triklinometric* Systems. There are also other

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[3] *Edin. Phil. Trans.* 1823, vols. xv. and xvi. [4] pp. 16, 42.
[5] Ibid. [6] Göttingen, 1821.

peculiarities of Symmetry, such, for instance, as that of the *plagihedral* faces of quartz, and other minerals.

The introduction of an arrangement of crystalline forms into systems, according to their degree of symmetry, was a step which was rather founded on a distinct and comprehensive perception of mathematical relations, than on an acquaintance with experimental facts, beyond what earlier mineralogists had possessed. This arrangement was, how ever, remarkably confirmed by some of the properties of minerals which attracted notice about the time now spoken of, as we shall see in the next chapter.

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## CHAPTER V.

### Reception and Confirmation of the Distinction of Systems of Crystallization.

DIFFUSION of the Distinction of Systems.—The distinction of systems of crystallization was so far founded on obviously true views, that it was speedily adopted by most mineralogists. I need not dwell on the steps by which this took place. Mr. Haidinger's translation of Mohs was a principal occasion of its introduction in England. As an indication of dates, bearing on this subject, perhaps I may be allowed to notice, that there appeared in the *Philosophical Transactions for* 1825, *A General Method of Calculating the Angels of Crystals*, which I had written, and in which I referred only to Haüy's views; but that in 1826,[1] I published a Memoir *On the Classification of Crystalline Combinations*, founded on the methods of Weiss and Mohs, especially the latter; with which I had in the mean time become acquainted, and which appeared to me to contain their own evidence and recommendation. General methods, such as was attempted in the Memoir just quoted, are part of that process in the history of sciences, by which, when the principles are once established, the mathematical operation of deducing their consequences is made more and more general and symmetrical: which we have seen already exemplified in the history of celestial mechanics after the time of Newton. It does not enter into our plan, to dwell upon the varions steps in this way

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[1] *Camb. Trans.* vol. ii. p. 391.

made by Levy, Naumann, Grassmann, Kupffer, Hessel, and by Professor Miller among ourselves. I may notice that one great improvement was, the method introduced by Monteiro and Levy, of determining the laws of derivation of forces by means of the *parallelisms of edges;* which was afterwards extended so that faces were considered as belonging to *zones*. Nor need I attempt to enumerate (what indeed it would be difficult to describe in words) the various methods of *notation* by which it has been proposed to represent the faces of crystals, and to facilitate the calculations which have reference to them.

[2nd Ed.] [My Memoir of 1825 depended on the views of Haüy in so far as that I started from his "primitive forms;" but being a general method of expressing all forms by co-ordinates, it was very little governed by these views. The mode of representing crystalline forms which I proposed seemed to contain its own evidence of being more true to nature than Haüy's theory of decrements, inasmuch as my method expressed the faces at much lower numbers. I determine a face by means of the dimensions of the primary form *divided* by certain numbers; Haüy had expressed the face virtually by the same dimensions *multiplied* by numbers. In cases where my notation gives such numbers as (3, 4, 1), (1, 3, 7), (5, 1, 19), his method involves the higher numbers (4, 3, 12), (21, 7, 3), (19, 95, 5). My method however has, I believe, little value as a method of "*calculating* the angles of crystals."

M. Neumann, of Königsberg, introduced a very convenient and elegant mode of representing the position of faces of crystals by corresponding points on the surface of a circumscribing sphere. He gave (in 1823) the laws of the derivation of crystalline faces, expressed geometrically by the intersection of zones, (*Beiträge zur Krystallonomie.*) The same method of indicating the position of faces of crystals was afterwards, together with the notation, re-invented by M. Grassmann, (*Zur Krystallonomie und Geometrischen Combinationslehre*, 1829.) Aiding himself by the suggestions of these writers, and partly adopting my method, Prof. Miller has produced a work on Crystallography remarkable for mathematical elegance and symmetry; and has given expressions really useful for calculating the angles of crystalline faces, (*A Treatise on Crystallography.* Cambridge, 1839.)]

*Confirmation of the Distinction of Systems by the Optical Properties of Minerals.—Brewster.*—I must not omit to notice the striking confirmation which the distinction of systems of crystallization received from optical discoveries, especially those of Sir D. Brewster. Of the history of this very rich and beautiful department of science, we have already given some account, in speaking of Optics. The first facts which were noticed, those relating to double refraction, belonged exclusively to crystals of the rhombohedral system. The splendid phenomena of the rings and lemniscates produced by dipolarizing crystals, were afterwards discovered; and these were, in 1817, classified by Sir David Brewster, according to the crystalline forms to which they belong. This classification, on comparison with the distinction of Systems of Crystallization, resolved itself into a necessary relation of mathematical symmetry: all crystals of the pyramidal and rhombohedral systems, which from their geometrical character have a single axis of symmetry, are also optically uniaxal, and produce by dipolarization circular rings; while the prismatic system, which has no such single axis, but three unequal axes of symmetry, is optically biaxal, gives lemniscates by dipolarized light, and according to Fresnel's theory, has three rectangular axes of unequal elasticity.

[2nd Ed.] [I have placed Sir David Brewster's arrangement of crystalline forms in this chapter, as an event belonging to the *confirmation* of the distinctions of forms introduced by Weiss and Mohs; because that arrangement was established, not on crystallographical, but on optical grounds. But Sir David Brewster's optical discovery was a much greater step in science than the systems of the two German crystallographers; and even in respect to the crystallographical principle, Sir D. Brewster had an independent share in the discovery. He divided crystalline forms into three classes, enumerating the Haüian "primitive forms" which belonged to each; and as he found some exceptions to this classification, (such as idocrase, &c.,) he ventured to pronounce that in those substances the received primitive forms were probably erroneous; a judgment which was soon confirmed by a closer crystallographical scrutiny. He also showed his perception of the mineralogical importance of his discovery by publishing it, not only in the *Phil. Trans.* (1818), but also in the *Transactions of the Wernerian Society of Natural History.* In a second paper inserted in this later series, read in 1820, he further notices Mohs's System of Crystallography, which had then recently appeared, and points out its agreement with his own.

Another reason why I do not make his great optical discovery a cardinal point in the history of crystallography is, that as a crystallographical system it is incomplete. Although we are thus led to distinguish the *tessular* and the *prismatic* systems (using Mohs's terms) from the *rhombohedral* and the *square prismatic*, we are not led to distinguish the latter two from each other; inasmuch as they have no optical difference of character. But this distinction is quite essential in crystallography; for these two systems have faces formed by laws as different as those of the other two systems.

Moreover, Weiss and Mohs not only divided crystalline forms into certain classes, but showed that by doing this, the derivation of all the existing forms from the fundamental ones assumed a new aspect of simplicity and generality; and this was the essential part of what they did.

On the other hand, I do not think it is too much to say as I have elsewhere said[3] that "Sir D. Brewster's optical experiments must have led to a classification of crystals into the above systems, or something nearly equivalent, even if crystals had not been so arranged by attention to their forms."]

Many other most curious trains of research have confirmed the general truth, that the degree and kind of geometrical symmetry corresponds exactly with the symmetry of the optical properties. As an instance of this, eminently striking for its singularity, we may notice the discovery of Sir John Herschel, that the *plagihedral* crystallization of quartz, by which it exhibits faces *twisted* to the right or the left, is accompanied by right-handed or left-handed circular polarization respectively. No one acquainted with the subject can now doubt, that the correspondence of geometrical and optical symmetry is of the most complete and fundamental kind.

[2nd Ed.] [Our knowledge with respect to the positions of the optical axes of the oblique prismatic crystals is still imperfect. It appears to be ascertained that, in singly oblique crystals, one of the axes of optical elasticity coincides with the rectangular crystallographic axis. In doubly oblique crystals, one of the axes of optical elasticity is, in many cases, coincident with the axis of a principal zone. I believe no more determinate laws have been discovered.]

Thus the highest generalization at which mathematical crystallographers have yet arrived, may be considered as fully established; and the science of Crystallography, in the condition in which these place it, is fit to be employed as one of the members of Mineralogy, and thus to fill its appropriate place and office.

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[3] *Philosophy of the Inductive Sciences*, B. viii. C. iii. Art. 3.

## CHAPTER VI.

### Correction of the Law of the same Angle for the same Substance.

DISCOVERY of Isomorphism. Mitscherlich.—The discovery of which we now have to speak may appear at first sight too large to be included in the history of crystallography, and may seem to belong rather to chemistry. But it is to be recollected that crystallography, from the time of its first assuming importance in the hands of Haüy, founded its claim to notice entirely upon its connexion with chemistry; crystalline forms were properties of *something;* but *what* that something was, and how it might be modified without becoming something else, no crystallographer could venture to decide, without the aid of chemical analysis. Haüy had assumed, as the general result of his researches, that the same chemical elements, combined in the same proportions, would always exhibit the same crystalline form; and reciprocally, that the same form and angles (except in the obvious case of the tessular system, in which the angles are determined by its *being* the tessular system,) implied the same chemical constitution. But this dogma could only be considered as an approximate conjecture; for there were many glaring and unexplained exceptions to it. The explanation of several of these was beautifully described by the discovery that there are various elements which are *isomorphous* to each other; that is, such that one may take the place of another without altering the crystalline form; and thus the chemical composition may be much changed, while the crystallographic character is undisturbed.

This truth had been caught sight of, probably as a guess only, by Fuchs as early as 1815 In speaking of a mineral which had been called Gehlenite, he says, "I hold the oxide of iron, not for an essential component part of this genus, but only as a *vicarious* element, replacing so much lime. We shall find it necessary to consider the results of several analyses of mineral bodies in this point of view, if we wish, on the one hand, to bring them into agreement with the doctrine of chemical proportions, and on the other, to avoid unnecessarily splitting up genera." In a lecture *On the Mutual Influence of*

*Chemistry and Mineralogy*,[1] he again draws attention to his term *vicarious* (*vicarirende*), which undoubtedly expresses the nature of the general law afterwards established by Mitscherlich in 1822.

But Fuchs's conjectural expression was only a prelude to Mitscherlich's experimental discovery of isomorphism. Till many careful analyses had given substance and signification to this conception of vicarious elements, it was of small value. Perhaps no one was more capable than Berzelius of turning to the best advantage any ideas which were current in the chemical world; yet we find him,[2] in 1820, dwelling upon a certain vague view of these cases,—that "oxides which contain equal doses of oxygen must have their general properties common;" without tracing it to any definite conclusions. But his scholar, Mitscherlich, gave this proposition a real crystallographical import. Thus he found that the carbonates of lime (calcspar,) of magnesia, of protoxide of iron, and of protoxide of manganese, agree in many respects of form, while the homologous angles vary through one or two degrees only; so again the carbonates of baryta, strontia, lead, and lime (arragonite), agree nearly; the different kinds of felspar vary only by the substitution of one alkali for another; the phosphates are almost identical with the arseniates of several bases. These, and similar results, were expressed by saying that, in such cases, the bases, lime, protoxide of iron, and the rest, are *isomorphous;* or in the latter instance, that the arsenic and phosphoric acids are isomorphous.

Since, in some of these cases, the substitution of one element of the isomorphous group for another does alter the angle, though slightly, it has since been proposed to call such groups *plesiomorphous*.

This discovery of isomorphism was of great importance, and excited much attention among the chemists of Europe. The history of its reception, however, belongs, in part, to the classification of minerals; for its effect was immediately to metamorphose the existing chemical systems of arrangement. But even those crystallographers and chemists who cared little for general systems of classification, received a powerful impulse by the expectation, which was now excited, of discovering definite laws connecting chemical constitution with crystalline form. Such investigations were soon carried on with great activity. Thus, at a recent period, Abich analysed a number of tessular minerals, spinelle, pleonaste, gahnite, franklinite, and chromic iron oxide; and

[1] Munich, 1820. [2] *Essay on the Theory of Chemical Proportions*, p. 122.

seems to have had some success in given a common type to their chemical formulæ, as there is a common type in their crystallization.

[2nd Ed.] [It will be seen by the above account that Prof. Mitscherlich's merit in the great discovery of Isomorphism is not at all narrowed by the previous conjectures of M. Fuchs. I am informed, moreover, that M. Fuchs afterwards (in Schweigger's *Journal*) retracted the opinions he had put forward on this subject.]

*Dimorphism.*—My business is, to point out the connected truths which have been obtained by philosophers, rather than insulated difficulties which still stand out to perplex them. I need not, therefore, dwell on the curious cases of *dimorphism;* cases in which the same definite chemical compound of the same elements appears to have two different forms; thus the carbonate of lime has two forms, *calcspar* and *arragonite*, which belong to different systems of crystallization. Such facts may puzzle us; but they hardly interfere with any received general truths, because we have as yet no truths of very high order respecting the connexion of chemical constitution and crystalline form Dimorphism does not interfere with isomorphism; the two classes of facts stand at the same stage of inductive generalization, and we wait for some higher truth which shall include both, and rise above them.

[2nd Ed.] [For additions to our knowledge of the Dimorphism of Bodies, see Professor Johnstone's valuable *Report* on that subject in the *Reports of the British Association* for 1837. Substances have also been found which are *trimorphous.* We owe to Professor Mitscherlich the discovery of dimorphism, as well as of isomorphism: and to him also we owe the greater part of the knowledge to which these discoveries have led.]

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## CHAPTER VII.

### Attempts to Establish the Fixity of other Physical Properties.—Werner.

THE reflections from which it appeared, (at the end of the last Book,) that in order to obtain general knowledge respecting bodies, we must give scientific fixity to our appreciation of their properties, applies to their other properties as well as to their crystalline form. And though none of the other properties have yet been referred to standards so definite as that which geometry supplies for crystals, a system has been introduced which makes their measures far more constant and precise than they are to a common undisciplined sense.

The author of this system was Abraham Gottlob Werner, who had been educated in the institutions which the Elector of Saxony had established at the mines of Freiberg. Of an exact and methodical intellect, and of great acuteness of the senses, Werner was well fitted for the task of giving fixity to the appreciation of outward impressions; and this he attempted in his *Dissertation on the external Characters of Fossils*, which was published at Leipzig in 1774. Of the precision of his estimation of such characters, we may judge from the following story, told by his biographer Frisch.[1] One of his companions had received a quantity of pieces of amber, and was relating to Werner, then very young, that he had found in the lot one piece from which he could extract no signs of electricity. Werner requested to be allowed to put his hand in the bag which contained these pieces, and immediately drew out the unelectrical piece. It was yellow chalcedony, which is distinguishable from amber by its weight and coldness.

The principal external characters which were subjected by Werner to a systematic examination were color, lustre, hardness, and specific gravity. His subdivisions of the first character (*Color*), were very numerous; yet it cannot be doubted that if we recollect them by the eye, and not by their names, they are definite and valuable characters, and especially the metallic colors. Breithaupt, merely by the aid of this character, distinguished two new compounds among the small grains found along with the grains of platinum, and usually confounded with them. The kinds of *Lustre*, namely, *glassy*, *fatty*, *adamantine*, *metallic*, are, when used in the same manner, equally valuable. *Specific Gravity* obviously admits of a numerical measure; and the *Hardness* of a mineral was pretty exactly defined by the substances which it would scratch, and by which it was capable of being scratched.

Werner soon acquired a reputation as a mineralogist, which drew persons from every part of Europe to Freiberg in order to hear his lectures; and thus diffused very widely his mode of employing external characters. It was, indeed, impossible to attend so closely to

[1] *Werner's Leben*, p. 26.

these characters as the Wernerian method required, without finding that they were more distinctive than might at first sight be imagined; and the analogy which this mode of studying Mineralogy established between that and other branches of Natural History, recommended the method to those in whom a general inclination to such studies was excited. Thus Professor Jameson of Edinburgh, who had been one of the pupils of Werner at Freiberg, not only published works in which he promulgated the mineralogical doctrines of his master, but established in Edinburgh a "Wernerian Society," having for its object the general cultivation of Natural History.

Werner's standards and nomenclature of external characters were somewhat modified by Mohs, who, with the same kinds of talents and views, succeeded him at Freiberg. Mohs reduced hardness to numerical measure by selecting ten known minerals, each harder than the other in order, from *talc* to *corundum* and *diamond*, and by making the place which these minerals occupy in the list, the numerical measure of the hardness of those which are compared with them. The result of the application of this fixed measurement and nomenclature of external characters will appear in the History of Classification, to which we now proceed.

# SYSTEMATIC MINERALOGY

## CHAPTER VIII.

### Attempts at the Classification of Minerals.

*Sect.* 1.—*Proper object of Classification.*

THE fixity of the crystalline and other physical properties of minerals is turned to account by being made the means of classifying such objects. To use the language of Aristotle,[1] Classification is the *architectonic* science, to which Crystallography and the Doctrine of External Characters are subordinate and ministerial, as the art of the bricklayer and carpenter are to that of the architect. But classification itself is useful only as subservient to an ulterior science, which shall furnish us with knowledge concerning things so classified. To classify is to divide and to name; and the value of the Divisions which we thus make, and of the names which we give them, is this;—that they render exact knowledge and general propositions possible. Now the knowledge which we principally seek concerning minerals is a knowledge of their chemical composition; the general propositions to which we hope to be led are such as assert relations between their intimate constitution and their external attributes. Thus our Mineralogical Classification must always have an eye turned towards Chemistry. We cannot get rid of the fundamental conviction, that the elementary composition of bodies, since it fixes their essence, must determine their properties. Hence all mineralogical arrangements, whether they profess it or not, must be, in effect, chemical; they must have it for their object to bring into view a set of relations, which, whatever else they may be, are at least chemical relations. We may begin with the outside, but it is only in order to reach the inner struc

[1] *Eth. Nicom.* i. 2.

ture. We may classify without reference to chemistry; but if we do so, it is only that we may assert chemical propositions with reference to our classification.

But, as we have already attempted to show, we not only may, but we *must* classify, by other than chemical characters, in order to be able to make our classification the basis of chemical knowledge. In order to assert chemical truths concerning bodies, we must have the bodies known by some tests not chemical. The chemist cannot assert that Arragonite does or does not contain Strontia, except the mineralogist can tell him whether any given specimen is or is not *Arragonite*. If chemistry be called upon to supply the *definitions* as well as the *doctrines* of mineralogy, the science can only consist of identical propositions.

Yet chemistry has been much employed in mineralogical classifications, and, it is generally believed, with advantage to the science: How is this consistent with what has been said?

To this the answer is, that when this *has* been done with advantage, the authority of external characters, as well as of chemical constitution, has really been brought into play. We have two sets of properties to compare, chemical and physical; to exhibit the connexion of these is the object of scientific mineralogy. And though this connexion would be most distinctly asserted, if we could keep the two sets of properties distinct, yet it may be brought into view in a great degree, by classifications in which both are referred to as guides. Since the governing principle of the attempts at classification is the conviction that the chemical constitution and the physical properties have a definite relation to each other, we appear entitled to use both kinds of evidence, in proportion as we can best obtain each; and then the general consistency and convenience of our system will be the security for its containing substantial knowledge, though this be not presented in a rigorously logical or systematic form.

Such *mixed systems* of classification, resting partly on chemical and partly on physical characters, naturally appeared as the earliest attempts in this way, before the two members of the subject had been clearly separated in men's minds; and these systems, therefore, we must first give an account of.

### *Sect. 2.—Mixed Systems of Classification.*

*Early Systems.*—The first attempts at classifying minerals went upon the ground of those differences of general aspect which had been recognized in the formation of common language; as *earths*, *stones*, *metals*. But such arrangements were manifestly vague and confused; and when chemistry had advanced to power and honor, her aid was naturally called in to introduce a better order. "Hiarne and Bromell were, as far as I know," says[2] Cronstedt, "the first who founded any mineral system upon chemical principles; to them we owe the three known divisions of the most simple mineral bodies; viz., the *calcarei*, *vitrescentes*, and *apyri*." But Cronstedt's own *Essay towards a System of Mineralogy*, published in Swedish in 1758, had perhaps more influence than any other, upon succeeding systems. In this, the distinction of earths and stones, and also of vitrescent and non-vitrescent earths (*apyri*), is rejected. The earths are classed as *calcareous*, *siliceous*, *argillaceous*, and the like. Again, calcareous earth is pure (*calc spar*), or united with acid of vitriol (*gypsum*), or united with the muriatic acid (*sal ammoniac*), and the like. It is easy to see that this is the method, which, in its general principle, has been continued to our own time. In such methods, it is supposed that we can recognize the substance by its general appearance, and on this assumption, its place in the system conveys to us chemical knowledge concerning it.

But as the other branches of Natural History, and especially Botany, assumed a systematic form, many mineralogists became dissatisfied with this casual and superficial mode of taking account of external characters; they became convinced, that in Mineralogy as in other sciences, classification must have its system and its rules. The views which Werner ascribes to his teacher, Pabst van Ohain,[3] show the rise of those opinions which led through Werner to Mohs: "He was of opinion that a natural mineral system must be constructed by chemical determinations, and external characters at the same time (*methodus mixta*); but that along with this, mineralogists ought also to construct and employ what he called an *artificial system*, which might serve us as a guide (*loco indicis*) how to introduce newly-discovered fossils into the system, and how to find easily and quickly those already known and introduced." Such an artificial system, containing not the grounds of classification, but marks for recognition, was afterwards attempted by Mohs, and termed by him the *Characteristic* of his system.

*Werner's System.*—But, in the mean time, Werner's classification had an extensive reign, and this was still a mixed system. Werner himself, indeed, never published a system of mineralogy. "We might

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[2] *Mineralogy*, Pref. p. viii.

[3] Frisch *Werner's Leben*, p. 15.

almost imagine," Cuvier says,[4] "that when he had produced his nomenclature of external characters, he was affrighted with his own creation, and that the reason of his writing so little after his first essay, was to avoid the shackles which he had imposed upon others." His system was, indeed, made known both in and out of Germany, by his pupils; but in consequence of Werner's unwillingness to give it on his own authority, it assumed, in its published forms, the appearance of an extorted secret imperfectly told. A *Notice of the Mineralogical Cabinet of Mine-Director Pabst von Ohain*, was, in 1792, published by Karsten and Hoffman, under Werner's direction; and conveyed by example, his views of mineralogical arrangement; and[5] in 1816 his *Doctrine of Classification* was surreptitiously copied from his manuscript, and published in a German Journal, termed *The Hesperus*. But it was only in 1817, after his death, that there appeared *Werner's Last Mineral System*, edited from his papers by Breithaupt and Köhler: and by this time, as we shall soon see, other systems were coming forwards on the stage.

A very slight notice of Werner's arrangement will suffice to show that it was, as we have termed it, a Mixed System. He makes four great Classes of fossils, *Earthy*, *Saline*, *Combustible*, *Metallic:* the earthy fossils are in eight Genera—Diamond, Zircon, Silica, Alumina, Talc, Lime, Baryta, Hallites. It is clear that these genera are in the main chemical, for chemistry alone can definitely distinguish the different Earths which characterize them. Yet the Wernerian arrangement supposed the distinctions to be practically made by reference to those external characters which the teacher himself could employ with such surpassing skill. And though it cannot be doubted, that the chemical views which prevailed around him had a latent influence on his classification in some cases, he resolutely refused to bend his system to the authority of chemistry. Thus,[6] when he was blamed for having, in opposition to the chemists, placed diamond among the earthy fossils, he persisted in declaring that, mineralogically considered, it was a stone, and could not be treated as anything else.

This was an indication to that tendency, which, under his successor, led to a complete separation of the two grounds of classification. But before we proceed to this, we must notice what was doing at this period in other parts of Europe.

*Haüy's System.*—Though Werner, on his own principles, ought to

[4] Cuv. *El.* ii. 314. [5] Frisch. p. 52. [6] Frisch. p. 62.

have been the first person to see the immense value of the most marked of external characters, crystalline form, he did not, in fact, attach much importance to it. Perhaps he was in some measure fascinated by a fondness for those characters which he had himself systematized, and the study of which did not direct him to look for geometrical relations. However this may be, the glory of giving to Crystallography its just importance in Mineralogy is due to France: and the Treatise of Haüy, published in 1801, is the basis of the best succeeding works of mineralogy. In this work, the arrangement is professedly chemical; and the classification thus established is employed as the means of enunciating crystallographic and other properties. "The principal object of this Treatise," says the author,[7] "is the exposition and development of a method founded on certain principles, which may serve as a framework for all the knowledge which Mineralogy can supply, aided by the different sciences which can join hands with her and march on the same line. It is worthy of notice, as characteristic of this period of Mixed Systems, that the classification of Haüy, though founded on principles so different from the Wernerian ones, deviates little from it in the general character of the divisions. Thus, the first Order of the first Class of Haüy is *Acidiferous Earthy Substances;* the first genus is *Lime;* the species are, *Carbonate of Lime*, *Phosphate of Lime*, *Fluate of Lime*, *Sulphate of Lime*, and so on.

*Other Systems.*—Such mixed methods were introduced also into this country, and have prevailed, we may say, up to the present time. The *Mineralogy* of William Phillips, which was published in 1824, and which was an extraordinary treasure of crystallographic facts, was arranged by such a mixed system; that is, by a system professedly chemical; but, inasmuch as a rigid chemical system is impossible, and the assumption of such a one leads into glaring absurdities, the system was, in this and other attempts of the same kind, corrected by the most arbitrary and lax application of other considerations.

It is a curious example of the difference of national intellectual character, that the manifest inconsistencies of the prevalent systems, which led in Germany, as we shall see, to bold and sweeping attempts at reform, produced in England a sort of contemptuous despair with regard to systems in general;—a belief that no system could be consistent or useful;—and a persuasion that the only valuable knowledge is the accumulation of particular facts. This is not the place to

[7] Disc. Prél. p. xvii.

explain how erroneous and unphilosophical such an opinion is. But we may notice that while such a temper prevails among us, our place in this science can never be found in advance of that position which we are now considering as exemplified in the period of Werner and Haüy. So long as we entertain such views respecting the objects of Mineralogy, we can have no share in the fortunes of the succeeding period of its history, to which I now proceed.

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## CHAPTER IX.

### Attempts at the Reform of Mineralogical Systems.—Separation of the Chemical and Natural History Methods.

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#### *Sect.* 1.—*Natural History System of Mohs.*

THE chemical principle of classification, if pursued at random, as in the cases just spoken of, leads to results at which a philosophical spirit revolts; it separates widely substances which are not distinguishable; joins together bodies the most dissimilar; and in hardly any instance does it bring any truth into view. The vices of classifications like that of Haüy could not long be concealed; but even before time had exposed the weakness of his system, Haüy himself had pointed out, clearly and without reserve,[1] that a chemical system is only one side of the subject, and supposes, as its counterpart, a science of external characters. In the mean time, the Wernerians were becoming more and more in love with the form which they had given to such a science. Indeed, the expertness which Werner and his scholars acquired in the use of external characters, justified some partiality for them. It is related of him,[2] that, by looking at a piece of iron-ore, and poising it in his hand, he was able to tell, almost precisely, the proportion of pure metal which it contained. And in the last year of his life,[3] he had marked out, as the employment of the ensuing winter, the study of the system of Berzelius, with a view to find out the laws of combination as disclosed by external characters. In the same spirit, his pupil Breit-

[1] See his Disc. Prél. [2] Frisch. *Werner's Leben*, p. 78. [3] Frisch. 3

aaupt[4] attempted to discover the ingredients of minerals by their peculiarities of crystallization. The persuasion that there must be *some* connexion between composition and properties, transformed itself, in their minds, into a belief that they could seize the nature of the connexion by a sort of instinct.

This opinion of the independency of the science of external characters, and of its sufficiency for its own object, at last assumed its complete form in the bold attempt to construct a system which should borrow nothing from chemistry. This attempt was made by Frederick Mohs, who had been the pupil of Werner, and was afterwards his successor in the school of Freiberg; and who, by the acute and methodical character of his intellect, and by his intimate knowledge of minerals, was worthy of his predecessor. Rejecting altogether all divisions of which the import was chemical, Mohs turned for guidance, or at least for the light of analogy, to botany. His object was to construct a *Natural System* of mineralogy. What the conditions and advantages of a natural system of any province of nature are, we must delay to explain till we have before us, in botany, a more luminous example of such a scheme. But further; in mineralogy, as in botany, besides the Natural System, by which we *form* our classes, it is necessary to have an *Artificial System*, by which we *recognize* them;—a principle which, we have seen, had already taken root in the school of Freiberg. Such an artificial system Mohs produced in his *Characteristic of the Mineral Kingdom*, which was published at Dresden in 1820; and which, though extending only to a few pages, excited a strong interest in Germany, where men's minds were prepared to interpret the full import of such a work. Some of the traits of such a "Characteristic" had, indeed, been previously drawn by others; as for example, by Haüy, who notices that each of his Classes has peculiar characters. For instance, his First Class (acidiferous substances,) alone possesses these combinations of properties; "division into a regular octohedron, without being able to scratch glass; specific gravity above 3·5, without being able to scratch glass." The extension of such characters into a scheme which should exhaust the whole mineral kingdom, was the undertaking of Mohs.

Such a collection of marks of classes, implied a classification previously established, and accordingly, Mohs had created his own mineral system. His aim was to construct it, as we shall hereafter see that other natural systems are constructed, by taking into account *all* the

[4] *Dresdn. Auswahl*, vol. ii. p. 97

resemblances and differences of the objects classified. It is obvious that to execute such a work, implied a most intimate and universal acquaintance with minerals;—a power of combining in one vivid survey the whole mineral kingdom. To illustrate the spirit in which Professor Mohs performed his task, I hope I may be allowed to refer to my own intercourse with him. At an early period of my mineralogical studies, when the very conception of a Natural System was new to me, he, with great kindliness of temper, allowed me habitually to propose to him the scruples which arose in my mind, before I could admit principles which appeared to me then so vague and indefinite; and answered my objections with great patience and most instructive clearness. Among other difficulties, I one day propounded to him this;—"You have published a Treatise on Mineralogy, in which you have described *all* the important properties of all known minerals. On your principles, then, it ought to be possible, merely by knowing the descriptions in your book, and without seeing any minerals, to construct a natural system; and this natural system ought to turn out identical with that which you have produced, by so careful an examination of the minerals themselves." He pondered a moment, and then he answered, "It is true; but what an enormous *imagination* (*einbildungskraft*, *power of inward imagining*), a man must have for such a work!" Vividness of conception of sensible properties, and the steady intuition (*anschauung*) of objects, were deemed by him, and by the Wernerian school in general, to be the most essential conditions of complete knowledge.

It is not necessary to describe Mohs's system in detail; it may sufficiently indicate its form to state that the following substances, such as I before gave as examples of other arrangements, calcspar, gypsum, fluor spar, apatite, heavy spar, are by Mohs termed respectively, *Rhombohedral Lime Haloide*, *Gyps Haloide*, *Octohedral Fluor Haloide*, *Rhombohedral Fluor Haloide*, *Prismatic Hal Baryte*. These substances are thus referred to the *Orders* Haloide, and Baryte; to *Genera* Lime Haloide, Fluor Haloide, Hal Baryte; and the *Species* is an additional particularization.

Mohs not only aimed at framing such a system, but was also ambitious of giving to all minerals *Names* which should accord with the system. This design was too bold to succeed. It is true, that a new nomenclature was much needed in mineralogy: it is true, too, that it was reasonable to expect, from an improved classification, an improved nomenclature, such as had been so happily obtained in botany by the reform of Linnæus. But besides the defects of Mohs's system, he had not prepared his verbal novelties with the temperance and skill of the great botanical reformer. He called upon mineralogists to change the name of almost every mineral with which they were acquainted; and the proposed appellations were mostly of a cumbrous form, as the above example may serve to show. Such names could have obtained general currency, only after a general and complete acceptance of the system; and the system did not possess, in a sufficient degree, that evidence which alone could gain it a home in the belief of philosophers,—the coincidence of its results with those of Chemistry. But before I speak finally of the fortunes of the Natural-history System, I will say something of the other attempt which was made about the same time to introduce a Reform into Mineralogy from the opposite extremity of the science.

### *Sect.* 2.—*Chemical System of Berzelius and others.*

If the students of external characters were satisfied of the independence of their method, the chemical analysts were naturally no less confident of the legitimate supremacy of their principles: and when the beginning of the present century had been distinguished by the establishment of the theory of definite proportions, and by discoveries which pointed to the electro-chemical theory, it could not appear presumption to suppose, that the classification of bodies, so far as it depended on chemistry, might be presented in a form more complete and scientific than at any previous time.

The attempt to do this was made by the great Swedish chemist Jacob Berzelius. In 1816, he published his *Essay to establish a purely Scientific System of Mineralogy, by means of the Application of the Electro-chemical Theory and the Chemical Doctrine of Definite Proportions.* It is manifest that, for minerals which are constituted by the law of Definite Proportions, this constitution must be a most essential part of their character. The electro-chemical theory was called in aid, in addition to the composition, because, distinguishing the elements of all compounds as electro-positive and electro-negative, and giving to every element a place in a series, and a place defined by the degree of these relations, it seemed to afford a rigorous and complete principle of arrangement. Accordingly, Berzelius, in his First System, arranged minerals according to their electro-positive element, and the elements according to their electro-positive rank;

and supposed that he had thus removed all that was arbitrary and vague in the previous chemical systems of mineralogy.

Though the attempt appeared so well justified by the state of che mical science, and was so plausible in its principle, it was not long before events showed that there was some fallacy in these specious appearances. In 1820, Mitscherlich discovered Isomorphism: by that discovery it appeared that bodies containing very different electro-positive elements could not be distinguished from each other; it was impossible, therefore, to put them in distant portions of the classification;—and thus the first system of Berzelius crumbled to pieces.

But Berzelius did not so easily resign his project. With the most unhesitating confession of his first failure, but with undaunted courage, he again girded himself to the task of rebuilding his edifice. Defeated at the electro-positive position, he now resolved to make a stand at the electro-negative element. In 1824, he published in the Transactions of the Swedish Academy, a Memoir *On the Alterations in the Chemical Mineral System, which necessarily follow from the Property exhibited by Isomorphous Bodies, of replacing each other in given Proportions.* The alteration was, in fact, an inversion of the system, with an attempt still to preserve the electro-chemical principle of arrangement. Thus, instead of arranging metallic minerals according to the *metal*, under iron, copper, &c., all the *sulphurets* were classed together, all the *oxides* together, all the *sulphates* together, and so in other respects. That such an order was a great improvement on the preceding one, cannot be doubted; but we shall see, I think, that as a strict scientific system it was not successful. The discovery of isomorphism, however, naturally led to such attempts. Thus Gmelin also, in 1825, published a mineral system,[6] which, like that of Berzelius, founded its leading distinctions on the electro-negative, or, as it was sometimes termed, the *formative* element of bodies; and, besides this, took account of the *numbers* of atoms or proportions which appear in the composition of the body; distinguishing, for instance, Silicates, as simple silicates, double silicates, and so on, to *quintuple* silicate (*Pechstein*) and *sextuple* silicate (*Perlstein*). In like manner, Nordenskiöld devised a system resting on the same bases, taking into account also the crystalline form. In 1824, Beudant published his *Traité Elémentaire de Minéralogie*, in which he professes to found his arrangement on the electro-negative element, and on Ampère's circular arrange

[6] *Zeitsch. der Min.* 1825, p. 435.

ment of elementary substances. Such schemes exhibit rather a play of the mere logical faculty, exercising itself on assumed principles, than any attempt at the real interpretation of nature. Other such pure chemical systems may have been published, but it is not necessary to accumulate instances. I proceed to consider their result.

### *Sect.* 3.—*Failure of the Attempts at Systematic Reform.*

It may appear presumptuous to speak of the failure of those whom, like Berzelius and Mohs, we acknowledge as our masters, at a period when, probably, they and some of their admirers still hold them to have succeeded in their attempt to construct a consistent system. But I conceive that my office as an historian requires me to exhibit the fortunes of this science in the most distinct form of which they admit, and that I cannot evade the duty of attempting to seize the true aspect of recent occurrences in the world of science. Hence I venture to speak of the failure of both the attempts at framing a pure scientific system of mineralogy,—that founded on the chemical, and that founded on the natural-history principle; because it is clear that they have not obtained that which alone we could, according to the views here presented, consider as success,—a coincidence of each with the other. A Chemical System of arrangement, which should bring together, in all cases, the substances which come nearest each other in external properties;—a Natural-history System, which should be found to arrange bodies in complete accordance with their chemical constitution:—if such systems existed, they might, with justice, claim to have succeeded. Their agreement would be their verification. The interior and exterior system are the type and the antitype, and their entire correspondence would establish the mode of interpretation beyond doubt. But nothing less than this will satisfy the requisitions of science. And when, therefore, the chemical and the natural-history system, though evidently, as I conceive, tending towards each other, are still far from coming together, it is impossible to allow that either method has been successful in regard to its proper object.

But we may, I think, point out the fallacy of the principles, as well as the imperfection of the results, of both of those methods. With regard to that of Berzelius, indeed, the history of the subject obviously betrays its unsoundness. The electro-positive principle was, in a very short time after its adoption, proved and acknowledged to be utterly untenable: what security have we that the electro-negative element is more trustworthy? Was not the necessity of an entire change of system, a proof that the ground, whatever that was, on which the electro-chemical principle was adopted, was an unfounded assumption? And, in fact, do we not find that the same argument which was allowed to be fatal to the First System of Berzelius, applies in exactly the same manner against the Second? If the electro-positive elements be often isomorphous, are not the electro-negative elements sometimes isomorphous also? for instance, the arsenic and phosphoric acids. But to go further, what *is* the ground on which the electro-chemical arrangement is adopted? Granted that the electrical relations of bodies are important; but how do we come to know that these relations have anything to do with mineralogy? How does it appear that on them, principally, depend those external properties which mineralogy must study? How does it appear that because sulphur is the electro-negative part of one body, and an acid the electro-negative part of another, these two elements similarly affect the compounds? How does it appear that there is any analogy whatever in their functions? We allow that the composition must, in *some way*, determine the classified place of the mineral,—but why in *this* way?

I do not dwell on the remark which Berzelius himself[6] makes on Nordenskiöld's system;—that it assumes a perfect knowledge of the composition in every case; although, considering the usual discrepancies of analyses of minerals, this objection must make all pure chemical systems useless. But I may observe, that mineralogists have not yet determined what characters are sufficiently affixed to determine a species of minerals. We have seen that the ancient notion of the composition of a species, has been unsettled by the discovery of isomorphism. The tenet of the constancy of the angle is rendered doubtful by cases of plesiomorphism. The optical properties, which are so closely connected with the crystalline, are still so imperfectly known, that they are subject to changes which appear capricious and arbitrary. Both the chemical and the optical mineralogists have constantly, of late, found occasion to separate species which had been united, and to bring together those which had been divided. Everything shows that, in this science, we have our classification still to begin. The detection of that fixity of characters, on which a right establishment of species must rest, is not yet complete, great as the progress is which we have made, by acquiring a knowledge of the laws of crystallization and of

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[6] *Jahres Bericht.* viii. 188.

definite chemical constitution. Our ignorance may surprise us; but it may diminish our surprise to recollect, that the knowledge which we seek is that of the laws of the physical constitution of all bodies whatever; for to us, as mineralogists, all chemical compounds are minerals.

The defect of the principle of the natural-history classifiers may be thus stated:—in studying the external characters of bodies, they take for granted that they can, without any other light, discover the relative value and importance of those characters. The grouping of Species into a Genus, of Genera into an Order, according to the method of this school, proceeds by no definite rules, but by a latent talent of appreciation,—a sort of classifying instinct. But this course cannot reasonably be expected to lead to scientific truth; for it can hardly be hoped, by any one who looks at the general course of science, that we shall discover the relation between external characters and chemical composition, otherwise than by tracing their association in cases where both are known. It is urged that in other classificatory sciences, in botany, for example, we obtain a natural classification from external characters without having recourse to any other source of knowledge. But this is not true in the sense here meant. In framing a natural system of botany, we have constantly before our eyes the principles of physiology; and we estimate the value of the characters of a plant by their bearing on its functions,—by their place in its organization. In an unorganic body, the chemical constitution is the law of its being; and we shall never succeed in framing a science of such bodies but by studiously directing our efforts to the interpretation of that law.

On these grounds, then, I conceive, that the bold attempts of Mohs and of Berzelius to give new forms to mineralogy, cannot be deemed successful in the manner in which their authors aspired to succeed. Neither of them can be marked as a permanent reformation of the science. I shall not inquire how far they have been accepted by men of science, for I conceive that their greatest effect has been to point out improvements which might be made in mineralogy without going the whole length either of the *pure* chemical, or of the *pure* natural-history system.

### *Sect. 4.—Return to Mixed Systems with Improvements.*

In spite of the efforts of the purists, mineralogists returned to mixed systems of classification; but these systems are much better than they were before such efforts were made.

The Second System of Berzelius, though not tenable in its rigorous form, approaches far nearer than any previous system to a complete character, bringing together like substances in a large portion of its extent. The System of Mohs also, whether or not unconsciously swayed by chemical doctrines, forms orders which have a community of chemical character; thus, the minerals of the order *Haloide* are salts of oxides, and those of the order *Pyrites* are sulphurets of metals. Thus the two methods appear to be converging to a common centre; and though we are unable to follow either of them to this point of union, we may learn from both in what direction we are to look for it. If we regard the best of the pure systems hitherto devised as indications of the nature of that system, perfect both as a chemical and as a natural-history system, to which a more complete condition of mineralogical knowledge may lead us, we may obtain, even at present, a tolerably good approximation to a complete classification; and such a one, if we recollect that it must be imperfect, and is to be held as provisional only, may be of no small value and use to us.

The best of the mixed systems produced by this compromise again comes from Freiberg, and was published by Professor Naumann in 1828. Most of his orders have both a chemical character and great external resemblances. Thus his *Haloides*, divided into *Unmetallic* and *Metallic*, and these again into *Hydrous* and *Anhydrous*, give good natural groups. The most difficult minerals to arrange in all systems are the siliceous ones. These M. Naumann calls *Silicides*, and subdivides them into *Metallic*, *Unmetallic*, and *Amphoteric* or mixed; and again, into *Hydrous* and *Anhydrous*. Such a system is at least a good basis for future researches; and this is, as we have said, all that we can at present hope for. And when we recollect that the natural-history principle of classification has begun, as we have already seen, to make its appearance in our treatises of chemistry, we cannot doubt that some progress is making towards the object which I have pointed out. But we know not yet how far we are from the end. The combination of chemical, crystallographical, physical and optical properties into some lofty generalization, is probably a triumph reserved for future and distant years.

*Conclusion.*—The history of Mineralogy, both in its successes and by its failures, teaches us this lesson;—that in the sciences of classification, the establishment of the fixity of characters, and the discovery of such characters as are fixed, are steps of the first importance in the progress of these sciences. The recollection of this maxim may aid us in shaping our course through the history of other sciences of this kind; in which, from the extent of the subject, and the mass of literature belonging to it, we might at first almost despair of casting the history into distinct epochs and periods. To the most prominent of such sciences, Botany, I now proceed.

# BOOK XVI

*CLASSIFICATORY SCIENCES.*

# HISTORY

OF

# SYSTEMATIC BOTANY AND ZOOLOGY.

. . . . . Vatem aspicies quæ rupe sub altâ
Fata canit, foliisque notas et nomina mandat.
Quæcunque in foliis descripsit carmina virgo
Digerit in numerum atque antro seclusa relinquit
Illa manent immorta locis neque ab ordine cedunt.

VIRGIL. *Æn.* iii. 443.

Behold the Sibyl !—Her who weaves a long,
A tangled, full, yet sweetly flowing song.
Wondrous her skill ; for leaf on leaf she frames
Unerring symbols and enduring names ;
And as her nicely measured line she binds,
For leaf on leaf a fitting place she finds ;
Their place once found, no more the leaves depart,
But fixed rest :—such is her magic art.

## INTRODUCTION.

WE now arrive at that study which offers the most copious and complete example of the sciences of classification, I mean Botany. And in this case, we have before us a branch of knowledge of which we may say, more properly than of any of the sciences which we have reviewed since Astronomy, that it has been constantly advancing, more or less rapidly, from the infancy of the human race to the present day. One of the reasons of this resemblance in the fortunes of two studies so widely dissimilar, is to be found in a simplicity of principle which they have in common; the ideas of Likeness and Difference, on which the knowledge of plants depends, are, like the ideas of Space and Time, which are the foundation of astronomy, readily apprehended with clearness and precision, even without any peculiar culture of the intellect. But another reason why, in the history of Botany, as in that of Astronomy, the progress of knowledge forms an unbroken line from the earliest times, is precisely the great difference of the kind of knowledge which has been attained in the two cases. In Astronomy, the discovery of general truths began at an early period of civilization; in Botany, it has hardly yet begun; and thus, in each of these departments of study, the lore of the ancient is homogeneous with that of the modern times, though in the one case it is science, in the other, the absence of science, which pervades all ages. The resemblance of the form of their history arises from the diversity of their materials.

I shall not here dwell further upon this subject, but proceed to trace rapidly the progress of *Systematic Botany*, as the classificatory science is usually denominated, when it is requisite to distinguish between that and Physiological Botany. My own imperfect acquaintance with this study admonishes me not to venture into its details, further than my purpose absolutely requires. I trust that, by taking my views principally from writers who are generally allowed to possess the best insight into the science, I may be able to draw the larger features of its history with tolerable correctness; and if I succeed in this, I shall attain an object of great importance in my general scheme.

## CHAPTER I.

### Imaginary Knowledge of Plants.

THE apprehension of such differences and resemblances as those by which we group together and discriminate the various kinds of plants and animals, and the appropriation of words to mark and convey the resulting notions, must be presupposed, as essential to the very beginning of human knowledge. In whatever manner we imagine man to be placed on the earth by his Creator, these processes must be conceived to be, as our Scriptures represent them, contemporaneous with the first exertion of reason, and the first use of speech. If we were to indulge ourselves in framing a hypothetical account of the origin of language, we should probably assume as the first-formed words, those which depend on the visible likeness or unlikeness of objects; and should arrange as of subsequent formation, those terms which imply, in the mind, acts of wider combination and higher abstraction. At any rate, it is certain that the names of the kinds of vegetables and animals are very abundant even in the most uncivilized stages of man's career. Thus we are informed[1] that the inhabitants of New Zealand have a distinct name of every tree and plant in their island, of which there are six or seven hundred or more different kinds. In the accounts of the rudest tribes, in the earliest legends, poetry, and literature of nations, pines and oaks, roses and violets, the olive and the vine, and the thousand other productions of the earth, have a place, and are spoken of in a manner which assumes, that in such kinds of natural objects, permanent and infallible distinctions had been observed and universally recognized.

For a long period, it was not suspected that any ambiguity or confusion could arise from the use of such terms; and when such inconveniences did occur, (as even in early times they did,) men were far from divining that the proper remedy was the construction of a science of classification. The loose and insecure terms of the language of common life retained their place in botany, long after their

[1] Yate's *New Zealand*, p. 238.

defects were severely felt: for instance, the vague and unscientific distinction of vegetables into *trees*, *shrubs*, and *herbs*, kept its ground till the time of Linnæus.

While it was thus imagined that the identification of a plant, by means of its name, might properly be trusted to the common uncultured faculties of the mind, and to what we may call the instinct of language, all the attention and study which were bestowed on such objects, were naturally employed in learning and thinking upon such circumstances respecting them as were supplied by any of the common channels through which knowledge and opinion flow into men's minds.

The reader need hardly be reminded that in the earlier periods of man's mental culture, he acquires those opinions on which he loves to dwell, not by the exercise of observation subordinate to reason; but, far more, by his fancy and his emotions, his love of the marvellous, his hopes and fears. It cannot surprise us, therefore, that the earliest lore concerning plants which we discover in the records of the past, consists of mythological legends, marvellous relations, and extraordinary medicinal qualities. To the lively fancy of the Greeks, the Narcissus, which bends its head over the stream, was originally a youth who in such an attitude became enamored of his own beauty: the hyacinth,[2] on whose petals the notes of grief were traced (A I, A I), recorded the sorrow of Apollo for the death of his favorite Hyacinthus: the beautiful lotus of India,[3] which floats with its splendid flower on the surface of the water, is the chosen seat of the goddess Lackshmi, the daughter of Ocean.[4] In Egypt, too,[5] Osiris swam on a lotus-leaf, and Harpocrates was cradled in one. The lotus-eaters of Homer lost immediately their love of home. Every one knows how easy it would be to accumulate such tales of wonder or religion.

Those who attended to the effects of plants, might discover in them some medicinal properties, and might easily imagine more; and when the love of the marvellous was added to the hope of health, it is easy to believe that men would be very credulous. We need not dwell upon the examples of this. In Pliny's Introduction to that book of his

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[2] Lilium martagon.

Ipse suos gemitus foliis inscribit et A I, A I,
Flos habet inscriptum funestaque litera ducta est.—OVID.

[3] Nelumbium speciosum.

[4] Sprengel, *Geschichte der Botanik*, i. 27.

[5] Ib. i. 28.

Natural History which treats of the medicinal virtues of plants, he says,[6] "Antiquity was so much struck with the properties of herbs, that it affirmed things incredible. Xanthus, the historian, says, that a man killed by a dragon, will be restored to life by an herb which he calls *balin;* and that Thylo, when killed by a dragon, was recovered by the same plant. Democritus asserted, and Theophrastus believed, that there was an herb, at the touch of which, the wedge which the woodman had driven into a tree would leap out again. Though we cannot credit these stories, most persons believe that almost anything might be effected by means of herbs, if their virtues were fully known." How far from a reasonable estimate of the reality of such virtues were the persons who entertained this belief, we may judge from the many superstitious observances which they associated with the gathering and using of medicinal plants. Theophrastus speaks of these;[7] "The drug-sellers and the rhizotomists (root-cutters) tell us," he says, "some things which may be true, but other things which are merely solemn quackery;[8] thus they direct us to gather some plants, standing from the wind, and with our bodies anointed; some by night, some by day, some before the sun falls on them. So far there may be something in their rules. But others are too fantastical and far fetched. It is, perhaps, not absurd to use a prayer in plucking a plant; but they go further than this. We are to draw a sword three times round the mandragora, and to cut it looking to the west: again, to dance round it, and to use obscene language, as they say those who sow cumin should utter blasphemies. Again, we are to draw a line round the black hellebore, standing to the east and praying; and to avoid an eagle either on the right or on the left; for, say they, 'if an eagle be near, the cutter will die in a year.'"

This extract may serve to show the extent to which these imaginations were prevalent, and the manner in which they were looked upon by Theophrastus, our first great botanical author. And we may now consider that we have given sufficient attention to these fables and superstitions, which have no place in the history of the progress of real knowledge, except to show the strange chaos of wild fancies and legends out of which it had to emerge. We proceed to trace the history of the knowledge of plants.

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[6] Lib. xxv. 5. [7] *De Plantis*, ix. 9. [8] Ἐπιτραγῳδοῦντες.

## CHAPTER II.

### Unsystematic Knowledge of Plants.

A STEP was made towards the formation of the Science of Plants, although undoubtedly a slight one, as soon as men began to collect information concerning them and their properties, from a love and reverence for knowledge, independent of the passion for the marvellous and the impulse of practical utility. This step was very early made. The "wisdom" of Solomon, and the admiration which was bestowed upon it, prove, even at that period, such a working of the speculative faculty: and we are told, that among other evidences of his being "wiser than all men," "he spake of trees, from the cedar-tree that is in Lebanon even unto the hyssop that springeth out of the wall."[1] The father of history, Herodotus, shows us that a taste for natural history had, in his time, found a place in the minds of the Greeks. In speaking of the luxuriant vegetation of the Babylonian plain,[2] he is so far from desiring to astonish merely, that he says, "the blades of wheat and barley are full four fingers wide; but as to the size of the trees which grow from millet and sesame, though I could mention it, I will not; knowing well that those who have not been in that country will hardly believe what I have said already." He then proceeds to describe some remarkable circumstances respecting the fertilization of the date-palms in Assyria.

This curious and active spirit of the Greeks led rapidly, as we have seen in other instances, to attempts at collecting and systematizing knowledge on almost every subject: and in this, as in almost every other department, Aristotle may be fixed upon, as the representative of the highest stage of knowledge and system which they ever attained. The vegetable kingdom, like every other province of nature, was one of the fields of the labors of this universal philosopher. But though his other works on natural history have come down to us, and are a most valuable monument of the state of such knowledge in his time, his Treatise on Plants is lost. The book *De Plantis*

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[1] 1 Kings iv. 33.

[2] Herod. i. 193.

which appears with his name, is an imposture of the middle ages, full of errors and absurdities.[3]

His disciple, friend, and successor, Theophrastus of Eresos, is, as we have said already, the first great writer on botany whose works we possess; and, as may be said in most cases of the first great writer, he offers to us a richer store of genuine knowledge and good sense than all his successors. But we find in him that the Greeks of his time, who aspired, as we have said, to collect and *systematize* a body of information on every subject, failed in one half of their object, as far as related to the vegetable world. Their attempts at a systematic distribution of plants were altogether futile. Although Aristotle's divisions of the animal kingdom are, even at this day, looked upon with admiration by the best naturalists, the arrangements and comparisons of plants which were contrived by Theophrastus and his successors, have not left the slightest trace in the modern form of the science; and, therefore, according to our plan, are of no importance in our history. And thus we can treat all the miscellaneous information concerning vegetables which was accumulated by the whole of this school of writers, in no other way than as something antecedent to the first progress towards systematic knowledge.

The information thus collected by the unsystematic writers is of various kinds; and relates to the economical and medicinal uses of plants, their habits, mode of cultivation, and many other circumstances: it frequently includes some description; but this is always extremely imperfect, because the essential conditions of description had not been discovered. Of works composed of materials so heterogeneous, it can be of little use to produce specimens; but I may quote a few words from Theophrastus, which may serve to connect him with the future history of the science, as bearing upon one of the many problems respecting the identification of ancient and modern plants. It has been made a question whether the following description does not refer to the potato.[4] He is speaking of the differences of roots: "Some roots," he says, "are still different from those which have been described; as that of the *arachidna*[5] plant: for this bears fruit underground as well as above: the fleshy part sends one thick root deep into the ground, but the others, which bear the fruit, are more slender

[3] Mirbel, *Botanique*, ii. 505. [4] Theoph. i. 11.

[5] Most probably the *Arachnis hypogæa*, or ground-nut.

and higher up, and ramified. It loves a sandy soil, and has no leaf whatever."

The books of Aristotle and Theophrastus soon took the place of the Book of Nature in the attention of the degenerate philosophers who succeeded them. A story is told by Strabo[6] concerning the fate of the works of these great naturalists. In the case of the wars and changes which occurred among the successors of Alexander, the heirs of Theophrastus tried to secure to themselves his books, and those of his master, by burying them in the ground. There the manuscripts suffered much from damp and worms; till Apollonicon, a book-collector of those days, purchased them, and attempted, in his own way, to supply what time had obliterated. When Sylla marched the Roman troops into Athens, he took possession of the library of Apollonicon; and the works which it contained were soon circulated among the learned of Rome and Alexandria, who were thus enabled to *Aristotelize*[7] on botany as on other subjects.

The library collected by the Attalic kings of Pergamus, and the Alexandrian Museum, founded and supported by the Ptolemies of Egypt, rather fostered the commentatorial spirit than promoted the increase of any real knowledge of nature. The Romans, in this as in other subjects, were practical, not speculative. They had, in the times of their national vigor, several writers on agriculture, who were highly esteemed; but no author, till we come to Pliny, who dwells on the mere knowledge of plants. And even in Pliny, it is easy to perceive that we have before us a writer who extracted his information principally from books. This remarkable man,[8] in the middle of a public and active life, of campaigns and voyages, contrived to accumulate, by reading and study, an extraordinary store of knowledge of all kinds. So unwilling was he to have his reading and note-making interrupted, that, even before day-break in winter, and from his litter as he travelled, he was wont to dictate to his amanuensis, who was obliged to preserve his hand from the numbness which the cold occasioned, by the use of gloves.[9]

It has been ingeniously observed, that we may find traces in the botanical part of his Natural History, of the errors which this hurried and broken habit of study produced; and that he appears frequently to have had books read to him and to have heard them amiss.[10] Thus,

Strabo, lib. xiii. c. i., § 54. [7] Αριστοτελιζειν. [8] Sprengel, i. 163.
Plin. Jun. Epist. 3, 5. [10] Sprengel, i. 163.

among several other instances, Theophrastus having said that the plane-tree is in Italy rare,[11] Pliny, misled by the similarity of the Greek word (*spanian*, rare), says that the tree occurs in Italy and Spain.[12] His work has, with great propriety, been called the Encyclopædia of Antiquity; and, in truth, there are few portions of the learning of the times to which it does not refer. Of the thirty-seven Books of which it consists, no less than sixteen (from the twelfth to the twenty-seventh) relate to plants. The information which is collected in these books, is of the most miscellaneous kind; and the author admits, with little distinction, truth and error, useful knowledge and absurd fables. The declamatory style, and the comprehensive and lofty tone of thought which we have already spoken of as characteristic of the Roman writers, are peculiarly observable in him. The manner of his death is well known: it was occasioned by the eruption of Vesuvius, A.D. 79, to which, in his curiosity, he ventured so near as to be suffocated.

Pliny's work acquired an almost unlimited authority, as one of the standards of botanical knowledge, in the middle ages; but even more than his, that of his contemporary, Pedanius Dioscorides, of Anazarbus in Cilicia. This work, written in Greek, is held by the best judges[13] to offer no evidence that the author observed for himself. Yet he says expressly in his Preface, that his love of natural history, and his military life, have led him into many countries, in which he has had opportunity to become acquainted with the nature of herbs and trees.[14] He speaks of six hundred plants, but often indicates only their names and properties, giving no description by which they can be identified. The main cause of his great reputation in subsequent times was, that he says much of the medicinal virtues of vegetables.

We come now to the ages of darkness and lethargy, when the habit of original thought seems to die away, as the talent of original observation had done before. Commentators and mystics succeed to the philosophical naturalists of better times. And though a new race, altogether distinct in blood and character from the Greek, appropriates to itself the stores of Grecian learning, this movement does not, as might be expected, break the chains of literary slavery. The Arabs

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[11] Theoph. iv. 7. Ἐν μὲν γὰρ τῷ Ἀδρίᾳ πλάτανον οὔ φασιν εἶναι πλὴν περὶ τὸ Διομήδους ἱερόν, σπανίαν δὲ καὶ ἐν Ἰταλίᾳ πάσῃ.

[12] Plin. *Nat. Hist.* xii. 3. Et alias (platanos) fuisse in Italia, ac nominatim *Hispania*, apud auctores invenitur.

[13] Mirbel, 510. [14] Sprengel, i. 136.

bring, to the cultivation of the science of the Greeks, their own oriental habit of submission, their oriental love of wonder; and thus, while they swell the herd of commentators and mystics, they produce no philosopher.

Yet the Arabs discharged an important function in the history of human knowledge,[15] by preserving, and transmitting to more enlightened times, the intellectual treasures of antiquity. The unhappy dissensions which took place in the Christian church had scattered these treasures over the East, at a period much antecedent to the rise of the Saracen power. In the fifth century, the adherents of Nestorius, bishop of Constantinople, were declared heretical by the Council of Ephesus (A.D. 431), and driven into exile. In this manner, many of the most learned and ingenious men of the Christian world were removed to the Euphrates, where they formed the *Chaldean* church, erected the celebrated Nestorian school of Edessa, and gave rise to many offsets from this in various regions. Already, in the fifth century, Hibas, Cumas, and Probus, translated the writings of Aristotle into Syriac. But the learned Nestorians paid an especial attention to the art of medicine, and were the most zealous students of the works of the Greek physicians. At Djondisabor, in Khusistan, they became an ostensible medical school, who distributed academical honors as the result of public disputations. The califs of Bagdad heard of the fame and the wisdom of the doctors of Djondisabor, summoned some of them to Bagdad, and took measures for the foundation of a school of learning in that city. The value of the skill, the learning, and the virtues of the Nestorians, was so strongly felt, that they were allowed by the Mohammedans the free exercise of the Christian religion, and intrusted with the conduct of the studies of those of the Moslemin, whose education was most cared for. The affinity of the Syriac and Arabic languages made the task of instruction more easy. The Nestorians translated the works of the ancients out of the former into the latter language: hence there are still found Arabic manuscripts of Dioscorides, with Syriac words in the margin. Pliny and Aristotle likewise assumed an Arabic dress; and were, as well as Dioscorides, the foundation of instruction in all the Arabian academies; of which a great number were established throughout the Saracen empire, from Bokhara in the remotest east, to Marocco and Cordova in the west. After some time, the Mohammedans themselves began to translate and

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[15] Sprengel, i. 203.

extract from their Syriac sources; and at length to write works of their own. And thus arose vast libraries, such as that of Cordova, which contained 250,000 volumes.

The Nestorians are stated[16] to have first established among the Arabs those collections of medicinal substances (*Apothecæ*), from which our term *Apothecary* is taken; and to have written books (*Dispensatoria*) containing systematic instructions for the employment of these medicaments; a word which long continued to be implied in the same sense, and which we also retain, though in a modified application (*Dispensary*).

The directors of these collections were supposed to be intimately acquainted with plants; and yet, in truth, the knowledge of plants owed but little to them; for the Arabic Dioscorides was the source and standard of their knowledge. The flourishing commerce of the Arabians, their numerous and distant journeys, made them, no doubt, practically acquainted with the productions of lands unknown to the Greeks and Romans. Their Nestorian teachers had established Christianity even as far as Chiua and Malabar; and their travellers mention[17] the camphor of Sumatra, the aloe-wood of Socotra near Java, the tea of Chiua. But they never learned the art of converting their practical into speculative knowledge. They treat of plants only in so far as their use in medicine is concerned,[18] and followed Dioscorides in the description, and even in the order of the plants, except when they arrange them according to the Arabic alphabet. With little clearness of view, they often mistake what they read:[19] thus when Dioscorides says that *ligusticon* grows on the *Apennine*, a mountain not far from the *Alps;* Avicenna, misled by a resemblance of the Arabic letters, quotes him as saying that the plant grows on *Akabis*, a mountain near *Egypt*.

It is of little use to enumerate such writers. One of the most noted of them was Mesuë, physician of the Calif of Kahirah. His work, which was translated into Latin at a later period, was entitled, *On Simple Medicines;* a title which was common to many medical treatises, from the time of Galen in the second century. Indeed, of this opposition of *simple* and *compound* medicines, we still have traces in our language:

16 Sprengel, i. 205.
17 Sprengel, i. 206
18 Ib. i. 207.
19 Ib. i. 211

He would ope his leathern scrip,
And show me *simples* of a thousand names,
Telling their strange and vigorous faculties.

MILTON, *Comus*.

Where the subject of our history is so entirely at a stand, it is unprofitable to dwell on a list of names. The Arabians, small as their science was, were able to instruct the Christians. Their writings were translated by learned Europeans, for instance Michael Scot, and Constantine of Africa, a Carthaginian who had lived forty years among the Saracens,[20] and who died A.D. 1087. Among his works, is a Treatise, *De Gradibus*, which contains the Arabian medicinal lore. In the thirteenth century occur Encyclopædias, as that of Albertus Magnus, and of Vincent of Beauvais; but these contain no natural history except traditions and fables. Even the ancient writers were altogether perverted and disfigured. The Dioscorides of the middle ages varied materially from ours.[21] Monks, merchants, and adventurers travelled far, but knowledge was little increased. Simon of Genoa,[22] a writer on plants in the fourteenth century, boasts that he perambulated the East in order to collect plants. "Yet in his *Clavis Sanationis*," says a modern botanical writer,[23] "we discover no trace of an acquaintance with nature. He merely compares the Greek, Arabic, and Latin names of plants, and gives their medicinal effect after his predecessors:"—so little true is it, that the use of the senses alone necessarily leads to real knowledge.

Though the growing activity of thought in Europe, and the revived acquaintance with the authors of Greece in their genuine form, were gradually dispelling the intellectual clouds of the middle ages, yet during the fifteenth century, botany makes no approach to a scientific form. The greater part of the literature of this subject consisted of Herbals, all of which were formed on the same plan, and appeared under titles such as *Hortus*, or *Ortus Sanitatis*. There are, for example, three[24] such German Herbals, with woodcuts, which date about 1490. But an important peculiarity in these works is that they contain some indigenous species placed side by side with the old ones. In 1516, *The Grete Herbal* was published in England, also with woodcuts. It contains an account of more than four hundred vegetables, and their

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[20] Sprengel, i. 230. [21] Ib. i. 239. [22] Ib. i. 241. [23] Ib. ib.
[24] Augsburg, 1488. Mainz, 1491. Lubec, 1492.

products; of which one hundred and fifty are English, and are no way distinguished from the exotics by the mode in which they are inserted in the work.

We shall see, in the next chapter, that when the intellect of Europe began really to apply itself to the observation of nature, the progress towards genuine science soon began to be visible, in this as in other subjects; but before this tendency could operate freely, the history of botany was destined to show, in another instance, how much more grateful to man, even when roused to intelligence and activity, is the study of tradition than the study of nature. When the scholars of Europe had become acquainted with the genuine works of the ancients in the original languages, the pleasure and admiration which they felt, led them to the most zealous endeavors to illustrate and apply what they read. They fell into the error of supposing that the plants described by Theophrastus, Dioscorides, Pliny, must be those which grew in their own fields. And thus Ruellius,[25] a French physician, who only travelled in the environs of Paris and Picardy, imagined that he found there the plants of Italy and Greece. The originators of genuine botany in Germany, Brunfels and Tragus (Bock), committed the same mistake; and hence arose the misapplication of classical names to many genera. The labors of many other learned men took the same direction, of treating the ancient writers as if they alone were the sources of knowledge and truth.

But the philosophical spirit of Europe was already too vigorous to allow this superstitious erudition to exercise a lasting sway. Leonicenus, who taught at Ferrara till he was almost a hundred years old, and died in 1524,[26] disputed, with great freedom, the authority of the Arabian writers, and even of Pliny. He saw, and showed by many examples, how little Pliny himself knew of nature, and how many errors he had made or transmitted. The same independence of thought with regard to other ancient writers, was manifested by other scholars. Yet the power of ancient authority melted away but gradually. Thus Antonius Brassavola, who established on the banks of the Po the first botanical garden of modern times, published in 1536, his *Examen omnium Simplicium Medicamentorum;* and, as Cuvier says,[27] though he studied plants in nature, his book (written in the

[25] *De Natura Stirpium,* 1536.

[26] Sprengel, i. 252.

[27] *Hist. des Sc. Nat.* partie ii. 169.

Platonic form of dialogue), has still the character of a commentary on the ancients.

The Germans appear to have been the first to liberate themselves from this thraldom, and to publish works founded mainly on actual observation. The first of the botanists who had this great merit is Otho Brunfels of Mentz, whose work, *Herbarum Vivæ Icones*, appeared in 1530. It consists of two volumes in folio, with wood-cuts; and in 1532, a German edition was published. The plants which it contains are given without any arrangement, and thus he belongs to the period of unsystematic knowledge. Yet the progress towards the formation of a system manifested itself so immediately in the series of German botanists to which he belongs, that we might with almost equal propriety transfer him to the history of that progress; to which we now proceed.

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# CHAPTER III.

## Formation of a System of Arrangement of Plants.

### *Sect.* 1.—*Prelude to the Epoch of Cæsalpinus.*

THE arrangement of plants in the earliest works was either arbitrary, or according to their use, or some other extraneous circumstance, as in Pliny. This and the division of vegetables by Dioscorides into *aromatic*, *alimentary*, *medicinal*, *vinous*, is, as will be easily seen, a merely casual distribution. The Arabian writers, and those of the middle ages, showed still more clearly their insensibility to the nature of system, by adopting an alphabetical arrangement; which was employed also in the Herbals of the sixteenth century. Brunfels, as we have said, adopted no principle of order; nor did his successor, Fuchs. Yet the latter writer urged his countrymen to put aside their Arabian and barbarous Latin doctors, and to observe the vegetable kingdom for themselves; and he himself set the example of doing this, examined plants with zeal and accuracy, and made above fifteen hundred drawings of them.[1]

[1] His *Historia Stirpium* was published at Basil in 1542.

The difficulty of representing plants in any useful way by means of drawings, is greater, perhaps, than it at first appears. So long as no distinction was made of the importance of different organs of the plant, a picture representing merely the obvious general appearance and larger parts, was of comparatively small value. Hence we are not to wonder at the slighting manner in which Pliny speaks of such records. "Those who gave such pictures of plants," he says, "Crateuas, Dionysius, Metrodorus, have shown nothing clearly, except the difficulty of their undertaking. A picture may be mistaken, and is changed and disfigured by copyists; and, without these imperfections, it is not enough to represent the plant in one state, since it has four different aspects in the four seasons of the year."

The diffusion of the habit of exact drawing, especially among the countrymen of Albert Durer and Lucas Cranach, and the invention of wood-cuts and copper-plates, remedied some of these defects. Moreover, the conviction gradually arose in men's minds that the structure of the flower and the fruit are the most important circumstances in fixing the identity of the plant. Theophrastus speaks with precision of the organs which he describes, but these are principally the leaves, roots, and stems. Fuchs uses the term *apices* for the anthers, and *gluma* for the blossom of grasses, thus showing that he had noticed these parts as generally present.

In the next writer whom we have to mention, we find some traces of a perception of the real resemblances of plants beginning to appear. It is impossible to explain the progress of such views without assuming in the reader some acquaintance with plants; but a very few words may suffice to convey the requisite notions. Even in plants which most commonly come in our way, we may perceive instances of the resemblances of which we speak. Thus, Mint, Marjoram, Basil, Sage, Lavender, Thyme, Dead-nettle, and many other plants, have a tubular flower, of which the mouth is divided into two lips; hence they are formed into a family, and termed *Labiatæ*. Again, the Stock, the Wall-flower, the Mustard, the Cress, the Lady-smock, the Shepherd's-purse, have, among other similarities, their blossoms with four petals arranged crosswise; these are all of the order *Cruciferæ*. Other flowers, apparently more complex, still resemble each other, as Daisy, Marigold, Aster, and Chamomile; these belong to the order *Compositæ*. And though the members of each such family may differ widely in their larger parts, their stems and leaves, the close study of nature leads the botanist irresistibly to consider their resemblances as occupying a far more important place than their differences. It is the general establishment of this conviction and its consequences which we have now to follow.

The first writer in whom we find the traces of an arrangement depending upon these natural resemblances, is Hieronymus Tragus, (Jerom Bock,) a laborious German botanist, who, in 1551, published a herbal. In this work, several of the species included in those natural families to which we have alluded,[2] as for instance the Labiatæ, the Cruciferæ, the Compositæ, are for the most part brought together; and thus, although with many mistakes as to such connexions, a new principle of order is introduced into the subject.

In pursuing the development of such principles of natural order, it is necessary to recollect that the principles lead to an assemblage of divisions and groups, successively subordinate, the lower to the higher, like the brigades, regiments, and companies of an army, or the provinces, towns, and parishes of a kingdom. Species are included in Genera, Genera in Families or Orders, and orders in Classes. The perception that there is some connexion among the species of plants, was the first essential step; the detection of different marks and characters which should give, on the one hand, limited groups, on the other, comprehensive divisions, were other highly important parts of this advance. To point out every successive movement in this progress would be a task of extreme difficulty, but we may note, as the most prominent portions of it, the establishment of the groups which immediately include Species, that is, *the formation of Genera;* and the invention of a method which should distribute into consistent and distinct divisions the whole vegetable kingdom, that is, *the construction of a System.*

To the second of these two steps we have no difficulty in assigning its proper author. It belongs to Cæsalpinus, and marks the first great epoch of this science. It is less easy to state to what botanist is due the establishment of Genera; yet we may justly assign the greater part of the merit of this invention, as is usually done, to Conrad Gessner of Zurich. This eminent naturalist, after publishing his great work on animals, died[3] of the plague in 1565, at the age of forty-nine, while he was preparing to publish a History of Plants, a sequel to his History of Animals. The fate of the work thus left un-

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[2] Sprengel, i. 270.

[3] Cuvier, *Leçons sur l'Hist. des Sciences Naturelles,* partie ii p. 193.

finished was remarkable. It fell into the hands of his pupil, Gaspard Wolf, who was to have published it, but wanting leisure for the office, sold it to Joachim Camerarius, a physician and botanist of Nuremberg, who made use of the engravings prepared by Gessner, in an Epitome which he published in 1586. The text of Gessner's work, after passing through various hands, was published in 1754 under the title of *Gessneri Opera Botanica per duo Sœcula desiderata, &c.*, but is very incomplete.

The imperfect state in which Gessner left his botanical labors, makes it necessary to seek the evidence of his peculiar views in scattered passages of his correspondence and other works. One of his great merits was, that he saw the peculiar importance of the flower and fruit as affording the characters by which the affinities of plants were to be detected; and that he urged this view upon his contemporaries. His plates present to us, by the side of each plant, its flower and its fruit, carefully engraved. And in his communications with his botanical correspondents, he repeatedly insists on these parts. Thus[4] in 1565 he writes to Zuinger concerning some foreign plants which the latter possessed: "Tell me if your plants have fruit and flower, as well as stalk and leaves, for those are of much the greater consequence. By these three marks,—flower, fruit, and seed,—I find that Saxifraga and Consolida Regalis are related to Aconite." These characters, derived from the *fructification* (as the assemblage of flower and fruit is called), are the means by which genera are established, and hence, by the best botanists, Gessner is declared to be the inventor of genera.[5]

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[4] *Epistolæ*, fol. 113 a; see also fol. 65 b.

[5] Haller, *Biblio Botanica*, i. 284. Methodi Botanicæ rationem primus pervidit;—dari nempe et genera quæ plures species comprehenderent et classes quæ multa genera. Varias etiam classes naturales expressit. Characterem in flore inque semine posuit, &c.—*Rauwolfio Socio Epist.* Wolf, p. 39.

Linnæus, *Genera Plantarum*, Pref. xiii. "A fructificatione plantas distinguere in genera, infinitæ sapientiæ placuisse, detexit posterior ætas, et quidem primus, sæculi sui ornamentum, Conradus Gessnerus, uti patet ex Epistolis ejus postremis, et Tabulis per Carmerarium editis."

Cuvier says (*Hist. des Sc. Nat.* 2$^{e}$ p$^{e}$, p. 193), after speaking to the same effect, "Il fit voir encore que toutes les plantes qui ont des fleurs et des fruits semblables se ressemblent par leurs propriétés, et que quand on rapproche ces plantes on obtient ainsi une classification naturelle." I do not know if he here refers to any particular passages of Gessner's work.

The labors of Gessner in botany, both on account of the unfinished state in which he left the application of his principles, and on account of the absence of any principlés manifestly applicable to the whole extent of the vegetable kingdom, can only be considered as a prelude to the epoch in which those defects were supplied. To that epoch we now proceed.

### *Sect. 2.—Epoch of Cæsalpinus.—Formation of a System of Arrangement.*

If any one were disposed to question whether Natural History truly belongs to the domain of Inductive Science;—whether it is to be prosecuted by the same methods, and requires the same endowments of mind as those which lead to the successful cultivation of the Physical Sciences,—the circumstances under which Botany has made its advance appear fitted to remove such doubts. The first decided step in this study was merely the construction of a classification of its subjects. We shall, I trust, be able to show that such a classification includes, in reality, the establishment of one general principle, and leads to more. But without here dwelling on this point, it is worth notice that the person to whom we owe this classification, Andreas Cæsalpinus of Arezzo, was one of the most philosophical men of his time, profoundly skilled in the Aristotelian lore which was then esteemed, yet gifted with courage and sagacity which enabled him to weigh the value of the Peripatetic doctrines, to reject what seemed error, and to look onwards to a better philosophy. "How are we to understand," he inquires, "that we must proceed from universals to particulars (as Aristotle directs), when particulars are better known?"[6] Yet he treats the Master with deference, and, as has been observed,[7] we see in his great botanical work deep traces of the best features of the Aristotelian school, logic and method; and, indeed, in this work he frequently refers to his *Quæstiones Peripateticæ.* His book, entitled *De Plantis libri* xvi. appeared at Florence in 1583. The aspect under which his task presented itself to his mind appears to me to possess so much interest, that I will transcribe a few of his reflections. After speaking of the splendid multiplicity of the productions of nature, and the confusion which has hitherto prevailed among writers on plants,

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[6] *Quæstiones Peripateticæ,* (1569,) lib. i. quæst. i.

[7] Cuvier, p. 198.

the growing treasures of the botanical world; he adds,[8] "In this immense multitude of plants, I see that want which is most felt in any other unordered crowd: if such an assemblage be not arranged into brigades like an army, all must be tumult and fluctuation. And this accordingly happens in the treatment of plants: for the mind is overwhelmed by the confused accumulation of things, and thus arise endless mistake and angry altercation." He then states his general view, which, as we shall see, was adopted by his successors. "*Since all science consists in the collection of similar, and the distinction of dissimilar things*, and since the consequence of this is a distribution into genera and species, which are to be natural classes governed by real differences, I have attempted to execute this task in the whole range of plants;—ut si quid pro ingenii mei tenuitate in hujusmodi studio profecerim, ad communem utilitatem proferam." We see here how clearly he claims for himself the credit of being the first to execute this task of arrangement.

After certain preparatory speculations, he says,[9] "Let us now endeavor to mark the kinds of plants by essential circumstances in the fructification." He then observes, "In the constitution of organs three things are mainly important—the number, the position, the figure." And he then proceeds to exemplify this: "Some have under one flower, ONE *seed*, as *Amygdala*, or ONE seed-*receptacle*, as *Rosa;* or TWO *seeds*, as *Ferularia*, or TWO seed-*receptacles*, as *Nasturtium;* or three, as the *Tithymalum* kind have THREE *seeds*, the *Bulbaceæ* THREE *receptacles;* or four, as *Marrubium*, FOUR *seeds*, *Siler* FOUR *receptacles;* or more, as *Cicoraceæ*, and *Acanaceæ* have MORE *seeds*, *Pinus*, MORE *receptacles*."

It will be observed that we have here ten classes made out by means of number alone, added to the consideration of whether the seed is alone in its covering, as in a cherry, or contained in a receptacle with several others, as in a berry, pod, or capsule. Several of these divisions are, however, further subdivided according to other circumstances, and especially according as the vital part of the seed, which he called the heart (*cor*[10]), is situated in the upper or lower part of the seed. As our object here is only to indicate the principle of the method of Cæsalpinus, I need not further dwell on the details, and still less on the defects by which it is disfigured, as, for instance, the retention of the old distinction of Trees, Shrubs, and Herbs.

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[8] Dedicatio, a 2. [9] Lib. i. c. 13, 14. [10] *Corculum* of Linnæus.

To some persons it may appear that this arbitrary distribution of the vegetable kingdom, according to the number of parts of a particular kind, cannot deserve to be spoken of as a great discovery. And if, indeed, the distribution had been arbitrary, this would have been true; the real merit of this and of every other system is, that while it is artificial in its form, it is natural in its results. The plants which are associated by the arrangement of Cæsalpinus, are those which have the closest resemblances in the most essential points. Thus, as Linnæus says, though the first in attempting to form natural orders, he observed as many as the most successful of later writers. Thus his *Legumina*[11] correspond to the natural order *Leguminosæ;* his *genus Ferulaceum*[12] to the *Umbellatæ;* his *Bulbaceæ*[13] to *Liliaceæ;* his *Anthemides*[14] to the *Compositæ;* in like manner, the *Boragineæ* are brought together,[15] and the *Labiatæ.* That such assemblages are produced by the application of his principles, is a sufficient evidence that they have their foundation in the general laws of the vegetable world. If this had not been the case, the mere application of number or figure alone as a standard of arrangement, would have produced only intolerable anomalies. If, for instance, Cæsalpinus had arranged plants by the number of flowers on the same stalk, he would have separated individuals of the same species; if he had distributed them according to the number of leaflets which compose the leaves, he would have had to place far asunder different species of the same genus. Or, as he himself says,[16] "If we make one genus of those which have a round root, as Rapum, Aristolochia, Cyclaminus, Aton, we shall separate from this genus those which most agree with it, as Napum and Raphanum, which resemble Rapum, and the long Aristolochia, which resembles the round; while we shall join the most remote kinds, for the nature of Cyclaminus and Rapum is altogether diverse in all other respects. Or if we attend to the differences of stalk, so as to make one genus of those which have a naked stalk, as the Junci, Cæpe, Aphacæ, along with Cicoraceæ, Violæ, we shall still connect the most unlike things, and disjoin the closest affinities. And if we note the differences of leaves, or even flowers, we fall into the same difficulty; for many plants very different in kind have leaves very similar, as Polygonum and Hypericum, Ernea and Sesamois, Apium and Ranunculus; and plants of the same genus have sometimes very different

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[11] Lib. vi. [12] Lib. vii. [13] Lib. x. [14] Lib. xii.
[15] Lib. xi. [16] Lib. i. cap. xii. p. 25.

leaves, as the several species of Ranunculus and of Lactuca. Nor will color or shape of the flowers help us better; for what has Vitis in common with Œnanthe, except the resemblance of the flower?" He then goes on to say, that if we seek a too close coincidence of all the characters we shall have no Species; and thus shows us that he had clearly before his view the difficulty which he had to attack, and which it is his glory to have overcome, that of constructing Natural Orders.

But as the principles of Cæsalpinus are justified, on the one hand, by their leading to *Natural Orders*, they are recommended on the other by their producing a *System* which applies through the whole extent of the vegetable kingdom. The parts from which he takes his characters must occur in all flowering-plants, for all such plants have seeds. And these seeds, if not very numerous for each flower, will be of a certain definite number and orderly distribution. And thus every plant will fall into one part or other of the same system.

It is not difficult to point out, in this induction of Cæsalpinus, the two elements which we have so often declared must occur in all inductive processes; the exact acquaintance with *facts*, and the general and applicable *ideas* by which these facts are brought together. Cæsalpinus was no mere dealer in intellectual relations or learned traditions, but a laborious and persevering collector of plants and of botanical knowledge. "For many years," he says in his Dedication, "I have been pursuing my researches in various regions, habitually visiting the places in which grew the various kinds of herbs, shrubs, and trees; I have been assisted by the labors of many friends, and by gardens established for the public benefit, and containing foreign plants collected from the most remote regions." He here refers to the first garden directed to the public study of Botany, which was that of Pisa,[17] instituted in 1543, by order of the Grand Duke Cosmo the First. The management of it was confided first to Lucas Ghini, and afterwards to Cæsalpinus. He had collected also a herbarium of dried plants, which he calls the rudiment of his work. "Tibi enim," he says, in his dedication to Francis Medici, Grand Duke of Etruria, "apud quem extat ejus rudimentum ex plantis libro agglutinatis a me compositum." And, throughout, he speaks with the most familiar and vivid acquaintance of the various vegetables which he describes.

But Cæsalpinus also possessed fixed and general views concerning the relation and functions of the parts of plants, and ideas of symmetry

[17] Cuv. 187.

and system; without which, as we see in other botanists of his and succeeding times, the mere accumulation of a knowledge of details does not lead to any advance in science. We have already mentioned his reference to general philosophical principles, both of the Peripatetics and of his own. The first twelve chapters of his work are employed in explaining the general structure of plants, and especially that point to which he justly attaches so much importance, the results of the different situation of the *cor* or *corculum* of the seed. He shows[18] that if we take the root, or stem, or leaves, or blossom, as our guide in classification, we shall separate plants obviously alike, and approximate those which have merely superficial resemblances. And thus we see that he had in his mind ideas of fixed resemblance and symmetrical distribution, which he sedulously endeavored to apply to plants; while his acquaintance with the vegetable kingdom enabled him to see in what manner these ideas were not, and in what manner they were, really applicable.

The great merit and originality of Cæsalpinus have been generally allowed, by the best of the more modern writers on Botany. Linnæus calls him one of the founders of the science; "Primus verus systematicus;"[19] and, as if not satisfied with the expression of his admiration in prose, hangs a poetical garland on the tomb of his hero. The following distich concludes his remarks on this writer:

> Quisquis hic extiterit primos concedet honores
> Cæsalpine tibi; primaque serta dabit:

and similar language of praise has been applied to him by the best botanists up to Cuvier,[20] who justly terms his book "a work of genius."

Perhaps the great advance made in this science by Cæsalpinus, is most strongly shown by this; that no one appeared, to follow the path which he had opened to system and symmetry, for nearly a century. Moreover, when the progress of this branch of knowledge was resumed, his next successor, Morison, did not choose to acknowledge that he had borrowed so much from so old a writer; and thus, hardly mentions his name, although he takes advantage of his labors, and even transcribes his words without acknowledgement, as I shall show. The pause between the great invention of Cæsalpinus, and its natural sequel, the developement and improvement of his method, is so marked, that I

[18] Lib. i. cap. xii. [19] *Philosoph. Bot.* p. 19. [20] Cuv. *Hist.* 193.

will, in order to avoid too great an interruption of chronological order, record some of its circumstances in a separate section.

### *Sect. 3.—Stationary Interval.*

The method of Cæsalpinus was not, at first, generally adopted. It had, indeed, some disadvantages. Employed in drawing the boundary-lines of the larger divisions of the vegetable kingdom, he had omitted those smaller groups, Genera, which were both most obvious to common botanists, and most convenient in the description and comparison of plants. He had also neglected to give the Synonyms of other authors for the plants spoken of by him; an appendage to botanical descriptions, which the increase of botanical information and botanical books had now rendered indispensable. And thus it happened, that a work, which must always be considered as forming a great epoch in the science to which it refers, was probably little read, and in a short time could be treated as if it were quite forgotten.

In the mean time, the science was gradually improved in its details. Clusius, or Charles de l'Ecluse, first taught botanists to describe well. "Before him," says Mirbel,[21] "the descriptions were diffuse, obscure, indistinct; or else concise, incomplete, vague. Clusius introduced exactitude, precision, neatness, elegance, method: he says nothing superfluous; he omits nothing necessary." He travelled over great part of Europe, and published various works on the more rare of the plants which he had seen. Among such plants, we may note now one well known, the potato; which he describes as being commonly used in Italy in 1586;[22] thus throwing doubt, at least, on the opinion which ascribes the first introduction of it into Europe to Sir Walter Raleigh, on his return from Virginia, about the same period. As serving to illustrate, both this point, and the descriptive style of Clusius, I quote, in a note, his description of the flower of this plant.[23]

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[21] *Physiol. Veg.* p. 525.

[22] Clusius. *Exotic.* iv. c. 52, p. lxxix.

[23] "Papas Peruanorum. Arachidna, Theoph. forte. Flores elegantes, uncialis amplitudinis aut majores, angulosi, singulari folio constantes, sed ita complicato ut quinque folia discreta videantur, coloris exterius ex purpura candicantis, interius purpurascentis, radiis quinque herbaceis ex umbilico stellæ instar prodeuntibus, et totidem staminibus flavis in umbonem coeuntibus."

He says that the Italians do not know whence they had the plant, and that they call it *Taratoufli*. The name *Potato* was, in England, previously applied to the Sweet Potato (*Convolvulus batatas*), which was the *common* Potato, in

The addition of exotic species to the number of known plants was indeed going on rapidly during the interval which we are now considering. Francis Hernandez, a Spaniard, who visited America towards the end of the sixteenth century, collected and described many plants of that country, some of which were afterwards published by Recchi.[24] Barnabas Cobo, who went as a missionary to America in 1596, also described plants.[25] The Dutch, among other exertions which they made in their struggle with the tyranny of Spain, sent out an expedition which, for a time, conquered the Brazils; and among other fruits of this conquest, they published an account of the natural history of the country.[26] To avoid interrupting the connexion of such labors, I will here carry them on a little further in the order of time. Paul Herman, of Halle, in Saxony, went to the Cape of Good Hope and to Ceylon; and on his return, astonished the botanists of Europe by the vast quantity of remarkable plants which he introduced to their knowledge.[27] Rheede, the Dutch governor of Malabar, ordered descriptions and drawings to be made of many curious species, which were published in a large work in twelve folio volumes.[28] Rumphe, another Dutch consul at Amboyna,[29] labored with zeal and success upon the plants of the Moluccas. Some species which occur in Madagascar figured in a description of that island composed by the French Commandant Flacourt.[30] Shortly afterwards, Engelbert Kæmpfer,[31] a Westphalian of great acquirements and undaunted courage, visited Persia, Arabia Felix, the Mogul Empire, Ceylon, Bengal, Sumatra, Java, Siam, Japan; Wheler travelled in Greece and Asia Minor; and Sherard, the English consul, published an account of the plants of the neighborhood of Smyrna.

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distinction to the *Virginian* Potato, at the time of Gerard's Herbal. (1597?) Gerard's figures of both plants are copied from those of Clusius.

It may be seen by the description of Arachidna, already quoted from Theophrastus, (above,) that there is little plausibility in Clusius's conjecture of the plant being known to the ancients. I need not inform the botanist that this opinion is untenable.

[24] *Nova Plantarum Regni Mexicana Historia*, Rom. 1651, fol.

[25] Sprengel, *Gesch. der Botanik*, ii. 62.

[26] *Historia Naturalis Brasiliæ*, L. B. 1648, fol. (Piso and Marcgraf).

[27] *Museum Zeylanicum*, L. B. 1726.

[28] *Hortus Malabaricus*, 1670–1703.

[29] *Herbarium Amboinense*, Amsterdam, 1741–51, fol.

[30] *Histoire de la grande Isle Madagascar*, Paris, 1661.

[31] *Amœnitates Exoticæ*, Lemgov. 1712. 4to.

At the same time, the New World excited also the curiosity of botanists. Hans Sloane collected the plants of Jamaica; John Banister those of Virginia; William Vernon, also an Englishman, and David Kriege, a Saxon, those of Maryland; two Frenchmen, Surian and Father Plumier, those of Saint Domingo.

We may add that public botanical gardens were about this time established all over Europe. We have already noticed the institution of that of Pisa in 1543; the second was that of Padua in 1545; the next, that of Florence in 1556; the fourth, that of Bologna, 1568; that of Rome, in the Vatican, dates also from 1568.

The first transalpine garden of this kind arose at Leyden in 1577; that of Leipzig in 1580. Henry the Fourth of France established one at Montpellier in 1597. Several others were instituted in Germany; but that of Paris did not begin to exist till 1626; that of Upsal, afterwards so celebrated, took its rise in 1657, that of Amsterdam in 1684. Morison, whom we shall soon have to mention, calls himself, in 1680, the first Director of the Botanical Garden at Oxford.

[2nd Ed.] [To what is above said of Botanical Gardens and Botanical Writers, between the times of Cæsalpinus and Morison, I may add a few circumstances. The first academical garden in France was that at Montpellier, which was established by Peter Richier de Belleval, at the end of the sixteenth century. About the same period, rare flowers were cultivated at Paris, and pictures of them made, in order to supply the embroiderers of the court-robes with new patterns. Thus figures of the most beautiful flowers in the garden of Peter Robins were published by the court-embroiderer Peter Vallet, in 1608, under the title of *Le Jardin du Roi Henry IV.* But Robins' works were of great service to botany; and his garden assisted the studies of Renealmus (Paul Reneaulme), whose *Specimen Historiæ Plantarum* (Paris, 1611), is highly spoken of by the best botanists. Recently, Mr. Robert Brown has named after him a new genus of *Irideæ* (RENEALMIA); adding, "Dixi in memoriam PAULI RENEALMI, botanici sui ævi accuratissimi, atque staminum primi scrutatoris; qui non modo eorum numerum et situm, sed etiam filamentorum proportionem passim descripsit, et characterem tetradynamicum siliquosarum perspexit." (*Prodromus Floræ Novæ Hollandiæ*, p. 448.)

The oldest Botanical Garden in England is that at Hampton Court, founded by Queen Elizabeth, and much enriched by Charles II. and William III. (Sprengel, *Gesch. d. Bot.* vol. ii. p. 96.)]

In the mean time, although there appeared no new system which commanded the attention of the botanical world, the feeling of the importance of the affinities of plants became continually more strong and distinct.

Lobel, who was botanist to James the First, and who published his *Stirpium Adversaria Nova* in 1571, brings together the natural families of plants more distinctly than his predecessors, and even distinguishes (as Cuvier states,[32]) monocotyledonous from dicotyledonous plants; one of the most comprehensive division-lines of botany, of which succeeding times discovered the value more completely. Fabius Columna,[33] in 1616, gave figures of the fructification of plants on copper, as Gessner had before done on wood. But the elder Bauhin (John), notwithstanding all that Cæsalpinus had done, retrograded, in a work published in 1619, into the less precise and scientific distinctions of—trees with nuts; with berries; with acorns; with pods; creeping plants, gourds, &c.: and no clear progress towards a system was anywhere visible among the authors of this period.

While this continued to be the case, and while the materials, thus destitute of order, went on accumulating, it was inevitable that the evils which Cæsalpinus had endeavored to remedy, should become more and more grievous. "The nomenclature of the subject[34] was in such disorder, it was so impossible to determine with certainty the plants spoken of by preceding writers, that thirty or forty different botanists had given to the same plant almost as many different names. Bauhin called by one appellation, a species which Lobel or Matheoli designated by another. There was an actual chaos, a universal confusion, in which it was impossible for men to find their way." We can the better understand such a state of things, from having, in our own time, seen another classificatory science, Mineralogy, in the very condition thus described. For such a state of confusion there is no remedy but the establishment of a true system of classification; which by its real foundation renders a reason for the place of each species; and which, by the fixity of its classes, affords a basis for a standard nomenclature, as finally took place in Botany. But before such a remedy is obtained, men naturally try to alleviate the evil by tabulating the synonyms of different writers, as far as they are able to do so. The task of constructing such a *Synonymy* of botany at the period of which we speak, was undertaken by Gaspard Bauhin, the brother of John, but nineteen years younger. This work, the *Pinax Theatri Botanici*, was printed

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[32] Cuv. *Leçons, &c.* 198. [33] Ib. 206. [34] Ib. 212.

at Basil in 1623. It was a useful undertaking at the time; but the want of any genuine order in the *Pinax* itself, rendered it impossible that it should be of great permanent utility.

After this period, the progress of almost all the sciences became languid for a while; and one reason of this interruption was, the wars and troubles which prevailed over almost the whole of Europe. The quarrels of Charles the First and his parliament, the civil wars and the usurpation, in England; in France, the war of the League, the stormy reign of Henry the Fourth, the civil wars of the minority of Louis the Thirteenth, the war against the Protestants and the war of the Fronde in the minority of Louis the Fourteenth; the bloody and destructive Thirty Years' War in Germany; the war of Spain with the United Provinces and with Portugal;—all these dire agitations left men neither leisure nor disposition to direct their best thoughts to the promotion of science. The baser spirits were brutalized; the better were occupied by high practical aims and struggles of their moral nature. Amid such storms, the intellectual powers of man could not work with their due calmness, nor his intellectual objects shine with their proper lustre.

At length a period of greater tranquillity gleamed forth, and the sciences soon expanded in the sunshine. Botany was not inert amid this activity, and rapidly advanced in a new direction, that of physiology; but before we speak of this portion of our subject, we must complete what we have to say of it as a classificatory science.

### *Sect.* 4.—*Sequel to the Epoch of Cæsalpinus. Further Formation and Adoption of Systematic Arrangement.*

Soon after the period of which we now speak, that of the restoration of the Stuarts to the throne of England, systematic arrangements of plants appeared in great numbers; and in a manner such as to show that the minds of botanists had gradually been ripening for this improvement, through the influence of preceding writers, and the growing acquaintance with plants. The person whose name is usually placed first on this list, Robert Morison, appears to me to be much less meritorious than many of those who published very shortly after him; but I will give him the precedence in my narrative He was a Scotchman, who was wounded fighting on the royalist side in the civil wars of England. On the triumph of the republicans, he withdrew to France, when he became director of the garden of Gaston, Duke of Orléans at Blois; and there he came under the notice of our Charles the Second; who, on his restoration, summoned Morison to England, where he became Superintendent of the Royal Gardens, and also of the Botanic Garden at Oxford. In 1669, he published *Remarks on the Mistakes of the two Bauhins*, in which he proves that many plants in the *Pinax* are erroneously placed, and shows considerable talent for appreciating natural families and genera. His great systematic work appeared from the University press at Oxford in 1680. It contains a system, but a system, Cuvier says,[35] which approaches rather to a natural method than to a rigorous distribution, like that of his predecessor Cæsalpinus, or that of his successor Ray. Thus the herbaceous plants are divided into *climbers*, *leguminous*, *siliquose*, *unicapsalar*, *bicapsular*, *tricapsular*, *quadricapsular*, *quinquecapsular;* this division being combined with characters derived from the number of petals. But along with these numerical elements, are introduced others of a loose and heterogeneous kind, for instance, the classification of herbs as *lactescent* and *emollient*. It is not unreasonable to say, that such a scheme shows no talent for constructing a complete system; and that the most distinct part of it, that dependent on the fruit, was probably borrowed from Cæsalpinus. That this is so, we have, I think, strong proof; for though Morison nowhere, I believe, mentions Cæsalpinus, except in one place in a loose enumeration of botanical writers,[36] he must have made considerable use of his work. For he has introduced into his own preface a passage copied literally[37] from the dedication of Cæsalpinus; which passage we have already quoted (p. 374,) beginning, "Since all science consists in the collection of similar, and the distinction of dissimilar things." And that the mention of the original is not omitted by accident, appears from this; that Morison appropriates also the conclusion of the passage, which has a personal reference, "*Conatus sum id præstare in universa plantarum historia, ut si quid pro ingenii mei tenuitate in hujusmodi studio profecerim, ad communem utilitatem proferrem.*" That Morison, thus, at so long an interval after the publication of the work of Cæsalpinus, borrowed from him without acknowledgement, and adopted his system so as to mutilate it, proves that he had neither the temper nor the talent of a discoverer; and justifies us withholding from him the credit which belongs to those, who, in his time, resumed the great undertaking of constructing a vegetable system.

Among those whose efforts in this way had the greatest and earliest

[35] Cuv. *Leçons*, &c. p. 486. [36] Pref. p. i. [37] Ib. p. ii.

influence, was undoubtedly our countryman, John Ray, who was Fellow of Trinity College, Cambridge, at the same time with Isaac Newton. But though Cuvier states[38] that Ray was the model of the systematists during the whole of the eighteenth century, the Germans claim a part of his merit for one of their countrymen, Joachim Jung, of Lubeck, professor at Hamburg.[39] Concerning the principles of this botanist, little was known during his life. But a manuscript of his book was communicated[40] to Ray in 1660, and from this time forwards, says Sprengel, there might be noticed in the writings of Englishmen, those better and clearer views to which Jung's principles gave birth. Five years after the death of Jung, his *Doxoscopia Physica* was published, in 1662; and in 1678, his *Isagoge Phytoscopica*. But neither of these works was ever much read; and even Linnæus, whom few things escaped which concerned botany, had, in 1771, seen none of Jung's works.

I here pass over Jung's improvements of botanical language, and speak only of those which he is asserted to have suggested in the arrangement of plants. He examines, says Sprengel,[41] the value of characters of species, which, he holds, must not be taken from the thorns, nor from color, taste, smell, medicinal effects, time and place of blossoming. He shows, in numerous examples, what plants must be separated, though called by a common name, and what must be united, though their names are several.

I do not see in this much that interferes with the originality of Ray's method,[42] of which, in consequence of the importance ascribed to it by Cuvier, as we have already seen, I shall give an account, following that great naturalist.[43] I confine myself to the ordinary plants, and omit the more obscure vegetables, as mushrooms, mosses, ferns, and the like.

Such plants are *composite* or *simple*. The *composite* flowers are those which contain many florets in the same *calyx*.[44] These are subdivided according as they are composed altogether of complete florets,

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[38] *Leçons Hist. Sc.* p. 487.

[39] Sprengel, ii. 27.

[40] Ray acknowledges this in his *Index Plant. Agri Cantab.* p. 87, and quotes from it the definition of *caulis*.

[41] Sprengel, ii. 29.

[42] *Methodus Plantarum Nova*, 1682. *Historia Plantarum*, 1686.

[43] Cuv. *Leçons Hist. Sc. Nat.* 488.

[44] *Involucrum*, in modern terminology.

or of half florets, or of a centre of complete florets, surrounded by a circumference or ray of demi-florets. Such are the divisions of the *corymbiferæ*, or *compositæ*.

In the *simple* flowers, the seeds are *naked*, or in a *pericarp*. Those with *naked* seeds are arranged according to the number of the seeds, which may be one, two, three, four, or more. If there is only one, no subdivision is requisite: if there are two, Ray makes a subdivision, according as the flower has five petals, or a continuous corolla. Here we come to several natural families. Thus, the flowers with two seeds and five petals are the *Umbelliferous* plants; the monopetalous flowers with two seeds are the *Stellatæ*. He founds the division of four-seeded flowers on the circumstance of the leaves being opposite, or alternate; and thus again, we have the natural families of *Asperifoliæ*, as *Echium*, &c., which have the leaves alternate, and the *Verticillatæ*, as *Salvia*, in which the leaves are opposite. When the flower has more than four seeds, he makes no subdivision.

So much for simple flowers with naked seeds. In those where the seeds are surrounded by a *pericarp*, or fruit, this fruit is large, soft, and fleshy, and the plants are *pomiferous;* or it is small and juicy, and the fruit is a berry, as a Gooseberry.

If the fruit is not juicy, but *dry*, it is multiple or simple. If it be simple, we have the *leguminose* plants. If it be multiple, the form of the flower is to be attended to. The flower may be *monopetalous*, or *tetrapetalous*, or *pentapetalous*, or with still *more* divisions. The monopetalous may be *regular* or *irregular;* so may the tetrapetalous. The regular tetrapetalous flowers are, for example, the *Cruciferæ*, as Stock and Cauliflower; the irregular, are the *papilionaceous* plants, Peas, Beans, and Vetches; and thus we again come to natural families. The remaining plants are divided in the same way, into those with *imperfect*, and those with *perfect*, flowers. Those with *imperfect* flowers are the *Grasses*, the *Rushes* (*Junci*), and the like; among those with *perfect* flowers, are the *Palmaceæ*, and the *Liliaceæ*.

We see that the division of plants is complete as a system; all flowers must belong to one or other of the divisions. Fully to explain the characters and further subdivisions of these families, would be to write a treatise on botany; but it is easily seen that they exhaust the subject as far as they go.

Thus Ray constructed his system partly on the fruit and partly on the flower; or more properly, according to the expression of Linnæus.

comparing his earlier with his later system, he began by being a *fructicist*, and ended by being a *corollist*.[45]

As we have said, a number of systems of arrangement of plants were published about this time, some founded on the fruit, some on the corolla, some on the calyx, and these employed in various ways. Rivinus[46] (whose real name was Bachman,) classified by the flower alone; instead of combining it with the fruit, as Ray had done.[47] He had the further merit of being the first who rejected the old division, of *woody* and *herbaceous* plants; a division which, though at variance with any system founded upon the structure of the plant, was employed even by Tournefort, and only finally expelled by Linnæus.

It would throw little light upon the history of botany, especially for our purpose, to dwell on the peculiarities of these transitory systems. Linnæus,[48] after his manner, has given a classification of them. Rivinus, as we have just seen, was a *corollist*, according to the regularity and number of the petals; Hermann was a *fructicist*. Christopher Knaut[49] adopted the system of Ray, but inverted the order of its parts; Christian Knaut did nearly the same with regard to that of Rivinus, taking number before regularity in the flower.[50]

Of the systems which prevailed previous to that of Linnæus, Tournefort's was by far the most generally accepted. Joseph Pitton de Tournefort was of a noble family in Provence, and was appointed professor at the Jardin du Roi in 1683. His well-known travels in the Levant are interesting on other subjects, as well as botany. His *Institutio Rei Herbariæ*, published in 1700, contains his method, which is that of a *corollist*. He is guided by the regularity or irregularity of the flowers, by their form, and by the situation of the receptacle of the seeds below the calyx, or within it. Thus his classes are —those in which the flowers are *campaniform*, or bell-shaped; those in which they are *infundibuliform*, or funnel-shaped, as Tobacco; then the irregular flowers, as the *Personatæ*, which resemble an ancient mask; the *Labiatæ*, with their two lips; the *Cruciform;* the *Rosaceæ*, with flowers like a rose; the *Umbelliferæ;* the *Caryophylleæ*, as the

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[45] Ray was a most industrious herbalizer, and I cannot understand on what ground Mirbel asserts (*Physiol. Veg.*, tom. ii. p. 531,) that he was better acquainted with books than with plants.

[46] Cuv. *Leçons*, 491.

[47] *Historia Generalis ad rem Herbariam*, 1690.

[48] *Philos. Bot.* p. 21. [49] *Enumeratio Plantarum*, &c., 1687. [50] Linn.

Pink; the *Liliaceæ*, with six petals, as the Tulip, Narcissus, Hyacinth, Lily; the *Papilionaceæ*, which are leguminous plants, the flower of which resembles a butterfly, as Peas and Beans; and finally, the *Anomalous*, as Violet, Nasturtium, and others.

Though this system was found to be attractive, as depending, in an evident way, on the most conspicuous part of the plant, the flower, it is easy to see that it was much less definite than systems like that of Rivinus, Hermann, and Ray, which were governed by number. But Tournefort succeeded in giving to the characters of genera a degree of rigor never before attained, and abstracted them in a separate form. We have already seen that the reception of botanical Systems has depended much on their arrangement into Genera.

Tournefort's success was also much promoted by the author inserting in his work a figure of a flower and fruit belonging to each genus; and the figures, drawn by Aubriet, were of great merit. The study of botany was thus rendered easy, for it could be learned by turning over the leaves of a book. In spite of various defects, these advantages gave this writer an ascendancy which lasted, from 1700, when his book appeared, for more than half a century. For though Linnæus began to publish in 1735, his method and his nomenclature were not generally adopted till 1760.

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## CHAPTER IV.

### The Reform of Linnæus.

#### *Sect.* 1.—*Introduction of the Reform.*

ALTHOUGH, perhaps, no man of science ever exercised a greater sway than Linnæus, or had more enthusiastic admirers, the most intelligent botanists always speak of him, not as a great discoverer, but as a judicious and strenuous *Reformer*. Indeed, in his own lists of botanical writers, he places himself among the "Reformatores;" and it is apparent that this is the nature of his real claim to admiration; for the doctrine of the sexes of plants, even if he had been the first to establish it, was a point of botanical physiology, a province of the science which no one would select as the peculiar field of Linnæus's glory; and the formation of a system of arrangement on the basis of this doctrine, though attended with many advantages, was not an improvement of any higher order than those introduced by Ray and Tournefort. But as a Reformer of the state of Natural History in his time, Linnæus was admirable for his skill, and unparaleled in his success. And we have already seen, in the instance of the reform of mineralogy, as attempted by Mohs and Berzelius, that men of great talents and knowledge may fail in such an undertaking.

It is, however, only by means of the knowledge which he displays, and of the beauty and convenience of the improvements which he proposes, that any one can acquire such an influence as to procure his snggestions to be adopted. And even if original circumstances of birth or position could invest any one with peculiar prerogatives and powers in the republic of science, Karl Linné began his career with no such advantages. His father was a poor curate in Smaland, a province of Sweden; his boyhood was spent in poverty and privation; it was with great difficulty that, at the age of twenty-one, he contrived to subsist at the University of Upsal, whither a strong passion for natural history had urged him. Here, however, he was so far fortunate, that Olaus Rudbeck, the professor of botany, committed to him the care of the Botanic Garden.[1] The perusal of the works of Vaillant and Patrick Blair suggested to him the idea of an arrangement of plants, formed upon the sexual organs, the stamens and pistils; and of such an arrangement he published a sketch in 1731, at the age of twenty-four.

But we must go forwards a few years in his life, to come to the period to which his most important works belong. University and family quarrels induced him to travel; and, after various changes of scene, he was settled in Holland, as the curator of the splendid botanical garden of George Clifford, an opulent banker. Here it was[2] that he laid the foundation of his future greatness. In the two years of his residence at Harlecamp, he published nine works. The first, the *Systema Naturæ*, which contained a comprehensive sketch of the whole domain of Natural History, excited general astonishment, by the acuteness of the observations, the happy talent of combination, and the clearness of the systematic views. Such a work could not fail to procure considerable respect for its author. His *Hortus Cliffortiana*

[1] Sprengel, ii. 232.

[2] Ibid. 234.

and *Musa Cliffortiana* added to this impression. The weight which he had thus acquired, he proceeded to use for the improvement of botany. His *Fundamenta Botanica* and *Bibliotheca Botanica* appeared in 1736; his *Critica Botanica* and *Genera Plantarum* in 1737; his *Classes Plantarum* in 1738; his *Species Plantarum* was not published till 1753; and all these works appeared in many successive editions, materially modified.

This circulation of his works showed that his labors were producing their effect. His reputation grew; and he was soon enabled to exert a personal, as well as a literary, influence, on students of natural history. He became Botanist Royal, President of the Academy of Sciences at Stockholm, and Professor in the University of Upsal; and this office he held for thirty-six years with unrivalled credit; exercising, by means of his lectures, his constant publications, and his conversation, an extraordinary power over a multitude of zealous naturalists, belonging to every part of the world.

In order to understand more clearly the nature and effect of the reforms introduced by Linnæus into botany, I shall consider them under the four following heads;—*Terminology*, *Nomenclature*, *Artificial System*, and *Natural System*.

### *Sect. 2.—Linnæan Reform of Botanical Terminology.*

It must be recollected that I designate as *Terminology*, the system of *terms* employed in the *description* of objects of natural history; while by *Nomenclature*, I mean the collection of the *names* of *species*. The reform of the descriptive part of botany was one of the tasks first attempted by Linnæus; and his terminology was the instrument by which his other improvements were effected.

Though most readers, probably, entertain, at first, a persuasion that a writer ought to content himself with the use of common words in their common sense, and feel a repugnance to technical terms and arbitrary rules of phraseology, as pedantic and troublesome; it is soon found, by the student of any branch of science that, without technical terms and fixed rules, there can be no certain or progressive knowledge. The loose and infantine grasp of common language cannot hold objects steadily enough for scientific examination, or lift them from one stage of generalization to another. They must be secured by the rigid mechanism of a scientific phraseology. This necessity had been felt in all the sciences, from the earliest periods of their progress. But the conviction had never been acted upon so as to produce a distinct and adequate descriptive botanical language. Jung, indeed,[3] had already attempted to give rules and precepts which should answer this purpose; but it was not till the *Fundamenta Botanica* appeared, that the science could be said to possess a fixed and complete terminology.

To give an account of such a terminology, is, in fact, to give a description of a dictionary and grammar, and is therefore what cannot here be done in detail. Linnæus's work contains about a thousand terms of which the meaning and application are distinctly explained; and rules are given, by which, in the use of such terms, the botanist may avoid all obscurity, ambiguity, unnecessary prolixity and complexity, and even inelegance and barbarism. Of course the greater part of the words which Linnæus thus recognized had previously existed in botanical writers; and many of them had been defined with technical precision. Thus Jung[4] had already explained what was a *composite*, what a *pinnate* leaf; what kind of a bunch of flowers is a *spike*, a *panicle*, an *umbel*, a *corymb*, respectively. Linnæus extended such distinctions, retaining complete clearness in their separation. Thus, with him, composite leaves are further distinguished as *digitate*, *pinnate*, *bipinnate*, *pedate*, and so on; pinnate leaves are *abruptly* so, or *with an odd* one, or *with a tendril;* they are pinnate *oppositely*, *alternately*, *interruptedly*, *articulately*, *decursively*. Again, the *inflorescence*, as the mode of assemblage of the flowers is called, may be a *tuft* (fasciculus), a *head* (capitulum), a *cluster* (racemus), a *bunch* (thyrsus), a *panicle*, a *spike*, a *catkin* (amentum), a *corymb*, an *umbel*, a *cyme*, a *whorl* (verticillus). And the rules which he gives, though often apparently arbitrary and needless, are found, in practice, to be of great service by their fixity and connexion. By the good fortune of having had a teacher with so much delicacy of taste as Linnæus, in a situation of so much influence, Botany possesses a descriptive language which will long stand as a model for all other subjects.

It may, perhaps, appear to some persons, that such a terminology as we have here described must be enormously cumbrous; and that, since the terms are arbitrarily invested with their meaning, the invention of them requires no knowledge of nature. With respect to the former doubt, we may observe, that technical description is, in reality, the only description which is clearly intelligible; but that technical language cannot be understood without being learnt as any other lan-

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[3] *Isagoge Phytoscopica*, 1679. [4] Sprengel, ii. 28.

guage is learnt; that is, the reader must connect the terms immediately with his own sensations and notions, and not mediately, through a verbal explanation; he must not have to guess their meaning, or to discover it by a separate act of interpretation into more familiar language as often as they occur. The language of botany must be the botanist's most familiar tongue. When the student has thus learnt to *think* in botanical language, it is no idle distinction to tell him that a *bunch* of grapes is not a *cluster;* that is, a *thyrsus* not a *raceme.* And the terminology of botany is then felt to be a useful implement, not an oppressive burden. It is only the schoolboy that complains of the irksomeness of his grammar and vocabulary. The accomplished student possesses them without effort or inconvenience.

As to the other question, whether the construction of such a botanical grammar and vocabulary implies an extensive and accurate acquaintance with the facts of nature, no one can doubt who is familiar with any descriptive science. It is true, that a person might construct an arbitrary scheme of distinctions and appellations, with no attention to natural objects; and this is what shallow and self-confident persons often set about doing, in some branch of knowledge with which they are imperfectly acquainted. But the slightest attempt to use such a phraseology leads to confusion; and any continued use of it leads to its demolition. Like a garment which does not fit us, if we attempt to work in it we tear it in pieces.

The formation of a good descriptive language is, in fact, an inductive process of the same kind as those which we have already noticed in the progress of natural history. It requires the *discovery of fixed characters*, which discovery is to be marked and fixed, like other inductive steps, by appropriate *technical terms.* The characters must be so far fixed, that the things which they connect must have a more permanent and real association than the things which they leave unconnected. If one bunch of grapes were really a racemus, and another a thyrsus, according to the definition of these terms, this part of the Linnæan language would lose its value; because it would no longer enable us to assert a general proposition with respect to one kind of plants.

### *Sect.* 3.—*Linnæan Reform of Botanical Nomenclature.*

In the ancient writers each recognized kind of plants had a distinct name. The establishment of Genera led to the practice of designating

Species by the name of the genus, with the addition of a "phrase" to distinguish the species. These phrases, (expressed in Latin in the ablative case,) were such as not only to mark, but to describe the species, and were intended to contain such features of the plant as were sufficient to distinguish it from others of the same genus. But in this way the designation of a plant often became a long and inconvenient assemblage of words. Thus different kinds of Rose were described as,

Rosa campestris, spinis carens, biflora (*Rosa alpina.*)
Rosa aculeata, foliis odoratis subtus rubiginosis (*R. eglanteria.*)
Rosa carolina fragrans, foliis medio tenus serratis (*R. carolina.*)
Rosa sylvestris vulgaris, flore odorato incarnato (*R. canina.*)

And several others. The prolixity of these appellations, their variety in every different author, the insufficiency and confusion of the distinctions which they contained, were felt as extreme inconveniences. The attempt of Bauhin to remedy this evil by a Synonymy, had, as we have seen, failed at the time, for want of any directing principle; and was become still more defective by the lapse of years and the accumulation of fresh knowledge and new books. Haller had proposed to distinguish the species of each genus by the numbers 1, 2, 3, and so on; but botanists found that their memory could not deal with such arbitrary abstractions. The need of some better nomenclature was severely felt.

The remedy which Linnæus finally introduced was the use of *trivial* names; that is, the designation of each species by the name of the genus along with a *single* conventional word, imposed without any general rule. Such names are added above in parentheses, to the specimens of the names previously in use. But though this remedy was found to be complete and satisfactory, and is now universally adopted in every branch of natural history, it was not one of the reforms which Linnæus at first proposed. Perhaps he did not at first see its full value; or, if he did, we may suppose that it required more self-confidence than he possessed, to set himself to introduce and establish ten thousand new names in the botanical world. Accordingly, the first attempts of Linnæus at the improvement of the nomenclature of botany were, the proposal of fixed and careful rules for the generic name, and for the descriptive phrase. Thus, in his *Critica Botanica*, he gives many precepts concerning the selection of the names of genera, intended to secure convenience or elegance. For instance, that they are to be single words;[5] he substitutes *atropa* for *bella donna*, and *leontodon* for *dens leonis;* that they are not to depend upon the name of another genus,[6] as *acriviola*, *agrimonoides;* that they are not[7] to be "sesquipedalia;" and, says he, any word is sesquipedalian to me, which has more than twelve letters, as *kalophyllodendron*, for which he substitutes *calophyllon*. Though some of these rules may seem pedantic, there is no doubt that, taken altogether, they tend exceedingly, like the labors of purists in other languages, to exclude extravagance, caprice, and barbarism in botanical speech.

The precepts which he gives for the matter of the "descriptive phrase," or, as it is termed in the language of the Aristotelian logicians, the "differentia," are, for the most part, results of the general rule, that the most fixed characters which can be found are to be used; this rule being interpreted according to all the knowledge of plants which had then been acquired. The language of the rules was, of course, to be regulated by the terminology, of which we have already spoken.

Thus, in the *Critica Botanica*, the name of a plant is considered as consisting of a generic *word* and a specific *phrase;* and these are, he says,[8] the right and left hands of the plant. But he then speaks of another kind of name; the *trivial* name, which is opposed to the scientific. Such names were, he says,[9] those of his predecessors, and especially of the most ancient of them. Hitherto[10] no rules had been given for their use. He manifestly, at this period, has small regard for them. "Yet," he says, "trivial names may, perhaps, be used on this account,—that the *differentia* often turns out too long to be convenient in common use, and may require change as new species are discovered. However," he continues, "in this work we set such names aside altogether, and attend only to the *differentiæ*."

Even in the *Species Plantarum*, the work which gave general currency to these trivial names, he does not seem to have yet dared to propose so great a novelty. They only stand in the margin of the work. "I have placed them there," he says in his Preface, "that, without circumlocution, we may call every herb by a single name; I have done this without selection, which would require more time. And I beseech all sane botanists to avoid most religiously ever pro-

[5] *Phil. Bot.* 224. [6] Ib. 228, 229. [7] Ib. 252.
[8] Ib. 266. [9] Ib. 261. [10] Ib. 260.

posing a trivial name without a sufficient specific distinction, lest the science should fall into its former barbarism."

It cannot be doubted, that the general reception of these trivial names of Linnæus, as the current language among botanists, was due, in a very great degree, to the knowledge, care, and skill with which his characters, both of genera and of species, were constructed. The rigorous rules of selection and expression which are proposed in the *Fundamenta Botanica* and *Critica Botanica*, he himself conformed to; and this scrupulosity was employed upon the results of immense labor. "In order that I might make myself acquainted with the species of plants," he says, in the preface to his work upon them, "I have explored the Alps of Lapland, the whole of Sweden, a part of Norway, Denmark, Germany, Belgium, England, France: I have examined the Botanical Gardens of Paris, Oxford, Chelsea, Harlecamp, Leyden, Utrecht, Amsterdam, Upsal, and others: I have turned over the Herbals of Burser, Hermann, Clifford, Burmann, Oldenland, Gronovius, Royer, Sloane, Sherard, Bobart, Miller, Tournefort, Vaillant, Jussieu, Surien, Beck, Brown, &c.: my dear disciples have gone to distant lands, and sent me plants from thence; Kerlen to Canada, Hasselquist to Egypt, Asbech to China, Toren to Surat, Solander to England, Alstrœmer to Southern Europe, Martin to Spitzbergen, Pontin to Malabar, Kœhler to Italy, Forskähl to the East, Lœfling to Spain, Montin to Lapland: my botanical friends have sent me many seeds and dried plants from various countries: Lagerström many from the East Indies; Gronovius most of the Virginian; Gmelin all the Siberian; Burmann those of the Cape." And in consistency with this habit of immense collection of materials, is his maxim,[11] that "a person is a better botanist in proportion as he knows more species." It will easily be seen that this maxim, like Newton's declaration that discovery requires patient thought alone, refers only to the exertions of which the man of genius is conscious; and leaves out of sight his peculiar endowments, which he does not see because they are part of his power of vision. With the taste for symmetry which dictated the *Critica Botanica*, and the talent for classification which appears in the *Genera Plantarum*, and the *Systema Naturæ*, a person must undoubtedly rise to higher steps of classificatory knowledge and skill, as he became acquainted with a greater number of facts.

The acknowledged superiority of Linnæus in the knowledge of the

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[11] *Phil. Bot.* 259.

matter of his science, induced other persons to defer to him in what concerned its form; especially when his precepts were, for the most part, recommended strongly both by convenience and elegance. The trivial names of the *Species Plantarum* were generally received; and though some of the details may have been altered, the immense advantage of the scheme ensures its permanence.

### *Sect. 4.—Linnæus's Artificial System.*

We have already seen, that, from the time of Cæsalpinus, botanists had been endeavoring to frame a systematic arrangement of plants. All such arrangements were necessarily both artificial and natural: they were *artificial*, inasmuch as they depended upon assumed principles, the number, form, and position of certain parts, by the application of which the whole vegetable kingdom was imperatively subdivided; they were *natural*, inasmuch as the justification of this division was, that it brought together those plants which were naturally related. No system of arrangement, for instance, would have been tolerated which, in a great proportion of cases, separated into distant parts of the plan the different species of the same genus. As far as the main body of the genera, at least, all systems are natural.

But beginning from this line, we may construct our systems with two opposite purposes, according as we endeavor to carry our assumed principle of division rigorously and consistently through the system, or as we wish to associate natural families of a wider kind than genera. The former propensity leads to an artificial, the latter to a natural method. Each is a *System* of *Plants;* but in the first, the emphasis is thrown on the former word of the title, in the other, on the latter.

The strongest recommendation of an artificial system, (besides its approaching to a natural method,) is, that it shall be capable of easy use; for which purpose, the facts on which it depends must be apparent in their relations, and universal in their occurrence. The system of Linnæus, founded upon the number, position, and other circumstances of the stamina and pistils, the reproductive organs of the plants, possessed this merit in an eminent degree, as far as these characters are concerned; that is, as far as the *classes* and *orders*. In its further subdivision into genera, its superiority was mainly due to the exact observation and description, which we have already had to notice as talents which Linnæus peculiarly possessed.

The Linnæan system of plants was more definite than that of Tour nefort, which was governed by the corolla; for number is more definite than irregular form. It was more readily employed than any of those which depend on the fruit, for the flower is a more obvious object, and more easily examined. Still, it can hardly be doubted, that the circumstance which gave the main currency to the system of Linnæus was its physiological signification: it was the *Sexual System.* The relation of the parts to which it directed the attention, interested both the philosophical faculty and the imagination. And when, soon after the system had become familiar in our own country, the poet of *The Botanic Garden* peopled the bell of every flower with " Nymphs" and " Swains," his imagery was felt to be by no means forced and far-fetched.

The history of the doctrine of the sexes of plants, as a point of physiology, does not belong to this place; and the Linnæan system of classification need not be longer dwelt upon for our present purpose. I will only explain a little further what has been said, that it is, up to a certain point, a natural system. Several of Linnæus's classes are, in a great measure, natural associations, kept together in violation of his own artificial rules. Thus the class *Diadelphia*, in which, by the system, the filaments of the stamina should be bound together in two parcels, does, in fact, contain many genera which are *monadelphous*, the filaments of the stamina all cohering so as to form one bundle only; as in *Genista*, *Spartium*, *Anthyllis*, *Lupinus*, *&c.* And why is this violation of rule? Precisely because these genera all belong to the natural tribe of Papilionaceous plants, which the author of the system could not prevail upon himself to tear asunder. Yet in other cases Linnæus was true to his system, to the injury of natural alliances, as he was, for instance, in another portion of this very tribe of *Papilionaceæ;* for there are plants which undoubtedly belong to the tribe, but which have ten separate stamens; and these he placed in the order *Decandria.* Upon the whole, however, he inclines rather to admit transgression of art than of nature.

The reason of this inclination was, that he rightly considered an artificial method as instrumental to the investigation of a natural one; and to this part of his views we now proceed.

### *Sect. 5.—Linnæus's Views on a Natural Method.*

The admirers of Linnæus, the English especially, were for some time in the habit of putting his Sexual System in opposition to the Natural Method, which about the same time was attempted in France. And as they often appear to have imagined that the ultimate object of botanical methods was to know the name of plants, they naturally preferred the Swedish method, which is excellent as a *finder*. No person, however, who wishes to know botany as a science, that is, as a body of general truths, can be content with making names his ultimate object. Such a person will be constantly and irresistibly led on to attempt to catch sight of the natural arrangement of plants, even before he discovers, as he will discover by pursuing such a course of study, that the knowledge of the natural arrangement is the knowledge of the essential construction and vital mechanism of plants. He will consider an artificial method as a means of arriving at a natural method. Accordingly, however much some of his followers may have overlooked this, it is what Linnæus himself always held and taught. And though what he executed with regard to this object was but little,[12] the distinct manner in which he presented the relations of an artificial and natural method, may justly be looked upon as one of the great improvements which he introduced into the study of his science.

Thus in the *Classes Plantarum* (1747), he speaks of the difficulty of the task of discovering the natural orders, and of the attempts made by others. "Yet," he adds, "I too have labored at this, have done something, have much still to do, and shall labor at the object as long as I live." He afterwards proposed sixty-seven orders, as the fragments of a natural method, always professing their imperfection.[13] And in others of his works[14] he lays down some antitheses on the subject after his manner. "The natural orders teach us the nature of plants; the artificial orders enable us to recognize plants. The natural orders, without a key, do not constitute a Method; the Method ought to be available without a master."

That extreme difficulty must attend the formation of a Natural Method, may be seen from the very indefinite nature of the Aphorisms upon this subject which Linnæus has delivered, and which the best botanists of succeeding times have assented to. Such are these;—the Natural Orders must be formed by attention, not to one or two, but to *all* the parts of plants;—the same organs are of great importance in regulating the divisions of one part of the system, and

[12] The natural orders which he proposed are a bare enumeration of genera, and have not been generally followed.

[13] *Phil. Bot.* p. 80.

[14] *Genera Plantarum*, 1764. See *Prælect. in Ord. Nat.* p. xlviii.

of small importance in another part;[15]—the Character does not constitute the Genus, but the Genus the Character;—the Character is necessary, not to make the Genus, but to recognize it. The vagueness of these maxims is easily seen; the rule of attending to all the parts, implies, that we are to estimate their relative importance, either by physiological considerations (and these again lead to arbitrary rules, as, for instance, the superiority of the function of nutrition to that of reproduction), or by a sort of latent naturalist instinct, which Linnæus in some passages seems to recognize. "The Habit of a plant," he says,[16] "must be secretly consulted. A practised botanist will distinguish, at the first glance, the plants of different quarters of the globe, and yet will be at a loss to tell by what mark he detects them. There is, I know not what look,—sinister, dry, obscure in African plants; superb and elevated, in the Asiatic; smooth and cheerful, in the American; stunted and indurated, in the Alpine."

Again, the rule that the same parts are of very different value in different Orders, not only leaves us in want of rules or reasons which may enable us to compare the marks of different Orders, but destroys the systematic completeness of the natural arrangement. If some of the Orders be regulated by the flower and others by the fruit, we may have plants, of which the flower would place them in one Order, and the fruit in another. The answer to this difficulty is the maxim already stated;—that no Character *makes* the Order; and that if a Character do not enable us to recognize the Order, it does not answer its purpose, and ought to be changed for another.

This doctrine, that the Character is to be employed as a servant and not as a master, was a stumbling-block in the way of those disciples who looked only for dogmatical and universal rules. One of Linnæus's pupils, Paul Dietrich Giseke, has given us a very lively account of his own perplexity on having this view propounded to him, and of the way in which he struggled with it. He had complained of the want of intelligible grounds, in the collection of natural orders given by Linnæus. Linnæus[17] wrote in answer, "You ask me for the characters of the Natural Orders: I confess I cannot give them." Such a reply naturally increased Giseke's difficulties. But afterwards, in 1771, he had the good fortune to spend some time at Upsal; and he narrates a conversation which he held with the great

[15] *Phil. Bot.* p. 172. [16] Ib. p. 171.

[17] *Linnæi Prælectiones,* Pref. p. xv.

teacher on this subject, and which I think may serve to show the nature of the difficulty;—one by no means easily removed, and by the general reader, not even readily comprehended with distinctness. Giseke began by conceiving that an Order *must* have that attribute from which its name is derived;—that the *Umbellatæ* must have their flower disposed in an umbel. The "mighty master" smiled,[18] and told him not to look at names, but at nature. "But" (said the pupil) "what is the use of the name, if it does not mean what it professes to mean?" "It is of small import" (replied Linnæus) "*what* you *call* the Order, if you take a proper series of plants and give it some name, which is clearly understood to apply to the plants which you have associated. In such cases as you refer to, I followed the logical rule, of borrowing a name *a potiori*, from the principal member. Can you" (he added) "give me the character of any single Order?" *Giseke.* "Surely, the character of the *Umbellatæ* is, that they have an umbel?" *Linnæus.* "Good; but there are plants which have an umbel, and are not of the *Umbellatæ*." *G.* "I remember. We must therefore add, that they have two naked seeds." *L.* "Then, *Echinophora*, which has only one seed, and *Eryngium*, which has not an umbel, will not be *Umbellatæ;* and yet they are of the Order." *G.* "I would place *Eryngium* among the *Aggregatæ*." *L.* "No; both are beyond dispute *Umbellatæ. Eryngium* has an involucrum, five stamina, two pistils, &c. Try again for your Character." *G.* "I would transfer such plants to the end of the Order, and make them form the transition to the next Order. *Eryngium* would connect the *Umbellatæ* with the *Aggregatæ*." *L.* "Ah! my good friend, the *Transition* from Order to Order is one thing; the *Character* of an Order is another. The Transitions I could indicate; but a Character of a Natural Order is impossible. I will not give my reasons for the distribution of Natural Orders which I have published. You or some other person, after twenty or after fifty years, will discover them, and see I was in the right."

I have given a portion of this curious conversation in order to show that the attempt to establish Natural Orders leads to convictions which are out of the domain of the systematic grounds on which they profess to proceed. I believe the real state of the case to be that the systematist, in such instances, is guided by an unformed and undeveloped apprehension of physiological functions. The ideas of the form, num-

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[18] "Subrisit ὁ πανυ."

ber, and figure of parts are, in some measure, overshadowed and superseded by the rising perception of organic and vital relations; and the philosopher who aims at a Natural Method, while he is endeavoring merely to explore the apartment in which he had placed himself, that of Arrangement, is led beyond it, to a point where another light begins, though dimly, to be seen; he is brought within the influence of the ideas of Organization and Life.

The sciences which depend on these ideas will be the subject of our consideration hereafter. But what has been said may perhaps serve to explain the acknowledged and inevitable imperfection of the unphysiological Linnæan attempts towards a natural method. "Artificial Classes are," Linnæus says, "a substitute for Natural, till Natural are detected." But we have not yet a Natural Method. "Nor," he says, in the conversation above cited, "can we have a Natural Method; for a Natural Method implies Natural Classes and Orders; and these Orders must have Characters." "And they," he adds in another place,[19] "who, though they cannot obtain a complete Natural Method, arrange plants according to the fragments of such a method, to the rejection of the Artificial, seem to me like persons who pull down a convenient vaulted room, and set about building another, though they cannot turn the vault which is to cover it."

How far these considerations deterred other persons from turning their main attention to a natural method, we shall shortly see; but in the mean time, we must complete the history of the Linnæan Reform.

### *Sect.* 6.—*Reception and Diffusion of the Linnæan Reform.*

We have already seen that Linnæus received, from his own country, honors and emoluments which mark his reputation as established, as early as 1740; and by his publications, his lectures, and his personal communications, he soon drew round him many disciples, whom he impressed strongly with his own doctrines and methods. It would seem that the sciences of classification tend, at least in modern times more than other sciences, to collect about the chair of the teacher a large body of zealous and obedient pupils; Linnæus and Werner were by far the most powerful heads of schools of any men who appeared in the course of the last century. Perhaps one reason of this is, that in these sciences, consisting of such an enormous multitude of species, of descriptive

[19] *Gen. Plant. in Prælect.* p. xii.

particulars, and of previous classifications, the learner is dependent upon the teacher more completely, and for a longer time than in other subjects of speculation: he cannot so soon or so easily cast off the aid and influence of the master, to pursue reasonings and hypotheses of his own. Whatever the cause may be, the fact is, that the reputation and authority of Linnæus, in the latter part of his life, were immense. He enjoyed also royal favor, for the King and Queen of Sweden were both fond of natural history. In 1753, Linnæus received from the hand of his sovereign the knighthood of the Polar Star, an honor which had never before been conferred for literary merit; and in 1756, was raised to the rank of Swedish nobility by the title of Von Linné; and this distinction was confirmed by the Diet in 1762. He lived, honored and courted, to the age of seventy-one; and in 1778 was buried in the cathedral of Upsal, with many testimonials of public respect and veneration.

De Candolle[20] assigns, as the causes of the successes of the Linnæan system,—the specific names,—the characteristic phrase,—the fixation of descriptive language,—the distinction of varieties and species,—the extension of the method to all the kingdoms of nature,—and the practice of introducing into it the species most recently discovered. This last course Linnæus constantly pursued; thus making his works the most valuable for matter, as they were the most convenient in form. The general diffusion of his methods over Europe may be dated, perhaps, a few years after 1760, when the tenth and the succeeding editions of the *Systema Naturæ* were in circulation, professing to include every species of organized beings. But his pupils and correspondents effected no less than his books, in giving currency to his system. In Germany,[21] it was defended by Ludwig, Gesner, Fabricius. But Haller, whose reputation in physiology was as great as that of Linnæus in methodology, rejected it as too merely artificial. In France, it did not make any rapid or extensive progress: the best French botanists were at this time occupied with the solution of the great problem of the construction of a Natural Method. And though the rhetorician Rousseau charmed, we may suppose, with the elegant precision of the *Philosophia Botanica*, declared it to be the most philosophical work he had ever read in his life, Buffon and Andanson, describers and philosophers of a more ambitious school, felt a repugnance to the rigorous rules, and limited, but finished, undertakings of the Swedish naturalist. To resist his

[20] *Théor. Elém.* p. 40.

[21] Sprengel, II. 244.

criticism and his influence, they armed themselves with dislike and contempt.

In England the Linnæan system was very favorably received:—perhaps the more favorably, for being a strictly artificial system. For the indefinite and unfinished form which almost inevitably clings to a natural method, appears to be peculiarly distasteful to our countrymen. It might seem as if the suspense and craving which comes with knowledge confessedly incomplete were so disagreeable to them, that they were willing to avoid it, at any rate whatever; either by rejecting system altogether, or by accepting a dogmatical system without reserve. The former has been their course in recent times with regard to Mineralogy; the latter was their proceeding with respect to the Linnæan Botany. It is in this country alone, I believe, that *Wernerian* and *Linnæan* Societies have been instituted. Such appellations somewhat remind us of the Aristotelian and Platonic schools of ancient Greece. In the same spirit it was, that the Artificial System was at one time here considered, not as subsidiary and preparatory to the Natural Orders, but as opposed to them. This was much as if the disposition of an army in a review should be considered as inconsistent with another arrangement of it in a battle.

When Linnæus visited England in 1736, Sloane, then the patron of natural history in this country, is said to have given him a cool reception, such as was perhaps most natural from an old man to a young innovator; and Dillenius, the Professor at Oxford, did not accept the sexual system. But as Pulteney, the historian of English Botany, says, when his works became known, "the simplicity of the classical characters, the uniformity of the generic notes, all confined to the parts of the fructification, and the precision which marked the specific distinctions, merits so new, soon commanded the assent of the unprejudiced."

Perhaps the progress of the introduction of the Linnæan System into England will be best understood from the statement of T. Martyn, who was Professor of Botany in the University of Cambridge, from 1761 to 1825. "About the year 1750," he says,[22] "I was a pupil of the school of our great countryman Ray; but the rich vein of knowledge, the profoundness and precision, which I remarked everywhere in the *Philosophia Botanica*, (published in 1751,) withdrew me from my first master, and I became a decided convert to that system of botany which has since been generally received. In 1753, the *Species*

[22] Pref. to *Language of Botany*, 3rd edit. 1807.

*Plantarum*, which first introduced the specific names, made me a Linnæan completely." In 1763, he introduced the system in his lectures at Cambridge, and these were the first Linnæan lectures in England. Stillingfleet had already, in 1757, and Lee, in 1760, called the attention of English readers to Linnæus. Sir J. Hill, (the king's gardener at Kew,) in his *Flora Britannica*, published in 1760, had employed the classes and generic characters, but not the nomenclature; but the latter was adopted by Hudson, in 1762, in the *Flora Anglica*.

Two young Swedes, pupils of Linnæus, Dryander and Solander, settled in England, and were in intimate intercourse with the most active naturalists, especially with Sir Joseph Banks, of whom the former was librarian, and the latter a fellow-traveller in Cook's celebrated voyage. James Edward Smith was also one of the most zealous disciples of the Linnæan school; and, after the death of Linnæus, purchased his Herbariums and Collections. It is related,[23] as a curious proof of the high estimation in which Linnæus was held, that when the Swedish government heard of this bargain, they tried, though too late, to prevent these monuments of their countryman's labor and glory being carried from his native land, and even went so far as to send a frigate in pursuit of the ship which conveyed them to England. Smith had, however, the triumph of bringing them home in safety. On his death they were purchased by the Linnæan Society. Such relics serve, as will easily be imagined, not only to warm the reverence of his admirers, but to illustrate his writings: and since they have been in this country, they have been the object of the pilgrimage of many a botanist, from every part of Europe.

I have purposely confined myself to the history of the Linnæan system in the cases in which it is most easily applicable, omitting all consideration of more obscure and disputed kinds of vegetables, as ferns, mosses, fungi, lichens, sea-weeds, and the like. The nature and progress of a classificatory science, which it is our main purpose to bring into view, will best be understood by attending, in the first place, to the cases in which such a science has been pursued with the most decided success; and the advances which have been made in the knowledge of the more obscure vegetables, are, in fact, advances in artificial classification, only in as far as they are advances in natural classification, and in physiology.

To these subjects we now proceed.

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[23] Trapp's *Transl. of Stower's Life of Linnæus*, p. 314.

## CHAPTER V.

### Progress towards a Natural System of Botany.

WE have already said, that the formation of a Natural System of classification must result from a comparison of *all* the resemblances and differences of the things classed; but that, in acting upon this maxim, the naturalist is necessarily either guided by an obscure and instinctive feeling, which is, in fact, an undeveloped recognition of physiological relations, or else acknowledges physiology for his guide, though he is obliged to assume arbitrary rules in order to interpret its indications. Thus all Natural Classification of organized beings, either begins or soon ends in Physiology; and can never advance far without the aid of that science. Still, the progress of the Natural Method in botany went to such a length before it was grounded entirely on the anatomy of plants, that it will be proper, and I hope instructive, to attempt a sketch of it here.

As I have already had occasion to remark, the earlier systems of plants were natural; and they only ceased to be so, when it appeared that the problem of constructing a *system* admitted of a very useful solution, while the problem of devising a *natural system* remained insoluble. But many botanists did not so easily renounce the highest object of their science. In France, especially, a succession of extraordinary men labored at it with no inconsiderable success: and they were seconded by worthy fellow-laborers in Germany and elsewhere.

The precept of taking into account all the parts of plants according to their importance, may be applied according to arbitrary rules. We may, for instance, assume that the fruit is the most important part; or we may make a long list of parts, and look for agreement in the greatest possible number of these, in order to construct our natural orders. The former course was followed by Gærtner;[1] the latter by Adanson. Gærtner's principles, deduced from the dissection of more than a thousand kinds of fruits,[2] exercised, in the sequel, a great and

[1] *De Fructibus et Seminibus Plantarum.* Stuttg. 1788–1791.

[2] Sprengel, ii. 29[illegible]

permanent influence on the formation of natural classes. Adanson's attempt, bold and ingenious, belonged, both in time and character, to a somewhat earlier stage of the subject.[3] Enthusiastic and laborious beyond belief, but self-confident, and contemptuous of the labors of others, Michael Adanson had collected, during five years spent in Senegal, an enormous mass of knowledge and materials; and had formed plans for the systems which he conceived himself thus empowered to reach, far beyond the strength and the lot of man.[4] In his *Families of Plants*, however, all agree that his labors were of real value to the science. The method which he followed is thus described by his eloquent and philosophical eulogist.[5]

Considering each organ by itself, he formed, by pursuing its various modifications, a system of division, in which he arranged all known species according to that organ alone. Doing the same for another organ, and another, and so for many, he constructed a collection of systems of arrangement, each artificial,—each founded upon one assumed organ. The species which come together in all these systems are, of all, naturally the nearest to each other; those which are separated in a few of the systems, but contiguous in the greatest number, are naturally near to each other, though less near than the former; those which are separated in a greater number, are further removed from each other in nature; and they are the more removed, the fewer are the systems in which they are associated.

Thus, by this method, we obtain the means of estimating precisely the degree of natural affinity of all the species which our systems include, independent of a physiological knowledge of the influence of the organs. But the method has, Cuvier adds, the inconvenience of presupposing another kind of knowledge, which, though it belongs only to descriptive natural history, is no less difficult to obtain;—the knowledge, namely, of all species, and of all the organs of each. A single one neglected, may lead to relations the most false; and Adanson himself, in spite of the immense number of his observations, exemplifies this in some instances.

We may add, that in the division of the structure into organs, and in the estimation of the gradations of these in each artificial system, there is still room for arbitrary assumption.

In the mean time, the two Jussieus had presented to the world a "Natural Method," which produced a stronger impression than the

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[3] *Familles des Plantes*, 1763. [4] Cuvier's *Eloge*. [5] Cuv. *Eloges*, tom. i. p. 282

"Universal Method" of Adanson. The first author of the system was Bernard de Jussieu, who applied it in the arrangement of the garden of the Trianon, in 1759, though he never published upon it. His nephew, Antoine Laurent de Jussieu, in his *Treatise of the Arrangement of the Trianon*,[6] gave an account of the principles and orders of his uncle, which he adopted when he succeeded him; and, at a later period, published his *Genera Plantarum secundum Ordines Naturales disposita;* a work, says Cuvier, which perhaps forms as important an epoch in the sciences of observation, as the *Chimie* of Lavoisier does in the sciences of experiment. The object of the Jussieus was to obtain a system which should be governed by the natural affinities of the plants, while, at the same time, the characters by which the orders were ostensibly determined, should be as clear, simple, and precise, as those of the best artificial system. The main points in these characters were the number of the cotyledons, and the structure of the seed; and subordinate to this, the insertion of the stamina, which they distinguished as *epigynous*, *perigynous*, and *hypogynous*, according as they were inserted over, about, or under, the germen. And the classes which were formed by the Jussieus, though they have since been modified by succeeding writers, have been so far retained by the most profound botanists, notwithstanding all the new care and new light which have been bestowed upon the subject, as to show that what was done at first, was a real and important step in the solution of the problem.

The merit of the formation of this natural method of plants must be divided between the two Jussieus. It has been common to speak of the nephew, Antoine Laurent, as only the publisher of his uncle's work.[7] But this appears, from a recent statement,[8] to be highly unjust. Bernard left nothing in writing but the catalogues of the garden of the Trianon, which he had arranged according to his own views; but these catalogues consist merely of a series of names without explanation or reason added. The nephew, in 1773, undertook and executed for himself the examination of a natural family, the *Ranunculaceæ;* and he was wont to relate (as his son informs us) that it

[6] *Mém. Ac. P.* 1774.

[7] *Prodromus Floræ Penins. Ind. Orient.* Wight and Walker-Arnott, Introd. p. xxxv.

By Adrien de Jussieu, son of Antoine Laurent, in the *Annales des Sc. Nat.*, Nov 1834.

was this employment which first opened his eyes and rendered him a botanist. In the memoir which he wrote, he explained fully the relative importance of the characters of plants, and the subordination of some to others;—an essential consideration, which Adanson's scheme had failed to take account of. The uncle died in 1777; and his nephew, in speaking of him, compares his arrangement to the *Ordines Naturales* of Linnæus: "Both these authors," he says, "have satisfied themselves with giving a catalogue of genera which approach each other in different points, without explaining the motives which induced them to place one order before another, or to arrange a genus under a certain order. These two arrangements may be conceived as problems which their authors have left for botanists to solve. Linnæus published his; that of M. de Jussieu is only known by the manuscript catalogues of the garden of the Trianon."

It was not till the younger Jussieu had employed himself for nineteen years upon botany, that he published, in 1789, his *Genera Plantarum;* and by this time he had so entirely formed his scheme in his head, that he began the impression without having written the book, and the manuscript was never more than two pages in advance of the printer's type.

When this work appeared, it was not received with any enthusiasm; indeed, at that time, the revolution of states absorbed the thoughts of all Europe, and left men little leisure to attend to the revolutions of science. The author himself was drawn into the vortex of public affairs, and for some years forgot his book. The method made its way slowly and with difficulty: it was a long time before it was comprehended and adopted in France, although the botanists of that country had, a little while before, been so eager in pursuit of a natural system. In England and Germany, which had readily received the Linnæan method, its progress was still more tardy.

There is only one point, on which it appears necessary further to dwell. A main and fundamental distinction in all natural systems, is that of the Monocotyledonous and Dicotyledonous plants; that is, plants which unfold themselves from an embryo with two little leaves, or with one leaf only. This distinction produces its effects in the systems which are regulated by numbers; for the flowers and fruit of the monocotyledons are generally referrible to some law in which the number *three* prevails; a type which rarely occurs in dicotyledons, these affecting most commonly an arrangement founded on the number *five*. But it appears, when we attempt to rise towards a natural method, that this division according to the cotyledons is of a higher order than the other divisions according to number; and corresponds to a distinction in the general structure and organization of the plant. The apprehension of the due rank of this distinction has gradually grown clearer. Cuvier[9] conceives that he finds such a division clearly marked in Lobel, in 1581, and employed by Ray as the basis of his classification a century later. This difference has had its due place assigned it in more recent systems of arrangement; but it is only later still that its full import has been distinctly brought into view. Desfontaines discovered[10] that the ligneous fibre is developed in an opposite manner in vegetables with one and with two cotyledons;—towards the inside in the former case, and towards the outside in the latter; and hence these two great classes have been since termed *endogenous* and *exogenous*.

Thus this division, according to the cotyledons, appears to have the stamp of reality put upon it, by acquiring a physiological meaning. Yet we are not allowed to forget, even at this elevated point of generalization, that *no one* character can be imperative in a natural method. Lamarck, who employed his great talents on botany, before he devoted himself exclusively to other branches of natural history, published his views concerning methods, systems,[11] and characters. His main principle is, that no single part of a plant, however essential, can be an absolute rule for classification; and hence he blames the Jussieuian method, as giving this inadmissible authority to the cotyledons. Roscoe[12] further urges that some plants, as *Orchis morio*, and *Limodorum verecundum*, have no visible cotyledons. Yet De Candolle, who labored along with Lamarck, in the new edition of the *Flore Française*, has, as we have already intimated, been led, by the most careful application of the wisest principles, to a system of Natural Orders, of which Jussieu's may be looked upon as the basis; and we shall find the greatest botanists, up to the most recent period, recognizing, and employing themselves in improving, Jussieu's Natural Families; so that in the progress of this part of our knowledge, vague and perplexing as it is, we have no exception to our general aphorism, that no real acquisition in science is ever discarded.

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[9] *Hist. Sc. Nat.* ii. 197.

[10] *Hist. Sc. Nat.* i. pp. 196, 290.

[11] Sprengel, ii. 296; and, there quoted, *Flore Française*, t. i. 3, 1778. *Mém. Ac. P.* 1785. *Journ. Hist. Nat.* t. i. For Lamarck's *Méthode Analytique*, see Dumeril, *Sc. Nat.* i. Art. 390.

[12] Roscoe, *Linn. Tr.* vol. xi. *Cuscuta* also has no cotyledons.

The reception of the system of Jussieu in this country was not so ready and cordial as that of Linnæus. As we have already noticed, the two systems were looked upon as rivals. Thus Roscoe, in 1810,[13] endeavored to show that Jussieu's system was not more natural than the Linnæan, and was inferior as an artificial system: but he argues his points as if Jussieu's characters were the grounds of his distribution; which, as we have said, is to mistake the construction of a natural system. In 1803, Salisbury[14] had already assailed the machinery of the system, maintaining that there are no cases of perigynous stamens, as Jussieu assumes; but this he urges with great expressions of respect for the author of the method. And the more profound botanists of England soon showed that they could appreciate and extend the natural method. Robert Brown, who had accompanied Captain Flinders to New Holland in 1801, and who, after examining that country, brought home, in 1805, nearly four thousand species of plants, was the most distinguished example of this. In his preface to the *Prodromus Floræ Novæ Hollandiæ*, he says, that he found himself under the necessity of employing the natural method, as the only way of avoiding serious error, when he had to deal with so many new genera as occur in New Holland; and that he has, therefore, followed the method of Jussieu; the greater part of whose orders are truly natural, "although their arrangement in classes, as is," he says, "conceded by their author, no less candid than learned, is often artificial, and, as appears to me, rests on doubtful grounds."

From what has already been said, the reader will, I trust, see what an extensive and exact knowledge of the vegetable world, and what comprehensive views of affinity, must be requisite in a person who has to modify the natural system so as to make it suited to receive and arrange a great number of new plants, extremely different from the genera on which the arrangement was first formed, as the New Holland genera for the most part were. He will also see how impossible it must be to convey by extract or description any notion of the nature of these modifications: it is enough to say, that they have excited the applause of botanists wherever the science is studied, and that they have induced M. de Humboldt and his fellow-laborers, themselves botanists of the first rank, to dedicate one of their works to him in terms of the strongest admiration.[15] Mr. Brown has also published

[13] *Linn. Tr.* vol. xi. p. 50.

[14] Ibid. vol. viii.

[15] Roberto Brown, Britanniarum gloriæ atque ornamento, totam Botanices scientiam ingenio mirifico complectenti, &c.

special disquisitions on parts of the Natural System; as on Jussieu's *Proteaceæ*:[16] on the *Asclepiadeæ*, a natural family of plants which must be separated from Jussieu's *Apocyneæ*:[17] and other similar labors.

We have, I think, been led, by our survey of the history of Botany, to this point;—that a Natural Method directs us to the study of Physiology, as the only means by which we can reach the object. This conviction, which in botany comes at the end of a long series of attempts at classification, offers itself at once in the natural history of animals, where the physiological signification of the resemblances and differences is so much more obvious. I shall not, therefore, consider any of these branches of natural history in detail as examples of mere classification. They will come before us, if at all, more properly when we consider the classifications which depend on the functions of organs, and on the corresponding modifications which they necessarily undergo; that is, when we trace the results of Physiology. But before we proceed to sketch the history of that part of our knowledge, there are a few points in the progress of Zoology, understood as a mere classificatory science, which appear to me sufficiently instructive to make it worth our while to dwell upon them.

[2nd Ed.] [Mr. Lindley's recent work, *The Vegetable Kingdom* (1846), may be looked upon as containing the best view of the recent history of Systematic Botany. In the Introduction to this work, Mr. Lindley has given an account of various recent works on the subject; as Agardh's *Classes Plantarum* (1826); Perleb's *Lehrbuch der Natur geschichte der Pflanzenreich* (1826); Dumortier's *Florula Belgica* (1827); Bartling's *Ordines Naturales Plantarum* (1830); Hess's *Uebersicht der Phanerogenischen Natürlichen Pflanzenfamilien* (1832); Schulz's *Natürliches System des Pflanzenreich's* (1832); Horaninow's *Primæ Lineæ Systematis Naturæ* (1834); Fries's *Corpus Florarum provincialium Sueciæ* (1835); Martins's *Conspectus Regni Vegetabilis secundum Characteres Morphologicos* (1835); Sir Edward F. Bromhead's System, as published in the *Edinburgh Journal* and other Journals (1836–1840); Endlicher's *Genera Plantarum secundum Ordines Naturales disposita* (1836–1840); Perleb's *Clavis Classicum Ordinum et Familiarum* (1838); Adolphe Brongniart's *Enumération des Genres de Plantes* (1843); Meisner's *Plantarum vascularium Genera secundum Ordines Naturales digesta* (1843); Horaninow's *Tetractys Naturæ, seu Systema quinquemembre omnium Naturalium*

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[16] *Linn. Tr.* vol. x. 1809. [17] *Mem. of Wernerian N. H. Soc.* vol. i. 1809.

(1843); Adrien de Jussieu's *Couërs Elmentaire d'Histoire Naturelle, Botanique* (1844).

Mr. Lindley, in this as in all his works, urges strongly the superior value of natural as compared with artificial systems; his principles being, I think, nearly such as I have attempted to establish in the *Philosophy of the Sciences*, Book viii., Chapter ii. He states that the leading idea which has been kept in view in the compilation of his work is this maxim of Fries: "Singula sphæra (sectio) *ideam quandam* exponit, indeque ejus character notione simplici optime exprimitur;" and he is hence led to think that the true characters of all natural assemblages are extremely simple.

One of the leading features in Mr. Lindley's system is that he has thrown the Natural Orders into groups subordinate to the higher divisions of Classes and Sub-classes. He had already attempted this, in imitation of Agardh and Bartling, in his *Nixus Plantarum* (1833). The groups of Natural Orders were there called *Nixus* (tendencies); and they were denoted by names ending in *ales;* but these groups were further subordinated to *Cohorts.* Thus the first member of the arrangement was Class 1. EXOGENÆ. Sub-class 1. POLYPETALÆ. Cohort 1. ALBUMINOSÆ. *Nixus* 1. *Ranales.* Natural Orders included in this *Nixus*, Ranunculaceæ, Saraceniceæ, Papaveraceæ, &c. In the *Vegetable Kingdom*, the groups of Natural Orders are termed *Alliances.* In this work, the Sub-classes of the EXOGENS are four: I. DICLINOUS; II. HYPOGYNOUS; III. PERIGYNOUS; IV. EPIGYNOUS; and the Alliances are subordinated to these without the intervention of *Cohorts.*

Mr. Lindley has also, in this as in other works, given English names for the Natural Orders. Thus for *Nymphaceæ*, *Ranunculaceæ*, *Tamaricaceæ*, *Zygophyllaceæ*, *Eleatrinaceæ*, he substitutes Water-Lilies, Crowfoots, Tamarisks, Bean-Capers, and Water-Peppers; for *Malvaceæ*, *Aurantiaceæ*, *Gentianaceæ*, *Primulaceæ*, *Urtiaceæ*, *Euphorbiaceæ*, he employs Mallow-worts, Citron-worts, Gentian-worts, Prim-worts, Nettle-worts, Spurge-worts; and the terms Orchids, Hippurids, Amaryllids, Irids, Typhads, Arads, Cucurbits, are taken as English equivalents for *Orchidaceæ*, *Haloragaceæ*, *Amaryllidaceæ*, *Iridaceæ*, *Typhaceæ*, *Araceæ*, *Cucurbitaceæ.* All persons who wish success to the study of botany in England must rejoice to see it tend to assume this idiomatic shape.]

## CHAPTER VI.

### The Progress of Systematic Zoology.

THE history of Systematic Botany, as we have presented it, may be considered as a sufficient type of the general order of progression in the sciences of classification. It has appeared, in the survey which we have had to give, that this science, no less than those which we first considered, has been formed by a series of inductive processes, and has, in its history, Epochs at which, by such processes, decided advances were made. The important step in such cases is, the seizing upon some artificial mark which conforms to natural resemblances;—some basis of arrangement and nomenclature by means of which true propositions of considerable generality can be enunciated. The advance of other classificatory sciences, as well as botany, must consist of such steps; and their course, like that of botany, must (if we attend only to the real additions made to knowledge,) be gradual and progressive, from the earliest times to the present.

To exemplify this continued and constant progression in the whole range of Zoology, would require vast knowledge and great labor; and is, perhaps, the less necessary, after we have dwelt so long on the history of Botany, considered in the same point of view. But there are a few observations respecting Zoology in general which we are led to make in consequence of statements recently promulgated; for these statements seem to represent the history of Zoology as having followed a course very different from that which we have just ascribed to the classificatory sciences in general. It is held by some naturalists, that not only the formation of a systematic classification in Zoology dates as far back as Aristotle; but that his classification is, in many respects, superior to some of the most admired and recent attempts of modern times.

If this were really the case, it would show that at least the idea of a Systematic Classification had been formed and developed long previous to the period to which we have assigned such a step; and it would be difficult to reconcile such an early maturity of Zoology with the conviction, which we have had impressed upon us by the other parts of our history, that not only labor but time, not only one man of genius but several, and those succeeding each other, are requisite to the formation of any considerable science.

But, in reality, the statements to which we refer, respecting the scientific character of Aristotle's Zoological system, are altogether without foundation; and this science confirms the lessons taught us by all the others. The misstatements respecting Aristotle's doctrines are on this account so important, and are so curious in themselves, that I must dwell upon them a little.

Aristotle's nine Books *On Animals* are a work enumerating the differences of animals in almost all conceivable respects;—in the organs of sense, of motion, of nutrition, the interior anatomy, the exterior covering, the manner of life, growth, generation, and many other circumstances. These differences are very philosophically estimated. "The corresponding parts of animals," he says,[1] "besides the differences of quality and circumstance, differ in being more or fewer, greater or smaller, and, speaking generally, in excess and defect. Thus some animals have crustaceous coverings, others hard shells; some have long beaks, some short; some have many wings, some have few; Some again have parts which others want, as crests and spurs." He then makes the following important remark: "Some animals have parts which correspond to those of others, not as being the same in species, nor by excess and defect, but by *analogy;* thus a claw is analogous to a thorn, and a nail to a hoof, and a hand to the nipper of a lobster, and a feather to a scale; for what a feather is in a bird, that is a scale in a fish."

It will not, however, be necessary, in order to understand Aristotle for our present purpose, that we should discuss his notion of Analogy. He proceeds to state his object,[2] which is, as we have said, to describe the differences of animals in their structure and habits. He then observes, that for structure, we may take Man for our type,[3] as being best known to us; and the remainder of the first Book is occupied with a description of man's body, beginning from the head, and proceeding to the extremities.

In the next Book, (from which are taken the principal passages in which his modern commentators detect his system,) he proceeds to compare the differences of parts in different animals, according to the order which he had observed in man. In the first chapter he speaks

[1] Lib. i. c. i. [2] Lib. i. c. ii. [3] c. iii.

of the head and neck of animals; in the second, of the parts analogous to arms and hands; in the third, of the breast and paps, and so on; and thus he comes, in the seventh chapter, to the legs, feet, and toes: and in the eleventh, to the teeth, and so to other parts.

The construction of a classification consists in the selection of certain parts, as those which shall eminently and peculiarly determine the place of each species in our arrangement. It is clear, therefore, that such an enumeration of differences as we have described, supposing it complete, contains the materials of all possible classifications. But we can with no more propriety say that the author of such an enumeration of differences is the author of any classification which can be made by means of them, than we can say that a man who writes down the whole alphabet writes down the solution of a given riddle or the answer to a particular question.

Yet it is on no other ground than this enumeration, so far as I can discover, that Aristotle's "System" has been so decidedly spoken of,[4] and exhibited in the most formal tabular shape. The authors of this *Systema Aristotelicum*, have selected, I presume, the following passages from the work *On Animals*, as they might have selected any other; and by arranging them according to a subordination unknown to Aristotle himself, have made for him a scheme which undoubtedly bears a great resemblance to the most complete systems of modern times.

Book I., chap. v.—"Some animals are viviparous, some oviparous, some vermiparous. The viviparous are such as man, and the horse, and all those animals which have hair; and of aquatic animals, the whale kind, as the dolphin and cartilaginous fishes."

Book II., chap. vii.—"Of quadrupeds which have blood and are viviparous, some are (as to their extremities,) many-cloven, as the hands and feet of man. For some are many-toed, as the lion, the dog, the panther; some are bifid, and have hoofs instead of nails, as the sheep, the goat, the elephant, the hippopotamus; and some have undivided feet, as the solid-hoofed animals, the horse and ass. The swine kind share both characters."

Chap. ii.—"Animals have also great differences in the teeth, both when compared with each other and with man. For all quadrupeds which have blood and are viviparous, have teeth. And in the first place, some are ambidental,[5] (having teeth in both jaws;) and some

[4] *Linnæan Transactions*, vol. xvi. p. 24.

[5] Ἀμφόδοντα.

are not so, wanting the front teeth in the upper jaw. Some have neither front teeth nor horns, as the camel; some have tusks,[6] as the boar, some have not. Some have serrated[7] teeth, as the lion, the panther, the dog; some have the teeth unvaried,[8] as the horse and the ox; for the animals which vary their cutting-teeth have all serrated teeth. No animal has both tusks and horns; nor has any animal with serrated teeth either of those weapons. The greater part have the front teeth cutting, and those within broad."

These passages undoubtedly contain most of the differences on which the asserted Aristotelian classification rests; but the classification is formed by using the characters drawn from the teeth, in order to subdivide those taken from the feet; whereas in Aristotle these two sets of characters stand side by side, along with dozens of others; any selection of which, employed according to any arbitrary method of subordination, might with equal justice be called Aristotle's system.

Why, for instance, in order to form subdivisions of animals, should we not go on with Aristotle's continuation of the second of the above quoted passages, instead of capriciously leaping to the third? "Of these some have horns, some have none . . . Some have a fetlock-joint,[9] some have none . . . Of those which have horns, some have them solid throughout, as the stag; others, for the most part, hollow . . . Some cast their horns, some do not." If it be replied, that we could not, by means of such characters, form a tenable zoological system; we again ask by what right we assume Aristotle to have made or attempted a systematic arrangement, when what he has written, taken in its natural order, does not admit of being construed into a system.

Again, what is the object of any classification? This, at least, among others. To enable the person who uses it to study and describe more conveniently the objects thus classified. If, therefore, Aristotle had formed or adopted any system of arrangement, we should see it in the order of the subjects in his work. Accordingly, so far as he has a system, he professes to make this use of it. At the beginning of the fifth Book, where he is proceeding to treat of the different modes of generation of animals, he says, "As we formerly made a Division of animals according to their kinds, we must now, in the same manner, give a general survey of their History (θεωρίαν). Except, indeed, that in the former case we made our commencement by a description

[6] Χαυλιόδοντα. [7] Καρχαρόδοντα. [8] Ἀνεπάλλακτα. [9] Ἀστράγαλον.

of man, but in the present instance we must speak of him last, because he requires most study. We must begin then with those animals which have shells; we must go on to those which have softer coverings, as crustacea, soft animals, and insects; after these, fishes, both viviparous and oviparous; then birds; then land animals, both viviparous and oviparous."

It is clear from this passage that Aristotle had certain wide and indefinite views of classification, which though not very exact, are still highly creditable to him; but it is equally clear that he was quite unconscious of the classification that has been ascribed to him. If he had adopted that or any other system, this was precisely the place in which he must have referred to and employed it.

The honor due to the stupendous accumulation of zoological knowledge which Aristotle's works contain, cannot be tarnished by our denying him the credit of a system which he never dreamt of, and which, from the nature of the progress of science, could not possibly be constructed at that period. But, in reality, we may exchange the mistaken claims which we have been contesting for a better, because a truer praise. Aristotle does show, as far as could be done at his time, a perception of the need of groups, and of names of groups, in the study of the animal kingdom; and thus may justly be held up as the great figure in the Prelude to the Formation of Systems which took place in more advanced scientific times.

This appears, in some measure, from the passage last quoted. For not only is there, in that, a clear recognition of the value and object of a method in natural history; but the general arrangement of the animal kingdom there proposed has considerable scientific merit, and is, for the time, very philosophical. But there are passages in his work in which he shows a wish to carry the principle of arrangement more into detail. Thus, in the first Book, before proceeding to his survey of the differences of animals,[10] after speaking of such classes as Quadrupeds, Birds, Fishes, Cetaceous, Testaceous, Crustaceous Animals, Mollusks, Insects, he says, (chap. vii.)

"Animals cannot be divided into large genera, in which one kind includes many kinds. For some kinds are unique, and have no difference of species, as *man*. Some have such kinds, but have no names for them. Thus all quadrupeds which have not wings, have blood. But of these, some are viviparous, some oviparous. Those which are

[10] Γένη.

viviparous have not all hair; those which are oviparous have scales.' We have here a manifestly intentional subordination of characters: and a kind of regret that we have not names for the classes here indicated; such, for instance, as viviparous quadrupeds having hair. But he follows the subject into further detail. "Of the class of viviparous quadrupeds," he continues, "there are many genera,[11] but these again are without names, except specific names, such as *man*, *lion*, *stag*, *horse*, *dog*, and the like. Yet there is a genus of animals that have names, as the horse, the ass, the *oreus*, the *ginnus*, the *innus*, and the animal which in Syria is called *heminus* (mule); for these are called *mules*, from their resemblance only; not being mules, for they breed of their own kind. Wherefore," he adds, that is, because we do not possess recognized genera and generic names of this kind, "we must take the species separately, and study the nature of each."

These passages afford us sufficient ground for placing Aristotle at the head of those naturalists to whom the first views of the necessity of a zoological system are due. It was, however, very long before any worthy successor appeared, for no additional step was made till modern times. When Natural History again came to be studied in Nature, the business of Classification, as we have seen, forced itself upon men's attention, and was pursued with interest in animals, as in plants. The steps of its advance were similar in the two cases;—by successive naturalists, various systems of artificial marks were selected with a view to precision and convenience;—and these artificial systems assumed the existence of certain natural groups, and of a natural system to which they gradually tended. But there was this difference between botany and zoology:—the reference to physiological principles, which, as we have remarked, influenced the natural systems of vegetables in a latent and obscure manner, botanists being guided by its light, but hardly aware that they were so, affected the study of systematic zoology more directly and evidently. For men can neither overlook the general physiological features of animals, nor avoid being swayed by them in their judgments of the affinities of different species. Thus the classifications of zoology tended more and more to a union with comparative anatomy, as the science was more and more improved.[12] But comparative anatomy belongs to the subject of the next Book; and anything it may be proper to say respecting its influence upon zoological arrangements, will properly find a place there.

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[11] Εἴδη.

[12] Cuvier, *Leç. d'Anat. Comp.* vol. i. p. 17.

It will appear, and indeed it hardly requires to be proved, that those steps in systematic zoology which are due to the light thrown upon the subject by physiology, are the result of a long series of labors by various naturalists, and have been, like other advances in science, led to and produced by the general progress of such knowledge. We can hardly expect that the classificatory sciences can undergo any material improvement which is not of this kind. Very recently, however, some authors have attempted to introduce into these sciences certain principles which do not, at first sight, appear as a continuation and extension of the previous researches of comparative anatomists. I speak, in particular, of the doctrines of a *Circular Progression* in the series of affinity; of a *Quinary Division* of such circular groups; and of a relation of *Analogy* between the members of such groups, entirely distinct from the relation of *Affinity*.

The doctrine of Circular Progression has been propounded principally by Mr. Macleay; although, as he has shown,[13] there are suggestions of the same kind to be found in other writers. So far as this view negatives the doctrine of a mere linear progression in nature, which would place each genus in contact only with the preceding and succeeding ones, and so far as it requires us to attend to more varied and ramified resemblances, there can be no doubt that it is supported by the result of all the attempts to form natural systems. But whether that assemblage of circles of arrangement which is now offered to naturalists, be the true and only way of exhibiting the natural relations of organized bodies, is a much more difficult question, and one which I shall not here attempt to examine; although it will be found, I think, that those analogies of science which we have had to study, would not fail to throw some light upon such an inquiry. The prevalence of an invariable numerical law in the divisions of natural groups, (as the number *five* is asserted to prevail by Mr. Macleay, the number *ten* by Fries, and other numbers by other writers), would be a curious fact, if established; but it is easy to see that nothing short of the most consummate knowledge of natural history, joined with extreme clearness of view and calmness of judgment, could enable any one to pronounce on the attempts which have been made to establish such a principle. But the doctrine of a relation of *Analogy* distinct from Affinity, in the manner which has recently been taught, seems to be obviously at variance with that gradual approximation of the classificatory to the phy-

[13] *Linn. Trans.* vol. xvi. p. 9.

siological sciences, which has appeared to us to be the general tendency of real knowledge. It seems difficult to understand how a reference to such relations as those which are offered as examples of analogy[14] can be otherwise than a retrograde step in science.

Without, however, now dwelling upon these points, I will treat a little more in detail of one of the branches of Zoology.

[2nd Ed.] [For the more recent progress of Systematic Zoology, see in the *Reports* of the British Association, in 1834, Mr. L. Jenyns's *Report on the Recent Progress and Present State of Zoology*, and in 1844, Mr. Strickland's *Report on the Recent Progress and Present State of Ornithology.* In these Reports, the questions of the Circular Arrangement, the Quinary System, and the relation of Analogy and Affinity are discussed.]

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## CHAPTER VII.

### The Progress of Ichthyology.

If it had been already observed and admitted that sciences of the same kind follow, and must follow, the same course in the order of their development, it would be unnecessary to give a history of any special branch of Systematic Zoology; since botany has already afforded us a sufficient example of the progress of the classificatory sciences. But we may be excused for introducing a sketch of the advance of one department of zoology, since we are led to the attempt by the peculiar advantage we possess in having a complete history of the subject written with great care, and brought up to the present time, by a naturalist of unequalled talents and knowledge. I speak of Cuvier's *Historical View of Ichthyology*, which forms the first chapter of his great work on that part of natural history. The place and office in the progress of this science, which is assigned to each person by Cuvier, will probably not be lightly contested. It will, therefore, be no small confirmation of the justice of the views on which the

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[14] For example, the goatsucker has an *affinity* with the swallow; but it has an *analogy* with the bat, because both fly at the same hour of the day, and feed in the same manner.—Swainson, *Geography and Classification of Animals* p. 129.

distribution of the events in the history of botany was founded, if Cuvier's representation of the history of ichthyology offers to us obviously a distribution almost identical.

We shall find that this is so;—that we have, in zoology as in botany, a period of unsystematic knowledge; a period of misapplied erudition; an epoch of the discovery of fixed characters; a period in which many systems were put forward; a struggle of an artificial and a natural method; and a gradual tendency of the natural method to a manifestly physiological character. A few references to Cuvier's history will enable us to illustrate these and other analogies.

*Period of Unsystematic Knowledge.*—It would be easy to collect a number of the fabulous stories of early times, which formed a portion of the *imaginary knowledge* of men concerning animals as well as plants. But passing over these, we come to a long period and a great collection of writers, who, in various ways, and with various degrees of merit, contributed to augment the knowledge which existed concerning fish, while as yet there was hardly ever any attempt at a classification of that province of the animal kingdom. Among these writers, Aristotle is by far the most important. Indeed he carried on his zoological researches under advantages which rarely fall to the lot of the naturalist; if it be true, as Athenæus and Pliny state,[1] that Alexander gave him sums which amounted to nine hundred talents, to enable him to collect materials for his history of animals, and put at his disposal several thousands of men to be employed in hunting, fishing, and procuring information for him. The works of his on Natural History which remain to us are, nine Books *Of the History of Animals*; four, *On the Parts of Animals*; five, *On the Generation of Animals*; one, *On the Going of Animals*; one, *Of the Sensations, and the Organs of them*; one, *On Sleeping and Waking*; one, *On the Motion of Animals*; one, *On the Length and Shortness of Life*; one, *On Youth and Old Age*; one, *On Life and Death*; one, *On Respiration.* The knowledge of the external and internal conformation of animals, their habits, instincts, and uses, which Aristotle displays in these works, is spoken of as something wonderful even to the naturalists of our own time. And he may be taken as a sufficient representative of the whole of the period of which we speak; for he is, says Cuvier,[2] not only the first, but the only one of the ancients who has treated of the natural history of fishes (the province to which

[1] Cuv. *Hist. Nat. des Poissons*, i. 13. [2] Cuv. p. 18.

we now confine ourselves,) in a scientific point of view, and in a way which shows genius.

We may pass over, therefore, the other ancient authors from whose writings Cuvier, with great learning and sagacity, has levied contributions to the history of ichthyology; as Theophrastus, Ovid, Pliny, Oppian, Athenæus, Ælian, Ausonius, Galen. We may, too, leave unnoticed the compilers of the middle ages, who did little but abstract and disfigure the portions of natural history which they found in the ancients. Ichthyological, like other knowledge, was scarcely sought except in books, and on that very account was not understood when it was found.

*Period of Erudition.*—Better times at length came, and men began to observe nature for themselves. The three great authors who are held to be the founders of modern ichthyology, appeared in the middle of the sixteenth century; these were Bélon, Rondelet, and Salviani, who all published about 1555. All the three, very different from the compilers who filled the interval from Aristotle to them, themselves saw and examined the fishes which they describe, and have given faithful representations of them. But, resembling in that respect the founders of modern botany, Brassavola, Ruellius, Tragus, and others, they resembled them in this also, that they attempted to make their own observations a commentary upon the ancient writers. Faithful to the spirit of their time, they are far more careful to make out the names which each fish bore in the ancient world, and to bring together scraps of their history from the authors in whom these names occur, than to describe them in a lucid manner; so that without their figures, says Cuvier, it would be almost as difficult to discover their species as those of the ancients.

The difficulty of describing and naming species so that they can be recognized, is little appreciated at first, although it is in reality the main-spring of the progress of the sciences of classification. Aristotle never dreamt that the nomenclature which was in use in his time could ever become obscure;[3] hence he has taken no precaution to enable his readers to recognize the species of which he speaks; and in him and in other ancient authors, it requires much labor and great felicity of divination to determine what the names mean. The perception of this difficulty among modern naturalists led to systems, and to nomenclature founded upon system; but these did not come into

[3] Cuvier, p. 17.

being immediately at the time of which we speak; nor till the evil had grown to a more inconvenient magnitude.

*Period of Accumulation of Materials. Exotic Collections.*—The fishes of Europe were for some time the principal objects of study; but those of distant regions soon came into notice.[4] In the seventeenth century the Dutch conquered Brazil, and George Margrave, employed by them, described the natural productions of the country, and especially the fishes. Bontius, in like manner, described some of those of Batavia. Thus these writers correspond to Rumphius and Rheede in the history of botany. Many others might be mentioned; but we must hasten tc the formation of systems, which is our main object of attention.

*Epoch of the Fixation of Characters. Ray and Willoughby.*—In botany, as we have seen, though Ray was one of the first who invented a connected system, he was preceded at a considerable interval by Cæsalpinus, who had given a genuine solution of the same problem. It is not difficult to assign reasons why a sound classification should be discovered for plants at an earlier period than for fishes. The vastly greater number of the known species, and the facilities which belong to the study of vegetables, give the botanist a great advantage; and there are numerical relations of a most definite kind (for instance, the number of parts of the seed-vessel employed by Cæsalpinus as one of the bases of his system), which are tolerably obvious in plants, but which are not easily discovered in animals. And thus we find that in ichthyology, Ray, with his pupil and friend Willoughby, appears as the first founder of a tenable system.[5]

The first great division in this system is into *cartilaginous* and *bony* fishes; a primary division, which had been recognized by Aristotle, and is retained by Cuvier in his latest labors. The subdivisions are determined by the general form of the fish (as long or flat), by the teeth, the presence or absence of ventral fins, the number of dorsal fins, and the nature of the spines of the fins, as soft or prickly. Most of these characters have preserved their importance in later systems; especially the last, which, under the terms *malacopterygian* and *acanthopterygian*, holds a place in the best recent arrangements.

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[4] Cuv. p. 43.

[5] Francisci Willoughbeii, Armigeri, *de Historia Piscium,* libri iv. jussu et sumptibus Societatis Regiæ Londinensis editi, &c. Totum opus recognovit, coaptavit, supplevit, librum etiam primum et secundum adjecit Joh. Raius Oxford, 1668.

That this system was a true first approximation to a solution of the problem, appears to be allowed by naturalists. Although, says Cuvier,[6] there are in it no genera well defined and well limited, still in many places the species are brought together very naturally, and in such a way that a few words of explanation would suffice to form, from the groups thus presented to us, several of the genera which have since been received. Even in botany, as we have seen, genera were hardly maintained with any degree of precision, till the binary nomenclature of Linnæus made this division a matter of such immense convenience.

The amount of this convenience, the value of a brief and sure nomenclature, had not yet been duly estimated. The work of Willoughby forms an epoch,[7] and a happy epoch, in the history of ichthyology; for the science, once systematized, could distinguish the new from the old, arrange methodically, describe clearly. Yet, because Willoughby had no nomenclature of his own, and no fixed names for his genera, his immediate influence was not great. I will not attempt to trace this influence in succeeding authors, but proceed to the next important step in the progress of system.

*Improvement of the System. Artedi.*—Peter Artedi was a countryman and intimate friend of Linnæus; and rendered to ichthyology nearly the same services which Linnæus rendered to botany. In his *Philosophia Ichthyologica*, he analysed[8] all the interior and exterior parts of animals; he created a precise terminology for the different forms of which these parts are susceptible; he laid down rules for the nomenclature of genera and species; besides his improvements of the subdivisions of the class. It is impossible not to be struck with the close resemblance between these steps, and those which are due to the *Fundamenta Botanica*. The latter work appeared in 1736, the former was published by Linnæus, after the death of the author, in 1738; but Linnæus had already, as early as 1735, made use of Artedi's manuscripts in the ichthyological part of his *Systema Naturæ*. We cannot doubt that the two young naturalists (they were nearly of the same age), must have had a great influence upon each other's views and labors; and it would be difficult now to ascertain what portion of the peculiar merits of the Linnæan reform was derived from Artedi. But we may remark that, in ichthyology at least, Artedi appears to have been a naturalist of more original views and profounder philosophy than his friend and editor, who afterwards himself took up the subject

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[6] Cuvier, p. 57. [7] p. 58. [8] p. 20.

The reforms of Linnæus, in all parts of natural history, appear as if they were mainly dictated by a love of elegance, symmetry, clearness and definiteness; but the improvement of the ichthyological system by Artedi seems to have been a step in the progress to a natural arrangement. His genera,[9] which are forty-five in number, are so well constituted, that they have almost all been preserved; and the subdivisions which the constantly-increasing number of species has compelled his successors to introduce, have very rarely been such that they have led to the transposition of his genera.

In its bases, however, Artedi's was an artificial system. His characters were positive and decisive, founded in general upon the number of rays of the membrane of the gills, of which he was the first to mark the importance;—upon the relative position of the fins, upon their number, upon the part of the mouth where the teeth are found, upon the conformation of the scales. Yet, in some cases, he has recourse to the interior anatomy.

Linnæus himself at first did not venture to deviate from the footsteps of a friend, who, in this science, had been his master. But in 1758, in the tenth edition of the *Systema Naturæ*, he chose to depend upon himself, and devised a new ichthyological method. He divided some genera, united others, gave to the species trivial names and characteristic phrases, and added many species to those of Artedi. Yet his innovations are for the most part disapproved of by Cuvier; as his transferring the *chondropterygian* fishes of Artedi to the class of reptiles, under the title of *Amphybia nantes;* and his rejecting the distinction of acanthopterygian and malacopterygian, which, as we have seen, had prevailed from the time of Willoughby, and introducing in its stead a distribution founded on the presence or absence of the ventral fins, and on their situation with regard to the pectoral fins. "Nothing," says Cuvier, "more breaks the true connexions of genera than these orders of *apodes*, *jugulares*, *thoracici*, and *abdominales*."

Thus Linnæus, though acknowledging the value and importance of natural orders, was not happy in his attempts to construct a system which should lead to them. In his detection of good characters for an artificial system he was more fortunate. He was always attentive to number, as a character; and he had the very great merit[10] of introducing into the classification the number of rays of the fins of each species. This mark is one of great importance and use. And this, as well as

[9] Cuvier, p. 71.

[10] p. 74.

other branches of natural history, derived incalculable advantages from the more general merits of the illustrious Swede;[11]—the precision of the characters, the convenience of a well-settled terminology, the facility afforded by the binary nomenclature. These recommendations gave him a pre-eminence which was acknowledged by almost all the naturalists of his time, and displayed by the almost universal adoption of his nomenclature, in zoology, as well as in botany; and by the almost exclusive employment of his distributions of classes, however imperfect and artificial they might be.

And even [12] if Linnæus had had no other merit than the impulse he gave to the pursuit of natural science, this alone would suffice to immortalize his name. In rendering natural history easy, or at least in making it appear so, he diffused a general taste for it. The great took it up with interest; the young, full of ardor, rushed forwards in all directions, with the sole intention of completing his system. The civilized world was eager to build the edifice which Linnæus had planned.

This spirit, among other results, produced voyages of natural historical research, sent forth by nations and sovereigns. George the Third of England had the honor of setting the example in this noble career, by sending out the expeditions of Byron, Wallis, and Carteret, in 1765. These were followed by those of Bougainville, Cook, Forster, and others. Russia also scattered several scientific expeditions through her vast dominions; and pupils of Linnæus sought the icy shores of Greenland and Iceland, in order to apply his nomenclature to the productions of those climes. But we need not attempt to convey any idea of the vast stores of natural historical treasures which were thus collected from every part of the globe.

I shall not endeavor to follow Cuvier in giving an account of the great works of natural history to which this accumulution of materials gave rise; such as the magnificent work of Bloch on Fishes, which appeared in 1782—1785; nor need I attempt, by his assistance, to characterize or place in their due position the several systems of classification proposed about this time. But in the course of these various essays, the distinction of the artificial and natural methods of classification came more clearly into view than before; and this is a point so important to the philosophy of the subject, that we must devote a few words to it.

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[11] Cuvier, p. 85.

[12] Ib. p. 88.

*Separation of the Artificial and Natural Methods in Ichthyology.*—It has already been said that all so-called *artificial methods* of classification must be natural, at least as to the narrowest members of the system; thus the artificial Linnæan method is natural as to species, and even as to genera. And on the other hand, all proposed natural methods, so long as they remain unmodified, are artificial as to their characteristic marks. Thus a Natural Method is an attempt to provide positive and distinct *characters* for the *wider* as well as for the narrower *natural groups.* These considerations are applicable to zoology as well as to botany. But the question, how we know natural groups before we find marks for them, was, in botany, as we have seen, susceptible only of vague and obscure answers :—the mind forms them, it was said, by taking the aggregate of all the characters; or by establishing a subordination of characters. And each of these answers had its difficulty, of which the solution appeared to be, that in attempting to form natural orders we are really guided by a latent undeveloped estimate of physiological relations. Now this principle, which was so dimly seen in the study of vegetables, shines out with much greater clearness when we come to the study of animals, in which the physiological relations of the parts are so manifest that they cannot be overlooked, and have so strong an attraction for our curiosity that we cannot help having our judgments influenced by them. Hence the superiority of natural systems in zoology would probably be far more generally allowed than in botany; and no arrangement of animals which, in a large number of instances, violated strong and clear natural affinities, would be tolerated because it answered the purpose of enabling us easily to find the name and place of the animal in the artificial system. Every system of zoological arrangement may be supposed to aspire to be a natural system. But according to the various habits of the minds of systematizers, this object was pursued more or less steadily and successfully; and these differences came more and more into view with the increase of knowledge and the multiplication of attempts.

Bloch, whose ichthyological labors have been mentioned, followed in his great work the method of Linnæus. But towards the end of his life he had prepared a general system, founded upon one single numerical principle ;—the number of fins; just as the sexual system of Linnæus is founded upon the number of stamina; and he made his subdivisions according to the position of the ventral and pectoral fins the same character which Linnæus had employed for his primary division. He could not have done better, says Cuvier,[13] if his object had been to turn into ridicule all artificial methods, and to show to what absurd combinations they may lead.

Cuvier himself, who always pursued natural systems with a singularly wise and sagacious consistency, attempted to improve the ichthyological arrangements which had been proposed before him. In his *Règne Animal*, published in 1817, he attempts the problem of arranging this class; and the views suggested to him, both by his successes and his failures, are so instructive and philosophical, that I cannot illustrate the subject better than by citing some of them.

"The class of fishes," he says,[14] "is, of all, that which offers the greatest difficulties, when we wish to subdivide it into orders, according to fixed and obvious characters. After many trials, I have determined on the following distribution, which in some instances is wanting in precision, bnt which possesses the advantage of keeping the natural families entire.

"Fish form two distinct series;—that of *chondropterygians* or *cartilaginous fish*, and that of *fish* properly so called.

"The *first* of these series has for its character, that the palatine bones replace, in it, the bones of the upper jaw: moreover the whole of its structure has evident analogies, which we shall explain.

"It divides itself into three ORDERS:

"The CYCLOSTOMES, in which the jaws are soldered (*soudées*) into an immovable ring, and the bronchiæ are open in numerous holes.

"The SELACIANS, which have the bronchiæ like the preceding, but not the jaws.

"The STURONIANS, in which the bronchiæ are open as usual by a slit furnished with an operculum.

"The second series, or that of *ordinary fishes*, offers me, in the first place, a primary division, into those of which the maxillary bone and the palatine arch are dovetailed (*engrenés*) to the skull. Of these I make an order of PECTOGNATHS, divided into two families; the *gymnodonts* and the *scleroderms*.

"After these I have the fishes with complete jaws, but with bronchiæ which, instead of having the form of combs, as in all the others, have the form of a series of little tufts (*houppes*). Of these I again form an order, which I call LOPHOBRANCHS, which only includes one family.

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[13] p. 108.

[14] *Règne Animal*, vol. ii. p. 110.

"There then remains an innumerable quantity of fishes, to which we can no longer apply any characters except those of the exterior organs of motion. After long examination, I have found that the least bad of these characters is, after all, that employed by Ray and Artedi, taken from the nature of the first rays of the dorsal and of the anal fin. Thus ordinary fishes are divided into MALACOPTERYGIANS, of which all the rays are soft, except sometimes the first of the dorsal fin or the pectorals;—and ACANTHOPTERYGIANS, which have always the first portion of the dorsal, or of the first dorsal when there are two, supported by spinous rays, and in which the anal has also some such rays, and the ventrals, at least, each one.

"The former may be subdivided without inconvenience, according to their ventral fins, which are sometimes situate behind the abdomen, sometimes adherent to the apparatus of the shoulder, or, finally, are sometimes wanting altogether.

"We thus arrive at the three orders of ABDOMINAL MALACOPTERYGIANS, of SUBBRACHIANS, and of APODES; each of which includes some natural families which we shall explain: the first, especially, is very numerous.

"But this basis of division is absolutely impracticable with the Acanthopterygians; and the problem of establishing among these any other subdivision than that of the natural families has hitherto remained for me insoluble. Fortunately several of these families offer characters almost as precise as those which we could give to true orders.

"In truth, we cannot assign to the families of fishes, ranks as marked, as for example, to those of mammifers. Thus the Chondropterygians on the one hand hold to reptiles by the organs of the senses, and by those of generation in some; and they are related to mollusks and worms by the imperfection of the skeleton in others.

"As to Ordinary Fishes, if any part of the organization is found more developed in some than in others, there does not result from this any pre-eminence sufficiently marked, or of sufficient influence upon their whole system, to oblige us to consult it in the methodical arrangement.

"We shall place them, therefore, nearly in the order in which we have just explained their characters."

I have extracted the whole of this passage, because, though it is too technical to be understood in detail by the general reader, those who have followed with any interest the history of the attempts at a natural classification in any department in nature, will see here a fine example of the problems which such attempts propose, of the difficulcies which it may present, and of the reasonings, labors, cautions, and varied resources, by means of which its solution is sought, when a great philosophical naturalist girds himself to the task. We see here most instructively, how different the endeavor to frame such a natura system, is from the procedure of an artificial system, which carries imperatively through the whole of a class of organized beings, a system of marks either arbitrary, or conformable to natural affinities in a partial degree. And we have not often the advantage of having the reasons for a systematic arrangement so clearly and fully indicated, as is done here, and in the descriptions of the separate orders.

This arrangement Cuvier adhered to in all its main points, both in the second edition of the *Règne Animal*, published in 1821, and in his *Histoire Naturelle des Poissons*, of which the first volume was published in 1828, but which unfortunately was not completed at the time of his death. It may be supposed, therefore, to be in accordance with those views of zoological philosophy, which it was the business of his life to form and to apply; and in a work like the present, where, upon so large a question of natural history, we must be directed in a great measure by the analogy of the history of science, and by the judgments which seem most to have the character of wisdom, we appear to be justified in taking Cuvier's ichthyological system as the nearest approach which has yet been made to a natural method in that department.

The true natural method is only one: artificial methods, and even good ones, there may be many, as we have seen in botany; and each of these may have its advantages for some particular use. On some methods of this kind, on which naturalists themselves have hardly yet had time to form a stable and distinct opinion, it is not our office to decide. But judging, as I have already said, from the general analogy of the natural sciences, I find it difficult to conceive that the ichthyological method of M. Agassiz, recently propounded with an especial reference to fossil fishes, can be otherwise than an artificial method. It is founded entirely on one part of the animal, its scaly covering, and even on a single scale. It does not conform to that which almost all systematic ichthyologists hitherto have considered as a permanent natural distinction of a high order; the distinction of bony and cartilaginous fishes; for it is stated that each order contains examples of both.[15] I do not know what general anatomical or physiological

[15] Dr. Buckland's *Bridgewater Treatise*, p. 270.

truths it brings into view; but they ought to be very important and striking ones, to entitle them to supersede those which led Cuvier to his system. To this I may add, that the new ichthyological classification does not seem to form, as we should expect that any great advance towards a natural system would form, a connected sequel to the past history of ichthyology;—a step to which anterior discoveries and improvements have led, and in which they are retained.

But notwithstanding these considerations, the method of M. Agassiz has probably very great advantages for his purpose; for in the case of fossil fish, the parts which are the basis of his system often remain, when even the skeleton is gone. And we may here again refer to a principle of the classificatory sciences which we cannot make too prominent;—all arrangements and nomenclatures are good, which enable us to assert general propositions. Tried by this test, we cannot fail to set a high value on the arrangement of M. Agassiz; for propositions of the most striking generality respecting fossil remains of fish, of which geologists before had never dreamt, are enunciated by means of his groups and names. Thus only the two first orders, the *Placoïdians* and *Ganoïdians*, existed before the commencement of the cretaceous formation: the third and fourth orders, the *Ctenoïdians* and *Cycloïdians*, which contain three-fourths of the eight thousand known species of living Fishes, appear for the first time in the cretaceous formation: and other geological relations of these orders, no less remarkable, have been ascertained by M. Agassiz.

But we have now, I trust, pursued these sciences of classification sufficiently far; and it is time for us to enter upon that higher domain of Physiology to which, as we have said, Zoology so irresistibly directs us.

[2nd Ed.] [I have retained the remarks which I ventured at first to make on the System of M. Agassiz; but I believe the opinion of the most philosophical ichthyologists to be that Cuvier's System was too exclusively based on the internal skeleton, as Agassiz's was on the external skeleton. In some degree both systems have been superseded, while all that was true in each has been retained. Mr. Owen, in his *Lectures on Vertebrata* (1846), takes Cuvierian characters from the endo-skeleton, Agassizian ones from the exo-skeleton, Linnæan ones from the ventral fins, Müllerian ones from the air-bladder, and combines them by the light of his own researches, with the view of forming a system more truly natural than any preceding one.

As I have said above, naturalists, in their progress towards a Natura.

System, are guided by physiological relations, latently in Botany, but conspicuously in Zoology. From the epoch of Cuvier's *Règne Animal*, the progress of Systematic Zoology is inseparably dependent on the progress of Comparative Anatomy. Hence I have placed Cuvier's Classification of animal forms in the next Book, which treats of Physiology.]

# BOOK XVII.

## *ORGANICAL SCIENCES.*

# HISTORY OF PHYSIOLOGY

AND

# COMPARATIVE ANATOMY

Fearful and wondrous is the skill which moulds
  Our body's vital plan,
And from the first dim hidden germ unfolds
  The perfect limbs of man.
Who, who can pierce the secret? tell us how
  Something is drawn from naught,
Life from the inert mass? Who, Lord! but thou,
  Whose hand the whole has wrought?
Of this corporeal substance, still to be,
  Thine eye a survey took;
And all my members, yet unformed by thee,
  Were written in thy book.

PSALM CXXXIX. 13–16.

# INTRODUCTION.

## *Of the Organical Sciences*

THOUGH the general notion of *life* is acknowledged by the most profound philosophers to be dim and mysterious, even up to the present time; and must, in the early stages of human speculation, have been still more obscure and confused; it was sufficient, even then, to give interest and connexion to men's observations upon their own bodies and those of other animals. It was seen, that in living things, certain peculiar processes were constantly repeated, as those of breathing and of taking food, for example; and that a certain conformation of the parts of the animal was subservient to these processes; and thus were gradually formed the notions of *Function* and of *Organization*. And the sciences of which these notions formed the basis are clearly distinguishable from all those which we have hitherto considered. We conceive an *organized* body to be one in which the parts are there for the sake of the whole, in a manner different from any mechanical or chemical connexion; we conceive a *function* to be not merely a process of change, but of change connected with the general vital process. When mechanical or chemical processes occur in the living body, they are instrumental to, and directed by, the peculiar powers of life. The sciences which thus consider organization and vital functions may be termed *organical* sciences.

When men began to speculate concerning such subjects, the general mode of apprehending the process in the cases of some functions, appeared to be almost obvious; thus it was conceived that the growth of animals arose from their frame appropriating to itself a part of the substance of the food through the various passages of the body. Under the influence of such general conceptions, speculative men were naturally led to endeavor to obtain more clear and definite views of the course of each of such processes, and of the mode in which the separate parts contributed to it. Along with the observation of the living person, the more searching examination which could be carried on in the dead body, and the comparison of various kinds of animals, soon showed that this pursuit was rich in knowledge and in interest.

Moreover, besides the interest which the mere speculative faculty gave to this study, the Art of Healing added to it a great practical value; and the effects of diseases and of medicines supplied new materials and new motives for the reasonings of the philosopher.

In this manner anatomy or physiology may be considered as a science which began to be cultivated in the earliest periods of civilization. Like most other ancient sciences, its career has been one of perpetual though variable progress; and as in others, so in this, each step has implied those which had been previously made, and cannot be understood aright except we understand them. Moreover, the steps of this advance have been very many and diverse; the cultivators of anatomy have in all ages been numerous and laborious; the subject is one of vast extent and complexity; almost every generation had added something to the current knowledge of its details; and the general speculations of physiologists have been subtle, bold, and learned. It must, therefore, be difficult or impossible for a person who has not studied the science with professional diligence and professional advantages, to form just judgments of the value of the discoveries of various ages and persons, and to arrange them in their due relation to each other. To this we may add, that though all the discoveries which have been made with respect to particular functions or organizations are understood to be subordinate to one general science, the Philosophy of Life, yet the principles and doctrines of this science nowhere exist in a shape generally received and assented to among physiologists; and thus we have not, in this science, the advantage which in some others we have possessed;—of discerning the true direction of its first movements, by knowing the point to which they ultimately tend;—of running on beyond the earlier discoveries, and thus looking them in the face, and reading their true features. With these disadvantages, all that we can have to say respecting the history of Physiology must need great indulgence on the part of the reader.

Yet here, as in other cases, we may, by guiding our views by those of the greatest and most philosophical men who have made the subject their study, hope to avoid material errors. Nor can we well evade making the attempt. To obtain some simple and consistent view of the progress of physiological science, is in the highest degree important to the completion of our views of the progress of physical science. For the physiological or organical sciences form a class to which the classes already treated of, the mechanical, chemical, and classificatory sciences, are subordinate and auxiliary. Again, another circumstance which makes physiology an important part of our survey of human knowledge is, that we have here a science which is concerned, indeed, about material combinations, but in which we are led almost beyond the borders of the material world, into the region of sensation and perception, thought and will. Such a contemplation may offer some suggestions which may prepare us for the transition from physical to metaphysical speculations.

In the survey which we must, for such purposes, take of the progress of physiology, it is by no means necessary that we should exhaust the subject, and attempt to give the history of every branch of the knowledge of the phenomena and laws of living creatures. It will be sufficient, if we follow a few of the lines of such researches, which may be considered as examples of the whole. We see that life is accompanied and sustained by many processes, which at first offer themselves to our notice as separate functions, however they may afterwards be found to be connected and identified; such are feeling, digestion, respiration, the action of the heart and pulse, generation, perception, voluntary motion. The analysis of any one of these functions may be pursued separately. And since in this, as in all genuine sciences, our knowledge becomes real and scientific, only in so far as it is verified in particular facts, and thus established in general propositions, such an original separation of the subjects of research is requisite to a true representation of the growth of real knowledge. The loose hypotheses and systems, concerning the connexion of different vital faculties and the general nature of living things, which have often been promulgated, must be excluded from this part of our plan. We do not deny all value and merit to such speculations; but they cannot be admitted in the earlier stages of the history of physiology, treated of as an inductive science. If the doctrine so propounded have a solid and permanent truth, they will again come before us when we have travelled through the range of more limited truths, and are prepared to ascend with security and certainty into the higher region of general physiological principles. If they cannot be arrived at by such a road, they are then, however plausible and pleasing, no portion of that real and progressive science with which alone our history is concerned.

We proceed, therefore, to trace the establishment of some of the more limited but certain doctrines of physiology.

## CHAPTER I.

### Discovery of the Organs of Voluntary Motion.

#### *Sect.* 1.—*Knowledge of Galen and his Predecessors.*

In the earliest conceptions which men entertained of their power of moving their own members, they probably had no thought of any mechanism or organization by which this was effected. The foot and the hand, no less than the head, were seen to be endowed with life; and this pervading life seemed sufficiently to explain the power of motion in each part of the frame, without its being held necessary to seek out a special seat of the will, or instruments by which its impulses were made effective. But the slightest inspection of dissected animals showed that their limbs were formed of a curious and complex collection of cordage, and communications of various kinds, running along and connecting the bones of the skeleton. These cords and communications we now distinguish as muscles, nerves, veins, arteries, &c.; and among these, we assign to the muscles the office of moving the parts to which they are attached, as cords move the parts of a machine. Though this action of the muscles on the bones may now appear very obvious, it was, probably, not at first discerned. It is observed that Homer, who describes the wounds which are inflicted in his battles with so much apparent anatomical precision, nowhere employs the word *muscle*. And even Hippocrates of Cos, the most celebrated physician of antiquity, is held to have had no distinct conception of such an organ.[1] He always employs the word *flesh* when he means *muscle*, and the first explanation of the latter word (μῦς) occurs in a spurious work ascribed to him. For nerves, sinews, ligaments,[2] he used indiscriminately the same terms; (τόνος or νεῦρον;) and of these nerves (νεῦρα) he asserts that they contract the limbs. Nor do we find much more distinctness on this subject even in Aristotle, a generation or two later. "The origin of the νεῦρα," he says,[3] "is from the heart; they connect

[1] Sprengel, *Geschichte der Arzneikunde*, i. 382.

[2] Sprengel, *Gesch. Arz.* i. 385.

[3] *Hist. Anim.* iii 5.

the bones, and surround the joints." It is clear that he means here the muscles, and therefore it is with injustice that he has been accused of the gross error of deriving the nerves from the heart. And he is held to have really had the merit[4] of discovering the nerves of sensation, which he calls the "canals of the brain" (πόροι τοῦ εγκεφάλου); but the analysis of the mechanism of motion is left by him almost untouched. Perhaps his want of sound mechanical notions, and his constant straining after verbal generalities, and systematic classifications of the widest kind, supply the true account of his thus missing the solution of one of the simplest problems of Anatomy.

In this, however, as in other subjects, his immediate predecessors were far from remedying the deficiencies of his doctrines. Those who professed to study physiology and medicine were, for the most part, studious only to frame some general system of abstract principles, which might give an appearance of connexion and profundity to their tenets. In this manner the successors of Hippocrates became a medical school, of great note in its day, designated as the *Dogmatic* school;[5] in opposition to which arose an *Empiric* sect, who professed to deduce their modes of cure, not from theoretical dogmas, but from experience. These rival parties prevailed principally in Asia Minor aud Egypt, during the time of Alexander's successors,—a period rich in names, but poor in discoveries; and we find no clear evidence of any decided advance in anatomy, such as we are here attempting to trace.

The victories of Lucullus and Pompeius, in Greece and Asia, made the Romans acquainted with the Greek philosophy; and the consequence soon was, that shoals of philosophers, rhetoricians, poets, and physicians[6] streamed from Greece, Asia Minor, and Egypt, to Rome and Italy, to traffic their knowledge and their arts for Roman wealth. Among these, was one person whose name makes a great figure in the history of medicine, Asclepiades of Prusa in Bithynia. This man appears to have been a quack, with the usual endowments of his class; —boldness, singularity, a contemptuous rejection of all previously esteemed opinions, a new classification of diseases, a new list of medicines, and the assertion of some wonderful cures. He would not, on such accounts, deserve a place in the history of science, but that he became the founder of a new school, the *Methodic*, which professed to hold itself separate both from the Dogmatics and the Empirics.

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[4] Ib. i. 456. [5] Sprengel, *Gesch. Arz.* i. 583. [6] Sprengel, *Gesch. Ar* ii. 5

I have noticed these schools of medicine, because, though I am not able to state distinctly their respective merits in the cultivation of anatomy, a great progress in that science was undoubtedly made during their domination, of which the praise must, I conceive, be in some way divided among them. The amount of this progress we are able to estimate, when we come to the works of Galen, who flourished under the Antonines, and died about A.D. 203. The following passage from his works will show that this progress in knowledge was not made without the usual condition of laborious and careful experiment, while it implies the curious fact of such experiment being conducted by means of family tradition and instruction, so as to give rise to a *caste* of dissectors. In the opening of his Second Book *On Anatomical Manipulations*, he speaks thus of his predecessors: "I do not blame the ancients, who did not write books on anatomical manipulation; though I praise Marinus, who did. For it was superfluous for them to compose such records for themselves or others, while they were, from their childhood, exercised by their parents in dissecting, just as familiarly as in writing and reading; so that there was no more fear of their forgetting their anatomy, than of forgetting their alphabet. But when grown men, as well as children, were taught, this thorough discipline fell off; and, the art being carried out of the family of the Asclepiads, and declining by repeated transmission, books became necessary for the student."

That the general structure of the animal frame, as composed of bones and muscles, was known with great accuracy before the time of Galen, is manifest from the nature of the mistakes and deficiencies of his predecessors which he finds it necessary to notice. Thus he observes, that some anatomists have made one muscle into two, from its having two heads;—that they have overlooked some of the muscles in the face of an ape, in consequence of not skinning the animal with their own hands;—and the like. Such remarks imply that the current knowledge of this kind was tolerably complete. Galen's own views of the general mechanical structure of an animal are very clear and sound. The skeleton, he observes, discharges[7] the office of the pole of a tent, or the walls of a house. With respect to the action of the muscles, his views were anatomically and mechanically correct; in some instances, he showed what this action was, by severing the muscle.[8] He himself added considerably to the existing knowledge of

[7] *De Anatom. Administ.* i. 2.

[8] Sprengel, ii. 157.

this subject; and his discoveries and descriptions, even of very minute parts of the muscular system, are spoken of with praise by modern anatomists.[9]

We may consider, therefore, that the doctrine of the muscular system, as a collection of cords and sheets, by the contraction of which the parts of the body are moved and supported, was firmly established, and completely followed into detail, by Galen and his predecessors. But there is another class of organs connected with voluntary motion, the nerves, and we must for a moment trace the opinions which prevailed respecting these. Aristotle, as we have said, noticed some of the nerves of sensation. But Herophilus, who lived in Egypt in the time of the first Ptolemy, distinguished nerves as the organs of the will,[10] and Rufus, who lived in the time of Trajan,[11] divides the nerves into sensitive and motive, and derives them all from the brain. But this did not imply that men had yet distinguished the nerves from the muscles. Even Galen maintained that every muscle consists of a bundle of nerves and sinews.[12] But the important points, the necessity of the nerve, and the origination of all this apparatus of motion from the brain, he insists upon with great clearness and force. Thus he proved the necessity experimentally, by cutting through some of the bundles of nerves,[13] and thus preventing the corresponding motions. And it is, he says,[14] allowed by all, both physicians and philosophers, that where the origin of the nerve is, there the seat of the soul (ἡγημονικὸν τῆς ψυχῆς) must be: now this, he adds, is in the brain, and not in the heart.

Thus the general construction and arrangement of the organization by which voluntary motion is effected, was well made out at the time of Galen, and is found distinctly delivered in his works. We cannot, perhaps, justly ascribe any large portion of the general discovery to him: indeed, the conception of the mechanism of the skeleton and muscles was probably so gradually unfolded in the minds of anatomical students, that it would be difficult, even if we knew the labors of each person, to select one, as peculiarly the author of the discovery. But it is clear that all those who did materially contribute to the establishment of this doctrine, must have possessed the qualifications which we find in Galen for such a task; namely, clear mechanical views of what the

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[9] Sprengel, ii. 150. [10] Ib. i. 534. [11] Ib. ii. 67.

[12] Ibid. ii. 152. Galen, *De Motu Musc.* p. 553. [13] Ib. 157.

[14] *De Hippocr. et Plat. Dog.* viii. 1.

tensions of collections of strings could do, and an exact practical acquaintance with the muscular cordage which exists in the animal frame;—in short, in this as in other instances of real advance in science, there must have been clear ideas and real facts, unity of thought and extent of observation, brought into contact.

### *Sect. 2.—Recognition of Final Causes in Physiology. Galen.*

There is one idea which the researches of the physiologist and the anatomist so constantly force upon him, that he cannot help assuming it as one of the guides of his speculations; I mean, the idea of a *purpose*, or, as it is called in Aristotelian phrase, a *final cause*, in the arrangements of the animal frame. It is impossible to doubt that the motive nerves run along the limbs, *in order that* they may convey to the muscles the impulses of the will; and that the muscles are attached to the bones, *in order that* they may move and support them. This conviction prevails so steadily among anatomists, that even when the use of any part is altogether unknown, it is still taken for granted that it has some use. The developement of this conviction,—of a purpose in the parts of animals,—of a function to which each portion of the organization is subservient,—contributed greatly to the progress of physiology; for it constantly urged men forwards in their researches respecting each organ, till some definite view of its purpose was obtained. The assumption of hypothetical final causes in Physics may have been, as Bacon asserts it to have been, prejudicial to science; but the assumption of unknown final causes in Physiology, has given rise to the science. The two branches of speculation, Physics and Physiology, were equally led, by every new phenomenon, to ask their question, "Why?" But, in the former case, "why" meant "through what cause?" in the latter, "for what end?" And though it may be possible to introduce into physiology the doctrine of efficient causes, such a step can never obliterate the obligations which the science owes to the pervading conception of a purpose contained in all organization.

This conception makes its appearance very early. Indeed, without any special study of our structure, the thought, that we are fearfully and wonderfully made, forces itself upon men, with a mysterious impressiveness, as a suggestion of our Maker. In this bearing, the thought is developed to a considerable extent in the well-known passage in Xenophon's *Conversations of Socrates*. Nor did it ever lose its hold on sober-minded and instructed men. The Epicureans, indeed, held that the eye was not made for seeing, nor the ear for hearing; and Asclepiades, whom we have already mentioned as an impudent pretender, adopted this wild dogma.[15] Such assertions required no labor. "It is easy," says Galen,[16] "for people like Asclepiades, when they come to any difficulty, to say that Nature has worked to no purpose." The great anatomist himself pursues his subject in a very different temper. In a well-known passage, he breaks out into an enthusiastic scorn of the folly of the atheistical notions.[17] "Try," he says, "if you can imagine a shoe made with half the skill which appears in the skin of the foot." Some one had spoken of a structure of the human body which he would have preferred to that which it now has. "See," Galen exclaims, after pointing out the absurdity of the imaginary scheme, "see what brutishness there is in this wish. But if I were to spend more words on such cattle, reasonable men might blame me for desecrating my work, which I regard as a religious hymn in honor of the Creator."

Galen was from the first highly esteemed as an anatomist. He was originally of Pergamus; and after receiving the instructions of many medical and philosophical professors, and especially of those of Alexandria, which was then the metropolis of the learned and scientific world, he came to Rome, where his reputation was soon so great as to excite the envy and hatred of the Roman physicians. The emperors Marcus Aurelius and Lucius Verus would have retained him near them; but he preferred pursuing his travels, directed principally by curiosity. When he died, he left behind him numerous works, all of them of great value for the light they throw on the history of anatomy and medicine; and these were for a long period the storehouse of all the most important anatomical knowledge which the world possessed. In the time of intellectual barrenness and servility, among the Arabians and the Europeans of the dark ages, the writings of Galen had almost unquestioned authority;[18] and it was only by an uncommon effort of independent thinking that Abdollatif ventured to assert, that even Galen's assertions must give way to the evidence of the senses. In more modern times, when Vesalius, in the sixteenth century, accused Galen of mistakes, he drew upon himself the hostility of the whole body of physicians. Yet the mistakes were such as might have

[15] Sprengel, ii. 15.

[16] *De Usu Part.* v. 5, (on the kidneys.)

[17] *De Usu Part.* iii. 10.

[18] Sprengel, ii. 359.

been pointed out and confessed[19] without acrimony, if, in times of revolution, mildness and moderation were possible; but an impatience of the superstition of tradition on the part of the innovators, and an alarm of the subversion of all recognized truths on the part of the established teachers, inflame and pervert all such discussions. Vesalius's main charge against Galen is, that his dissections were performed upon animals, and not upon the human body. Galen himself speaks of the dissection of apes as a very familiar employment, and states that he killed them by drowning. The natural difficulties which, in various ages, have prevented the unlimited prosecution of human dissection, operated strongly among the ancients, and it would have been difficult, under such circumstances, to proceed more judiciously than Galen did.

I shall now proceed to the history of the discovery of another and less obvious function, the circulation of the blood, which belongs to modern times.

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## CHAPTER II.

### Discovery of the Circulation of the Blood.

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#### *Sect.* 1.—*Prelude to the Discovery.*

THE blood-vessels, the veins and arteries, are as evident and peculiar in their appearance as the muscles; but their function is by no means so obvious. Hippocrates[1] did not discriminate Veins and Arteries; both are called by the same name (φλέβες); and the word from which *artery* comes (ἀρτηρίη) means, in his works, the windpipe. Aristotle, scanty as was his knowledge of the vessels of the body, has yet the merit of having traced the origin of all the veins to the heart. He expressly contradicts those of his predecessors who had derived the veins from the head;[2] and refers to dissection for the proof. If the book *On the Breath* be genuine (which is doubted), Aristotle was aware of the distinction between veins and arteries. "Every artery,"

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[19] Cuv. *Leçons sur l'Hist. des Sc. Nat.* p. 25.

[1] Sprengel, i. 383.

[2] *Hist. Animal.* iii. 3.

it is there asserted, "is accompanied by a vein; the former are filled only with breath or air."[3] But whether or no this passage be Aristotle's, he held opinions equally erroneous; as, that the windpipe conveys air into the heart.[4] Galen[5] was far from having views respecting the blood-vessels, as sound as those which he entertained concerning the muscles. He held the liver to be the origin of the veins, and the heart of the arteries. He was, however, acquainted with their junctions, or *anastomoses.* But we find no material advance in the knowledge of this subject, till we overleap the blank of the middle ages, and reach the dawn of modern science.

The father of modern anatomy is held to be Mondino,[6] who dissected and taught at Bologna in 1315. Some writers have traced in him the rudiments of the doctrine of the circulation of the blood; for he says that the heart transmits blood to the lungs. But it is allowed, that he afterwards destroys the merit of his remark, by repeating the old assertion that the left ventricle ought to contain spirit or air, which it generates from the blood.

Anatomy was cultivated with great diligence and talent in Italy by Achillini, Carpa, and Messa, and in France by Sylvius and Stephanus (Dubois and Etienne). Yet still these empty assumptions respecting the heart and blood-vessels kept their ground. Vesalius, a native of Brussels, has been termed the founder of human anatomy, and his great work *De Humani Corporis Fabricâ* is, even yet, a splendid monument of art, as well as science. It is said that his figures were designed by Titian; and if this be not exactly true, says Cuvier,[7] they must, at least, be from the pencil of one of the most distinguished pupils of the great painter; for to this day, though we have more finished drawings, we have no designs that are more artistlike. Fallopius, who succeeded Vesalius at Padua, made some additions to the researches of his predecessor; but in his treatise *De Principio Venarum*, it is clearly seen[8] that the circulation of the blood was unknown to him. Eustachius also, whom Cuvier groups with Vesalius and Fallopius, as the three great founders of modern anatomy, wrote a treatise on the vein *azygos*[9] which is a little treatise on comparative anatomy; but the discovery of the functions of the veins came from a different quarter.

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[3] *De Spiritu*, v. 1078. [4] Spr. i. 501. [5] Ib. ii. 152.
[6] *Encyc. Brit.* 692. Anatomy. [7] *Leçons sur l'Hist. des Sc. Nat.* p. 21.
[8] Cuv. *Sc. Nat.* p. 32. [9] Ib. p. 34

The unfortunate Servetus, who was burnt at Geneva as a heretic in 1553, is the first person who speaks distinctly of the small circulation, or that which carries the blood from the heart to the lungs, and back again to the heart. His work entitled *Christianismi Restitutio* was also burnt; and only two copies are known to have escaped the flames. It is in this work that he asserts the doctrine in question, as a collateral argument or illustration of his subject. "The communication between the right and left ventricle of the heart, is made," he says, "not as is commonly believed, through the partition of the heart, but by a remarkable artifice (*magno artificio*) the blood is carried from the right ventricle by a long circuit through the lungs; is elaborated by the lungs, made yellow, and transfused from the *vena arteriosa* into the *arteria venosa*." This truth is, however, mixed with various of the traditional fancies concerning the "*vital spirit*, which has its origin in the left ventricle." It may be doubted, also, how far Servetus formed his opinion upon conjecture, and on a hypothetical view of the formation of this vital spirit. And we may, perhaps, more justly ascribe the real establishment of the pulmonary circulation as an inductive truth, to Realdus Columbus, a pupil and successor of Vesalius at Padua, who published a work *De Re Anatomica* in 1559, in which he claims this discovery as his own.[10]

Andrew Cæsalpinus, who has already come under our notice as one of the fathers of modern inductive science, both by his metaphysical and his physical speculations, described the pulmonary circulation still more completely in his *Quæstiones Peripateticæ*, and even seemed to be on the eve of discovering the great circulation; for he remarked the swelling of veins below ligatures, and inferred from it a refluent motion of blood in these vessels.[11] But another discovery of structure was needed, to prepare the way for this discovery of function; and this was made by Fabricius of Acquapendente, who succeeded in the grand list of great professors at Padua, and taught there for fifty years.[12] Sylvius had discovered the existence of the valves of the veins; but Fabricius remarked that they are all turned towards the heart. Combining this disposition with that of the valves of the heart, and with the absence of valves in the arteries, he might have come to the conclusion[13] that the blood moves in a different direction in the arteries and in the veins, and might thus have discovered the circulation: but this glory was reserved for William Harvey: so true

[10] *Encyc. Brit.* [11] Ib. [12] Cuv. p. 44. [13] p. 45.

is it, observes Cuvier, that we are often on the brink of a discovery without suspecting that we are so;—so true is it, we may add, that a certain succession of time and of persons is generally necessary to familiarize men with one thought, before they can advance to that which is the next in order.

### *Sect. 2.—The Discovery of the Circulation made by Harvey.*

WILLIAM HARVEY was born in 1578, at Folkestone in Kent.[14] He first studied at Cambridge: he afterwards went to Padua, where the celebrity of Fabricius of Acquapendente attracted from all parts those who wished to be instructed in anatomy and physiology. In this city, excited by the discovery of the valves of the veins, which his master had recently made, and reflecting on the direction of the valves which are at the entrance of the veins into the heart, and at the exit of the arteries from it, he conceived the idea of making experiments, in order to determine what is the course of the blood in its vessels. He found that when he tied up veins in various animals, they swelled below the ligature, or in the part furthest from the heart; while arteries, with a like ligature, swelled on the side next the heart. Combining these facts with the direction of the valves, he came to the conclusion that the blood is impelled, by the left side of the heart, in the arteries to the extremities, and thence returns by the veins into the right side of the heart. He showed, too, how this was confirmed by the phenomena of the pulse, and by the results of opening the vessels. He proved, also, that the circulation of the lungs is a continuation of the larger circulation; and thus the whole doctrine of the double circulation was established.

Harvey's experiments had been made in 1616 and 1618; it is commonly said that he first promulgated his opinion in 1619; but the manuscript of the lectures, delivered by him as lecturer to the College of Physicians, is extant in the British Museum, and, containing the propositions on which the doctrine is founded, refers them to April, 1616. It was not till 1628 that he published, at Frankfort, his *Exercitatio Anatomica de Motu Cordis et Sanguinis;* but he there observes that he had for above nine years confirmed and illustrated his opinion in his lectures, by arguments grounded upon ocular demonstrations.

[14] Cuv. p. 51.

### *Sect. 3.—Reception of the Discovery.*

Without dwelling long upon the circumstances of the general reception of this doctrine, we may observe that it was, for the most part, readily accepted by his countrymen, but that abroad it had to encounter considerable opposition. Although, as we have seen, his predecessors had approached so near to the discovery, men's minds were by no means as yet prepared to receive it. Several physicians denied the truth of the opinion, among whom the most eminent was Riolan, professor at the Collège de France. Other writers, as usually happens in the case of great discoveries, asserted that the doctrine was ancient, and even that it was known to Hippocrates. Harvey defended his opinion with spirit and temper; yet he appears to have retained a lively recollection of the disagreeable nature of the struggles in which he was thus involved. At a later period of his life, Ent,[15] one of his admirers, who visited him, and urged him to publish the researches on generation, on which he had long been engaged, gives this account of the manner in which he received the proposal: "And would you then advise me, (smilingly replies the doctor,) to quit the tranquillity of this haven, wherein I now calmly spend my days, and again commit myself to the unfaithful ocean? You are not ignorant how great troubles my lucubrations, formerly published, have raised. Better it is, certainly, at some time, to endeavor to grow wise at home in private, than by the hasty divulgation of such things to the knowledge whereof you have attained with vast labor, to stir up tempests that may deprive you of your leisure and quiet for the future."

His merits were, however, soon generally recognized. He was[16] made physician to James the First, and afterwards to Charles the First, and attended that unfortunate monarch in the civil war. He had the permission of the parliament to accompany the king on his leaving London; but this did not protect him from having his house plundered in his absence, not only of its furniture, but, which he felt more, of the records of his experiments. In 1652, his brethren of the College of Physicians placed a marble bust of him in their hall, with an inscription recording his discoveries; and two years later, he was nominated to the office of President of the College, which however he

[15] Epist. Dedic. to *Anatom. Exercit.*

[16] *Biog. Brit.*

declined in consequence of his age and infirmities. His doctrine soon acquired popular currency; it was, for instance, taken by Descartes[17] as the basis of his physiology in his work *On Man;* and Harvey had the pleasure, which is often denied to discoverers, of seeing his discovery generally adopted during his lifetime.

### *Sect. 4.—Bearing of the Discovery on the Progress of Physiology.*

In considering the intellectual processes by which Harvey's discoveries were made, it is impossible not to notice, that the recognition of a creative purpose, which, as we have said, appears in all sound physiological reasonings, prevails eminently here. "I remember," says Boyle, "that when I asked our famous Harvey what were the things that induced him to think of a circulation of the blood, he answered me, that when he took notice that the valves in the veins of so many parts of the body were so placed, that they gave a free passage to the blood towards the heart, but opposed the passage of the venal blood the contrary way; he was incited to imagine that so provident a cause as Nature had not placed so many valves without design; and no design seemed more probable than that the blood should be sent through the arteries, and return through the veins, whose valves did not oppose its course that way."

We may notice further, that this discovery implied the usual conditions, distinct general notions, careful observation of many facts, and the mental act of bringing together these elements of truth. Harvey must have possessed clear views of the motions and pressures of a fluid circulating in ramifying tubes, to enable him to see how the position of valves, the pulsation of the heart, the effects of ligatures, of bleeding, and of other circumstances, ought to manifest themselves in order to confirm his view. That he referred to a multiplied and varied experience for the evidence that it was so confirmed, we have already said. Like all the best philosophers of his time, he insists rigidly upon the necessity of such experience. "In every science," he says,[18] "be it what it will, a diligent observation is requisite, and sense itself must be frequently consulted. We must not rely upon other men's experience, but our own, without which no man is a proper disciple of any part of natural knowledge." And by publishing his experiments, he trusts, he adds, that he has enabled his reader "to be an equitable

[17] Cuv. 53.

[18] *Generation of Animals*, Pref.

umpire between Aristotle and Galen;" or rather, he might have said, to see how, in the promotion of science, sense and reason. observation and invention, have a mutual need of each other.

We may observe further, that though Harvey's glory, in the case now before us, rested upon his having proved the reality of certain mechanical movements and actions in the blood, this discovery, and all other physiological truths, necessarily involved the assumption of some peculiar agency belonging to living things, different both from mechanical agency, and from chemical; and in short, something *vital*, and not physical merely. For when it was seen that the pulsation of the heart, its *systole* and *diastole*, caused the circulation of the blood, it might still be asked, what force caused this constantly-recurring contraction and expansion. And again, circulation is closely connected with respiration; the blood is, by the circulation, carried to the lungs, and is there, according to the expression of Columbus and Harvey, mixed with air. But by what mechanism does this *mixture* take place, and what is the real nature of it? And when succeeding researches had enabled physiologists to give an answer to this question, as far as chemical relations go, and to say, that the change consists in the abstraction of the carbon from the blood by means of the oxygen of the atmosphere; they were still only led to ask further, how this chemical change was effected, and how such a change of the blood fitted it for its uses. Every function of which we explain the course, the mechanism, or the chemistry, is connected with other functions,—is subservient to them, and they to it; and all together are parts of the general vital system of the animal, ministering to its life, but deriving their activity from the life. Life is not a collection of forces, or polarities, or affinities, such as any of the physical or chemical sciences contemplate; it has powers of its own, which often supersede those subordinate relations; and in the cases where men have traced such agents in the animal frame, they have always seen, and usually acknowledged, that these agents were ministerial to some higher agency, more difficult to trace than these, but more truly the cause of the phenomena.

The discovery of the mechanical and chemical conditions of the vital functions, as a step in physiology, may be compared to the discovery of the laws of phenomena in the heavens by Kepler and his predecessors, while the discovery of the force by which they were produced was still reserved in mystery for Newton to bring to light. The subordinate relation of the facts, their dependance on space and time, their reduction to order and cycle, had been fully performed; but the reference of them to distinct ideas of causation, their interpretation as the results of mechanical force, was omitted or attempted in vain. The very notion of such Force, and of the manner in which motions were determined by it, was in the highest degree vague and vacillating; and a century was requisite, as we have seen, to give to the notion that clearness and fixity which made the Mechanics of the Heavens a possible science. In like manner, the notion of Life, and of Vital Forces, is still too obscure to be steadily held. We cannot connect it distinctly with severe inductions from facts. We can trace the motions of the animal fluids as Kepler traced the motions of the planets; but when we seek to render a reason for these motions, like him, we recur to terms of a wide and profound, but mysterious import; to Virtues, Influences, undefined Powers. Yet we are not on this account to despair. The very instance to which I am referring shows us how rich is the promise of the future. Why, says Cuvier,[19] may not Natural History one day have its Newton? The idea of the vital forces may gradually become so clear and definite as to be available in science; and future generations may include, in their physiology, propositions elevated as far above the circulation of the blood, as the doctrine of universal gravitation goes beyond the explanation of the heavenly motions by epicycles.

If, by what has been said, I have exemplified sufficiently the nature of those steps in physiology, which, like the discovery of the Circulation, give an explanation of the process of some of the animal functions, it is not necessary for me to dwell longer on the subject; for to write a history, or even a sketch of the history of Physiology, would suit neither my powers nor my purpose. Some further analysis of the general views which have been promulgated by the most eminent physiologists, may perhaps be attempted in treating of the Philosophy of Inductive Science; but the estimation of the value of recent speculations and investigations must be left to those who have made this vast subject the study of their lives. A few brief notices may, however, be here introduced.

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[19] *Ossem. Foss.* Introd.

# CHAPTER III.

## Discovery of the Motion of the Chyle, and consequent Speculations.

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### *Sect.* 1.—*The Discovery of the Motion of the Chyle.*

IT may have been observed in the previous course of this History of the Sciences, that the discoveries in each science have a peculiar physiognomy: something of a common type may be traced in the progress of each of the theories belonging to the same department of knowledge. We may notice something of this common form in the various branches of physiological speculation. In most, or all of them, we have, as we have noticed the case to be with respect to the circulation of the blood, clear and certain discoveries of mechanical and chemical processes, succeeded by speculations far more obscure, doubtful, and vague, respecting the relation of these changes to the laws of life. This feature in the history of physiology may be further instanced, (it shall be done very briefly), in one or two other cases. And we may observe, that the lesson which we are to collect from this narrative, is by no means that we are to confine ourselves to the positive discovery, and reject all the less clear and certain speculations. To do this, would be to lose most of the chances of ulterior progress; for though it may be, that our conceptions of the nature of organic life are not yet sufficiently precise and steady to become the guides to positive inductive truths, still the only way in which these peculiar physiological ideas can be made more distinct and precise, and thus brought more nearly into a scientific form, is by this struggle with our ignorance or imperfect knowledge. This is the lesson we have learnt from the history of physical astronomy and other sciences. We must strive to refer facts which are known and understood, to higher principles, of which we cannot doubt the existence, and of which, in some degree, we can see the place; however dim and shadowy may be the glimpses we have hitherto been able to obtain of their forms. We may often fail in such attempts, but without the attempt we can never succeed.

That the food is received into the stomach, there undergoes a change of its consistence, and is then propelled along the intestines, are obvious facts in the animal economy. But a discovery made in the course of the seventeenth century brought into clearer light the sequel of this series of processes, and its connexion with other functions. In the year 1622, Asellius or Aselli[1] discovered certain minute vessels, termed *lacteals*, which absorb a white liquid (the *chyle*) from the bowels, and pour it into the blood. These vessels had, in fact, been discovered by Eristratus, in the ancient world,[2] in the time of Ptolemy; but Aselli was the first modern who attended to them. He described them in a treatise entitled *De Venis Lacteis, cum figuris elegantissimis*, printed at Milan in 1627, the year after the death of the author. The work is remarkable as the first which exhibits *colored* anatomical figures; the arteries and veins are represented in red, the lacteals in black.

Eustachius,[3] at an earlier period, had described (in the horse) the thoracic duct by which the chyle is poured into the subclavian vein, on the right side of the neck. But this description did not excite so much notice as to prevent its being forgotten, and rediscovered in 1550, after the knowledge of the circulation of the blood had given more importance to such a discovery. Up to this time,[4] it had been supposed that the lacteals carried the chyle to the liver, and that the blood was manufactured there. This opinion had prevailed in all the works of the ancients and moderns; its falsity was discovered by Pecquet, a French physician, and published in 1651, in his *New Anatomical Experiments;* in which are discovered a receptacle of the chyle, unknown till then, and the vessel which conveys it to the subclavian vein. Pecquet himself, and other anatomists, soon connected this discovery with the doctrine, then recently promulgated, of the circulation of the blood. In 1665, these vessels, and the *lymphatics* which are connected with them, were further illustrated by Ruysch in his exhibition of their valves. (*Dilucidatio valvularum in vasis lymphaticis et lacteis.*)

### *Sect. 2.—The Consequent Speculations. Hypotheses of Digestion.*

Thus it was shown that aliments taken into the stomach are, by its action, made to produce *chyme;* from the chyme, gradually changed

[1] Mayo, *Physiology*, p. 156.
[2] Cuv. *Hist. Sc.* p. 50.
[3] Cuv. *Hist.* p. 34.
[4] Ib. p. 365.

in its progress through the intestines, *chyle* is absorbed by the lacteals; and this, poured into the blood by the thoracic duct, repairs the waste and nourishes the growth of the animal. But by what powers is the food made to undergo these transformations? Can we explain them on mechanical or on chemical principles? Here we come to a part of physiology less certain than the discovery of vessels, or of the motion of fluids. We have a number of opinions on this subject, but no universally acknowledged truth. We have a collection of *Hypotheses of Digestion* and *Nutrition.*

I shall confine myself to the former class; and without dwelling long upon these, I shall mention some of them. The philosophers of the Academy *del Cimento*, and several others, having experimented on the stomach of gallinaceous birds, and observed the astonishing force with which it breaks and grinds substances, were led to consider the digestion which takes place in the stomach as a kind of *trituration.*[5] Other writers thought it was more properly described as *fermentation;* others again spoke of it as a *putrefaction.* Varignon gave a merely physical account of the first part of the process, maintaining that the division of the aliments was the effect of the disengagement of the *air* introduced into the stomach, and dilated by the heat of the body. The opinion that digestion is a *solution* of the food by the gastric juice has been more extensively entertained.

Spallanzani and others made many experiments on this subject. Yet it is denied by the best physiologists, that the changes of digestion can be adequately represented as chemical changes only. The nerves of the stomach (the *pneumo-gastric*) are said to be essential to digestion. Dr. Wilson Philip has asserted that the influence of these nerves, when they are destroyed, may be replaced by a galvanic current.[6] This might give rise to a supposition that digestion depends on galvanism. Yet we cannot doubt that all these hypotheses,—mechanical, physical, chemical, galvanic—are altogether insufficient. "The stomach must have," as Dr. Prout says,[7] "the power of organiz-

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[5] Bourdon, *Physiol. Comp.* p. 514.

[6] Müller (*Manual of Physiology*, B. iii. Sect. 1, Chap. iii.) speaks of Dr. Wilson Philip's assertion that the nerves of the stomach being cut, and a galvanic current kept up in them, digestion is still accomplished. He states that he and other physiologists have repeated such experiments on an extensive scale, and have found no effect of this kind.

[7] *Bridgewater Tr.* p. 493

ing and vitalizing the different elementary substances. It is impossible to imagine that this organizing agency of the stomach can be chemical. This agency is *vital*, and its nature completely unknown."

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## CHAPTER IV.

### Examination of the Process of Reproduction in Animals and Plants, and Consequent Speculations.

#### *Sect.* 1.—*The Examination of the Process of Reproduction in Animals.*

IT would not, perhaps, be necessary to give any more examples of what has hitherto been the general process of investigations on each branch of physiology; or to illustrate further the combination which such researches present, of certain with uncertain knowledge;—of solid discoveries of organs and processes, succeeded by indefinite and doubtful speculation concerning vital forces. But the reproduction of organized beings is not only a subject of so much interest as to require some notice, but also offers to us laws and principles which include both the vegetable and the animal kingdom; and which, therefore, are requisite to render intelligible the most general views to which we can attain, respecting the world of organization.

The facts and laws of reproduction were first studied in detail in animals. The subject appears to have attracted the attention of some of the philosophers of antiquity in an extraordinary degree: and indeed we may easily imagine that they hoped, by following this path, if any, to solve the mystery of creation. Aristotle appears to have pursued it with peculiar complacency; and his great work *On animals* contains[1] an extraordinary collection of curious observations relative to this subject. He had learnt the modes of reproduction of most of the animals with which he was acquainted; and his work is still, as a writer of our own times has said,[2] "original after so many copies, and young after two thousand years." His observations referred principally to the external circumstances of generation: the anatomical examination was

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[1] Bourdon, p. 161.

[2] Ib. p. 101.

left to his successors. Without dwelling on the intermediate labors, we come to modern times, and find that this examination owes its greatest advance to those who had the greatest share in the discovery of the circulation of the blood;—Fabricius of Acquapendente, and Harvey. The former[3] published a valuable work on the Egg and the Chick. In this are given, for the first time, figures representing the developement of the chick, from its almost imperceptible beginning, to the moment when it breaks the shell. Harvey pursued the researches of his teacher. Charles[4] the First had supplied him with the means of making the experiments which his purpose required, by sacrificing a great number of the deer in Windsor Park in the state of gestation: but his principal researches were those respecting the egg, in which he followed out the views of Fabricius. In the troubles which succeeded the death of the unfortunate Charles the house of Harvey was pillaged; and he lost the whole of the labors he had bestowed on the generation of insects. His work, *Exercitationes de Generatione Animalium*, was published at London in 1651; it is more detailed and perfect than that of Fabricius; but the author was prevented by the unsettled condition of the country from getting figures engraved to accompany his descriptions.

Many succeeding anatomists pursued the examination of the series of changes in generation, and of the organs which are concerned in them, especially Malpighi, who employed the microscope in this investigation, and whose work on the Chick was published in 1673. It is impossible to give here any general view of the result of these laborious series of researches: but we may observe, that they led to an extremely minute and exact survey of all the parts of the fœtus, its envelopes and appendages, and, of course, to a designation of these by appropriate names. These names afterwards served to mark the attempts which were made to carry the analogy of animal generation into the vegetable kingdom.

There is one generalization of Harvey which deserves notice.[5] He was led by his researches to the conclusion, that all living things may be properly said to come from eggs: "Omne vivum ex ovo." Thus not only do oviparous animals produce by means of eggs, but in those which are viviparous, the process of generation begins with the developement of a small vesicle, which comes from the ovary, and which exists before the embryo: and thus viviparous or suckling-beasts, not-

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[3] Cuv. *Hist. Sc. Nat.* p. 46. [4] Ib. p. 53. [5] Exerc. lxiii.

withstanding their name, are born from eggs, as well as birds, fishes, and reptiles.[6] This principle also excludes that supposed production of organized beings without parents (of worms in corrupted matter, for instance,) which was formerly called *spontaneous generation;* and the best physiologists of modern times agree in denying the reality of such a mode of generation.[7]

## *Sect. 2.—The Examination of the Process of Reproduction in Vegetables.*

The extension of the analogies of animal generation to the vegetable world was far from obvious. This extension was however made;—with reference to the embryo plant, principally by the microscopic observers, Nehemiah Grew, Marcello Malpighi, and Antony Leeuwenhoek;—with respect to the existence of the sexes, by Linnæus and his predecessors.

The microscopic labors of Grew and Malpighi were patronized by the Royal Society of London in its earliest youth. Grew's book, *The Anatomy of Plants*, was ordered to be printed in 1670. It contains plates representing extremely well the process of germination in various seeds, and the author's observations exhibit a very clear conception of the relation and analogies of different portions of the seed. On the day on which the copy of this work was laid before the Society, a communication from Malpighi of Bologna, *Anatomes Plantarum Idea*, stated his researches, and promised figures which should illustrate them. Both authors afterwards went on with a long train of valuable observations, which they published at various times, and which contain much that has since become a permanent portion of the science.

Both Grew and Malpighi were, as we have remarked, led to apply to vegetable generation many terms which imply an analogy with the generation of animals. Thus, Grew terms the innermost coat of the seed, the *secundine;* speaks of the *navel-fibres*, &c. Many more such terms have been added by other writers. And, as has been observed by a modern physiologist,[8] the resemblance is striking. Both in the vegetable seed and in the fertilized animal egg, we have an *embryo*, *chalazæ*, a *placenta*, an *umbilical cord*, a *cicatricula*, an *amnios*, *membranes*, *nourishing vessels*. The *cotyledons* of the seed are the equivalent of the *vitellus* of birds, or of the *umbilical vesicle* of sucking-beasts:

[6] Bourdon, p. 221. [7] Ib. p. 49. [8] Ib. p. 384.

the *albumen* or *perisperm* of the grain is analogous to the *white of the egg* of birds, or the *allantoid* of viviparous animals.

*Sexes of Plants.*—The attribution of sexes to plants, is a notion which was very early adopted; but only gradually unfolded into distinctness and generality.[9] The ancients were acquainted with the fecundation of vegetables. Empedocles, Aristotle, Theophrastus, Pliny, and some of the poets, make mention of it; but their notions were very incomplete, and the conception was again lost in the general shipwreck of human knowledge. A Latin poem, composed in the fifteenth century by Jovianus Pontanus, the preceptor of Alphonso, King of Naples, is the first modern work in which mention is made of the sex of plants. Pontanus sings the loves of two date-palms, which grew at the distance of fifteen leagues from each other: the male at Brundusium, the female at Otranto. The distance did not prevent the female from becoming fruitful, as soon as the palms had raised their heads above the surrounding trees, so that nothing intervened directly between them, or, to speak with the poet, so that they were able to see each other.

Zaluzian, a botanist who lived at the end of the fifteenth century, says that the greater part of the species of plants are *androgynes*, that is, have the properties of the male and of the female united in the same plant; but that some species have the two sexes in separate individuals; and he adduces a passage of Pliny relative to the fecundation of the date-palm. John Bauhin, in the middle of the seventeenth century, cites the expressions of Zaluzian; and forty years later, a professor of Tübingen, Rudolph Jacob Camerarius, pointed out clearly the organs of generation, and proved by experiments on the mulberry, on maize, and on the plant called Mercury (*mercurialis*), that when by any means the action of the stamina upon the pistils is intercepted, the seeds are barren. Camerarius, therefore, a philosopher in other respects of little note, has the honor assigned him of being the author of the discovery of the sexes of plants in modern times.[10]

The merit of this discovery will, perhaps, appear more considerable when it is recollected that it was rejected at first by very eminent botanists. Thus Tournefort, misled by insufficient experiments, maintained that the stamina are excretory organs; and Reaumur, at the beginning of the eighteenth century, inclined to the same doctrine.

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[9] Mirbel, *El.* ii. 538.

[10] Mirbel, ii. 539

Upon this, Geoffroy, an apothecary at Paris, scrutinized afresh the sexual organs; he examined the various forms of the pollen, already observed by Grew and Malpighi; he pointed out the excretory canal, which descends through the style, and the *micropyle*, or minute orifice in the coats of the ovule, which is opposite to the extremity of this canal; though he committed some mistakes with regard to the nature of the pollen. Soon afterwards, Sebastian Vaillant, the pupil of Tournefort, but the corrector of his error on this subject, explained in his public lectures the phenomenon of the fecundation of plants, described the explosion of the anthers, and showed that the *florets* of composite flowers, though formed on the type of an *androgynous* flower, are sometimes male, sometimes female, and sometimes neuter.

But though the sexes of plants had thus been noticed, the subject drew far more attention when Linnæus made the sexual parts the basis of his classification. Camerarius and Burkard had already entertained such a thought, but it was Linnæus who carried into effect, and thus made the notion of the sexes of vegetables almost as familiar to us as that of the sexes of animals.

### *Sect. 3.—The Consequent Speculations.—Hypotheses of Generation.*

The views of the processes of generation, and of their analogies throughout the whole of the organic world, which were thus established and diffused, form an important and substantial part of our physiological knowledge. That a number of curious but doubtful hypotheses should be put forward, for the purpose of giving further significance and connexion to these discoveries, was to be expected. We must content ourselves with speaking of these very briefly. We have such hypotheses in the earliest antiquity of Greece; for as we have already said, the speculations of cosmogony were the source of the Greek philosophy; and the laws of generation appeared to offer the best promise of knowledge respecting the mystery of creation. Hippocrates explained the production of a new animal by the *mixture of seed* of the parents; and the offspring was male or female as the seminal principle of the father or of the mother was the more powerful. According to Aristotle, the mother supplied the *matter*, and the father the *form*. Harvey's doctrine was, that the ovary of the female is fertilized by a *seminal contagion* produced by the seed of the male. But an opinion which obtained far more general reception was, that the *embryo pre-existed* in the mother, before any union of the sexes. It is easy to see that this doctrine is accompanied with great difficulties;[12] for if the mother, at the beginning of life, contain in her the embryos of all her future children; these embryos again must contain the children which they are capable of producing; and so on indefinitely; and thus each female of each species contains in herself the germs of infinite future generations. The perplexity which is involved in this notion of an endless series of creatures, thus encased one within another, has naturally driven inquirers to attempt other suppositions. The microscopic researches of Leeuwenhoek and others led them to the belief that there are certain animalcules contained in the seed of the male, which are the main agents in the work of reproduction. This system ascribes almost everything to the male, as the one last mentioned does to the female. Finally, we have the system of Buffon;—the famous hypothesis of *organic molecules.* That philosopher asserted that he found, by the aid of the microscope, all nature full of moving globules, which he conceived to be, not animals as Leeuwenhoek imagined, but bodies capable of producing, by their combination, either animals or vegetables, in short, all organized bodies. These globules he called *organic molecules.*[13] And if we inquire how these organic molecules, proceeding from all parts of the two parents, unite into a whole, as perfect as either of the progenitors, Buffon answers, that this is the effect of the *interior mould;* that is, of a system of internal laws and tendencies which determine the form of the result as an external mould determines the shape of the cast.

An admirer of Buffon, who has well shown the untenable character of this system, has urged, as a kind of apology for the promulgation of the hypothesis,[14] that at the period when its author wrote, he could not present his facts with any hope of being attended to, if he did not connect them by some common tie, some dominant idea which might gratify the mind; and that, acting under this necessity, he did well to substitute for the extant theories, already superannuated and confessedly imperfect, conjectures more original and more probable. Without dissenting from this view, we may observe, that Buffon's theory, like those which preceded it, is excusable, and even deserving of admiration, so far as it groups the facts consistently; because in doing this, it exhibits the necessity, which the physiological speculator ought to feel, of aspiring to definite and solid general principles; and that thus, though

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[11] Bourdon, p. 204. [12] Ib. p 209. [13] Ib. p. 219. [14] Ib. p. 221.

the theory may not be established as true, it may be useful by bringing into view the real nature and application of such principles.

It is, therefore, according to our views, unphilosophical to derive despair, instead of hope, from the imperfect success of Buffon and his predecessors. Yet this is what is done by the writer to whom we refer. "For me," says he,[15] "I vow that, after having long meditated on the system of Buffon,—a system so remarkable, so ingenious, so well matured, so wonderfully connected in all its parts, at first sight so probable;—I confess that, after this long study, and the researches which it requires, I have conceived in consequence, a distrust of myself, a skepticism, a disdain of hypothetical systems, a decided predilection and exclusive taste for pure and rational observation, in short, a disheartening, which I had never felt before."

The best remedy of such feelings is to be found in the history of science. Kepler, when he had been driven to reject the solid epicycles of the ancients, or a person who had admired Kepler as M. Bourdon admires Buffon, but who saw that his magnetic virtue was an untenable fiction, might, in the same manner, have thrown up all hope of a sound theory of the causes of the celestial motions. But astronomers were too wise and too fortunate to yield to such despondency. The predecessors of Newton substituted a solid science of Mechanics for the vague notions of Kepler; and the time soon came when Newton himself reduced the motions of the heavens to a Law as distinctly conceived as the Motions had been before.

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## CHAPTER V.

### Examination of the Nervous System, and Consequent Speculations.

#### *Sect.* 1.—*The Examination of the Nervous System.*

IT is hardly necessary to illustrate by further examples the manner in which anatomical observation has produced conjectural and hypothetical attempts to connect structure and action with some

[15] Bourdon, p. 274.

higher principle, of a more peculiarly physiological kind. But it may still be instructive to notice a case in which the principle, which is thus brought into view, is far more completely elevated above the domain of matter and mechanism than in those we have yet considered;—a case where we have not only Irritation, but Sensation;—not only Life, but Consciousness and Will. A part of science in which suggestions present themselves, brings us, in a very striking manner, to the passage from the physical to the hyperphysical sciences.

We have seen already (chap. i.) that Galen and his predecessors had satisfied themselves that the nerves are the channels of perception; a doctrine which had been distinctly taught by Herophilus[1] in the Alexandrian school. Herophilus, however, still combined, under the common name of Nerves, the Tendons; though he distinguished such Nerves from those which arise from the brain and the spinal marrow, and which are subservient to the will. In Galen's time this subject had been prosecuted more into detail. That anatomist has left a Treatise expressly upon *The Anatomy of the Nerves;* in which he describes the successive *Pairs* of Nerves: thus, the First Pair are the visual nerves: and we see, in the language which Galen uses, the evidence of the care and interest with which he had himself examined them. "These nerves," he says, "are not resolved into many fibres, like all the other nerves, when they reach the organs to which they belong; but spread out in a different and very remarkable manner, which it is not easy to describe or to believe, without actually seeing it." He then gives a description of the retina. In like manner he describes the Second Pair, which is distributed to the muscles of the eyes; the Third and Fourth Pairs, which go to the tongue and palate; and so on to the Seventh Pair. This division into Seven Pairs was established by Marinus,[2] but Vesalius found it to be incomplete. The examination which is the basis of the anatomical enumeration of the Nerves at present recognized was that of Willis. His book, entitled *Cerebri Anatome, cui accessit Nervorum descriptio et usus*, appeared at London in 1664. He made important additions to the knowledge of this subject.[3] Thus he is the first who describes in a distinct manner what has been called the *Nervous Centre*,[4] the pyramidal eminences which, according to more recent anatomists, are the communication of the brain with the spinal marrow: and of which the *Decussation*, described by Santorini, affords the explanation of the action of a part

[1] Spr. i. 534. *Dic. Sc. Med.* xxxv. 467. [3] Cuv. *Sc. Nat.* p. 385. [4] Ibid.

of the brain upon the nerves of the opposite side. Willis proved also that the *Rete Mirabile*, the remarkable net-work of arteries at the base of the brain, observed by the ancients in ruminating animals, does not exist in man. He described the different Pairs of Nerves with more care than his predecessors; and his mode of numbering them is employed up to the present time. He calls the Olfactory Nerves the First Pair; previously to him, these were not reckoned a Pair: and thus the optic nerves were, as we have seen, called the first. He added the Sixth and the Ninth Pairs, which the anatomists who preceded him did not reckon. Willis also examined carefully the different *Ganglions*, or knots which occur upon the nerves. He traced them wherever they were to be found, and he gave a general figure of what Cuvier calls the *nervous skeleton*, very superior to that of Vesalius, which was coarse and inexact. Willis also made various efforts to show the connexion of the parts of the brain. In the earlier periods of anatomy, the brain had been examined by slicing it, so as to obtain a section. Varolius endeavored to unravel it, and was followed by Willis. Vicq d'Azyr, in modern times, has carried the method of section to greater perfection than had before been given it;[5] as Vieussens and Gall have done with respect to the method of Varolius and Willis. Recently Professor Chaussier[6] makes three kinds of Nerves:—the *Encephalic*, which proceed from the head, and are twelve on each side;—the *Rachidian*, which proceed from the spinal marrow, and are thirty on each side;—and *Compound Nerves*, among which is the *Great Sympathetic* Nerve.

One of the most important steps ever made in our knowledge of the nerves is, the distinction which Bichat is supposed to have established, of a *ganglionic system*, and a *cerebral system*. And we may add, to the discoveries in nervous anatomy, the remarkable one, made in our own time, that the two offices—of conducting the motive impressions from the central seat of the will to the muscles, and of propagating sensations from the surface of the body and the external organs of sense to the sentient mind—reside in two distinct portions of the nervous substance:—a discovery which has been declared[7] to be "doubtless the most important accession to physiological (anatomical) knowledge since the time of Harvey." This doctrine was first published and taught by Sir Charles Bell: after an interval of some

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[5] Cuv. p. 40.

[6] *Dict. Sc. Nat.* xxxv. 467.

[7] Dr. Charles Henry's *Report of Brit. Assoc.* iii. p. 62.

years, it was more distinctly delivered in the publications of Mr. John Shaw, Sir C. Bell's pupil. Soon afterwards it was further confirmed, and some part of the evidence corrected, by Mr. Mayo, another pupil of Sir C. Bell, and by M. Majendie.[8]

### *Sect. 2.—The Consequent Speculations. Hypotheses respecting Life, Sensation, and Volition.*

I SHALL not attempt to explain the details of these anatomical investigations; and I shall speak very briefly of the speculations which have been suggested by the obvious subservience of the nerves to life, sensation, and volition. Some general inferences from their distribution were sufficiently obvious; as, that the seat of sensation and volition is in the brain. Galen begins his work, *On the Anatomy of the Nerves*, thus: "That none of the members of the animal either exercises voluntary motion, or receives sensation, and that if the nerve be cut, the part immediately becomes inert and insensible, is acknowledged by all physicians. But that the origin of the nerves is partly from the brain, and partly from the spinal marrow, I proceed to explain." And in his work *On the Doctrines of Plato and Hippocrates*, he proves at

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[8] As authority for the expressions which I have now used in the text, I will mention Müller's *Manual of Physiology* (4th edition, 1844). In Book iii. Section 2, Chap. i., "On the Nerves of Sensation and Motion," Müller says, "Charles Bell was the first who had the ingenious thought that the posterior roots of the nerves of the spine—those which are furnished with a ganglion—govern sensation only; that the anterior roots are appointed for motion; and that the primitive fibres of these roots, after being united in a single nervous cord, are mingled together in order to supply the wants of the skin and muscles. He developed this idea in a little work (*An Idea of a new Anatomy of the Brain*, London, 1811), which was not intended to travel beyond the circle of his friends." Müller goes on to say, that eleven years later, Majendie prosecuted the same theory. But Mr. Alexander Shaw, in 1839, published *A Narrative of the Discoveries of Sir Charles Bell in the Nervous System*, in which it appears that Sir Charles Bell had further expounded his views in his lectures to his pupils (p. 89), and that one of these, Mr. John Shaw, had in various publications, in 1821 and 1822, further insisted upon the same views; especially in a Memoir *On Partial Paralysis* (p. 75). MM. Mayo and Majendie both published Memoirs in August, 1822; and these and subsequent works confirmed the doctrine of Bell. Mr. Alexander Shaw states (p. 97), that a mistake of Sir Charles Bell's, in an experiment which he had made to prove his doctrine, was discovered through the joint labors of M. Majendie and Mr. Mayo

great length[9] that the brain is the origin of sensation and motion, refuting the opinions of earlier days, as that of Chrysippus,[10] who placed the *hegemonic*, or master-principle of the soul, in the heart. But though Galen thought that the rational soul resides in the brain, he was disposed to agree with the poets and philosophers, according to whom the heart is the seat of courage and anger, and the liver the seat of love.[11] The faculties of the soul were by succeeding physiologists confined to the brain; but the disposition still showed itself, to attribute to them distinct localities. Thus Willis[12] places the imagination in the *corpus callosum*, the memory in the folds of the *hemispheres*, the perception in the *corpus striatum*. In more recent times, a system founded upon a similar view has been further developed by Gall and his followers. The germ of Gall's system may be considered as contained in that of Willis; for Gall represents the hemispheres as the folds of a great membrane which is capable of being unwrapped and spread out, and places the different faculties of man in the different regions of this membrane. The chasm which intervenes between matter and motion on the one side, and thought and feeling on the other, is brought into view by all such systems; but none of the hypotheses which they involve can effectually bridge it over.

The same observation may be made respecting the attempts to explain the manner in which the nerves operate as the instruments of sensation and volition. Perhaps a real step was made by Glisson,[13] professor of medicine in the University of Cambridge, who distinguished in the fibres of the muscles of motion a peculiar property, different from any merely mechanical or physical action. His work *On the Nature of the Energetic Substance, or on the Life of Nature and of its Three First Faculties, The Perceptive, Appetitive, and Motive*, which was published in 1672, is rather metaphysical than physiological. But the principles which he establishes in this treatise he applies more specially to physiology in a treatise *On the Stomach and Intestines* (Amsterdam, 1677). In this he ascribes to the fibres of the animal body a peculiar power which he calls *Irritability*. He divides *irritation* into natural, vital, and animal; and he points out, though briefly, the gradual differences of irritability in different organs. "It is hardly comprehensible," says Sprengel,[14] "how this

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[9] Lib. vii.

[10] Lib. iii. c. 1.

[11] Lib. vi. c. 8.

[12] Cuv. *Sc. Nat.* p. 384.

[13] Cuv *Sc. Nat.* p. 434.

[14] Spr. iv. 47.

lucid and excellent notion of the Cambridge teacher was not accepted with greater alacrity, and further unfolded by his contemporaries." It has, however, since been universally adopted.

But though the discrimination of muscular irritability as a peculiar power might be a useful step in physiological research, the explanations hitherto offered, of the way in which the nerves operate on this irritability, and discharge their other offices, present only a series of hypotheses. Glisson[15] assumed the existence of certain vital spirits, which, according to him, are a mild, sweet fluid, resembling the spirituous part of white of egg, and residing in the nerves.—This hypothesis, of a very subtle humor or spirit existing in the nerves, was indeed very early taken up.[16] This nervous spirit had been compared to air by Erasistratus, Asclepiades, Galen, and others. The chemical tendencies of the seventeenth century led to its being described as acid, sulphureous or nitrous. At the end of that century. the hypothesis of an *ether* attracted much notice as a means of accounting for many phenomena; and this ether was identified with the nervous fluid. Newton himself inclines to this view, in the remarkable Queries which are annexed to his *Opticks*. After ascribing many physical effects to his ether, he adds (Query 23), "Is not vision performed chiefly by the vibrations of this medium, excited in the bottom of the eye by the rays of light, and propagated through the solid, pellucid, and uniform capillamenta of the nerves into the place of sensation?" And (Query 24), "Is not animal motion performed by the vibrations of this medium, excited in the brain by the power of the will, and propagated from thence through the capillamenta of the nerves into the muscles for contracting and dilating them?" And an opinion approaching this has been adopted by some of the greatest of modern physiologists; as Haller, who says,[17] that, though it is more easy to find what this nervous spirit is not than what it is, he conceives that, while it must be far too fine to be perceived by the sense, it must yet be more gross than fire, magnetism, or electricity; so that it may be contained in vessels, and confined by boundaries. And Cuvier speaks to the same effect:[18] "There is a great probability that it is by an imponderable fluid that the nerve acts on the fibre, and that this nervous fluid is drawn from the blood, and secreted by the medullary matter."

Without presuming to dissent from such authorities on a point of

[15] Spr. iv. 38.

[16] Haller, *Physiol.* iv. 365.

[17] *Physiol.* iv. 381, lib. x. sect. viii. § 15.

[18] *Règne Animal*, Introd. p. 30

anatomical probability, we may venture to observe, that these hypotheses do not tend at all to elucidate the physiological principle which is here involved; for this principle cannot be mechanical, chemical, or physical, and therefore cannot be better understood by embodying it in a fluid; the difficulty we have in conceiving what the moving force *is*, is not got rid of by explaining the machinery by which it is merely *transferred.* In tracing the phenomena of sensation and volition to their cause, it is clear that we must call in some peculiar and hyperphysical principle. The hypothesis of a fluid is not made more satisfactory by attenuating the fluid; it becomes subtle, spirituous, ethereal, imponderable, to no purpose; it must cease to be a fluid, before its motions can become sensation and volition. This, indeed, is acknowledged by most physiologists; and strongly stated by Cuvier.[19] "The impression of external objects upon the ME, the production of a sensation, of an image, is a mystery impenetrable for our thoughts." And in several places, by the use of this peculiar phrase, "*the me*," (*le moi,*) for the sentient and volent faculty, he marks, with peculiar appropriateness and force, that phraseology borrowed from the world of matter will, in this subject, no longer answer our purpose. We have here to go from Nouns to Pronouns, from Things to Persons. We pass from the Body to the Soul, from Physics to Metaphysics. We are come to the borders of material philosophy; the next step is into the domain of Thought and Mind. Here, therefore, we begin to feel that we have reached the boundaries of our present subject. The examination of that which lies beyond them must be reserved for a philosophy of another kind, and for the labors of the future; if we are ever enabled to make the attempt to extend into that loftier and wider scene, the principles which we gather on the ground we are now laboriously treading.

Such speculations as I have quoted respecting the nervous fluid, proceeding from some of the greatest philosophers who ever lived, prove only that hitherto the endeavor to comprehend the mystery of perception and will, of life and thought, have been fruitless and vain. Many anatomical truths have been discovered, but, so far as our survey has yet gone, no genuine physiological principle. All the trains of physiological research which we have followed have begun in exact examination of organization and function, and have ended in wide conjectures and arbitrary hypotheses. The stream of knowledge in all such cases is

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[19] *Règne Animal,* Introd. p. 47.

clear and lively at its outset; but, instead of reaching the great ocean of the general truths of science, it is gradually spread abroad among sands and deserts till its course can be traced no longer.

Hitherto, therefore, we must consider that we have had to tell the story of the *failures* of physiological speculation. But of late there have come into view and use among physiologists certain principles which may be considered as peculiar to organized subjects; and of which the introduction forms a real advance in organical science. Though these have hitherto been very imperfectly developed, we must endeavor to exhibit, in some measure, their history and bearing.

[2nd Ed.] [In order to show that I am not unaware how imperfect the sketch given in this work is, as a History of Physiology, I may refer to the further discussions on these subjects contained in the *Philosophy of the Inductive Sciences*, Book ix. I have there (Chap. ii.) noticed the successive *Biological Hypotheses* of the Mystical, the Iatrochemical, and Iatromathematical Schools, the Vital-Fluid School, and the Psychical School. I have (Chaps. iii., iv., v.) examined several of the attempts which have been made to analyze the Idea of Life, to classify Vital Functions, and to form Ideas of Separate Vital Forces. I have considered in particular, the attempts to form a distinct conception of Assimilation and Secretion, of Generation, and of Voluntary Motion; and I have (Chap. vi.) further discussed the Idea of Final Causes as employed in Biology.]

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## CHAPTER VI.

### Introduction of the Principle of Developed and Metamorphosed Symmetry.

#### *Sect.* 1.—*Vegetable Morphology. Göthe. De Candolle.*

BEFORE we proceed to consider the progress of principles which belong to animal and human life, such as have just been pointed at, we must look round for such doctrines, if any such there be, as apply alike to all organized beings, conscious or unconscious, fixed or locomotive;—to the laws which regulate vegetable as well as animal forms and functions. Though we are very far from being able to present a clear and connected code of such laws, we may refer to one law, at least, which appears to be of genuine authority and validity; and which is worthy our attention as an example of a properly organical or physiological principle, distinct from all mechanical, chemical, or other physical forces; and such as cannot even be conceived to be resolvable into those. I speak of the tendency which produces such results as have been brought together in recent speculations upon *Morphology*.

It may perhaps be regarded as indicating how peculiar are the principles of organic life, and how far removed from any mere mechanical action, that the leading idea in these speculations was first strongly and effectively apprehended, not by a laborious experimenter and reasoner, but by a man of singularly brilliant and creative fancy; not by a mathematician or chemist, but by a poet. And we may add further, that this poet had already shown himself incapable of rightly apprehending the relation of physical facts to their principles; and had, in trying his powers on such subjects, exhibited a signal instance of the ineffectual and perverse operation of the method of philosophizing to which the constitution of his mind led him. The person of whom we speak, is John Wolfgang Göthe, who is held, by the unanimous voice of Europe, to have been one of the greatest poets of our own, or of any time, and whose *Doctrine of Colors* we have already had to describe, in the History of Optics, as an entire failure. Yet his views on the laws which connect the forms of plants into one simple system, have been generally accepted and followed up. We might almost be led to think that this writer's poetical endowments had contributed to this scientific discovery;—the love of beauty of form, by fixing the attention upon the symmetry of plants; and the creative habit of thought, by making constant developement of a familiar process.[1]

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[1] We may quote some of the poet's own verses as an illustration of his feelings on this subject. They are addressed to a lady.

> Dich verwirret, geliebte, die tausendfältige mischung
>   Dieses blumengewühls über dem garten umher
> Viele namen hörest du an, und immer verdränget,
>   Mit barbarischem klang, einer den andern im ohr.
> Alle gestalten sind ählich und keine gleichet der andern;
>   Und so deutet das chor auf ein geheimes gesetz,
> Auf ein heiliges räthsel. O! könnte ich dich, liebliche freundinn,
>   Ueberliefern so gleich glücklich das lösende wort.

But though we cannot but remark the peculiarity of our being indebted to a poet for the discovery of a scientific principle, we must not forget that he himself held, that in making this step, he had been guided, not by his invention, but by observation. He repelled, with extreme repugnance, the notion that he had substituted fancy for fact, or imposed ideal laws on actual things. While he was earnestly pursuing his morphological speculations, he attempted to impress them upon Schiller. "I expounded to him, in as lively a manner as possible, the metamorphosis of plants, drawing on paper, with many characteristic strokes, a symbolic plant before his eyes. He heard me," Göthe says,[a] "with much interest and distinct comprehension; but when I had done, he shook his head, and said, 'That is not Experience; that is an Idea:' I stopt with some degree of irritation; for the point which separated us was marked most luminously by this expression." And in the same work he relates his botanical studies and his habit of observation, from which it is easily seen that no common amount of knowledge and notice of details, were involved in the course of thought which led him to the principle of the Metamorphosis of Plants.

Before I state the history of this principle, I may be allowed to endeavor to communicate to the reader, to whom this subject is new, some conception of the principle itself. This will not be difficult, if he will imagine to himself a flower, for instance, a common wild-rose, or the blossom of an apple-tree, as consisting of a series of parts disposed in *whorls*, placed one over another on an *axis*. The lowest whorl is the calyx with its five sepals; above this is the corolla with its five petals; above this are a multitude of stamens, which may be considered as separate whorls of five each, often repeated; above these is a whorl composed of the ovaries, or what become the seed-vessels in the fruit, which are five united together in the apple, but indefinite in number and separate in the rose. Now the morphological view is this;—

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Thou, my love, art perplext with the endless seeming confusion
  Of the luxuriant wealth which in the garden is spread;
Name upon name thou hearest, and in thy dissatisfied hearing,
  With a barbarian noise one drives another along.
All the forms resemble, yet none is the same as another;
  Thus the whole of the throng points at a deep hidden law,
Points at a sacred riddle. Oh! could I to thee, my beloved friend,
  Whisper the fortunate word by which the riddle is read!

[a] *Zur Morphologie*. p. 24.

that the members of each of these whorls are in their nature identical, and the same as if they were whorls of ordinary leaves, brought together by the shortening their common axis, and modified in form by the successive elaboration of their nutriment. Further, according to this view, a wherl of leaves itself is to be considered as identical with several detached leaves dispersed spirally along the axis, and brought together because the axis is shortened. Thus all the parts of a plant are, or at least represent, the successive metamorphoses of the same elementary member. The root-leaves thus pass into the common leaves;—these into bracteæ;—these into the sepals;—these into the petals;—these into the stamens with their anthers;—these into the ovaries with their styles and stigmas;—these ultimately become the fruit; and thus we are finally led to the seed of a new plant.

Moreover the same notion of metamorphosis may be applied to explain the existence of flowers which are not symmetrical like those we have just referred to, but which have an irregular corolla or calyx. The papilionaceous flower of the pea tribe, which is so markedly irregular, may be deduced by easy gradations from the regular flower, (through the *mimoseæ*,) by expanding one petal, joining one or two others, and modifying the form of the intermediate ones.

Without attempting to go into detail respecting the proofs of that identity of all the different organs, and all the different forms of plants, which is thus asserted, we may observe, that it rests on such grounds as these;—the transformations which the parts of flowers undergo by accidents of nutriment or exposure. Such changes, considered as monstrosities where they are very remarkable, show the tendencies and possibilities belonging to the organization in which they occur. For instance, the single wild-rose, by culture, transforms many of its numerous stamens into petals, and thus acquires the deeply folded flower of the double garden-rose. We cannot doubt of the reality of this change, for we often see stamens in which it is incomplete. In other cases we find petals becoming leaves, and a branch growing out of the centre of the flower. Some pear-trees, when in blossom, are remarkable for their tendencies to such monstrosities.[3] Again, we find that flowers which are usually irregular, occasionally become regular, and conversely. The common snap-dragon (*Linaria vulgaris*) affords a curious instance of this.[4] The usual form of this plant is "personate," the corolla being divided into two lobes, which differ in form, and

[3] Lindley, *Nat. Syst.* p. 84. [4] Henslow, *Principles of Botany*, p. 116.

together present somewhat the appearance of an animal's face; and the upper portion of the corolla is prolonged backwards into a tube like "spur." No flower can be more irregular; but there is a singular variety of this plant, termed *Peloria*, in which the corolla is strictly symmetrical, consisting of a conical tube, narrowed in front, elongated behind into five equal spurs, and containing five stamens of equal length, instead of the two unequal pairs of the didynamous Linaria. These and the like appearances show that there is in nature a capacity for, and tendency to, such changes as the doctrine of metamorphosis asserts.

Göthe's *Metamorphosis of Plants* was published 1790: and his system was the result of his own independent course of thought. The view which it involved was not, however, absolutely new, though it had never before been unfolded in so distinct and persuasive a manner. Linnæus considered the leaves, calyx, corolla, stamens, each as evolved in succession from the other; and spoke of it as *prolepsis* or *anticipation*,[5] when the leaves changed accidentally into bracteæ, these into a calyx, this into a corolla, the corolla into stamens, or these into the pistil. And Caspar Wolf apprehended in a more general manner the same principle. "In the whole plant," says he,[6] "we see nothing but leaves and stalk;" and in order to prove what is the situation of the leaves in all their later forms, he adduces the cotyledons as the first leaves.

Göthe was led to his system on this subject by his general views of nature. He saw, he says,[7] that a whole life of talent and labor was requisite to enable any one to arrange the infinitely copious organic forms of a single kingdom of nature. "Yet I felt," he adds, "that for me there must be another way, analogous to the rest of my habits. The appearance of the changes, round and round, of organic creatures had taken strong hold on my mind. Imagination and Nature appeared to me to vie with each other which could go on most boldly yet most consistently." His observation of nature, directed by such a thought, led him to the doctrine of the metamorphosis.

In a later republication of his work (*Zur Morphologie*, 1817,) he gives a very agreeable account of the various circumstances which affected the reception and progress of his doctrine. Willdenow[8] quoted

[5] Sprengel, *Bot.* ii. 302. *Amœn. Acad.* vi. 324, 365.

[6] *Nov. Con. Ac. Petrop.* xii. 403, xiii. 478.

[7] *Zur Morph.* i. 30.

[8] *Zur Morph.* i. 121.

him thus:—"The life of plants is, as Mr. Göthe very prettily says, an expansion and contraction, and these alternations make the various periods of life." "This '*prettily*,'" says Göthe, "I can be well content with, but the '*egregie*' of Usteri is much more pretty and obliging." Usteri had used this term respecting Göthe in an edition of Jussieu.

The application of the notion of metamorphosis to the explanation of double and monstrous flowers had been made previously by Jussieu.

Göthe's merit was, to have referred to it the *regular* formation of the flower. And as Sprengel justly says,[9] his view had so profound a meaning, made so strong an appeal by its simplicity, and was so fruitful in the most valuable consequences, that it was not to be wondered at if it occasioned further examination of the subject; although many persons pretend to slight it. The task of confirming and verifying the doctrine by a general application of it to all cases,—a labor so important and necessary after the promulgation of any great principle,—Göthe himself did not execute. At first he collected specimens and made drawings with some such view,[10] but he was interrupted and diverted to other matters. "And now," says he, in his later publication, "when I look back on this undertaking, it is easy to see that the object which I had before my eyes was, for me, in my position, with my habits and mode of thinking, unattainable. For it was no less than this: that I was to take that which I had stated in general, and presented to the conception, to the mental intuition, in words; and that I should, in a particularly visible, orderly, and gradual manner, present it to the eye; so as to show to the outward sense that out of the germ of this idea might grow a tree of physiology fit to overshadow the world."

Voigt, professor at Jena, was one of the first who adopted Göthe's view into an elementary work, which he did in 1808. Other botanists labored in the direction which had thus been pointed out. Of those who have thus contributed to the establishment and developement of the metamorphic doctrine, Professor De Candolle, of Geneva, is perhaps the most important. His Theory of Developement rests upon two main principles, *abortion* and *adhesion*. By considering some parts as degenerated or absent through the abortion of the buds which might have formed them, and other parts as adhering together, he holds that all plants may be reduced to perfect symmetry: and the actual and constant occurrence of such incidents is shown beyond

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[9] *Gesch Botan.* ii. 304. [1] *Zur Morph.* i. 229.

all doubt. And thus the snap-dragon, of which we have spoken above, is derived from the Peloria, which is the normal condition of the flower, by the abortion of one stamen, and the degeneration ot two others. Such examples are too numerous to need to be dwelt on.

### *Sect. 2.—Application of Vegetable Morphology.*

The doctrine, being thus fully established, has been applied to solve different problems in botany; for instance, to explain the structure of flowers which appear at first sight to deviate widely from the usual forms of the vegetable world. We have an instance of such an application in Mr. Robert Brown's explanation of the real structure of various plants which had been entirely misunderstood: as, for example, the genus *Euphorbia.* In this plant he showed that what had been held to be a jointed filament, was a pedicel with a filament above it, the intermediate corolla having evanesced. In *Orchideæ* (the orchis tribe), he showed that the peculiar structure of the plant arose from its having six stamens (two sets of three each), of which five are usually abortive. In *Coniferæ* (the cone-bearing trees), it was made to appear that the seed was naked, while the accompanying appendage, corresponding to a seed-vessel, assumed all forms, from a complete leaf to a mere scale. In like manner it was proved that the *pappus*, or down of *composite* plants (as thistles), is a transformed calyx.

Along with this successful application of a profound principle, it was natural that other botanists should make similar attempts. Thus Mr. Lindley was led to take a view[11] of the structure of *Reseda* (mignonette) different from that usually entertained; which, when published, attracted a good deal of attention, and gained some converts among the botanists of Germany and France. But in 1833, Mr. Lindley says, with great candor, "Lately, Professor Henslow has satisfactorily proved, in part by the aid of a monstrosity in the common *Mignonette*, in part by a severe application of morphological rules, that my hypothesis must necessarily be false." Such an agreement of different botanists respecting the consequences of morphological rules, proves the reality and universality of the rules.

We find, therefore, that a principle which we may call the *Principle of Developed and Metamorphosed Symmetry*, is firmly established

[11] Lindley, *Brit. Assoc. Report,* iii. 50.

and recognized, and familiarly and successfully applied by botanists And it will be apparent, on reflection, that though *symmetry* is a notion which applies to inorganic as well as to organic things, and is, in fact, a conception of certain relations of space and position, such *developement* and *metamorphosis* as are here spoken of, are ideas entirely different from any of those to which the physical sciences have led us in our previous survey; and are, in short, genuine *organical* or *physiological* ideas;—real elements of the philosophy of *life*.

We must, however imperfectly, endeavor to trace the application of this idea in the other great department of the world of life; we must follow the history of Animal Morphology.

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## CHAPTER VII.

### Progress of Animal Morphology.

#### *Sect. 1.—Rise of Comparative Anatomy.*

THE most general and constant relations of the form of the organs, both in plants and animals, are the most natural grounds of classification. Hence the first scientific classifications of animals are the first steps in animal morphology. At first, a *zoology* was constructed by arranging animals, as plants were at first arranged, according to their external parts. But in the course of the researches of the anatomists of the seventeenth century, it was seen that the internal structure of animals offered resemblances and transitions of a far more coherent and philosophical kind, and the Science of *Comparative Anatomy* rose into favor and importance. Among the main cultivators of this science[1] at the period just mentioned, we find Francis Redi, of Arezzo; Guichard-Joseph Duvernay, who was for sixty years Professor of Anatomy at the Jardin du Roi at Paris, and during this lapse of time had for his pupils almost all the greatest anatomists of the greater part of the eighteenth century; Nehemiah Grew, secretary to the Royal Society of London, whose *Anatomy of Plants* we have already noticed.

But Comparative Anatomy, which had been cultivated with ardoı

[1] Cuv. *Leçons sur l'Hist. des Sc. Nat.* 414, 420.

to the end of the seventeenth century, was, in some measure, neglected during the first two-thirds of the eighteenth. The progress of botany was, Cuvier sagaciously suggests,[2] one cause of this; for that science had made its advances by confining itself to external characters, and rejecting anatomy; and though Linnæus acknowledged the dependence of zoology upon anatomy[3] so far as to make the number of teeth his characters, even this was felt, in his method, as a bold step. But his influence was soon opposed by that of Buffon, Daubenton, and Pallas; who again brought into view the importance of comparative anatomy in Zoology; at the same time that Haller proved how much might be learnt from it in Physiology. John Hunter in England, the two Monros in Scotland, Camper in Holland, and Vicq d'Azyr in France, were the first to follow the path thus pointed out. Camper threw the glance of genius on a host of interesting objects, but almost all that he produced was a number of sketches; Vicq d'Azyr, more assiduous, was stopt in the midst of a most brilliant career by a premature death.

Such is Cuvier's outline of the earlier history of comparative anatomy. We shall not go into detail upon this subject; but we may observe that such studies had fixed in the minds of naturalists the conviction of the possibility and the propriety of considering large divisions of the animal kingdom as modifications of one common *type*. Belon, as early as 1555, had placed the skeleton of a man and a bird side by side, and shown the correspondence of parts. So far as the case of vertebrated animals extends, this correspondence is generally allowed; although it required some ingenuity to detect its details in some cases; for instance, to see the analogy of parts between the head of a man and a fish.

In tracing these less obvious correspondencies, some curious steps have been made in recent times. And here we must, I conceive, again ascribe no small merit to the same remarkable man who, as we have already had to point out, gave so great an impulse to vegetable morphology. Göthe, whose talent and disposition for speculating on all parts of nature were truly admirable, was excited to the study of anatomy by his propinquity to the Duke of Weimar's cabinet of natural history. In 1786, he published a little essay, the object of which was to show that in man, as well as in beasts, the upper jaw contains an intermaxillary bone, although the sutures are obliterated. After 1790,[4] animated and impelled by the same passion for natural

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[2] Cuv. *Hist. Sc. Nat.* i. 301. [3] Ib. [4] *Zur Morphologie*, i. 234.

observation and for general views, which had produced his Metamorphosis of Plants, he pursued his speculations on these subjects eagerly and successfully. And in 1795, he published a *Sketch of a Universal Introduction into Comparative Anatomy, beginning with Osteology*; in which he attempts to establish an "osteological type," to which skeletons of all animals may be referred. I do not pretend that Göthe's anatomical works have had any influence on the progress of the science comparable with that which has been exercised by the labors of professional anatomists; but the ingenuity and value of the views which they contained was acknowledged by the best authorities; and the clearer introduction and application of the principle of developed and metamorphosed symmetry may be dated from about this time. Göthe declares that, at an early period of these speculations, he was convinced[5] that the bony head of beasts is to be derived from six vertebræ. In 1807, Oken published a "Program" *On the Signification of the Bones of the Skull*, in which he maintained that these bones are equivalent to four vertebræ; and Meckel, in his *Comparative Anatomy*, in 1811, also resolved the skull into vertebræ. But Spix, in his elaborate work *Cephalogenesis*, in 1815, reduced the vertebræ of the head to three. "Oken," he says,[6] "published opinions merely theoretical, and consequently contrary to those maintained in this work, which are drawn from observation." This resolution of the head into vertebræ is assented to by many of the best physiologists, as explaining the distribution of the nerves, and other phenomena. Spix further extended the application of the vertebral theory to the heads of all classes of vertebrate animals; and Bojanus published a Memoir expressly on the vertebral structure of the skulls of fishes in Oken's *Isis* for 1818. Geoffroy Saint-Hilaire presented a lithographic plate to the French Academy in February 1824, entitled *Composition de la Tête osseuze chez l'Homme et les Animaux*, and developed his views of the vertebral composition of the skull in two Memoirs published in the *Annales des Sciences Naturelles* for 1824. We cannot fail to recognize here the attempt to apply to the skeleton of animals the principle which leads botanists to consider all the parts of a flower as transformations of the same organs. How far the application of the principle, as here proposed, is just, I must leave philosophical physiologists to decide.

By these and similar researches, it is held by the best physiologists

[5] *Zur Morphologie*, 250.

[6] Spix, *Cephalogenesis*.

that the skull of all vertebrate animals is pretty well reduced to a uniform structure, and the laws of its variations nearly determined.[7]

The vertebrate animals being thus reduced to a single type, the question arises how far this can be done with regard to other animals, and how many such types there are. And here we come to one of the important services which Cuvier rendered to natural history.

### *Sect. 2.—Distinction of the General Types of the Forms of Animals. —Cuvier.*

Animals were divided by Lamarck into vertebrate and invertebrate; and the general analogies of all vertebrate animals are easily made manifest. But with regard to other animals, the point is far from clear. Cuvier was the first to give a really philosophical view of the animal world in reference to the plan on which each animal is constructed. There are,[8] he says, four such plans;—four forms on which animals appear to have been modelled; and of which the ulterior divisions, with whatever titles naturalists have decorated them, are only very slight modifications, founded on the development or addition of some parts which do not produce any essential change in the plan.

These four great branches of the animal world are the *vertebrata*, *mollusca*, *articulata*, *radiata;* and the differences of these are so important that a slight explanation of them may be permitted.

The *vertebrata* are those animals which (as man and other sucklers, birds, fishes, lizards, frogs, serpents) have a backbone and a skull with lateral appendages, within which the viscera are included, and to which the muscles are attached.

The *mollusca*, or soft animals, have no bony skeleton; the muscles are attached to the skin, which often includes stony plates called *shells;* such molluscs are shell-fish; others are cuttle-fish, and many pulpy sea-animals.

The *articulata* consist of *crustacea* (lobsters, &c.), *insects*, *spiders*, and *annulose worms*, which consist of a head and a number of successive annular portions of the body *jointed* together (to the interior of which the muscles are attached), whence the name.

Finally, the *radiata* include the animals known under the name of *zoophytes*. In the preceding three branches the organs of motion and of sense were distributed symmetrically on the two sides of an axis,

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[7] Cuv. *Hist. Sc. Nat.* iii. 442.

[8] *Règne Animal*, p. 57.

so that the animal has a right and a left side. In the radiata the similar members radiate from the axis in a circular manner, like the petals of a regular flower.

The whole value of such a classification cannot be understood without explaining its use in enabling us to give general descriptions, and general laws of the animal functions of the classes which it includes; but in the present part of our work our business is to exhibit it as an exemplification of the reduction of animals to laws of Symmetry. The bipartite Symmetry of the form of vertebrate and articulate animals is obvious; and the reduction of the various forms of such animals to a common type has been effected, by attention to their anatomy, in a manner which has satisfied those who have best studied the subject. The molluscs, especially those in which the head disappears, as oysters, or those which are rolled into a spiral, as snails, have a less obvious Symmetry, but here also we can apply certain general types. And the Symmetry of the radiated zoophytes is of a nature quite different from all the rest, and approaching, as we have suggested, to the kind of Symmetry found in plants. Some naturalists have doubted whether[9] these zoophytes are not referrible to two types (*acrita* or polypes, and true *radiata*,) rather than to one.

This fourfold division was introduced by Cuvier.[10] Before him, naturalists followed Linnæus, and divided non-vertebrate animals into two classes, insects and worms. "I began," says Cuvier, "to attack this view of the subject, and offered another division, in a Memoir read at the Society of Natural History of Paris, the 21st of Floreal, in the year III. of the Republic (May 10, 1795,) printed in the *Décade Philosophique:* in this, I mark the characters and the limits of molluscs, insects, worms, echinoderms, and zoophytes. I distinguish the red-blooded worms or annelides, in a Memoir read to the Institute, the 11th Nivose, year X. (December 31, 1801.) I afterwards distributed these different classes into three branches, each co-ordinate to the branch formed by the vertebrate animals, in a Memoir read to the Institute in July, 1812, printed in the *Annales du Muséum d'Histoire Naturelle*, tom. xix." His great systematic work, the *Règne Animal*, founded on this distribution, was published in 1817; and since that time the division has been commonly accepted among naturalists.

[2nd Ed.] [The question of the Classification of Animals is discussed in the first of Prof. Owen's *Lectures on the Invertebrate Ani-*

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[9] *Brit. Assoc. Rep.* iv. 227.

[1] *R`gne A.* 61.

*mals* (1843). Mr. Owen observes that the arrangement of animals into *Vertebrate* and *Invertebrate* which prevailed before Cuvier, was necessarily bad, inasmuch as no *negative* character in Zoology gives true natural groups. Hence the establishment of the *sub-kingdoms*, *Mollusca*, *Articulata*, *Radiata*, as co-ordinate with *Vertebrata*, according to the arrangement of the nervous system, was a most important advance. But Mr. Owen has seen reason to separate the *Radiata* of Cuvier into two divisions; the *Nematoneura*, in which the nervous system can be traced in a filamentary form (including *Echinoderma*, *Ciliobrachiata*, *Cœlelmintha*, *Rotifera*,) and the *Acrita* or lowest division of the animal kingdom, including *Acalepha*, *Nudibrachiata*, *Sterelmintha*, *Polygastria*.]

### *Sect.* 3.—*Attempts to establish the Identity of the Types of Animal Forms.*

Supposing this great step in Zoology, of which we have given an account,—the reduction of all animals to four types or plans,—to be quite secure, we are then led to ask whether any further advance is possible;—whether several of these types can be referred to one common form by any wider effort of generalization. On this question there has been a considerable difference of opinion. Geoffroy Saint-Hilaire,[11] who had previously endeavored to show that all vertebrate animals were constructed so exactly upon the same plan as to preserve the strictest analogy of parts in respect to their osteology, thought to extend this unity of plan by demonstrating, that the hard parts of crustaceans and insects are still only modifications of the skeleton of higher animals, and that therefore the type of vertebrata must be made to include them also:—the segments of the articulata are held to be strictly analogous to the vertebræ of the higher animals, and thus the former live *within* their vertebral column in the same manner as the latter live *without* it. Attempts have even been made to reduce molluscous and vertebrate animals to a community of type, as we shall see shortly.

Another application of the principle, according to which creatures the most different are developments of the same original type, may be discerned[12] in the doctrine, that the embryo of the higher forms of animal life passes by gradations through those forms which are perma-

[11] Mr. Jenyns, *Brit. Assoc. Rep.* iv. 150. [12] Dr. Clark, *Report*, Ib. iv. 113

nent in inferior animals. Thus, according to this view, the human fœtus assumes successively the plan of the zoophyte, the worm, the fish, the turtle, the bird, the beast. But it has been well observed, that "in these analogies we look in vain for the precision which can alone support the inference that has been deduced;"[13] and that at each step, the higher embryo and the lower animal which it is supposed to resemble, differ in having each different organs suited to their respective destinations.

Cuvier[14] never assented to this view, nor to the attempts to refer the different divisions of his system to a common type. "He could not admit," says his biographer, "that the lungs or gills of the vertebrates are in the same connexion as the branchiæ of molluscs and crustaceans, which in the one are situated at the base of the feet, or fixed on the feet themselves, and in the other often on the back or about the arms. He did not admit the analogy between the skeleton of the vertebrates and the skin of the articulates; he could not believe that the tænia and the sepia were constructed on the same plan; that there was a similarity of composition between the bird and the echinus, the whale and the snail; in spite of the skill with which some persons sought gradually to efface their discrepancies."

Whether it may be possible to establish, among the four great divisions of the "Animal Kingdom," some analogies of a higher order than those which prevail within each division, I do not pretend to conjecture. If this can be done, it is clear that it must be by comparing the types of these divisions under their most general forms: and thus Cuvier's arrangement, so far as it is itself rightly founded on the unity of composition of each branch, is the surest step to the discovery of a unity pervading and uniting these branches. But those who generalize surely, and those who generalize rapidly, may travel in the same direction, they soon separate so widely, that they appear to move from each other. The partisans of a universal "unity of composition" of animals, accused Cuvier of being too inert in following the progress of physiological and zoological science. Borrowing their illustration from the political parties of the times, they asserted that he belonged to the science of *resistance*, not to the science of the *movement*. Such a charge was highly honorable to him; for no one acquainted with the history of zoology can doubt that he had a great share in the impulse by which the "movement" was occasioned; or that he him-

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[13] Dr. Clark, p. 114.

[14] Laurillard, *Elog. de Cuvier*, p. 66.

self made a large advance with it; and it was because he was so poised by the vast mass of his knowledge, so temperate in his love of doubtful generalizations, that he was not swept on in the wilder part of the stream. To such a charge, moderate reformers, who appreciate the value of the good which exists, though they try to make it better, and who know the knowledge, thoughtfulness, and caution, which are needful in such a task, are naturally exposed. For us, who can only decide on such a subject by the general analogies of the history of science, it may suffice to say, that it appears doubtful whether the fundamental conceptions of affinity, analogy, transition, and developement, have yet been fixed in the minds of physiologists with sufficient firmness and clearness, or unfolded with sufficient consistency and generality, to make it likely that any great additional step of this kind can for some time be made.

We have here considered the doctrine of the identity of the seemingly various types of animal structure, as an attempt to extend the correspondencies which were the basis of Cuvier's division of the animal kingdom. But this doctrine has been put forward in another point of view, as the antithesis to the doctrine of final causes. This question is so important a one, that we cannot help attempting to give some view of its state and bearings.

## CHAPTER VIII.

### The Doctrine of Final Causes in Physiology.

#### *Sect.* 1.—*Assertion of the Principle of Unity of Plan.*

We have repeatedly seen, in the course of our historical view of Physiology, that those who have studied the structure of animals and plants, have had a conviction forced upon them, that the organs are constructed and combined in subservience to the life and functions of the whole. The parts have a *purpose*, as well as a *law*;—we can trace Final Causes, as well as Laws of Causation. This principle is peculiar to physiology; and it might naturally be expected that, in the progress of the science, it would come under special consideration. This accordingly has happened; and the principle has been drawn into a prominent position by the struggle of two antagonistic schools of physiologists. On the one hand, it has been maintained that this doctrine of final causes is altogether unphilosophical, and requires to be replaced by a more comprehensive and profound principle : on the other hand, it is asserted that the doctrine is not only true, but that, in our own time, it has been fixed and developed so as to become the instrument of some of the most important discoveries which have been made. Of the views of these two schools we must endeavor to give some account.

The disciples of the former of the two schools express their tenets by the phrases *unity of plan*, *unity of composition ;* and the more detailed developement of these doctrines has been termed the *Theory of Analogies*, by Geoffroy Saint-Hilaire, who claims this theory as his own creation. According to this theory, the structure and functions of animals are to be studied by the guidance of their analogy only ; our attention is to be turned, not to the fitness of the organization for any end of life or action, but to its resemblance to other organizations by which it is gradually derived from the original type.

According to the rival view of this subject, we must not assume, and cannot establish, that the plan of all animals is the same, or their composition similar. The existence of a single and universal system of analogies in the construction of all animals is entirely unproved, and therefore cannot be made our guide in the study of their properties. On the other hand, the plan of the animal, the purpose of its organization in the support of its life, the necessity of the functions to its existence, are truths which are irresistibly apparent, and which may therefore be safely taken as the bases of our reasonings. This view has been put forward as the doctrine of the *conditions of existence :* it may also be described as the principle of *a purpose in organization ;* the structure being considered as having the function for its end. We must say a few words on each of these views.

It had been pointed out by Cuvier, as we have seen in the last chapter, that the animal kingdom may be divided into four great branches; in each of which the *plan* of the animal is different, namely, *vertebrata*, *articulata*, *mollusca*, *radiata*. Now the question naturally occurs, is there really no resemblance of construction in these different classes? It was maintained by some, that there is such a resemblance. In 1820,[1] M. Audouin, a young naturalist of Paris,

[1] Cuv. *Hist. Sc. Nat.* iii. 422.

endeavored to fill up the chasm which separates insects from other animals; and by examining carefully the portions which compose the solid frame-work of insects, and following them through their various transformations in different classes, he conceived that he found relations of position and function, and often of number and form, which might be compared with the relations of the parts of the skeleton in vertebrate animals. He thought that the first segment of an insect, the head,[2] represents one of the three vertebræ which, according to Spix and others, compose the vertebrate head: the second segment of the insects, (the *prothorax* of Audouin,) is, according to M. Geoffroy, the second vertebra of the head of the vertebrata, and so on. Upon this speculation Cuvier[3] does not give any decided opinion; observing only, that even if false, it leads to active thought and useful research.

But when an attempt was further made to identify the plan of another branch of the animal world, the mollusca, with that of the vertebrata, the radical opposition between such views and those of Cuvier, broke out into an animated controversy.

Two French anatomists, MM. Laurencet and Meyranx, presented to the Academy of Sciences, in 1830, a Memoir containing their views on the organization of molluscous animals; and on the sepia or cuttle-fish in particular, as one of the most complete examples of such animals. These creatures, indeed, though thus placed in the same division with shell-fish of the most defective organization and obscure structure, are far from being scantily organized. They have a brain,[4] often eyes, and these, in the animals of this class, (*cephalopoda*) are more complicated than in any vertebrates;[5] they have sometimes ears, salivary glands, multiple stomachs, a considerable liver, a bile, a complete double circulation, provided with auricles and ventricles; in short, their vital activity is vigorous, and their senses are distinct.

But still, though this organization, in the abundance and diversity of its parts, approaches that of vertebrate animals, it had not been considered as composed in the same manner, or arranged in the same order. Cuvier had always maintained that the plan of molluscs is not a continuation of the plan of vertebrates.

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[2] Ib. 437. [3] Cuv. *Hist. Sc. Nat.* iii. 441.

[4] Geoffroy Saint-Hilaire denies this. *Principes de Phil. Zoologique discutés en* 1830, p. 68.

[5] Geoffroy Saint-Hilaire, *Principes de Phil. Zoologie discutés en* 1830, p. 55.

MM. Laurencet and Meyranx, on the contrary, conceived that the sepia might be reduced to the type of a vertebrate creature, by considering the back-bone of the latter bent double backwards, so as to bring the root of the tail to the nape of the neck; the parts thus brought into contact being supposed to coalesce. By this mode of conception, these anatomists held that the viscera were placed in the same connexion as in the vertebrate type, and the functions exercised in an analogous manner.

To decide on the reality of the analogy thus asserted, clearly belonged to the jurisdiction of the most eminent anatomists and physiologists. The Memoir was committed to Geoffroy Saint-Hilaire and Latreille, two eminent zoologists, in order to be reported on. Their report was extremely favorable; and went almost to the length of adopting the views of the authors.

Cuvier expressed some dissatisfaction with this report on its being read;[6] and a short time afterwards,[7] represented Geoffroy Saint-Hilaire as having asserted that the new views of Laurencet and Meyranx refuted completely the notion of the great interval which exists between molluscous and vertebrate animals. Geoffroy protested against such an interpretation of his expressions; but it soon appeared, by the controversial character which the discussions on this and several other subjects assumed, that a real opposition of opinions was in action.

Without attempting to explain the exact views of Geoffroy, (we may, perhaps, venture to say that they are hardly yet generally understood with sufficient distinctness to justify the mere historian of science in attempting such an explanation,) their general tendency may be sufficiently collected from what has been said; and from the phrases in which his views are conveyed.[8] *The principle of connexions, the elective affinities of organic elements, the equilibrization of organs;*—such are the designations of the leading doctrines which are unfolded in the preliminary discourse of his *Anatomical Philosophy.* Elective affinities of organic elements are the forces by which the vital structures and varied forms of living things are produced; and the principles of connexion and equilibrium of these forces in the various parts of the organization prescribe limits and conditions to the variety and developement of such forms.

The character and tendency of this philosophy will be, I think,

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[6] *Princ. de Phil. Zool. discutés en* 1830, p. 36. [7] p. 50.

[8] *Phil. Zool.* 15.

much more clear, if we consider what it excludes and denies. It rejects altogether all conception of a plan and purpose in the organs of animals, as a principle which has determined their forms, or can be of use in directing our reasonings. "I take care," says Geoffroy, "not to ascribe to God any intention."[9] And when Cuvier speaks of the combination of organs in such order that they may be in consistence with the part which the animal *has to play* in nature; his rival rejoins,[10] I "know nothing of animals which *have to play* a part in nature." Such a notion is, he holds, unphilosophical and dangerous. It is an abuse of final causes which makes the cause to be engendered by the effect. And to illustrate still further, his own view, he says, "I have read concerning fishes, that because they live in a medium which resists more than air, their motive forces are calculated so as to give them the power of progression under those circumstances. By this mode of reasoning, you would say of a man who makes use of crutches, that he was originally destined to the misfortune of having a leg paralysed or amputated."

How far this doctrine of unity in the plan in animals, is admissible or probable in physiology when kept within proper limits, that is, when not put in opposition to the doctrine of a purpose involved in the plan of animals, I do not pretend even to conjecture. The question is one which appears to be at present deeply occupying the minds of the most learned and profound physiologists; and such persons alone, adding to their knowledge and zeal, judicial sagacity and impartiality, can tell us what is the general tendency of the best researches on this subject.[11] But when the anatomist expresses such opinions, and defends them by such illustrations as those which I have just quoted,[12] we perceive that he quits the entrenchments of his superior science, in which he might

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[9] "Je me garde de prêter à Dieu aucune intention." *Phil. Zool.* 10.

[10] "Je ne connais point d'animal qui DOIVE jouer un rôle dans la nature." p. 65.

[11] So far as this doctrine is generally accepted among the best physiologists, we cannot doubt the propriety of Meckel's remark, (*Comparative Anatomy*, 1821, Pref. p. xi.) that it cannot be truly asserted either to be new, or to be peculiarly due to Geoffroy Saint-Hilaire.

[12] It is hardly worth while answering such illustrations, but I may remark, that the one quoted above, irrelevant and unbecoming as it is, tells altogether against its author. The fact that the wooden leg is of the same length as the other, proves, and would satisfy the most incredulous man, that it was *intended* for walking.

have remained unassailable so long as the question was a professional one; and the discussion is open to those who possess no peculiar knowledge of anatomy. We shall, therefore, venture to say a few words upon it.

### *Sect.* 2.—*Estimate of the Doctrine of Unity of Plan.*

It has been so often repeated, and so generally allowed in modern times, that Final Causes ought not to be made our guides in natural philosophy, that a prejudice has been established against the introduction of any views to which this designation can be applied, into physical speculations. Yet, in fact, the assumption of an end or purpose in the structure of organized beings, appears to be an intellectual habit which no efforts can cast off. It has prevailed from the earliest to the latest ages of zoological research; appears to be fastened upon us alike by our ignorance and our knowledge; and has been formally accepted by so many great anatomists, that we cannot feel any scruple in believing the rejection of it to be the superstition of a false philosophy, and a result of the exaggeration of other principles which are supposed capable of superseding its use. And the doctrine of unity of plan of all animals, and the other principles associated with this doctrine, so far as they exclude the conviction of an intelligible scheme and a discoverable end, in the organization of animals, appear to be utterly erroneous. I will offer a few reasons for an opinion which may appear presumptuous in a writer who has only a general knowledge of the subject.

1. In the first place, it appears to me that the argumentation on the case in question, the Sepia, does by no means turn out to the advantage of the new hypothesis. The arguments in support of the hypothetical view of the structure of this mollusc were, that by this view the relative position of the parts was explained, and confirmations which had appeared altogether anomalous, were reduced to rule; for example, the beak, which had been supposed to be in a position the reverse of all other beaks, was shown, by the assumed posture, to have its upper mandible longer than the lower, and thus to be regularly placed. "But," says Cuvier,[13] "supposing the posture, in order that the side on which the funnel of the sepia is folded should be the back of the animal, considered as similar to a vertebrate, the brain with re-

[13] *G. S. H. Phil. Zool.* p. 70.

gard to the beak, and the œsophagus with regard to the liver, should have positions corresponding to those in vertebrates; but the positions of these organs are exactly contrary to the hypothesis. How, then, can you say," he asks, "that the cephalopods and vertebrates have *identity of composition*, *unity of composition*, without using words in a sense entirely different from their common meaning?"

This argument appears to be exactly of the kind on which the value of the hypothesis must depend.[14] It is, therefore, interesting to see the reply made to it by the theorist. It is this: "I admit the facts here stated, but I deny that they lead to the notion of a different sort of animal composition. Molluscous animals had been placed too high in the zoological scale; but if they are only the embryos of its lower stages, if they are only beings in which far fewer organs come into play, it does not follow that the organs are destitute of the relations which the power of successive generations may demand. The organ A will be in an unusual relation with the organ C, if B has not been produced;—if a stoppage of the developement has fallen upon this latter organ, and has thus prevented its production. And thus," he says, "we see how we may have different arrangements, and divers constructions as they appear to the eye."

It seems to me that such a concession as this entirely destroys the theory which it attempts to defend; for what arrangement does the principle of unity of composition *exclude*, if it admits unusual, that is, various arrangements of some organs, accompanied by the total absence of others? Or how does this differ from Cuvier's mode of stating the conclusion, except in the introduction of certain arbitrary hypotheses of developement and stoppage? "I reduce the facts," Cuvier says, "to their true expression, by saying that Cephalopods have several organs which are common to them and vertebrates, and which discharge the same offices; but that these organs are in them differently distributed, and often constructed in a different manner;

[14] I do not dwell on other arguments which were employed. It was given as a circumstance suggesting the supposed posture of the type, that in this way the back was colored, and the belly was white. On this Cuvier observes (*Phil. Zool.* pp. 93, 68), "I must say, that I do not know any naturalist so ignorant as to suppose that the back is determined by its dark color, or even by its position when the animal is in motion; they all know that the badger has a black belly and a white back; that an infinity of other animals, especially among insects, are in the same case; and that many fishes swim on their side, or with their belly upwards."

and they are accompanied by several other organs which vertebrates have not; while these on the other hand have several which are wanting in cephalopods.'

We shall see afterwards the general principles which Cuvier himself considered as the best guides in these reasonings. But I will first add a few words on the disposition of the school now under consideration, to reject all assumption of an end.

2. That the parts of the bodies of animals are made in order to discharge their respective offices, is a conviction which we cannot believe to be otherwise than an irremovable principle of the philosophy of organization, when we see the manner in which it has constantly forced itself upon the minds of zoologists and anatomists in all ages; not only as an inference, but as a guide whose indications they could not help following. I have already noticed expressions of this conviction in some of the principal persons who occur in the history of physiology, as Galen and Harvey. I might add many more, but I will content myself with adducing a contemporary of Geoffroy's whose testimony is the more remarkable, because he obviously shares with his countryman in the common prejudice against the use of final causes. "I consider," he says, in speaking of the provisions for the reproduction of animals,[16] "with the great Bacon, the philosophy of final causes as sterile; but I have elsewhere acknowledged that it was very difficult for the most cautious man never to have recourse to them in his explanations." After the survey which we have had to take of the history of physiology, we cannot but see that the assumption of final causes in this branch of science is so far from being sterile, that it has had a large share in every discovery which is included in the existing mass of real knowledge. The use of every organ has been discovered by starting from the assumption that it must have *some* use. The doctrine of the circulation of the blood was, as we have seen, clearly and professedly due to the persuasion of a purpose in the circulatory apparatus. The study of comparative anatomy is the study of the adaption of animal structures to their purposes. And we shall soon have to show that this conception of final causes has, in our own times, been so far from barren, that it has, in the hands of Cuvier and others, enabled us to become intimately acquainted with vast departments of zoology to which we have no other mode of access. It has placed before us in a complete state

[16] Cabanis, *Rapports du Physique et du Morale de l'Homme*, i 229.

animals, of which, for thousands of years, only a few fragments have existed, and which differ widely from all existing animals; and it has given birth, or at least has given the greatest part of its importance and interest, to a science which forms one of the brightest parts of the modern progress of knowledge. It is, therefore, very far from being a vague and empty assertion, when we say that final causes are a real and indestructible element in zoological philosophy; and that the exclusion of them, as attempted by the school of which we speak, is a fundamental and most mischievous error.

3. Thus, though the physiologist may persuade himself that he ought not to refer to final causes, we find that, practically, he cannot help doing this; and that the event shows that his practical habit is right and well-founded. But he may still cling to the speculative difficulties and doubts in which such subjects may be involved by *à priori* considerations. He may say, as Saint-Hilaire does say,[16] "I ascribe no intention to God, for I mistrust the feeble powers of my reason. I observe facts merely, and go no further. I only pretend to the character of the historian of *what is*." "I cannot make Nature an intelligent being who does nothing in vain, who acts by the shortest mode, who does all for the best."

I am not going to enter at any length into this subject, which, thus considered, is metaphysical and theological, rather than physiological. If any one maintain, as some have maintained, that no manifestation of means apparently used for ends in nature, *can* prove the existence of design in the Author of nature, this is not the place to refute such an opinion in its general form. But I think it may be worth while to show, that even those who incline to such an opinion, still cannot resist the necessity which compels men to assume, in organized beings, the existence of an end.

Among the philosophers who have referred our conviction of the being of God to our moral nature, and have denied the possibility of demonstration on mere physical grounds, Kant is perhaps the most eminent. Yet he has asserted the reality of such a principle of physiology as we are now maintaining in the most emphatic manner. Indeed, this assumption of an end makes his very definition of an organized being. "An organized product of nature is that in which all the parts are mutually ends and means."[17] And this, he says, is a universal and necessary maxim. He adds, "It is well known that the

[16] *Phil. Zool.* p. 10.

[17] *Urtheilskraft*, p. 296.

anatomizers of plants and animals, in order to investigate their structure, and to obtain an insight into the grounds why and to what end such parts, why such a situation and connexion of the parts, and exactly such an internal form, come before them, assume, as indispensably necessary, this maxim, that in such a creature nothing is *in vain*, and proceed upon it in the same way in which in general natural philosophy we proceed upon the principle that *nothing happens by chance.* In fact, they can as little free themselves from this *teleological* principle as from the general physical one; for as, on omitting the latter, no experience would be possible, so on omitting the former principle, no clue could exist for the observation of a kind of natural objects which can be considered teleologically under the conception of natural ends."

Even if the reader should not follow the reasoning of this celebrated philosopher, he will still have no difficulty in seeing that he asserts, in the most distinct manner, that which is denied by the author whom we have before quoted, the propriety and necessity of assuming the existence of an end as our guide in the study of animal organization.

4. It appears to me, therefore, that whether we judge from the arguments, the results, the practice of physiologists, their speculative opinions, or those of the philosophers of a wider field, we are led to the same conviction, that in the organized world we may and must adopt the belief, that organization exists for its purpose, and that the apprehension of the purpose may guide us in seeing the meaning of the organization. And I now proceed to show how this principle has been brought into additional clearness and use by Cuvier.

In doing this, I may, perhaps, be allowed to make a reflection of a kind somewhat different from the preceding remarks, though suggested by them. In another work,[18] I endeavored to show that those who have been discoverers in science have generally had minds, the disposition of which was to believe in an intelligent Maker of the universe; and that the scientific speculations which produced an opposite tendency, were generally those which, though they might deal familiarly with known physical truths, and conjecture boldly with regard to the unknown, did not add to the number of solid generalizations. In order to judge whether this remark is distinctly applicable in the case now considered, I should have to estimate Cuvier in comparison with other physiologists of his time, which I do not presume to do. But I may

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[18] *Bridgewater Treatise*, B. iii. c. vii. and viii. On Inductive Habits of Thought, and on Deductive Habits of Thought.

observe, that he is allowed by all to have established, on an indestructible basis, many of the most important generalizations which zoology now contains; and the principal defect which his critics have pointed out, has been, that he did not generalize still more widely and boldly. It appears, therefore, that he cannot but be placed among the great discoverers in the studies which he pursued; and this being the case, those who look with pleasure on the tendency of the thoughts of the greatest men to an Intelligence far higher than their own, must be gratified to find that he was an example of this tendency; and that the acknowledgement of a creative purpose, as well as a creative power, not only entered into his belief, but made an indispensable and prominent part of his philosophy.

### *Sect. 3.—Establishment and Application of the Principle of the Conditions of Existence of Animals.—Cuvier.*

We have now to describe more in detail the doctrine which Cuvier maintained in opposition to such opinions as we have been speaking of; and which, in his way of applying it, we look upon as a material advance in physiological knowledge, and therefore give to it a distinct place in our history. "Zoology has," he says,[19] in the outset of his *Règne Animal*, "a principle of reasoning which is peculiar to it, and which it employs with advantage on many occasions: this is the principle of *the Conditions of Existence*, vulgarly the principle of *Final Causes.* As nothing can exist if it do not combine all the conditions which render its existence possible, the different parts of each being must be co-ordinated in such a manner as to render the total being possible, not only in itself, but in its relations to those which surround it; and the analysis of these conditions often leads to general laws, as clearly demonstrated as those which result from calculation or from experience."

This is the enunciation of his leading principle in general terms. To our ascribing it to him, some may object on the ground of its being self-evident in its nature,[20] and having been very anciently applied. But to this we reply, that the principle must be considered as a real discovery in the hands of him who first shows how to make it an instrument of other discoveries. It is true, in other cases as well as in this, that some vague apprehension, of true general principles, such as *à*

[19] *Règne An.* p. 6. [20] Swainson, *Study of Nat. Hist.* p. 85.

*priori* considerations can supply, has long preceded the knowledge of them as real and verified laws. In such a way it was seen, before Newton, that the motions of the planets must result from attraction; and so, before Dufay and Franklin, it was held that electrical actions must result from a fluid. Cuvier's merit consisted, not in seeing that an animal cannot exist without combining all the conditions of its existence; but in perceiving that this truth may be taken as a guide in our researches concerning animals;—that the mode of their existence may be collected from one part of their structure, and then applied to interpret or detect another part. He went on the supposition not only that animal forms have *some* plan, *some* purpose, but that they have an intelligible plan, a discoverable purpose. He proceeded in his investigations like the decipherer of a manuscript, who makes out his alphabet from one part of the context, and then applies it to read the rest. The proof that his principle was something very different from an identical proposition, is to be found in the fact, that it enabled him to understand and arrange the structures of animals with unprecedented clearness and completeness of order; and to restore the forms of the extinct animals which are found in the rocks of the earth, in a manner which has been universally assented to as irresistibly convincing. These results cannot flow from a trifling or barren principle; and they show us that if we are disposed to form such a judgment of Cuvier's doctrine, it must be because we do not fully apprehend its import.

To illustrate this, we need only quote the statement which he makes, and the uses to which he applies it. Thus in the Introduction to his great work on *Fossil Remains* he says, "Every organized being forms an entire system of its own, all the parts of which mutually correspond, and concur to produce a certain definite purpose by reciprocal reaction, or by combining to the same end. Hence none of these separate parts can change their forms without a corresponding change in the other parts of the same animal; and consequently each of these parts, taken separately, indicates all the other parts to which it has belonged. Thus, if the viscera of an animal are so organized as only to be fitted for the digestion of recent flesh, it is also requisite that the jaws should be so constructed as to fit them for devouring prey; the claws must be constructed for seizing it and tearing it to pieces; the teeth for cutting and dividing its flesh; the entire system of the limbs or organs of motion for pursuing and overtaking it; and the organs of sense for discovering it at a distance. Nature must also have endowed the brain of the animal with instincts sufficient for concealing itself, and for laying plans to catch its necessary victims."[21] By such considerations he has been able to reconstruct the whole of many animals of which parts only were given;—a positive result, which shows both the reality and the value of the truth on which he wrought.

Another great example, equally showing the immense importance of this principle in Cuvier's hands, is the reform which, by means of it, he introduced into the classification of animals. Here again we may quote the view he himself has given[22] of the character of his own improvements. In studying the physiology of the natural classes of vertebrate animals, he found, he says, "in the respective quantity of their respiration, the reason of the quantity of their motion, and consequently of the kind of locomotion. This, again, furnishes the reason for the forms of their skeletons and muscles; and the energy of their senses, and the force of their digestion, are in a necessary proportion to the same quantity. Thus a division which had till then been established, like that of vegetables, only upon observation, was found to rest upon causes appreciable, and applicable to other cases." Accordingly, he applied this view to invertebrates;—examined the modifications which take place in their organs of circulation, respiration, and sensation; and having calculated the necessary results of these modifications, he deduced from it a new division of those animals, in which they are arranged according to their true relations.

Such have been some of the results of the principle of the Conditions of Existence, as applied by its great assertor.

It is clear, indeed, that such a principle could acquire its practical value only in the hands of a person intimately acquainted with anatomical details, with the functions of the organs, and with their variety in different animals. It is only by means of such nutriment that the embryo truth could be developed into a vast tree of science. But it is not the less clear, that Cuvier's immense knowledge and great powers of thought led to their results, only by being employed under the guidance of this master-principle: and, therefore, we may justly consider it as the distinctive feature of his speculations, and follow it with a gratified eye, as the thread of gold which runs through, connects, and enriches his zoological researches:—gives them a deeper interest and a higher value than can belong to any view of the organical sciences, in which the very essence of organization is kept out of sight.

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[21] *Theory of the Earth*, p. 90.

[22] *Hist. Sc. Nat.* i. 293.

The real philosopher, who knows that all the kinds of truth are intimately connected, and that all the best hopes and encouragements which are granted to our nature must be consistent with truth, will be satisfied and confirmed, rather than surprised and disturbed, thus to find the Natural Sciences leading him to the borders of a higher region. To him it will appear natural and reasonable, that after journeying so long among the beautiful and orderly laws by which the universe is governed, we find ourselves at last approaching to a Source of order and law, and intellectual beauty:—that, after venturing into the region of life and feeling and will, we are led to believe the Fountain of life and will not to be itself unintelligent and dead, but to be a living Mind, a Power which aims as well as acts. To us this doctrine appears like the natural cadence of the tones to which we have so long been listening; and without such a final strain our ears would have been left craving and unsatisfied. We have been lingering long amid the harmonies of law and symmetry, constancy and development; and these notes, though their music was sweet and deep, must too often have sounded to the ear of our moral nature, as vague and unmeaning melodies, floating in the air around us, but conveying no definite thought, moulded into no intelligible announcement. But one passage which we have again and again caught by snatches, though sometimes interrupted and lost, at last swells in our ears full, clear, and decided; and the religious "Hymn in honor of the Creator," to which Galen so gladly lent his voice, and in which the best physiologists of succeeding times have ever joined, is filled into a richer and deeper harmony by the greatest philosophers of these later days, and will roll on hereafter the "perpetual song" of the temple of science.

# BOOK XVIII

*THE PALÆTIOLOGICAL SCIENCES*

# HISTORY OF GEOLOGY.

Di quibus imperium est animarum, Umbræque silentes,
Et Chaos, et Phlegethon, loca nocte silentia late,
Sit mihi fas audita loqui; sit, numine vestro
Pandere res alta terrâ et caligine mersas.

VIRGIL. *Æn.* vi. 264.

Ye Mighty Ones, who sway the Souls that go
Amid the marvels of the world below!
Ye, silent Shades, who sit and hear around!
Chaos! and Streams that burn beneath the ground!
All, all forgive, if by your converse stirred,
My lips shall utter what my ears have heard;
If I shall speak of things of doubtful birth,
Deep sunk in darkness, as deep sunk in earth.

## INTRODUCTION.

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### *Of the Palætiological Sciences.*

WE now approach the last Class of Sciences which enter into the design of the present work; and of these, Geology is the representative, whose history we shall therefore briefly follow. By the Class of Sciences to which I have referred it, I mean to point out those researches in which the object is, to ascend from the present state of things to a more ancient condition, from which the present is derived by intelligible causes.

The sciences which treat of causes have sometimes been termed *ætiological*, from αἰτία, *a cause:* but this term would not sufficiently describe the speculations of which we now speak; since it might include sciences which treat of Permanent Causality, like Mechanics, as well as inquiries concerning Progressive Causation. The investigations which I now wish to group together, deal, not only with the possible, but with the actual past; and a portion of that science on which we are about to enter, Geology, has properly been termed *Palæontology*, since it treats of beings which formerly existed.[1] Hence, combining these two notions,[2] *Palætiology* appears to be a term not inappropriate, to describe those speculations which thus refer to actual past events, and attempt to explain them by laws of causation.

Such speculations are not confined to the world of inert matter; we have examples of them in inquiries concerning the monuments of the art and labor of distant ages; in examinations into the origin and early progress of states and cities, customs and languages; as well as in researches concerning the causes and formations of mountains and rocks, the imbedding of fossils in strata, and their elevation from the bottom of the ocean. All these speculations are connected by this bond,—that they endeavor to ascend to a past state of things, by the aid of the evidence of the present. In asserting, with Cuvier, that

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[1] Πάλαι. ὄντα.

[2] Πάλαι. αἰτία.

"The geologist is an antiquary of a new order," we do not mark a fanciful and superficial resemblance of employment merely, but a real and philosophical connexion of the principles of investigation. The organic fossils which occur in the rock, and the medals which we find in the ruins of ancient cities, are to be studied in a similar spirit and for a similar purpose. Indeed, it is not always easy to know where the task of the geologist ends, and that of the antiquary begins. The study of ancient geography may involve us in the examination of the causes by which the forms of coasts and plains are changed; the ancient mound or scarped rock may force upon us the problem, whether its form is the work of nature or of man; the ruined temple may exhibit the traces of time in its changed level, and sea-worn columns; and thus the antiquarian of the earth may be brought into the very middle of the domain belonging to the antiquarian of art.

Such a union of these different kinds of archæological investigations has, in fact, repeatedly occurred. The changes which have taken place in the temple of Jupiter Serapis, near Puzzuoli, are of the sort which have just been described; and this is only one example of a large class of objects;—the monuments of art converted into records of natural events. And on a wider scale, we find Cuvier, in his inquiries into geological changes, bringing together historical and physical evidence. Dr. Prichard, in his *Researches into the Physical History of Man*, has shown that to execute such a design as his, we must combine the knowledge of the physiological laws of nature with the traditions of history and the philosophical comparison of languages. And even if we refuse to admit, as part of the business of geology, inquiries concerning the origin and physical history of the *present* population of the globe; still the geologist is compelled to take an interest in such inquiries, in order to understand matters which rigorously belong to his proper domain; for the ascertained history of the present state of things offers the best means of throwing light upon the causes of *past* changes. Mr. Lyell quotes Dr. Prichard's book more frequently than any geological work of the same extent.

Again, we may notice another common circumstance in the studies which we are grouping together as palætiological, diverse as they are in their subjects. In all of them we have the same kind of manifestations of a number of successive changes, each springing out of a preceding state; and in all, the phenomena at each step become more and more complicated, by involving the results of all that has preceded, modified by supervening agencies. The general aspect of all these trains of change is similar, and offers the same features for description. The relics and ruins of the earlier states are preserved, mutilated and dead, in the products of later times. The analogical figures by which we are tempted to express this relation are philosophically true. It is more than a mere fanciful description, to say that in languages, customs, forms of Society, political institutions, we see a number of formations super-imposed upon one another, each of which is, for the most part, an assemblage of fragments and results of the preceding condition. Though our comparison might be bold, it would be just, if we were to assert, that the English language is a conglomerate of Latin words, bound together in a Saxon cement; the fragments of the Latin being partly portions introduced directly from the parent quarry, with all their sharp edges, and partly pebbles of the same material, obscured and shaped by long rolling in a Norman or some other channel. Thus the study of palætiology in the materials of the earth, is only a type of similar studies with respect to all the elements, which, in the history of the earth's inhabitants, have been constantly undergoing a series of connected changes.

But, wide as is the view which such considerations give us of the class of sciences to which geology belongs, they extend still further. "The science of the changes which have taken place in the organic kingdoms of nature," (such is the description which has been given of Geology,[3]) may, by following another set of connexions, be extended beyond "the modifications of the surface of our own planet." For we cannot doubt that some resemblance of a closer or looser kind, has obtained between the changes and causes of change, on other bodies of the universe, and on our own. The appearances of something of the kind of volcanic action on the surface of the moon, are not to be mistaken. And the inquiries concerning the origin of our planet and of our solar system, inquiries to which Geology irresistibly impels her students, direct us to ask what information the rest of the universe can supply, bearing upon this subject. It has been thought by some, that we can trace systems, more or less like our solar system, in the process of formation; the nebulous matter, which is at first expansive and attenuated, condensing gradually into suns and planets. Whether this *Nebular Hypothesis* be tenable or not, I shall not here inquire; but the discussion of such a question would be closely connected with

[3] Lyell, *Principles of Geology*, p. 1.

geology, both in its interests and in its methods. If men are ever able to frame a science of the past changes by which the universe has been brought into its present condition, this science will be properly described as *Cosmical Palætiology*.

These palætiological sciences might properly be called *historical*, if that term were sufficiently precise: for they are all of the nature of history, being concerned with the succession of events: and the part of history which deals with the past causes of events, is, in fact, a moral palætiology. But the phrase *Natural History* has so accustomed us to a use of the word *history* in which we have nothing to do with time, that, if we were to employ the word *historical* to describe the palætiological sciences, it would be in constant danger of being misunderstood. The fact is, as Mohs has said, that Natural History, when systematically treated, rigorously excludes all that is *historical;* for it classes objects by their permanent and universal properties, and has nothing to do with the narration of particular and casual facts. And this is an inconsistency which we shall not attempt to rectify.

All palætiological sciences, since they undertake to refer changes to their causes, assume a certain classification of the phenomena which change brings forth, and a knowledge of the operation of the causes of change. These phenomena, these causes, are very different, in the branches of knowledge which I have thus classed together. The natural features of the earth's surface, the works of art, the institutions of society, the forms of language, taken together, are undoubtedly a very wide collection of subjects of speculation; and the kinds of causation which apply to them are no less varied. Of the causes of change in the inorganic and organic world,—the peculiar principles of Geology,—we shall hereafter have to speak. As these must be studied by the geologist, so, in like manner, the tendencies, instincts, faculties, principles, which direct man to architecture and sculpture, to civil government, to rational and grammatical speech, and which have determined the circumstances of his progress in these paths, must be in a great degree known to the Palætiologist of Art, of Society, and of Language, respectively, in order that he may speculate soundly upon his peculiar subject. With these matters we shall not here meddle, confining ourselves, in our exemplification of the conditions and progress of such sciences, to the case of Geology.

The journey of survey which we have attempted to perform over the field of human knowledge, although carefully directed according to the paths and divisions of the physical sciences, has already conducted us to the boundaries of physical science, and gives us a glimpse of the region beyond. In following the history of Life, we found ourselves led to notice the perceptive and active faculties of man; it appeared that there was a ready passage from physiology to psychology, from physics to metaphysics. In the class of sciences now under notice, we are, at a different point, carried from the world of matter to the world of thought and feeling,—from things to men. For, as we have already said, the science of the causes of change includes the productions of Man as well as of Nature. The history of the earth, and the history of the earth's inhabitants, as collected from phenomena, are governed by the same principles. Thus the portions of knowledge which seek to travel back towards the origin, whether of inert things or of the works of man, resemble each other. Both of them treat of events as connected by the thread of time and causation. In both we endeavor to learn accurately what the present is, and hence what the past has been. Both are *historical* sciences in the same sense.

It must be recollected that I am now speaking of history as ætiological;—as it investigates causes, and as it does this in a scientific, that is, in a rigorous and systematic, manner. And I may observe here, though I cannot now dwell on the subject, that all ætiological sciences will consist of three portions; the Description of the facts and phenomena;—the general Theory of the causes of change appropriate to the case;—and the Application of the theory to the facts. Thus, taking Geology for our example, we must have, first *Descriptive* or *Phenomenal Geology;* next, the exposition of the general principles by which such phenomena can be produced, which we may term *Geological Dynamics;* and, lastly, doctrines hence derived, as to what have been the causes of the existing state of things, which we may call *Physical Geology.*

These three branches of geology may be found frequently or constantly combined in the works of writers on the subject, and it may not always be easy to discriminate exactly what belongs to each subject.[4] But the analogy of this science with others, its present

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[4] The Wernerians, in distinguishing their study from *Geology*, and designating it as *Geognosy*, the *knowledge* of the earth, appear to have intended to select Descriptive Geology for their peculiar field. In like manner, the original aim of the Geological Society of London, which was formed (1807) "with a view to record and multiply observations," recognized the possibility of a Descriptive Geology separate from the other portions of the science.

condition and future fortunes, will derive great illustration from such a distribution of its history; and in this point of view, therefore, we shall briefly treat of it; dividing the history of Geological Dynamics, for the sake of convenience, into two Chapters, one referring to inorganic, and one to organic, phenomena.

# DESCRIPTIVE GEOLOGY.

## CHAPTER I.

### Prelude to Systematic Descriptive Geology.

*Sect.* 1.—*Ancient Notices of Geological Facts.*

THE recent history of Geology, as to its most important points, is bound up with what is doing at present from day to day; and that portion of the history of the science which belongs to the past, has been amply treated by other writers.[1] I shall, therefore, pass rapidly over the series of events of which this history consists; and shall only attempt to mention what may seem to illustrate and confirm my own view of its state and principles.

Agreeably to the order already pointed out, I shall notice, in the first place, Phenomenal Geology, or the description of the facts, as distinct from the inquiry into their causes. It is manifest that such a merely descriptive kind of knowledge may exist; and it probably will not be contested, that such knowledge ought to be collected, before we attempt to frame theories concerning the causes of the phenomena. But it must be observed, that we are here speaking of the formation of a *science;* and that it is not a collection of miscellaneous, unconnected, unarranged knowledge that can be considered as constituting science; but a methodical, coherent, and, as far as possible, complete body of facts, exhibiting fully the condition of the earth as regards those circumstances which are the subject matter of geological speculation. Such a Descriptive Geology is a pre-requisite to Physical Geology, just as Phenomenal Astronomy necessarily preceded Physical Astronomy, or as Classificatory Botany is a necessary accompaniment to Botanical Physiology. We may observe also that Descriptive Geology, such as we now speak of, is one of the classificatory sciences, like

[1] As MM. Lyell, Fitton, Conybeare, in our own country.

Mineralogy or Botany: and will be found to exhibit some of the features of that class of sciences.

Since, then, our History of Descriptive Geology is to include only systematic and scientific descriptions of the earth or portions of it, we pass over, at once, all the casual and insulated statements of facts, though they may be geological facts, which occur in early writers; such, for instance, as the remark of Herodotus,[2] that there are shells in the mountains of Egypt; or the general statements which Ovid puts in the mouth of Pythagoras:[3]

> Vidi ego quod fuerat solidissima tellus,
> Esse fretum; vidi factas ex æquore terras,
> Et procul a pelago conchæ jacuere marinæ.

We may remark here already how generally there are mingled with descriptive notices of such geological facts, speculations concerning their causes. Herodotus refers to the circumstance just quoted, for the purpose of showing that Egypt was formerly a gulf of the sea; and the passage of the Roman poet is part of a series of exemplifications which he gives of the philosophical tenet, that nothing perishes but everything changes. It will be only by constant attention that we shall be able to keep our provinces of geology distinct.

### *Sect. 2.—Early Descriptions and Collections of Fossils.*

If we look, as we have proposed to do, for systematic and exact knowledge of geological facts, we find nothing which we can properly adduce till we come to modern times. But when facts such as those already mentioned, (that sea-shells and other marine objects are found imbedded in rocks,) and other circumstances in the structure of the earth, had attracted considerable attention, the exact examination, collection, and record of these circumstances began to be attempted. Among such steps in Descriptive Geology, we may notice descriptions and pictures of fossils, descriptions of veins and mines, collections of organic and inorganic fossils, maps of the mineral structure of countries, and finally, the discoveries concerning the superposition of strata, the constancy of their organic contents, their correspondence in different countries, and such great general relations of the materials and features of the earth as have been discovered up to the present time.

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[2] ii. 12.

[3] Met. xv. 262.

Without attempting to assign to every important advance its author, I shall briefly exemplify each of the modes of contributing to descriptive geology which I have just enumerated.

The study of organic fossils was first pursued with connexion and system in Italy. The hills which on each side skirt the mountain-range of the Apennines are singularly rich in remains of marine animals. When these remarkable objects drew the attention of thoughtful men, controversies soon arose whether they really were the remains of living creatures, or the productions of some capricious or mysterious power by which the forms of such creatures were mimicked; and again, if the shells were really the spoils of the sea, whether they had been carried to the hills by the deluge of which the Scripture speaks, or whether they indicated revolutions of the earth of a different kind. The earlier works which contain the descriptions of the phenomena have, in almost all instances, by far the greater part of their pages occupied with these speculations; indeed, the facts could not be studied without leading to such inferences, and would not have been collected but for the interest which such reasonings possessed. As one of the first persons who applied a sound and vigorous intellect to these subjects, we may notice the celebrated painter Leonardo da Vinci, whom we have already had to refer to as one of the founders of the modern mechanical sciences. He strenuously asserts the contents of the rocks to be real shells, and maintains the reality of the changes of the domain of land and sea which these spoils of the ocean imply. "You will tell me," he says, "that nature and the influence of the stars have formed these shelly forms in the mountains; then show me a place in the mountains where the stars at the present day make shelly forms of different ages, and of different species in the same place. And how, with that, will you explain the gravel which is hardened in stages at different heights in the mountains?" He then mentions several other particulars respecting these evidences that the existing mountains were formerly in the bed of the sea. Leonardo died in 1519. At present we refer to geological essays like his, only so far as they are descriptive. Going onwards with this view, we may notice Fracastoro, who wrote concerning the petrifactions which were brought to light in the mountains of Verona, when, in 1517, they were excavated for the purpose of repairing the city. Little was done in the way of collection of facts for some time after this. In 1669, Steno, a Dane resident in Italy, put forth his treatise, *De Solido intra Solidum naturaliter contento;* and the fol lowing year, Augustino Scilla, a Sicilian painter, published a Latin epistle, *De Corporibus Marinis Lapidescentibus*, illustrated by good engravings of fossil-shells, teeth, and corals.[4] After another interval of speculative controversy, we come to Antonio Vallisneri, whose letters, *De' Corpi Marini che su' Monti si trovano*, appeared at Venice in 1721. In these letters he describes the fossils of Monte Bolca, and attempts to trace the extent of the marine deposits of Italy,[5] and to distinguish the most important of the fossils. Similar descriptions and figures were published with reference to our own country at a later period. In 1766, Brander's *Fossilia Hantoniensia*, or Hampshire Fossils, appeared; containing excellent figures of fossil shells from a part of the south coast of England; and similar works came forth in other parts of Europe.

However exact might be the descriptions and figures thus produced, they could not give such complete information as the objects themselves, collected and permanently preserved in museums. Vallisneri says,[6] that having begun to collect fossils for the purpose of forming a grotto, he selected the best, and preserved them "as a noble diversion for the more curious." The museum of Calceolarius at Verona contained a celebrated collection of such remains. A copious description of it appeared in 1622. Such collections had been made from an earlier period, and catalogues of them published. Thus Gessner's work, *De Rerum Fossilium, Lapidum et Gemmarum Figuris* (1565), contains a catalogue of the cabinet of petrifactions collected by John Kentman; many catalogues of the same kind appeared in the seventeenth century.[7] Lhwyd's *Lythophylaccii Britannici Iconographia*, published at Oxford in 1669, and exhibiting a very ample catalogue of English Fossils contained in the Ashmolean Museum, may be noticed as one of these.

One of the most remarkable occurrences in the progress of descriptive geology in England, was the formation of a geological museum by William Woodward as early as 1695. This collection, formed with great labor, systematically arranged, and carefully catalogued, he bequeathed to the University of Cambridge; founding and endowing

[4] Augustine Scilla's original drawings of fossil shells, teeth, and corals, from which the engravings mentioned in the text were executed, as well as the natural objects from which the drawings were made, were bought by Woodward, and are now in the Woodwardian Museum at Cambridge.

[5] p. 20. [6] p. 1. [7] Parkinson, *Organic Remains*, vol. i. p. 20.

at the same time a professorship of the study of geology. The Woodwardian Museum still subsists, a monument of the sagacity with which its author so early saw the importance of such a collection.

Collections and descriptions of fossils, including in the term specimens of minerals of all kinds, as well as organic remains, were frequently made, and especially in places where mining was cultivated; but under such circumstances, they scarcely tended at all to that general and complete knowledge of the earth of which we are now tracing the progress.

In more modern times, collections may be said to be the most important books of the geologist, at least next to the strata themselves. The identifications and arrangements of our best geologists, the immense studies of fossil anatomy by Cuvier and others, have been conducted mainly by means of collections of specimens. They are more important in this study than in botany, because specimens which contain important geological information are both more rare and more permanent. Plants, though each individual is perishable, perpetuate and diffuse their kind; while the organic impression on a stone, if lost, may never occur in a second instance; but, on the other hand, if it be preserved in the museum, the individual is almost as permanent in this case, as the species in the other.

I shall proceed to notice another mode in which such information was conveyed.

### *Sect.* 3.—*First Construction of Geological Maps.*

Dr. Lister, a learned physician, sent to the Royal Society, in 1683, a proposal for maps of soils or minerals; in which he suggested that in the map of England, for example, each soil and its boundaries might be distinguished by color, or in some other way. Such a mode of expressing and connecting our knowledge of the materials of the earth was, perhaps, obvious, when the mass of knowledge became considerable. In 1720, Fontenelle, in his observations on a paper of De Reaumur's, which contained an account of a deposit of fossil-shells in Touraine, says, that in order to reason on such cases, "we must have a kind of geographical charts, constructed according to the collection of shells found in the earth." But he justly adds, "What a quantity of observations, and what time would it not require to form such maps!"

The execution of such projects required, not merely great labor, but several steps in generalization and classification, before it could take place. Still such attempts were made. In 1743, was published, *A new Philosophico-chorographical Chart of East Kent, invented and delineated* by Christopher Packe, M.D.; in which, however, the main object is rather to express the course of the valleys than the materials of the country. Guettard formed the project of a mineralogical map of France, and Monnet carried this scheme into effect in 1780,[8] "by order of the king." In these maps, however, the country is not considered as divided into soils, still less strata; but each part is marked with its predominant mineral only. The spirit of generalization which constitutes the main value of such a work is wanting.

Geological maps belong strictly to Descriptive Geology; they are free from those wide and doubtful speculations which form so large a portion of the earlier geological books. Yet even geological maps cannot be usefully or consistently constructed without considerable steps of classification and generalization. When, in our own time, geologists were become weary of controversies respecting theory, they applied themselves with extraordinary zeal to the construction of stratigraphical maps of various countries; flattering themselves that in this way they were merely recording incontestable facts and differences. Nor do I mean to intimate that their facts were doubtful, or their distinctions arbitrary. But still they were facts interpreted, associated, and represented, by means of the classifications and general laws which earlier geologists had established; and thus even Descriptive Geology has been brought into existence as a science by the formation of systems and the discovery of principles. At this we cannot be surprized, when we recollect the many steps which the formation of Classificatory Botany required. We must now notice some of the principal discoveries which tended to the formation of Systematic Descriptive Geology.

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[8] *Atlas et Description Minéralogique de la France, entrepris par ordre du Roi;* par MM. Guettard et Monnet, Paris, 1780, pp. 212, with 31 maps.

# CHAPTER II.

## FORMATION OF SYSTEMATIC DESCRIPTIVE GEOLOGY.

### *Sect.* 1.—*Discovery of the Order and Stratification of the Materials of the Earth.*

THAT the substances of which the earth is framed are not scattered and mixed at random, but possess identity and continuity to a considerable extent, Lister was aware, when he proposed his map. But there is, in his suggestions, nothing relating to stratification; nor any order of position, still less of time, assigned to these materials. Woodward, however, appears to have been fully aware of the general law of stratification. On collecting information from all parts, "the result was," he says, "that in time I was abundantly assured that the circumstances of these things in remoter countries were much the same with those of ours here: that the stone, and other terrestrial matter in France, Flanders, Holland, Spain, Italy, Germany, Denmark, and Sweden, was distinguished into *strata or layers*, as it is in England; that these strata were divided by parallel fissures; that there were enclosed in the stone and all the other denser kinds of terrestrial matter, great numbers of the shells, and other productions of the sea, in the same manner as in that of this island."[1] So remarkable a truth, thus collected from a copious collection of particulars by a patient induction, was an important step in the science.

These general facts now began to be commonly recognized, and followed into detail. Stukely the antiquary[2] (1724), remarked an important feature in the strata of England, that their *escarpments*, or steepest sides, are turned towards the west and north-west; and Strachey[3] (1719), gave a stratigraphical description of certain coal-mines near Bath.[4] Michell, appointed Woodwardian Professor at Cambridge

[1] *Natural History of the Earth*, 1723.

[2] *Itinerarium Curiosum*, 1724.

[3] *Phil. Trans.* 1719, and *Observations on Strata, &c.* 1729.

[4] Fitton *Annals of Philosophy*, N. S. vol. i. and ii. (1832, '3), p. 157.

in 1762, described this stratified structure of the earth far more distinctly than his predecessors, and pointed out, as the consequence of it, that "the same kinds of earths, stones, and minerals, will appear at the surface of the earth in long parallel slips, parallel to the long ridges of mountains; and so, in fact, we find them."[5]

Michell (as appeared by papers of his which were examined after his death) had made himself acquainted with the series of English strata which thus occur from Cambridge to York;—that is, from the chalk to the coal. These relations of position required that geological maps, to complete the information they conveyed, should be accompanied by geological *Sections*, or imaginary representations of the order and mode of superpositions, as well as of the superficial extent of the strata, as in more recent times has usually been done. The strata, as we travel from the higher to the lower, come from under each other into view; and this *out-cropping*, *basseting*, or by whatever other term it is described, is an important feature in their description.

It was further noticed that these relations of position were combined with other important facts, which irresistibly suggested the notion of a relation in time. This, indeed, was implied in all theories of the earth; but observations of the facts most require our notice. Steno is asserted by Humboldt[6] to be the first who (in 1669) distinguished between rocks anterior to the existence of plants and animals upon the globe, containing therefore no organic remains; and rocks super-imposed on these, and full of such remains; "turbidi maris sedimenta sibi invicem imposita."

Rouelle is stated by his pupil Desmarest, to have made some additional and important observations. "He saw," it is said, "that the shells which occur in rocks were not the same in all countries; that certain species occur together, while others do not occur in the same beds; that there is a constant order in the arrangement of these shells, certain species lying in distinct bands."[7]

Such divisions as these required to be marked by technical names. A distinction was made of *l'ancienne terre* and *la nouvelle terre*, to which Rouelle added a *travaille intermédiaire*. Rouelle died in 1770, having been known by lectures, not by books. Lehman, in 1756, claims for himself the credit of being the first to observe and describe correctly the structure of stratified countries; being ignorant, pro-

[5] *Phil. Trans.* 1760. [6] *Essai Géognastique.*

[7] *Encyl. Méthod. Geogr. Phys.* tom. i. p. 416, quoted by Fitton as above, p 159

bably, of the labors of Strachey in England. He divided mountains into three classes;[8] *primitive*, which were formed with the world;—those which resulted from a partial destruction of the primitive rocks—and a third class resulting from local or universal deluges. In 1759, also, Arduine,[9] in his Memoirs on the mountains of Padua, Vicenza, and Verona, deduced, from original observations, the distinction of rocks into *primary*, *secondary*, and *tertiary*.

The relations of position and fossils were, from this period, inseparably connected with opinions concerning succession in time. Odoardi remarked,[10] that the strata of the Sabapennine hills are *unconformable* to those of the Apennine, (as Strachey had observed, that the strata above the coal were unconformable to the coal;[11]) and his work contained a clear argument respecting the different ages of these two classes of hills. Fuchsel was, in 1762, aware of the distinctness of strata of different ages in Germany. Pallas and Saussure were guided by general views of the same kind in observing the countries which they visited: but, perhaps, the general circulation of such notions was most due to Werner.

### *Sect. 2.—Systematic form given to Descriptive Geology.—Werner.*

Werner expressed the general relations of the strata of the earth by means of classifications which, so far as general applicability is concerned, are extremely imperfect and arbitrary; he promulgated a theory which almost entirely neglected all the facts previously discovered respecting the grouping of fossils,—which was founded upon observations made in a very limited district of Germany,—and which was contradicted even by the facts of this district. Yet the acuteness of his discrimination in the subjects which he studied, the generality of the tenets he asserted, and the charm which he threw about his speculations, gave to Geology, or, as he termed it, *Geognosy*, a popularity and reputation which it had never before possessed. His system had asserted certain universal formations, which followed each other in a constant order;—granite the lowest,—then mica-slate and clay-slate;—upon these *primitive* rocks, generally highly inclined, rest other *transition* strata;—upon these, lie *secondary* ones, which being more nearly horizontal, are called *flötz* or flat. The term *formation*,

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[8] Lyell, i. 70. [9] Ib. 72.
[10] Ib. 74. [11] Fitton, p. 157.

which we have thus introduced, indicating groups which, by evidence of all kinds,—of their materials, their position, and their organic contents,—are judged to belong to the same period, implies no small amount of theory: yet this term, from this time forth, is to be looked upon as a term of classification solely, so far as classification can be separately attended to.

Werner's distinctions of strata were for the most part drawn from mineralogical constitution. Doubtless, he could not fail to perceive the great importance of organic fossils. "I was witness," says M. de Humboldt, one of his most philosophical followers, "of the lively satisfaction which he felt when, in 1792, M. de Schlotheim, one of the most distinguished geologists of the school of Freiberg, began to make the relations of fossils to strata the principal object of his studies." But Werner and the disciples of his school, even the most enlightened of them, never employed the characters derived from organic remains with the same boldness and perseverance as those who had from the first considered them as the leading phenomena: thus M. de Humboldt expresses doubts which perhaps many other geologists do not feel when, in 1823, he says, "Are we justified in concluding that all formations are characterized by particular species? that the fossil-shells of the chalk, the muschelkalk, the Jura limestone, and the Alpine limestone, are all different? I think this would be pushing the induction much too far."[12] In Prof. Jamieson's *Geognosy*, which may be taken as a representation of the Wernerian doctrines, organic fossils are in no instance referred to as characters of formations or strata. After the curious and important evidence, contained in organic fossils, which had been brought into view by the labors of Italian, English, and German writers, the promulgation of a system of Descriptive Geology, in which all this evidence was neglected, cannot be considered otherwise than as a retrograde step in science.

Werner maintained the aqueous deposition of all strata above the primitive rocks; even of those *trap* rocks, to which, from their resemblance to lava and other phenomena, Raspe, Arduino, and others, had already assigned a volcanic origin. The fierce and long controversy between the *Vulcanists* and *Neptunists*, which this dogma excited, does not belong to this part of our history; but the discovery of veins of granite penetrating the superincumbent slate, to which the controversy led, was an important event in descriptive geology. Hutton, the

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[12] *Gissement des Roches*, p. 41

author of the theory of igneous causation which was in this country opposed to that of Werner, sought and found this phenomenon in the Grampian hills, in 1785. This supposed verification of his system "filled him with delight, and called forth such marks of joy and exultation, that the guides who accompanied him were persuaded, says his biographer,[13] that he must have discovered a vein of silver or gold."[14]

Desmarest's examination of Auvergne (1768) showed that there was there an instance of a country which could not even be described without terms implying that the basalt, which covered so large a portion of it, had flowed from the craters of extinct volcanoes. His map of Auvergne was an excellent example of a survey of such a country, thus exhibiting features quite different from those of common stratified countries.[15]

The facts connected with metalliferous veins were also objects of Werner's attention. A knowledge of such facts is valuable to the geologist as well as to the miner, although even yet much difficulty attends all attempts to theorize concerning them. The facts of this nature have been collected in great abundance in all mining districts; and form a prominent part of the descriptive geology of such districts; as, for example, the Hartz, and Cornwall.

Without further pursuing the history of the knowledge of the inorganic phenomena of the earth, I turn to a still richer department of geology, which is concerned with organic fossils.

### *Sect. 3.—Application of Organic Remains as a Geological Character.—Smith.*

Rouelle and Odoardi had perceived, as we have seen, that fossils were grouped in bands: but from this general observation to the execution of a survey of a large kingdom, founded upon this principle, would have been a vast stride, even if the author of it had been aware of the doctrines thus asserted by these writers. In fact, however, William Smith executed such a survey of England, with no other guide or help than his own sagacity and perseverance. In his employments as a civil engineer, he noticed the remarkable continuity and constant order of the strata in the neighborhood of Bath, as discriminated by their fossils; and about the year 1793, he[16] drew up a Tabular View of the

[13] Playfair's *Works*, vol. iv. p. 75.
[14] Lyell, i. 90.
[15] Lyell, i. 86.
[16] Fitton, p. 148.

strata of that district, which contained the germ of his subsequent discoveries. Finding in the north of England the same strata and associations of strata with which he had become acquainted in the west, he was led to name them and to represent them by means of maps, according to their occurrence over the whole face of England. These maps appeared[17] in 1815; and a work by the same author, entitled *The English Strata identified by Organic Remains*, came forth later. But the views on which this identification of strata rests, belong to a considerably earlier date; and had not only been acted upon, but freely imparted in conversation many years before.

In the meantime the study of fossils was pursued with zeal in various countries. Lamarck and Defrance employed themselves in determining the fossil shells of the neighborhood of Paris;[18] and the interest inspired by this subject was strongly nourished and stimulated by the memorable work of Cuvier and Brongniart, *On the Environs of Paris*, published in 1811, and by Cuvier's subsequent researches on the subjects thus brought under notice. For now, not only the distinction, succession, and arrangement, but many other relations among fossil strata, irresistibly arrested the attention of the philosopher. Brongniart[19] showed that very striking resemblances occurred in their fossil remains, between certain strata of Europe and of North America; and proved that a rock may be so much disguised, that the identity of the stratum can only be recognized by geological characters.[20]

The Italian geologists had found in their hills, for the most part, the same species of shells which existed in their seas; but the German and English writers, as Gesner,[21] Raspe,[22] and Brander,[23] had perceived that the fossil-shells were either of unknown species, or of such as lived in distant latitudes. To decide that the animals and plants, of which we find the remains in a fossil state, were of species now extinct, obviously required an exact and extensive knowledge of natural history. And if this were so, to assign the relations of the past to the existing tribes of beings, and the peculiarities of their vital processes and habits, were tasks which could not be performed without the most consummate physiological skill and talent. Such tasks, however, have been the familiar employments of geologists, and naturalists incited and

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[17] Brit. Assoc. 1832. Conybeare, p. 373. [18] Humboldt, *Giss. d. R.* p. 35.
[19] *Hist. Nat. des Crustacés Fossiles*, pp. 57, 62.
[20] Humboldt, *Giss. d. R.* p. 45.
[21] Lyell, i. 70. [22] Ib. 74. [23] Ib. 76

appealed to by geologists, ever since Cuvier published his examination of the fossil inhabitants of the Paris basin. Without attempting a history of such labors, I may notice a few circumstances connected with them.

### *Sect.* 4.—*Advances in Palæontology.—Cuvier.*

So long as the organic fossils which were found in the strata of the earth were the remains of marine animals, it was very difficult for geologists to be assured that the animals were such as did not exist in any part or clime of the existing ocean. But when large land and river animals were discovered, different from any known species, the persuasion that they were of extinct races was forced upon the naturalist. Yet this opinion was not taken up slightly, nor acquiesced in without many struggles.

Bones supposed to belong to fossil elephants, were some of the first with regard to which this conclusion was established. Such remains occur in vast numbers in the soil and gravel of almost every part of the world; especially in Siberia, where they are called the bones of the *mammoth.* They had been noticed by the ancients, as we learn from Pliny;[24] and had been ascribed to human giants, to elephants imported by the Romans, and to many other origins. But in 1796, Cuvier had examined these opinions with a more profound knowledge than his predecessors; and he thus stated the result of his researches.[25] "With regard to what have been called the fossil remains of elephants, from Tentzelius to Pallas, I believe that I am in the condition to prove, that they belong to animals which were very clearly different in species from our existing elephants, although they resembled them sufficiently to be considered as belonging to the same genera." He had founded this conclusion principally on the structure of the teeth, which he found to differ in the Asiatic and African elephant; while, in the fossil animal, it was different from both. But he also reasoned in part on the form of the skull, of which the best-known example had been described in the *Philosophical Transactions* as early as 1737.[26] "As soon," says Cuvier, at a later period, "as I became acquainted with Messerschmidt's drawing, and joined to the differences which it presented, those which I had myself observed in the inferior jaw and the

[24] *Hist. Nat.* lib. xxxvi. 18.

[25] *Mém. Inst. Math. et Phys.* tom. ii. p. 4.

[26] Described by Breyne from a specimen found in Siberia by Messerschmidt in 1722. *Phil. Trans.* xl. 446.

molar teeth, I no longer doubted that the fossil elephants were of a species different from the Indian elephant. This idea, which I announced to the Institute in the month of January, 1796, opened to me views entirely new respecting the theory of the earth; and determined me to devote myself to the long researches and to the assiduous labors which have now occupied me for twenty-five years."[27]

We have here, then, the starting-point of those researches concerning extinct animals, which, ever since that time, have attracted so large a share of notice from geologists and from the world. Cuvier could hardly have anticipated the vast storehouse of materials which lay under his feet, ready to supply him occupation of the most intense interest in the career on which he had thus entered. The examination of the strata on which Paris stands, and of which its buildings consist, supplied him with animals, not only different from existing ones, but some of them of great size and curious peculiarities. A careful examination of the remains which these strata contain was undertaken soon after the period we have referred to. In 1802, Defrance had collected several hundreds of undescribed species of shells; and Lamarck[28] began a series of Memoirs upon them; remodelling the whole of Conchology, in order that they might be included in its classifications. And two years afterwards (1804) appears the first of Cuvier's grand series of Memoirs containing the restoration of the vertebrate animals of these strata. In this vast natural museum, and in contributions from other parts of the globe, he discovered the most extraordinary creatures:—the Palæotherium,[29] which is intermediate between the horse and the pig; the Anoplotherium, which stands nearest to the rhinoceros and the tapir; the Megalonix and Megatherium, animals of the sloth tribe, but of the size of the ox and the rhinoceros. The Memoirs which contained these and many other discoveries, set the naturalists to work in every part of Europe.

Another very curious class of animals was brought to light principally by the geologists of England; animals of which the bones, found in the *lias* stratum, were at first supposed to be those of crocodiles. But in 1816,[30] Sir Everard Home says, "In truth, on a consideration of this skeleton, we cannot but be inclined to believe, that among the animals destroyed by the catastrophes of remote antiquity, there had

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[27] *Ossemens Fossiles.* second edit. i. 178.

[28] *Annales du Muséum d'Hist. Nat.* tom. i. p. 308, and the following volumes.

[29] Daubuisson, ii. 411.

[30] *Phil. Trans.* 1816, p. 20.

been some at least that differ so entirely in their structure from any which now exist as to make it impossible to arrange their fossil remains with any known class of animals." The animal thus referred to, being clearly intermediate between fishes and lizards, was named by Mr. König, *Ichthyosaurus;* and its structure and constitution were more precisely determined by Mr. Conybeare in 1821, when he had occasion to compare with it another extinct animal of which he and Mr. de la Beche had collected the remains. This animal, still more nearly approaching the lizard tribe, was by Mr. Conybeare called *Plesiosaurus*.[31] Of each of these two genera several species were afterwards found.

Before this time, the differences of the races of animals and plants belonging to the past and the present periods of the earth's history, had become a leading subjeet of speculation among geological naturalists. The science produced by this study of the natural history of former states of the earth has been termed *Palæontology;* and there is no branch of human knowledge more fitted to stir men's wonder, or to excite them to the widest physiological speculations. But in the present part of our history this science requires our notice, only so far as it aims at the restoration of the types of ancient animals, on clear and undoubted principles of comparative anatomy. To show how extensive and how conclusive is the science when thus directed, we need only refer to Cuvier's *Ossemens Fossiles;*[32] a work of vast labor and profound knowledge, which has opened wide the doors of this part of geology. I do not here attempt even to mention the labors of the many other eminent contributors to Palæontology; as Brocchi, Des Hayes, Sowerby, Goldfuss, Agassiz, who have employed themselves on animals, and Schlottheim, Brongniart, Hutton, Lindley, on plants.

[2nd Ed.] [Among the many valuable contributions to Palæontology in more recent times, I may especially mention Mr. Owen's *Reports on British Fossil Reptiles*, *on British Fossil Mammalia*, and *on the Extinct Animals of Australia*, with descriptions of certain Fossils indicative of large Marsupial Pachydermata: and M. Agassiz's *Report on the Fossil Fishes of the Devonian System*, his *Synoptical Table of British Fossil Fishes*, and his *Report on the Fishes of the London Clay.* All these are contained in the volumes produced by the British Association from 1839 to 1845.

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[31] *Geol. Trans.* vol. v.

[32] The first edition appeared in 1812, consisting principally of the Memoirs to which reference has already been made.

A new and most important instrument of palæontological investigation has been put in the geologist's hand by Prof. Owen's discovery that the internal structure of teeth, as disclosed by the microscope, is a means of determining the kind of the animal. He has carried into every part of the animal kingdom an examination founded upon this discovery, and has published the results of this in his *Odontography.* As an example of the application of this character of animals, I may mention that a tooth brought from Riga by Sir R. Murchison was in this way ascertained by Mr. Owen to belong to a fish of the genus *Dendrodus.* (*Geology of Russia*, i. 67.)]

When it had thus been established, that the strata of the earth are characterized by innumerable remains of the organized beings which formerly inhabited it, and that anatomical and physiological considerations must be carefully and skilfully applied in order rightly to interpret these characters, the geologist and the palæontologist obviously had, brought before them, many very wide and striking questions. Of these we may give some instances; but, in the first place, we may add a few words concerning those eminent philosophers to whom the science owed the basis on which succeeding speculations were to be built.

### *Sect. 5.—Intellectual Characters of the Founders of Systematic Descriptive Geology.*

It would be in accordance with the course we have pursued in treating of other subjects, that we should attempt to point out in the founders of the science now under consideration, those intellectual qualities and habits to which we ascribe their success. The very recent date of the generalizations of geology, which has hardly allowed us time to distinguish the calm expression of the opinion of the wisest judges, might, in this instance, relieve us from such a duty; but since our plan appears to suggest it, we will, at least, endeavor to mark the characters of the founders of geology, by a few of their prominent lines.

The three persons who must be looked upon as the main authors of geological classification are, Werner, Smith, and Cuvier. These three men were of very different mental constitution; and it will, perhaps, not be difficult to compare them, in reference to those qualities which we have all along represented as the main features of the discoverer's genius, clearness of ideas, the possession of numerous facts, and the power of bringing these two elements into contact.

In the German, considering him as a geologist, the ideal element predominated. That Werner's powers of external discrimination were extremely acute, we have seen in speaking of him as a mineralogist; and his talent and tendency for classifying were, in his mineralogical studies, fully fed by an abundant store of observation; but when he came to apply this methodizing power to geology, the love of system, so fostered, appears to have been too strong for the collection of facts he had to deal with. As we have already said, he promulgated, as representing the world, a scheme collected from a province, and even too hastily gathered from that narrow field. Yet his intense spirit of method in some measure compensated for other deficiencies, and enabled him to give the character of a science to what had been before a collection of miscellaneous phenomena. The ardor of system-making produced a sort of fusion, which, however superficial, served to bind together the mass of incoherent and mixed materials, and thus to form, though by strange and anomalous means, a structure of no small strength and durability, like the ancient vitrified structures which we find in some of our mountain regions.

Of a very different temper and character was William Smith. No literary cultivation of his youth awoke in him the speculative love of symmetry and system; but a singular clearness and precision of the classifying power, which he possessed as a native talent, was exercised and developed by exactly those geological facts among which his philosophical task lay. Some of the advances which he made, had, as we have seen, been at least entered upon by others who preceded him: but of all this he was ignorant; and, perhaps, went on more steadily and eagerly to work out his own ideas, from the persuasion that they were entirely his own. At a later period of his life, he himself published an account of the views which had animated him in his earlier progress. In this account[33] he dates his attempts to discriminate and connect strata from the year 1790, at which time he was twenty years old. In 1792, he "had considered how he could best represent the order of superposition—continuity of course—and general eastern declination of the strata." Soon after, doubts which had arisen were removed by the "discovery of a mode of identifying the strata by the organized fossils respectively imbedded therein." And "thus stored with ideas," as he expresses himself, he began to communicate them to his friends. In all this, we see great vividness

[33] *Phil. Mag.* 1833, vol. i. p. 38.

of thought and activity of mind, unfolding itself exactly in proportion to the facts with which it had to deal. We are reminded of that cyclopean architecture in which each stone, as it occurs, is, with wonderful ingenuity, and with the least possible alteration of its form, shaped so as to fit its place in a solid and lasting edifice.

Different yet again was the character (as a geological discoverer) of the great naturalist of the beginning of the nineteenth century. In that part of his labors of which we have now to speak, Cuvier's dominant ideas were rather physiological than geological. In his views of past physical changes, he did not seek to include any ranges of facts which lay much beyond the narrow field of the Paris basin. But his sagacity in applying his own great principle of the Conditions of Existence, gave him a peculiar and unparalleled power in interpreting the most imperfect fossil records of extinct anatomy. In the constitution of his mind, all philosophical endowments were so admirably developed and disciplined, that it was difficult to say, whether more of his power was due to genius or to culture. The talent of classifying which he exercised in geology, was the result of the most complete knowledge and skill in zoology; while his views concerning the revolutions which had taken place in the organic and inorganic world, were in no small degree aided by an extraordinary command of historical and other literature. His guiding ideas had been formed, his facts had been studied, by the assistance of all the sciences which could be made to bear upon them. In his geological labors we seem to see some beautiful temple, not only firm and fair in itself, but decorated with sculpture and painting, and rich in all that art and labor, memory and imagination, can contribute to its beauty.

[2nd Ed.] [Sir Charles Lyell (B. i. c. iv.) has quoted with approval what I have elsewhere said, that the advancement of three of the main divisions of geology in the beginning of the present century was promoted principally by the three great nations of Europe,—the German, the English, and the French:—Mineralogical Geology by the German school of Werner:—Secondary Geology by Smith and his English successors;—Tertiary Geology by Cuvier and his fellow-laborers in France.]

## CHAPTER III.

### Sequel to the Formation of Systematic Descriptive Geology.

*Sect.* 1.—*Reception and Diffusion of Systematic Geology.*

IF our nearness to the time of the discoveries to which we have just referred, embarrasses us in speaking of their authors, it makes it still more difficult to narrate the reception with which these discoveries met. Yet here we may notice a few facts which may not be without their interest.

The impression which Werner made upon his hearers was very strong; and, as we have already said, disciples were gathered to his school from every country, and then went forward into all parts of the world, animated by the views which they had caught from him. We may say of him, as has been so wisely said of a philosopher of a very different kind,[1] "He owed his influence to various causes; at the head of which may be placed that genius for system, which, though it cramps the growth of knowledge, perhaps finally atones for that mischief by the zeal and activity which it rouses among followers and opponents, who discover truth by accident, when in pursuit of weapons for their warfare." The list of Werner's pupils for a considerable period included most of the principal geologists of Europe; Freisleben, Mohs, Esmark, d'Andrada, Raumer, Engelhart, Charpentier, Brocchi. Alexander von Humboldt and Leopold von Buch went forth from his school to observe America and Siberia, the Isles of the Atlantic, and the coast of Norway. Professor Jameson established at Edinburgh a Wernerian Society; and his lecture-room became a second centre of Wernerian doctrines, whence proceeded many zealous geological observers; among these we may mention as one of the most distinguished, M. Ami Boué, though, like several others, he soon cast away the peculiar opinions of the Wernerian school. The classifications of this school were, however, diffused over the civilized world with ex-

[1] Mackintosh *on Hobbes*, Dissert. p. 177.

traordinary success; and were looked upon with great respect, till the study of organic fossils threw them into the shade.

Smith, on the other hand, long pursued his own thoughts without aid and without sympathy. About 1799 he became acquainted with a few gentlemen (Dr. Anderson, Mr. Richardson, Mr. Townsend, and Mr. Davies), who had already given some attention to organic fossils, and who were astonished to find his knowledge so much more exact and extensive than their own. From this time he conceived the intention of publishing his discoveries; but the want of literary leisure and habits long prevented him. His knowledge was orally communicated without reserve to many persons; and thus gradually and insensibly became part of the public stock. When this diffusion of his views had gone on for some time, his friends began to complain that the author of them was deprived of his well-merited share of fame. His delay in publication made it difficult to remedy this wrong; for soon after he published his Geological Map of England, another appeared, founded upon separate observations; and though, perhaps, not quite independent of his, yet in many respects much more detailed and correct. Thus, though his general ideas obtained universal currency, he did not assume his due prominence as a geologist In 1818, a generous attempt was made to direct a proper degree of public gratitude to him, in an article in the *Edinburgh Review*, the production of Dr. Fitton, a distinguished English geologist. And when the eminent philosopher, Wollaston, had bequeathed to the Geological Society of London a fund from which a gold medal was to be awarded to geological services, the first of such medals was, in 1831, "given to Mr. William Smith, in consideration of his being a great original discoverer in English geology; and especially for his having been the first in this country to discover and to teach the identification of strata, and to determine their succession by means of their imbedded fossils."

Cuvier's discoveries, on the other hand, both from the high philosophic fame of their author, and from their intrinsic importance, arrested at once the attention of scientific Europe; and, notwithstanding the undoubted priority of Smith's labors, for a long time were looked upon as the starting-point of our knowledge of organic fossils. And, in reality, although Cuvier's memoirs derived the greatest part of their value from his zoological conclusions, they reflected back no small portion of interest on the classifications of strata which were involved in his inferences. And the views which he presented gave to geology an attractive and striking character, and a connexion with 'arge physiological as well as physical principles, which added incomparably to its dignity and charm.

In tracing the reception and diffusion of doctrines such as those of Smith and Cuvier, we ought not to omit to notice more especially the formation and history of the Geological Society of London, just mentioned. It was established in 1807, with a view to multiply and record observations, and patiently to await the result of some future period; that is, its founders resolved to apply themselves to Descriptive Geology, thinking the time not come for that theoretical geology which had then long fired the controversial ardor of Neptunists and Plutonists. The first volume of the Transactions of this society was published in 1811. The greater part of the contents of this volume [2] savor of the notions of the Wernerian school; and there are papers on some of the districts in England most rich in fossils, which Mr. Conybeare says, well exhibit the low state of secondary geology at that period. But a paper by Mr. Parkinson refers to the discoveries both of Smith and of Cuvier; and in the next volume, Mr. Webster gives an account of the Isle of Wight, following the admirable model of Cuvier and Brongniart's account of the Paris basin. "If we compare this memoir of Mr. Webster with the preceding one of Dr. Berger (also of the Isle of Wight), they at once show themselves to belong to two very distinct eras of science; and it is difficult to believe that the interval which elapsed between their respective publication was only three or four years."[3]

Among the events belonging to the diffusion of sound geological views in this country, we may notice the publication of a little volume entitled, *The Geology of England and Wales*, by Mr. Conybeare and Mr. Phillips, in 1821; an event far more important than, from the modest form and character of the work, it might at first sight appear. By describing in detail the geological structure and circumstances of England (at least as far downwards as the coal), it enabled a very wide class of readers to understand and verify the classifications which geology had then very recently established; while the extensive knowledge and philosophical spirit of Mr. Conybeare rendered it, under the guise of a topographical enumeration, in reality a profound and instructive scientific treatise. The vast impulse which it gave to the study of sound descriptive geology was felt and acknowledged in other countries, as well as in Britain.

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[2] Conybeare. *Report. Brit. Assoc* p. 372. [3] Conybeare, *Report,* p. 372.

Since that period, Descriptive Geology in England has constantly advanced. The advance has been due mainly to the labors of the members of the Geological Society; on whose merits as cultivators of their science, none but those who are themselves masters of the subject, have a right to dwell. Yet some parts of the scientific character of these men may be appreciated by the general speculator; for they have shown that there are no talents and no endowments which may not find their fitting employment in this science. Besides that they have united laborious research and comprehensive views, acuteness and learning, zeal and knowledge; the philosophical eloquence with which they have conducted their discussions has had a most beneficial influence on the tone of their speculations; and their researches in the field, which have carried them into every country and every class of society, have given them that prompt and liberal spirit, and that open and cordial bearing, which results from intercourse with the world on a large and unfettered scale. It is not too much to say, that in our time, Practical Geology has been one of the best schools of philosophical and general culture of mind.

### *Sect. 2.—Application of Systematic Geology. Geological Surveys and Maps.*

Such surveys as that which Conybeare and Phillips's book presented with respect to England, were not only a means of disseminating the knowledge implied in the classifications of such a work, but they were also an essential part of the Application and Extension of the principles established by the founders of Systematic Geology. As soon as the truth of such a system was generally acknowledged, the persuasion of the propriety of geological surveys and maps of each country could not but impress itself on men's minds.

When the earlier writers, as Lister and Fontenelle, spoke of mineralogical and fossilological maps, they could hardly be said to know the meaning of the terms which they thus used. But when subsequent classifications had shown how such a suggestion might be carried into effect, and to what important consequences it might lead, the task was undertaken in various countries in a vigorous and consistent manner. In England, besides Smith's map, another, drawn up by Mr. Greenough, was published by the Geological Society in 1819; and, being founded on very numerous observations of the author and his friends, made with great labor and cost, was not only an important correction and confirmation of Smith's labors, but a valuable storehouse and standard of what had then been done in English geology. Leopold von Buch had constructed a geological map of a large portion of Germany, about the same period; but, aware of the difficulty of the task he had thus attempted, he still forbore to publish it. At a later period, and as materials accumulated, more detailed maps of parts of Germany were produced by Hoffmann and others. The French government entrusted to a distinguished Professor of the School of Mines (M. Brochant de Villiers), the task of constructing a map of France on the model of Mr. Greenough's; associating with him two younger persons, selected for their energy and talents, MM. Beaumont and Dufrénoy. We shall have occasion hereafter to speak of the execution of this survey. By various persons, geological maps of almost every country and province of Europe, and of many parts of Asia and America, have been published. I need not enumerate these, but I may refer to the account given of them by Mr. Conybeare, in the *Reports of the British Association for* 1832, p. 384. These various essays may be considered as contributions, though hitherto undoubtedly very imperfect ones, to that at which Descriptive Geology ought to aim, and which is requisite as a foundation for sound theory;—a complete geological survey of the whole earth. But we must say a few words respecting the language in which such a survey must be written.

As we have already said, that condition which made such maps and the accompanying descriptions possible, was that the strata and their contents had previously undergone classification and arrangement at the hands of the fathers of geology. Classification, in this as in other cases, implied names which should give to the classes distinctness and permanence; and when the series of strata belonging to one country were referred to in the description of another, in which they appeared, as was usually the case, under an aspect at least somewhat different, the supposed identification required a peculiar study of each case; and thus Geology had arrived at the point, which we have before had to notice as one of the stages of the progress of Classificatory Botany, at which a technical *nomenclature* and a well-understood *synonymy* were essential parts of the science.

### *Sect.* 3.—*Geological Nomenclature.*

By Nomenclature we mean a *system* of names; and hence we can not speak of a Geological Nomenclature till we come to Werner and Smith. The earlier mineralogists had employed names, often artificial and arbitrary, for special minerals, but no technical and constant names for strata. The elements of Werner's names for the members of his geological series were words in use among miners, as *Gneiss*, *Grauwacke*, *Thonschiefer*, *Rothe todte liegende*, *Zechstein ;* or arbitrary names of the mineralogists, as Syenite, Serpentine, Porphyry, Granite. But the more technical part of his phraseology was taken from that which is the worst kind of name, arbitrary numeration. Thus he had his *first* sandstone formation, *second* sandstone, *third* sandstone ; *first* flötz limestone, *second* flötz limestone, *third* flötz limestone. Such names are, beyond all others, liable to mistake in their application, and likely to be expelled by the progress of knowledge ; and accordingly, though the Wernerian names for rocks mineralogically distinguished, have still some currency, his sandstones and limestones, after creating endless confusion while his authority had any sway, have utterly disappeared from good geological works.

The nomenclature of Smith was founded upon English provincial terms of very barbarous aspect, as *Cornbrash*, *Lias*, *Gault*, *Clunch Clay*, *Coral Rag*. Yet these terms were widely diffused when his classification was generally accepted ; they kept their place, precisely because they had no systematic signification ; and many of them are at present part of the geological language of the whole civilized world.

Another kind of names which has been very prevalent among geologists are those borrowed from places. Thus the Wernerians spoke of Alpine Limestone and Jura Limestone ; the English, of Kimmeridge Clay and Oxford Clay, Purbeck Marble, and Portland Rock. These names, referring to the stratum of a known locality as a type, were good, as far as an identity with that type had been traced ; but when this had been incompletely done, they were liable to great ambiguity. If the Alps or the Jura contain several formations of limestone, such terms as we have noticed, borrowed from those mountains, cease to be necessarily definite, and may give rise to much confusion.

Descriptive names, although they might be supposed to be the best, have, in fact, rarely been fortunate. The reason of this is obvious ;— the mark which has been selected for description may easily fail to be essential ; and the obvious connexions of natural facts may overleap the arbitrary definition. As we have already stated in the history of botany, the establishment of descriptive marks of real classes presupposes the important but difficult step, of the discovery of such marks.

Hence those descriptive names only have been really useful in geology which had been used without any scrupulous regard to the appropriateness of the description. The *Green Sand* may be white, brown, or red; the *Mountain Limestone* may occur only in valleys; the *Oolite* may have no roe-like structure; and yet these may be excellent geological names, if they be applied to formations geologically identical with those which the phrases originally designated. The signification may assist the memory, but must not be allowed to subjugate the faculty of natural classification.

The terms which have been formed by geologists in recent times have been drawn from sources similar to those of the older ones, and will have their fortune determined by the same conditions. Thus Mr. Lyell has given to the divisions of the tertiary strata the appellations *Pleiocene*, *Meiocene*, *Eocene*, accordingly as they contain a *majority* of recent species of shells, a *minority* of such species, or a small proportion of living species, which may be looked upon as indicating the *dawn* of the existing state of the animate creation. But in this case, he wisely treats his distinctions, not as definitions, but as the marks of natural groups. "The plurality of species indicated by the name *pleiocene*, must not," he says,[4] "be understood to imply an absolute majority of recent fossil shells in all cases, but a comparative preponderance wherever the pleiocene are contrasted with strata of the period immediately preceding."

Mr. Lyell might have added, that no precise percentage of recent species, nor any numerical criterion whatever, can be allowed to overbear the closer natural relations of strata, proved by evidence of a superior kind, if such can be found. And this would be the proper answer to the objection made by De la Beche to these names; namely, that it may happen that the *meiocene* rocks of one country may be of the same date as the *pleiocene* of another; the same formation having in one place a majority, in another a minority, of existing species. We are not to run into this incongruity, for we are not so to apply the names. The formation which has been called pleiocene, must continue to be so called, even where the majority of recent species fails; and all rocks that agree with that in date, without further reference to the numerical relations of their fossils, must also share in the name.

To invent good names for these large divisions of the series of strata is indeed extremely difficult. The term *Oolite* is an instance in which

[4] *Geol.* iii. 392.

a descriptive word has become permanent in a case of this kind; and, in imitation of it, *Pœcilite* (from ποικίλος, various, (has been proposed by Mr. Conybeare[5] as a name for the group of strata inferior to the oolites, of which the *Variegated* Sandstone (Bunter Sandstein, Grès Bigarré,) is a conspicuous member. For the series of formations which lies immediately over the rocks in which no organic remains are found, the term *Transition* was long used, but with extreme ambiguity and vagueness. When this series, or rather the upper part of it, was well examined in South Wales, where it consists of many well-marked members, and may be probably taken as a type for a large portion of the rest of the world, it became necessary to give to the group thus explored a name not necessarily leading to assumption or controversy. Mr. Murchison selected the term *Silurian*, borrowed from the former inhabitants of the country in which his types were found; and this is a term excellent in many respects; but one which will probably not quite supersede "Transition," because, in other places, transition rocks occur which correspond to none of the members of the Silurian region.

Though new names are inevitable accompaniments of new views of classification, and though, therefore, the geological discoverer must be allowed a right to coin them, this is a privilege which, for the sake of his own credit, and the circulation of his tokens, he must exercise with great temperance and judgment. M. Brongniart may be taken as an example of the neglect of this caution. Acting upon the principle, in itself a sound one, that inconveniences arise from geological terms which have a mineralogical signification, he has given an entirely new list of names of the members of the geological series. Thus the primitive unstratified rocks are *terrains agalysiens;* the transition semi-compact are *hemilysiens;* the sedimentary strata are *yzemiens;* the diluvial deposits are *clysmiens;* and these divisions are subdivided by designations equally novel; thus of the "terrains yzemiens," members are—the terrains *clastiques*, *tritoniens*, *protéïques*, *palæotheriens*, *epilymniques*, *thalassiques*.[6] Such a nomenclature appears to labor under great inconveniences, since the terms are descriptive in their derivation, yet are not generally intelligible, and refer to theoretical views yet have not the recommendation of systematic connexion.

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[5] *Report*, p. 379.

[6] Brongniart, *Tableau des Terrains*, 1829.

*Sect. 4.—Geological Synonymy, or Determination of Geological Equivalents.*

It will easily be supposed that with so many different sources of names as we have mentioned, the same stratum may be called by different designations; and thus a synonymy may be necessary for geology; as it was for botany in the time of Bauhin, when the same plants had been spoken of by so many different appellations in different authors. But in reality, the synonymy of geology is a still more important part of the subject than the analogy of botany would lead us to suppose. For in plants, the species are really fixed, and easily known when seen; and the ambiguity is only in the imperfect communication or confused ideas of the observers. But in geology, the identity of a stratum or formation in different places, though not an arbitrary, may be a very doubtful matter, even to him who has seen and examined. To assign its right character and place to a stratum in a new country, is, in a great degree, to establish the whole geological history of the country. To assume that the same names may rightly be applied to the strata of different countries, is to take for granted, not indeed the Wernerian dogma of universal formations, but a considerable degree of generality and uniformity in the known formations. And how far this generality and uniformity prevail, observation alone can teach. The search for geological synonyms in different countries brings before us two questions;—first, *are* there such synonyms? and only in the second place, and as far as they occur, *what* are they?

In fact, it is found that although formations which must be considered as geologically identical (because otherwise no classification is possible,) do extend over large regions, and pass from country to country, their identity includes certain modifications; and the determination of the identity and of the modifications are inseparably involved with each other, and almost necessarily entangled with theoretical considerations. And in two countries, in which we find this modified coincidence, instead of saying that the strata are identical, and that their designations are synonyms, we may, with more propriety, consider them as two corresponding series; of which the members of the one may be treated as the *Representatives* or *Equivalents* of the members of the other.

This doctrine of Representatives or Equivalents supposes that the geological phenomena in the two countries have been the results of similar series of events, which have, in some measure, coincided in time and order; and thus, as we have said, refers us to a theory. But yet, considered merely as a step in classification, the comparison of the geological series of strata in different countries is, in the highest degree, important and interesting. Indeed in the same manner in which the separation of Classificatory from Chemical Mineralogy is necessary for the completion of mineralogical science, the comparative Classification of the strata of different countries according to their resemblances and differences alone, is requisite as a basis for a Theory of their causes. But, as will easily be imagined from its nature, this part of descriptive geology deals with the most difficult and the most elevated problems; and requires a rare union of laborious observation with a comprehensive spirit of philosophical classification.

In order to give instances of this process (for of the vast labor and great talents which have been thus employed in England, France, and Germany, it is only instances that we can give,) I may refer to the geological survey of France, which was executed, as we have already stated, by order of the government. In this undertaking it was intended to obtain a knowledge of the whole mineral structure of France; but no small portion of this knowledge was brought into view, when a synonymy had been established between the Secondary Rocks of France and the corresponding members of the English and German series, which had been so well studied as to have become classical points of standard reference. For the purpose of doing this, the principal directors of the survey, MM. Brochant de Villiers, De Beaumont, and Dufrénoy, came to England in 1822, and following the steps of the best English geologists, in a few months made themselves acquainted with the English series. They then returned to France, and, starting from the chalk of Paris in various directions, travelled on the lines which carried them over the edges of the strata which emerge from beneath the chalk, identifying, as they could, the strata with their foreign analogues. They thus recognized almost all of the principal beds of the oolitic series of England.[7] At the same time they found differences as well as resemblances. Thus the Portland and Kimmeridge beds of France were found to contain in abundance a certain shell, the *gryphæa virgula*, which had not before been much remarked in those beds in England. With regard to the synonyms in Germany, on the other hand, a difference of opinion

[7] De la Beche, *Manual*, 305

arose between M. Elie de Beaumont and M. Voltz,[8] the former considering the *Grès de Vosges* as the equivalent of the *Rothe todte liegende*, which occurs beneath the Zechstein, while M. Voltz held that it was the lower portion of the Red or *Variegated Sandstone* which rests on the Zechstein.

In the same manner, from the first promulgation of the Wernerian system, attempts were made to identify the English with the German members of the geological alphabet; but it was long before this alphabet was rightly read. Thus the English geologists who first tried to apply the Wernerian series to this country, conceived the Old and New Red Sandstone of England to be the same with the Old and New Red Sandstone of Werner; whereas Werner's Old Red, the Rothe todte liegende, is above the coal, while the English Old Red is below it. This mistake led to a further erroneous identification of our Mountain Limestone with Werner's First Flötz Limestone; and caused an almost inextricable confusion, which, even at a recent period, has perplexed the views of German geologists respecting this country. Again, the Lias of England was, at first, supposed to be the equivalent of the Muschelkalk of Germany. But the error of this identification was brought into view by examinations and discussions in which MM. Œyenhausen and Dechen took the lead; and at a later period, Professor Sedgwick, by a laborious examination of the strata of England, was enabled to show the true relation of this part of the geology of the two countries. According to him, the New Red Sandstone of England, considered as one great complex formation, may be divided into seven members, composed of sandstones, limestones, and marls; five of which represent respectively the *Rothe todte liegende;* the *Kupfer schiefer;* the *Zechstein*, (with the *Rauchwacké*, *Asche*, and *Stinkstein* of the Thuringenwald;) the *Bunter sandstein;* and the *Keuper:* while the *Muschelkalk*, which lies between the two last members of the German list, has not yet been discovered in our geological series. "Such a coincidence," he observes,[9] "in the subdivisions of two distant mechanical deposits, even upon the supposition of their being strictly contemporaneous, is truly astonishing. It has not been assumed hypothetically, but is the fair result of the facts which are recorded in this paper."

As an example in which the study of geological equivalents becomes still more difficult, we may notice the attempts to refer the strata of

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[8] De la Beche, *Manual*, 381. [9] *Geol. Trans.* Second Series, iii. 121.

the Alps to those of the north-west of Europe. The dark-colcred marbles and schists resembling mica slate[10] were, during the prevalence of the Wernerian theory, referred, as was natural, to the transition class. The striking physical characters of this mountain region, and its long-standing celebrity as a subject of mineralogical examination, made a complete subversion of the received opinion respecting its place in the geological series, an event of great importance in the history of the science. Yet this was what occurred when Dr. Buckland, in 1820, threw his piercing glance upon this district. He immediately pointed out that these masses, by their fossils, approach to the Oolitic Series of this country. From this view it followed, that the geological equivalents of that series were to be found among rocks in which the mineralogical characters were altogether different, and that the loose limestones of England represent some of the highly-compact and crystalline marbles of Italy and Greece. This view was confirmed by subsequent investigations; and the correspondence was traced, not only in the general body of the formations, but in the occurrence of the Red Marl at its bottom, and the Green Sand and Chalk at its top.

The talents and the knowledge which such tasks require are of no ordinary kind; nor, even with a consummate acquaintance with the well-ascertained formations, can the place of problematical strata be decided without immense labor. Thus the examination and delineation of hundreds of shells by the most skilful conchologists, has been thought necessary in order to determine whether the calcareous beds of Maestricht and of Gosau are or are not intermediate, as to their organic contents, between the chalk and the tertiary formations. And scarcely any point of geological classification can be settled without a similar union of the accomplished naturalist with the laborious geological collector.

It follows from the views already presented, of this part of geology, that no attempt to apply to distant countries the names by which the well-known European strata have been described, can be of any value, if not accompanied by a corresponding attempt to show how far the European series is really applicable. This must be borne in mind in estimating the import of the geological accounts which have been given of various parts of Asia, Africa, and America. For instance, when the carboniferous group and the new red sandstone are stated to

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[10] De la Beche, *Manual*. 313.

be found in India, we require to be assured that these formations are, in some way, the equivalents of their synonyms in countries better explored. Till this is done, the results of observation in such places would be better conveyed by a nomenclature implying only those facts of resemblance, difference, and order, which have been ascertained in the country so described. We know that serious errors were incurred by the attempts made to identify the Tertiary strata of other countries with those first studied in the Paris basin. Fancied points of resemblance, Mr. Lyell observes, were magnified into undue importance, and essential differences in mineral character and organic contents were slurred over.

[2nd Ed.] [The extension of geological surveys, the construction of geological maps, and the determination of the geological equivalents which replace each other in various countries, have been carried on in continuation of the labors mentioned above, with enlarged activity, range, and means. It is estimated that one-third of the land of each hemisphere has been geologically explored; and that thus Descriptive Geology has now been prosecuted so far, that it is not likely that even the extension of it to the whole globe would give any material novelty of aspect to Theoretical Geology. The recent literature of the subject is so voluminous that it is impossible for me to give any account of it here; very imperfectly acquainted, as I am, even with the English portion, and still more, with what has been produced in other countries.

While I admire the energetic and enlightened labors by which the philosophers of France, Belgium, Germany, Italy, Russia, and America, have promoted scientific geology, I may be allowed to rejoice to see in the very phraseology of the subject, the evidence that English geologists have not failed to contribute their share to the latest advances in the science. The following order of strata proceeding upwards is now, I think, recognized throughout Europe. The *Silurian;* the *Devonian* (Old Red Sandstone;) the *Carboniferous;* the *Permian*, (Lower part of the new Red Sandstone series;) the *Trias*, (Upper three members of the New Red Sandstone series;) the *Lias;* the *Oolite*, (in which are reckoned by M. D'Orbigny the Etages *Bathonien*, *Oxonien*, *Kimmeridgien*, and *Portlandien;*) the *Neocomien*, (Lower Green Sand,) the Chalk; and above these, Tertiary and Supra-Tertiary beds. Of these, the Silurian, described by Sir R. Murchison from its types in South Wales, has been traced by European Geologists through the Ardennes, Servia, Turkey, the shores of the Gulf of Finland, the valley of the Mississippi, the west coast of North America, and the mountains of South America. Again, the labors of Prof. Sedgwick and Sir R. Murchison, in 1836, '7, and '8, aided by the sagacity of Mr. Lonsdale, led to their placing certain rocks of Devon and Cornwall as a formation intermediate between the Silurian and Carboniferous Series; and the *Devonian System* thus established has been accepted by geologists in general, and has been traced, not only in various parts of Europe, but in Australia and Tasmania, and in the neighborhood of the Alleganies.

Above the Carboniferous Series, Sir R. Murchison and his fellow laborers, M. de Verneuil and Count Keyserling, have found in Russia, a well-developed series of rocks occupying the ancient kingdom of Permia, which they have hence called the *Permian formation;* and this term also has found general acceptance. The next group, the Keuper, Muschelkalk, and Bunter Sandstein of Germany, has been termed *Trias* by the continental geologists. The *Neocomien* is so called from Neuchatel, where it is largely developed. Below all these rocks come, in England, the *Cambrian*, on which Prof. Sedgwick has expended so many years of valuable labor. The comparison of the Protozoic and Hypozoic rocks of different countries is probably still incomplete.

The geologists of North America have made great progress in decyphering and describing the structure of their own country; and they have wisely gone, in a great measure, upon the plan which I have commended at the end of the third Chapter;—they have compared the rocks of their own country with each other, and given to the different beds and formations names borrowed from their own localities.

This course will facilitate rather than impede the reduction of their classification to its synonyms and equivalents in the old world.

Of course it is not to be expected nor desired that books belonging to Descriptive Geology shall exclude the other two branches of the subject, Geological Dynamics and Physical Geology. On the contrary, among the most valuable contributions to both these departments have been speculations appended to descriptive works. And this is naturally and rightly more and more the case as the description embraces a wider field. The noble work *On the Geology of Russia and the Urals*, by Sir Roderick Murchison and his companions, is a great example of this, as of other merits in a geological book. The author introduces into his pages the various portions of geological dynamics of which I shall have to speak afterwards; and thus endeavors to make out the physical history of the region, the boundaries of its raised sea bottoms, the shores of the great continent on which the mammoths lived, the period when the gold ore was formed, and when the watershed of the Ural chain was elevated.]

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## CHAPTER IV.

### Attempts to Discover General Laws in Geology.

*Sect.* 1.—*General Geological Phenomena.*

BESIDES thus noticing such features in the rocks of each country as were necessary to the identification of the strata, geologists have had many other phenomena of the earth's surface and materials presented to their notice; and these they have, to a certain extent, attempted to generalize, so as to obtain on this subject what we have elsewhere termed the Laws of Phenomena, which are the best materials for physical theory. Without dwelling long upon these, we may briefly note some of the most obvious. Thus it has been observed that mountain ranges often consist of a ridge of subjacent rock, on which lie, on each side, strata sloping from the ridge. Such a ridge is an *Anticlinal Line*, a *Mineralogical Axis.* The sloping strata present their *Escarpements*, or steep edges, to this axis. Again, in mining countries, the *Veins* which contain the ore are usually a system of *parallel* and nearly vertical partitions in the rock; and these are, in very many cases, intersected by another system of veins parallel to each other and nearly *perpendicular* to the former. Rocky regions are often intersected by *Faults*, or fissures interrupting the strata, in which the rock on one side the fissure appears to have been at first continuous with that on the other, and shoved aside or up or down after the fracture. Again, besides these larger fractures, rocks have *Joints*,—separations, or tendencies to separate in some directions rather than in others; and a *slaty Cleavage*, in which the parallel subdivisions may be carried on, so as to produce laminæ of indefinite thinness. As an example of those laws of phenomena of which we have spoken, we may instance the general law asserted by Prof. Sedgwick (not, however, as free from exception), that in one particular class of rocks the slaty Cleavage *never* coincides with the Direction of the strata.

The phenomena of metalliferous veins may be referred to, as another large class of facts which demand the notice of the geologist. It would be difficult to point out briefly any general laws which prevail in such cases; but in order to show the curious and complex nature of the facts, it may be sufficient to refer to the description of the metallic veins of Cornwall by Mr. Carne;[1] in which the author maintains that their various contents, and the manner in which they cut across, and *stop*, or *shift*, each other, leads naturally to the assumption of veins of no less than six or eight different ages in one kind of rock.

Again, as important characters belonging to the physical history of the earth, and therefore to geology, we may notice all the general laws which refer to its temperature;—both the laws of climate, as determined by the *isothermal lines*, which Humboldt has drawn, by the aid of very numerous observations made in all parts of the world; and also those still more curious facts, of the increase of temperature which takes place as we descend in the solid mass. The latter circumstance, after being for a while rejected as a fable, or explained away as an accident, is now generally acknowledged to be the true state of things in many distant parts of the globe, and probably in all.

Again, to turn to cases of another kind: some writers have endeavored to state in a general manner laws according to which the members of the geological series succeed each other; and to reduce apparent anomalies to order of a wider kind. Among those who have written with such views, we may notice Alexander von Humboldt, always, and in all sciences, foremost in the race of generalization. In his attempt to extend the doctrine of geological equivalents from the rocks of Europe[2] to those of the Andes, he has marked by appropriate terms the general modes of geological succession. "I have insisted," he says,[3] "principally upon the phenomena of *alternation*, *oscillation*, and *local suppression*, and on those presented by the *passages* of formations from one to another, by the effect of an *interior developement*."

The phenomena of alternation to which M. de Humboldt here refers are, in fact, very curious: as exhibiting a mode in which the transitions from one formation to another may become gradual and insensible,

[1] *Transactions of the Geol. Soc. of Cornwall*, vol. ii.

[2] *Gissement des Roches dans les deux Hémisphères*, 1823. [3] Pref. p. vi.

instead of sudden and abrupt. Thus the coal measures in the south of England are above the mountain limestone; and the distinction of the formations is of the most marked kind. But as we advance northward into the coal-field of Yorkshire and Durham, the subjacent limestone begins to be subdivided by thick masses of sandstone and carbonaceous strata, and passes into a complex deposit, not distinguishable from the overlying coal measures; and in this manner the transition from the limestone to the coal is made by alternation. Thus, to use another expression of M. de Humboldt's in ascending from the limestone, the coal, before we quit the subjacent stratum, *preludes* to its fuller exhibition in the superior beds.

Again, as to another point: geologists have gone on up to the present time endeavoring to discover general laws and facts, with regard to the position of mountain and mineral masses upon the surface of the earth. Thus M. Von Buch, in his physical description of the Canaries, has given a masterly description of the lines of volcanic action and volcanic products, all over the globe. And, more recently, M. Elie de Beaumont has offered some generalizations of a still wider kind. In this new doctrine, those mountain ranges, even in distant parts of the world, which are of the same age, according to the classifications already spoken of, are asserted to be parallel[4] to each other, while those ranges which are of different ages lie in different directions. This very wide and striking proposition may be considered as being at present upon its trial among the geologists of Europe.[5]

Among the organic phenomena, also, which have been the subject of geological study, general laws of a very wide and comprehensive kind have been suggested, and in a greater or less degree confirmed by adequate assemblages of facts. Thus M. Adolphe Brongniart has not only, in his *Fossil Flora*, represented and skilfully restored a vast number of the plants of the ancient world; but he has also, in the *Prodromus* of the work, presented various important and striking views of the general character of the vegetation of former periods, as

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[4] We may observe that the notion of parallelism, when applied to lines drawn on *remote* portions of a globular surface, requires to be interpreted in so arbitrary a manner, that we can hardly imagine it to express a physical law.

[5] Mr. Lyell, in the sixth edition of his *Principles*, B. i. c. xii., has combated the hypothesis of M. Elie de Beaumont, stated in the text. He has argued both against the catastrophic character of the elevation of mountain chains, and the parallelism of the contemporaneous ridges. It is evident that the former doctrine may be true, though the latter be shown to be false.

insular or continental, tropical or temperate. And M. Agassiz, by the examination of an incredible number of specimens and collections of fossil fish, has been led to results which, expressed in terms of his own ichthyological classification, form remarkable general laws. Thus, according to him,[6] when we go below the lias, we lose all traces of two of the four orders under which he comprehends all known kinds of fish; namely, the *Cycloïdean* and the *Ctenoïdean;* while the other two orders, the *Ganoïdean* and *Placoïdean*, rare in our days, suddenly appear in great numbers, together with large sauroid and carnivorous fishes. Cuvier, in constructing his great work on ichthyology, transferred to M. Agassiz the whole subject of fossil fishes, thus showing how highly he esteemed his talents as a naturalist. And M. Agassiz has shown himself worthy of his great predecessor in geological natural history, not only by his acuteness and activity, but by the comprehensive character of his zoological philosophy, and by the courage with which he has addressed himself to the vast labors which lie before him. In his *Report on the Fossil Fish discovered in England*, published in 1835, he briefly sketches some of the large questions which his researches have suggested; and then adds,[7] "Such is the meagre outline of a history of the highest interest, full of curious episodes, but most difficult to relate. To unfold the details which it contains will be the business of my life."

[2nd Ed.] [In proceeding downwards through the series of formations into which geologists have distributed the rocks of the earth, one class of organic forms after another is found to disappear. In the Tertiary Period we find all the classes of the present world: Mammals, Birds, Reptiles, Fishes, Crustaceans, Mollusks, Zoophytes. In the Secondary Period, from the Chalk down to the New Red Sandstone, Mammals are not found, with the minute exception of the marsupial *amphitherium* and *phascolotherium* in the Stonesfield slate. In the Carboniferous and Devonian period we have no large Reptiles, with, again, a minute amount of exception. In the lower part of the Silurian rocks, Fishes vanish, and we have no animal forms but Mollusks, Crustaceans and Zoophytes.

The Carboniferous, Devonian and Silurian formations, thus containing the oldest forms of life, have been termed *palæozoic.* The boundaries of the life-bearing series have not yet been determined; but the series in which vertebrated animals do not appear has been

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[6] Greenough, *Address to Geol. Soc.* 1835, p. 19. [7] *Brit. Assoc. Report*, p 72.

provisionally termed *protozoic*, and the lower Silurian rocks may probably be looked upon as its upper members. Below this, geologists place a *hypozoic* or *azoic* series of rocks.

Geologists differ as to the question whether these changes in the inhabitants of the globe were made by determinate steps or by insensible gradations. M. Agassiz has been led to the conviction that the organized population of the globe was renewed in the interval of each principal member of its formations.[8] Mr. Lyell, on the other hand, conceives that the change in the collection of organized beings was gradual, and has proposed on this subject an hypothesis which I shall hereafter consider.]

### *Sect. 2.—Transition to Geological Dynamics.*

While we have been giving this account of the objects with which Descriptive Geology is occupied, it must have been felt how difficult it is, in contemplating such facts, to confine ourselves to description and classification. Conjectures and reasonings respecting the causes of the phenomena force themselves upon us at every step; and even influence our classification and nomenclature. Our Descriptive Geology impels us to endeavor to construct a Physical Geology. This close connexion of the two branches of the subject by no means invalidates the necessity of distinguishing them: as in Botany, although the formation of a Natural System necessarily brings us to physiological relations, we still distinguish Systematic from Physiological Botany.

Supposing, however, our Descriptive Geology to be completed, as far as can be done without considering closely the causes by which the strata have been produced, we have now to enter upon the other province of the science, which treats of those causes, and of which we have already spoken, as *Physical Geology*. But before we can treat this department of speculation in a manner suitable to the conditions of science, and to the analogy of other parts of our knowledge, a certain intermediate and preparatory science must be formed, of which we shall now consider the origin and progress.

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[8] *Brit. Assoc. Report* 1842, p. 83.

# GEOLOGICAL DYNAMICS.

## CHAPTER V.

### Inorganic Geological Dynamics.

*Sect.* 1.—*Necessity and Object of a Science of Geological Dynamics.*

WHEN the structure and arrangement which men observed in the materials of the earth instigated them to speculate concerning the past changes and revolutions by which such results had been produced, they at first supposed themselves sufficiently able to judge what would be the effects of any of the obvious agents of change, as water or volcanic fire. It did not at once occur to them to suspect, that their common and extemporaneous judgment on such points was far from sufficient for sound knowledge;—they did not foresee that they must create a special science, whose object should be to estimate the general laws and effects of assumed causes, before they could pronounce whether such causes had actually produced the particular facts which their survey of the earth had disclosed to them.

Yet the analogy of the progress of knowledge on other subjects points out very clearly the necessity of such a science. When phenomenal astronomy had arrived at a high point of completeness, by the labors of ages, and especially by the discovery of Kepler's laws, astronomers were vehemently desirous of knowing the causes of these motions; and sanguine men, such as Kepler, readily conjectured that the motions were the effects of certain virtues and influences, by which the heavenly bodies acted upon each other. But it did not at first occur to him and his fellow-speculators, that they had not ascertained what motions the influences of one body upon another could produce; and that, therefore, they were not prepared to judge whether such causes as they spoke of, did really regulate the motions of the planets. Yet such was found to be the necessary course of sound inference. Men needed a science of motion, in order to arrive at a science of the heavenly motions: they could not advance in the study of the Mechanics of the heavens, till they had learned the Mechanics of terrestrial bodies. And thus they were, in such speculations, at a stand for nearly a century, from the time of Kepler to the time of Newton, while the science of Mechanics was formed by Galileo and his successors. Till that task was executed, all the attempts to assign the causes of cosmical phenomena were fanciful guesses and vague assertions; after that was done, they became demonstrations. The science of *Dynamics* enabled philosophers to pass securely and completely from *Phenomenal Astronomy* to *Physical Astronomy.*

In like manner, in order that we may advance from Phenomenal Geology to Physical Geology, we need a science of *Geological Dynamics;*—that is, a science which shall investigate and determine the laws and consequences of the known causes of changes such as those which Geology considers:—and which shall do this, not in an occasional, imperfect, and unconnected manner, but by systematic, complete, and conclusive methods;—shall, in short, be a Science, and not a promiscuous assemblage of desultory essays.

The necessity of such a study, as a distinct branch of geology, is perhaps hardly yet formally recognized, although the researches which belong to it have, of late years, assumed a much more methodical and scientific character than they before possessed. Mr. Lyell's work (*Principles of Geology*), in particular, has eminently contributed to place Geological Dynamics in its proper prominent position. Of the four books of his Treatise, the second and third are upon this division of the subject; the second book treating of aqueous and igneous causes of change, and the third, of changes in the organic world.

There is no difficulty in separating this auxiliary geological science from theoretical Geology itself, in which we apply our principles to the explanation of the actual facts of the earth's surface. The former, if perfected, would be a demonstrative science dealing with general cases; the latter is an ætiological view having reference to special facts; the one attempts to determine what always must be under given conditions; the other is satisfied with knowing what is and has been, and why it has been; the first study has a strong resemblance to Mechanics, the other to philosophical Archæology.

Since this portion of science is still so new, it is scarcely possible to give any historical account of its progress, or any complete survey of its shape and component parts. I can only attempt a few notices, which may enable us in some measure to judge to what point this division of our subject is tending.

We may remark, in this as in former cases, that since we have here to consider the formation and progress of a *science*, we must treat as unimportant preludes to its history, the detached and casual observations of the effects of causes of change which we find in older writers. It is only when we come to systematic collections of information, such as may afford the means of drawing general conclusions; or to rigorous deductions from known laws of nature;—that we can recognize the separate existence of geological dynamics, as a path of scientific research.

The following may perhaps suffice, for the present, as a sketch of the subjects of which this science treats:—the aqueous causes of change, or those in which water adds to, takes from, or transfers, the materials of the land:—the igneous causes; volcanoes, and, closely connected with them, earthquakes, and the forces by which they are produced;—the calculations which determine, on physical principles, the effects of assumed mechanical causes acting upon large portions of the crust of the earth;—the effect of the forces, whatever they be, which produce the crystalline texture of rocks, their fissile structure, and the separation of materials, of which we see the results in metalliferous veins. Again, the estimation of the results of changes of temperature in the earth, whether operating by pressure, expansion, or in any other way;—the effects of assumed changes in the superficial condition, extent, and elevation, of terrestrial continents upon the climates of the earth;—the effect of assumed cosmical changes upon the temperature of this planet;—and researches of the same nature as these.

These researches are concerned with the causes of change in the inorganic world; but the subject requires no less that we should investigate the causes which may modify the forms and conditions of organic things; and in the large sense in which we have to use the phrase, we may include researches on such subjects also as parts of Geological Dynamics; although, in truth, this department of physiology has been cultivated, as it well deserves to be, independently of its bearing upon geological theories. The great problem which offers itself here, in reference to Geology, is, to examine the value of any hypotheses by which it may be attempted to explain the succession of different races of animals and plants in different strata; and though it may be difficult, in this inquiry, to arrive at any positive result, we may at least be able to show the improbability of some conjectures which have been propounded.

I shall now give a very brief account of some of the attempts made in these various departments of this province of our knowledge; and in the present chapter, of Inorganic Changes.

### *Sect.* 2.—*Aqueous Causes of Change.*

THE controversies to which the various theories of geologists gave rise, proceeding in various ways upon the effects of the existing causes of change, led men to observe, with some attention and perseverance, the actual operation of such causes. In this way, the known effect of the Rhine, in filling up the Lake of Geneva at its upper extremity, was referred to by De Luc, Kirwan, and others, in their dispute with the Huttonians; and attempts were even made to calculate how distant the period was, when this alluvial deposit first began. Other modern observers have attended to similar facts in the natural history of rivers and seas. But the subject may be considered as having first assumed its proper form, when taken up by Mr. Von Hoff; of whose *History of the Natural Changes of the Earth's surface which are proved by Tradition*, the first part, treating of aqueous changes, appeared in 1822. This work was occasioned by a Prize Question of the Royal Society of Göttingen, promulgated in 1818; in which these changes were proposed as the subject of inquiry, with a special reference to geology. Although Von Hoff does not attempt to establish any general inductions upon the facts which his book contains, the collection of such a body of facts gave almost a new aspect to the subject, by showing that changes in the relative extent of land and water were going on at every time, and almost at every place; and that mutability and fluctuation in the form of the solid parts of the earth, which had been supposed by most persons to be a rare exception to the common course of events, was, in fact, the universal rule. But it was Mr. Lyell's *Principles of Geology, being an attempt to explain the former Changes of the Earth's Surface by the causes now in action* (of which the first volume was published in 1830), which disclosed the full effect of such researches on geology; and which attempted to present such assemblages of special facts, as examples of general laws. Thus this work may, as we have said, be looked upon as the beginning of Geological Dynamics, at least among us. Such generalizations and applications as it contains give the most lively interest to a thousand observations respecting rivers and floods, mountains and morasses, which otherwise appear without aim or meaning; and thus this department of science cannot fail to be constantly augmented by contributions from every side. At the same time it is clear, that these contributions, voluminous as they must become, must, from time to time, be resolved into laws of greater and greater generality; and that thus alone the progress of this, as of all other sciences, can be furthered.

I need not attempt any detailed enumeration of the modes of aqueous action which are here to be considered. Some are destructive, as when the rivers erode the channels in which they flow; or when the waves, by their perpetual assault, shatter the shores, and carry the ruins of them into the abyss of the ocean. Some operations of the water, on the other hand, add to the land; as when *deltas* are formed at the mouths of rivers or when calcareous springs form deposits of *travertin*. Even when bound in icy fetters, water is by no means deprived of its active power; the *glacier* carries into the valley masses of its native mountain, and often, becoming ice-bergs, float with a lading of such materials far into the seas of the temperate zone. It is indisputable that vast beds of worn down fragments of the existing land are now forming into strata at the bottom of the ocean; and that many other effects are constantly produced by existing aqueous causes, which resemble some, at least, of the facts which geology has to explain.

[2nd Ed.] [The effects of glaciers above mentioned are obvious; but the mechanism of these bodies,—the mechanical cause of their motions,—was an unsolved problem till within a very few years. That they slide as rigid masses;—that they advance by the expansion of their mass;—that they advance as a collection of rigid fragments; were doctrines which were held by eminent physicists; though a very slight attention to the subject shows these opinions to be untenable. In Professor James Forbes's theory on the subject (published in his *Travels through the Alps*, 1843,) we find a solution of the problem, so simple, and yet so exact, as to produce the most entire conviction. In this theory, the ice of a glacier is, on a great scale, supposed to be a plastic or viscous mass, though small portions of it are sensibly rigid. It advances down the slope of the valley in which it lies as a plastic mass would do, accommodating itself to the varying shape and size of its bed, and showing by its crevasses its mixed character between fluid and rigid. It shows this character still more curiously by a *ribboned struc* *ture* on a small scale, which is common in the solid ice of the glacier. The planes of these *ribbons* are, for the most part, at right angles to the crevasses, near the sides of the glacier, while, near its central line, they *dip* towards the upper part of the glacier. This structure appears to arise from the difference of velocities of contiguous moving filaments of the icy mass, as the crevasses themselves arise from the tension of larger portions. Mr. Forbes has, in successive publications, removed the objections which have been urged against this theory. In the last of them, a Memoir in the *Phil. Trans.*, 1846, (*Illustrations of the Viscous Theory of Glacier Motion*,) he very naturally expresses astonishment at the opposition which has been made to the theory on the ground of the rigidity of small pieces of ice. He has himself shown that the ice of glaciers has a plastic flexibility, by marking forty-five points in a transverse straight line upon the Mer de Glace, and observing them for several days. The straight line in that time not only became oblique to the side, but also became visibly curved.

Both Mr. Forbes and other philosophers have made it in the highest degree probable that glaciers have existed in many places in which they now exist no longer, and have exercised great powers in transporting large blocks of rock, furrowing and polishing the rocks along which they slide, and leaving lines and masses of detritus or *moraine* which they had carried along with them or pushed before them. It cannot be doubted that extinct glaciers have produced some of the effects which the geologist has to endeavor to explain. But this part of the machinery of nature has been worked by some theorists into an exaggerated form, in which it cannot, as I conceive, have any place in an account of Geological Dynamics which aims at being permanent.

The great problem of the diffusion of drift and erratic blocks from their parent rocks to great distances, has driven geologists to the consideration of other hypothetical machinery by which the effects may be accounted for: especially the great *northern drift* and *boulders*,—the rocks from the Scandinavian chain which cover the north of Europe on a vast area, having a length of 2000 and breadth of from 400 to 800 miles. The diffusion of these blocks has been accounted for by supposing them to be imbedded in icebergs, detached from the shore, and floated into oceanic spaces, where they have grounded and been deposited by the melting of the ice. And this mode of action may to some extent be safely admitted into geological speculation. For it is a matter of fact, that our navigators in arctic and antarctic regions have repeatedly seen icebergs and icefloes sailing along laden with such materials.

The above explanation of the phenomena of drift supposes the land on which the travelled materials are found to have been the bottom of a sea where they were deposited. But it does not, even granting the conditions, account for some of the facts observed;—that the drift and the boulders are deposited in "trainées" or streaks, which, in direction, diverge from the parent rock;—and that the boulders are of smaller and smaller size, as they are found more remote from that centre. These phenomena rather suggest the notion of currents of water as the cause of the distribution of the materials into their present situations. And though the supposition that the whole area occupied by drift and boulders was a sea-bottom when they were scattered over it much reduces the amount of violence which it is necessary to assume in order to distribute the loose masses, yet still the work appears to be beyond the possible effect of ordinary marine currents, or any movements which would be occasioned by a slow and gradual rising of the centre of distribution.

It has been suggested that a *sudden* rise of the centre of distribution would cause a motion in the surrounding ocean sufficient to produce such an effect: and in confirmation of this reference has been made to Mr. Scott Russell's investigations with respect to waves, already referred to. (Book VIII.) The wave in this case would be the *wave of translation*, in which the motion of the water is as great at the bottom as at the top; and it has hence been asserted that by paroxysmal elevations of 100 or 200 feet, a current of 25 or 30 miles an hour might be accounted for. But I think it has not been sufficiently noted that at each point this "current" is transient: it lasts only while the wave is passing over the point, and therefore it would only either carry a single mass the whole way with its own velocity, or move through a short distance a series of masses over which it successively passed. It does not appear, therefore, that we have here a complete account of the transport of a collection of materials, in which each part is transferred through great distances:—except, indeed, we were to suppose a numerous succession of paroxysmal elevations. Such a *battery* might, by successive shocks, transmitting their force through the water, diffuse the fragments of the central mass over any area, however wide.

The fact that the erratic blocks are found to rest on the lower drift, is well explained by supposing the latter to have been spread on the sea bottom while rock-bearing ice-masses floated on the surface till they deposited their lading.

Sir R. Murchison has pointed out another operation of ice in producing mounds of rocky masses; namely, the effects of rivers and lakes, in climates where, as in Russia, the waters carry rocky fragments entangled in the winter ice, and leave them in heaps at the highest level which the waters attain.

The extent to which the effects of glaciers, now vanished, are apparent in many places, especially in Switzerland and in England, and other phenomena of the like tendency, have led some of the most eminent geologists to the conviction that, anterior to the period of our present temperature, there was a *Glacial Period*, at which the temperature of Europe was lower than it now is.]

Although the study of the common operations of water may give the geologist such an acquaintance with the laws of his subject as may much aid his judgment respecting the extent to which such effects may proceed, a long course of observation and thought must be requisite before such operations can be analysed into their fundamental principles, and become the subjects of calculation, or of rigorous reasoning in any manner which is as precise and certain as calculation. Various portions of Hydraulics have an important bearing upon these subjects, including some researches which have been pursued with no small labor by engineers and mathematicians; as the effects of currents and waves, the laws of tides and of rivers, and many similar problems. In truth, however, such subjects have not hitherto been treated by mathematicians with much success; and probably several generations must elapse before this portion of geological dynamics can become an exact science.

### *Sect. 3.—Igneous Causes of Change.—Motions of the Earth's Surface.*

The effects of volcanoes have long been noted as important and striking features in the physical history of our globe; and the probability of their connexion with many geological phenomena, had not escaped notice at an early period. But it was not till more recent times, that the full import of these phenomena was apprehended. The person who first looked at such operations with that commanding general view which showed their extensive connexion with physical geology was Alexander von Humboldt, who explored the volcanic phenomena of the New World, from 1799 to 1804. He remarked[1] the linear distribution of volcanic domes, considering them as vents placed along the edge of vast fissures communicating with reservoirs of igneous matter, and extending across whole continents. He observed, also, the frequent sympathy of volcanic and terremotive action in remote districts of the earth's surface, thus showing how deeply seated must be the cause of these convulsions. These views strongly excited and influenced the speculations of geologists; and since then, phenomena of this kind have been collected into a general view as parts of a natural-historical science. Von Hoff, in the second volume of the work already mentioned, was one of the first who did this; "At least," he himself says,[2] (1824,) "it was not known to him that any one before him had endeavored to combine so large a mass of facts with the general ideas of the natural philosopher, so as to form a whole." Other attempts were, however, soon made. In 1825, M. von Ungern-Sternberg published his book *On the Nature and Origin of Volcanoes*,[3] in which, he says, his object is, to give an empirical representation of these phenomena. In the same year, Mr. Poulett Scrope published a work in which he described the known facts of volcanic action; not, however, confining himself to description; his purpose being, as his title states, to consider "the probable causes of their phenomena, the laws which determine their march, the disposition of their products, and their connexion with the present state and past history of the globe; leading to the establishment of a new theory of the earth." And in 1826, Dr. Daubeny, of Oxford, produced *A Description of Active* and Extinct *Volcanoes*, including in the latter phrase the volcanic rocks of central France, of the Rhine, of northern and central Italy, and many other countries. Indeed, the near connexion between the volcanic effects now going on, and those by which the basaltic rocks of Auvergne and many other places had been produced, was, by this time, no longer doubted by any; and therefore the line which here separates the study of existing causes from that of past effects may seem to melt away. But yet it is manifest that the assumption of an identity of scale and mechanism between volcanoes now active, and the igneous catastrophes of which the products have sur

[1] Humboldt, *Relation Historique*: and his other works.

[2] Vol. ii. Prop. 5.

[3] *Werden und Seyn des Vulkanischen Gebirges.* Carlsruhe, 1825.

rived great revolutions on the earth's surface, is hypothetical; and all which depends on this assumption belongs to theoretical geology.

Confining ourselves, then, to volcanic effects, which have been produced, certainly or probably, since the earth's surface assumed its present form, we have still an ample exhibition of powerful causes of change, in the streams of lava and other materials emitted in eruptions; and still more in the earthquakes which, as men easily satisfied themselves, are produced by the same causes as the eruptions of volcanic fire.

Mr. Lyell's work was important in this as in other portions of this subject. He extended the conceptions previously entertained of the effects which such causes may produce, not only by showing how great these operations are historically known to have been, and how constantly they are going on, if we take into our survey the whole surface of the earth; but still more, by urging the consequences which would follow in a long course of time from the constant repetition of operations in themselves of no extraordinary amount. A lava-stream many miles long and wide, and several yards deep, a subsidence or elevation of a portion of the earth's surface of a few feet, are by no means extraordinary facts. Let these operations, said Mr. Lyell, be repeated thousands of times; and we have results of the same order with the changes which geology discloses.

The most mitigated earthquakes have, however, a character of violence. But it has been thought by many philosophers that there is evidence of a change of level of the land in cases where none of these violent operations are going on. The most celebrated of these cases is Sweden; the whole of the land from Gottenburg to the north of the Gulf of Bothnia has been supposed in the act of rising, slowly and insensibly, from the surrounding waters. The opinion of such a change of level has long been the belief of the inhabitants; and was maintained by Celsius in the beginning of the eighteenth century. It has since been conceived to be confirmed by various observations of marks cut on the face of the rock; beds of shells, such as now live in the neighboring seas, raised to a considerable height; and other indications. Some of these proofs appear doubtful; but Mr. Lyell, after examining the facts upon the spot in 1834, says, "In regard to the proposition that the land, in certain parts of Sweden, is gradually rising, I have no hesitation in assenting to it, after my visit to the districts above alluded to."[4] If this conclusion be generally accepted by

[4] *Phil. Trans.* 1835, p. 32.

geologists, we have here a daily example of the operation of some powerful agent which belongs to geological dynamics; and which, for the purposes of the geological theorist, does the work of the earthquake upon a very large scale, without assuming its terrors.

[2nd Ed.] [Examples of changes of level of large districts occurring at periods when the country has been agitated by earthquakes are well ascertained, as the rising of the coast of Chili in 1822, and the subsidence of the district of Cutch, in the delta of the Indus, in 1819. (Lyell, B. II. c. XV.) But the cases of more slow and tranquil movement seem also to be established. The gradual secular rise of the shore of the Baltic, mentioned in the text, has been confirmed by subsequent investigation. It appears that the rate of elevation increases from Stockholm, where it is only a few inches in a century, to the North Cape, where it is several feet. It appears also that several other regions are in a like state of secular change. The coast of Greenland is sinking. (Lyell, B. II. c. XVIII.) And the existence of "raised beaches" along various coasts is now generally accepted among geologists. Such beaches, anciently forming the margin of the sea, but now far above it, exist in many places; for instance, along a great part of the Scotch coast; and among the raised beaches of that country we ought probably, with Mr. Darwin, to include the "parallel roads" of Glenroy, the subject, in former days, of so much controversy among geologists and antiquaries.

Connected with the secular rise and fall of large portions of the earth's surface, another agency which plays an important part in Geological dynamics has been the subject of some bold yet singularly persuasive speculations by Mr. Darwin. I speak of the formation of Coral, and Coral Reefs. He says that the coral-building animal works only at small and definite distances below the surface. How then are we to account for the vast number of coral islands, rings, and reefs, which are scattered over the Pacific and Indian Oceans! Can we suppose that there are so many mountains, craters, and ridges, all exactly within a few feet of the same height through this vast portion of the globe's surface? This is incredible. How then are we to explain the facts? Mr. Darwin replies, that if we suppose the land to subside slowly beneath the sea, and at the same time suppose the coralline zoophytes to go on building, so that their structure constantly rises nearly to the surface of the water, we shall have the facts explained. A submerged island will produce a ring; a long coast, a barrier reef; and so on. Mr. Darwin also notes other phenomena, as elevated beds of coral, which, occurring in other places, indicate a recent rising of the land; and on such grounds as these he divides the surface of those parts of the ocean into regions of elevation and of depression.

The labors of coralline zoophytes, as thus observed, form masses of coral, such as are found fossilized in the strata of the earth. But our knowledge of the laws of life which have probably affected the distribution of marine remains in strata, has received other very striking accessions by the labors of Prof. Edward Forbes in observing the marine animals of the Ægean Sea. He found that, even in their living state, the mollusks and zoophytes are already distributed into strata. Dividing the depth into eight regions, from 2 to 230 fathoms, he found that each region had its peculiar inhabitants, which disappeared speedily either in ascending or in descending. The zero of animal life appeared to occur at about 300 fathoms. This curious result bears in various ways upon geology. Mr. Forbes himself has given an example of the mode in which it may be applied, by determining the depth at which the submarine eruption took place which produced the volcanic isle of Neokaimeni in 1707. By an examination of the fossils embedded in the pumice, he showed that it came from the fourth region.[5]

To the modes in which organized beings operate in producing the materials of the earth, we must add those pointed out by the extraordinary microscopic discoveries of Professor Ehrenberg. It appears that whole beds of earthy matter consist of the cases of certain infusoria, the remains of these creatures being accumulated in numbers which it confounds our thoughts to contemplate.]

Speculations concerning the *causes* of volcanoes and earthquakes, and of the rising and sinking of land, are a highly important portion of this science, at least as far as the calculation of the possible results of definite causes is concerned. But the various hypotheses which have been propounded on this subject can hardly be considered as sufficiently matured for such calculation. A mass of matter in a state of igneous fusion, extending to the centre of the earth, even if we make such an hypothesis, requires some additional cause to produce eruption. The supposition that this fire may be produced by intense chemical action between combining elements, requires further, not only some agency to bring together such elements, but some reason why

[5] *British Assoc. Reports*, 1843, p. 177.

they should be originally separate. And if any other causes have been suggested, as electricity or magnetism, this has been done so vaguely as to elude all possibility of rigorous deduction from the hypothesis. The doctrine of a Central Heat, however, has occupied so considerable a place in theoretical geology, that it ought undoubtedly to form an article in geological dynamics.

### *Sect.* 4.—*The Doctrine of Central Heat.*

The early geological theorists who, like Leibnitz and Buffon, assumed that the earth was originally a mass in a state of igneous fusion, naturally went on to deduce from this hypothesis, that the crust consolidated and cooled before the interior, and that there might still remain a central heat, capable of producing many important effects. But it is in more recent times that we have measures of such effects, and calculations which we can compare with measures. It was found, as we have said, that in descending below the surface of the earth, the temperature of its materials increased. Now it followed from Fourier's mathematical investigations of the distribution of heat in the earth, that if there be no primitive heat (*chaleur d'origine*), the temperature, when we descend below the crust, will be constant in each vertical line. Hence an observed increase of temperature in descending, appeared to point out a central heat resulting from some cause now no longer in action.

The doctrine of a central heat has usually been combined with the supposition of a central igneous fluidity; for the heat in the neighborhood of the centre must be very intense, according to any law of its increase in descending which is consistent with known principles. But to this central fluidity it has been objected that such a fluid must be in constant circulation by the cooling of its exterior. Mr. Daniell found this to be the case in all fused metals. It has also been objected that there must be, in such a central fluid, *tides* produced by the moon and sun; but this inference would require several additional suppositions and calculations to give it a precise form.

Again, the supposition of a central heat of the earth, considered as the effect of a more ancient state of its mass, appeared to indicate that its cooling must still be going on. But if this were so, the earth might contract, as most bodies do when they cool; and this contraction might lead to mechanical results, as the shortening of the day. Laplace satisfied himself, by reference to ancient astronomical records, that no such alteration in the length of the day had taken place, even to the amount of one two-hundredth of a second; and thus, there was here no confirmation of the hypothesis of a primitive heat of the earth.

Though we find no evidence of the secular contraction of the earth in the observations with which astronomy deals, there are some geological facts which at first appear to point to the reality of a refrigeration within geological periods; as the existence of the remains of plants and shells of tropical climates, in the strata of countries which are now near to or within the frigid zones. These facts, however, have given rise to theories of the changes of climate, which we must consider separately.

But we may notice, as connected with the doctrine of central heat, the manner in which this hypothesis has been applied to explain volcanic and geological phenomena. It does not enter into my plan, to consider explanations in which this central heat is supposed to give rise to an expansive force,[6] without any distinct reference to known physical laws. But we may notice, as more likely to become useful materials of the science now before us, such speculations as those of Mr. Babbage; in which he combines the doctrine of central heat with other physical laws;[7] as, that solid rocks *expand* by being heated, but that clay contracts; that different rocks and strata *conduct* heat differently; that the earth *radiates* heat differently, or at different parts of its surface, according as it is covered with forests, with mountains, with deserts, or with water. These principles, applied to large masses, such as those which constitute the crust of the earth, might give rise to changes as great as any which geology discloses. For example: when the bed of a sea is covered by a thick deposit of new matter worn from the shores, the strata below the bed, being protected by a bad conductor of heat, will be heated, and, being heated, may be expanded; or, as Sir J. Herschel has observed, may produce explosion by the conversion of their moisture into steam. Such speculations, when founded on real data and sound calculations, may hereafter be of material use in geology.

The doctrine of central heat and fluidity has been rejected by some eminent philosophers. Mr. Lyell's reasons for this rejection belong

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[6] Scrope *On Volcanoes*, p. 192.

[7] *On the Temple of Serapis*, 1834. See also *Journal of the Royal Inst.* vol. ii., quoted in Conyb. and Ph. p. xv. Lyell, B. ii. c. xix. p. 383, (4th ed.) on Expansion of Stone.

rather to Theoretical Geology; but I may here notice M. Poissons opinion. He does not assent to the conclusion of Fourier, that since the temperature increases in descending, there must be some primitive central heat. On the contrary, he considers that such an increase may arise from this;—that the earth, at some former period, passed (by the motion of the solar system in the universe,) though a portion of space which was warmer than the space in which it now revolves (by reason, it may be, of the heat of other stars to which it was then nearer). He supposes that, since such a period, the surface has cooled down by the influence of the surrounding circumstances; while the interior, for a certain unknown depth, retains the trace of the former elevation of temperature. But this assumption is not likely to expel the belief in the terrestrial origin of the subterraneous heat. For the supposition of such an inequality in the temperature of the different regions in which the solar system is placed at different times, is altogether arbitrary; and, if pushed to the amount to which it must be carried, in order to account for the phenomenon, is highly improbable.[8] The doctrine of central heat, on the other hand, (which need not be conceived as implying the *universal* fluidity of the mass,) is not only naturally suggested by the subterraneous increase of temperatures, but explains the spheroidal figure of the earth; and falls in with almost any theory which can be devised, of volcanoes, earthquakes, and great geological changes.

### *Sect. 5.—Problems respecting Elevations and Crystalline Forces.*

Other problems respecting the forces by which great masses of the earth's crust have been displaced, have also been solved by various mathematicians. It has been maintained by Von Buch that there occur, in various places, *craters of elevation;* that is, mountain-masses resembling the craters of volcanoes, but really produced by an expansive force from below, bursting an aperture through horizontal strata,

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[8] For this hypothesis would make it necessary to suppose that the earth has, at some former period, derived from some other star or stars more heat than she now derives from the sun. But this would imply, as highly probable, that at some period some other star or stars must have produced also a *mechanical* effect upon the solar system, greater than the effect of the sun. Now such a past operation of forces, fitted to obliterate all order and symmetry, is quite inconsistent with the simple, regular, and symmetrical relation which the whole solar system, as far as Uranus, bears to the present central body.

and elevating them in a conical form. Against this doctrine, as exemplified in the most noted instances, strong arguments have been adduced by other geologists. Yet the protrusion of fused rock by subterraneous forces upon a large scale is not denied: and how far the examples of such operations may, in any cases, be termed craters of elevation, must be considered as a question not yet decided. On the supposition of the truth of Von Buch's doctrine, M. de Beaumont has calculated the relations of position, the fissures, &c., which would arise. And Mr. Hopkins,[9] of Cambridge, has investigated in a much more general manner, upon mechanical principles, the laws of the elevations, fissures, faults, veins, and other phenomena which would result from an elevatory force, acting simultaneously at every point beneath extensive portions of the crust of the earth. An application of mathematical reasoning to the illustration of the phenomena of veins had before been made in Germany by Schmidt and Zimmerman.[10] The conclusion which Mr. Hopkins has obtained, respecting the two sets of fissures, at right angles to each other, which would in general be produced by such forces as he supposes, may suggest interesting points of examination respecting the geological phenomena of fissured districts.

[2nd Ed.] [The theory of craters of elevation probably errs rather by making the elevation of a point into a particular class of volcanic agency, than by giving volcanic agency too great a power of elevation.

A mature consideration of the subject will make us hesitate to ascribe much value to the labors of those writers who have applied mathematical reasoning to geological questions. Such reasoning, when it is carried to the extent which requires symbolical processes, has always been, I conceive, a source, not of knowledge, but of error, and confusion; for in such applications the real questions are slurred over in the hypothetical assumptions of the mathematician, while the calculation misleads its followers by a false aspect of demonstration. All symbolical reasonings concerning the fissures of a semi-rigid mass produced by elevatory or other forces, appear to me to have turned out valueless. At the same time it cannot be too strongly borne in mind, that mathematical and mechanical habits of thought are requisite to all clear thinking on such subjects.]

Other forces, still more secure in their nature and laws, have played a very important part in the formation of the earth's crust. I speak of the forces by which the crystalline, slaty, and jointed structure of

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[9] *Trans. Camb. Phil. Soc.* vol. vi. 1836. [10] *Phil. Mag.* July, 1836, p. 2

mineral masses has been produced. These forces are probably identical, on the one hand, with the cohesive forces from which rocks derive their solidity and their physical properties; while, on the other hand, they are closely connected with the forces of chemical attraction. No attempts, of any lucid and hopeful kind, have yet been made to bring such forces under definite mechanical conceptions: and perhaps mineralogy, to which science, as the point of junction of chemistry and crystallography, such attempts would belong, is hardly yet ripe for such speculations. But when we look at the universal prevalence of crystalline forms and cleavages, at the extent of the phenomena of slaty cleavage, and at the *segregation* of special minerals into veins and nodules, which has taken place in some unknown manner, we cannot doubt that the forces of which we now speak have acted very widely and energetically. Any elucidation of their nature would be an important step in Geological Dynamics.

[2nd Ed.] [A point of Geological Dynamics of great importance is, the change which rocks undergo in structure after they are deposited, either by the action of subterraneous heat, or by the influence of crystalline or other corpuscular forces. By such agencies, sedimentary rocks may be converted into crystalline, the traces of organic fossils may be obliterated, a slaty cleavage may be produced, and other like effects. The possibility of such changes was urged by Dr. Hutton in his Theory; and Sir James Hall's very instructive and striking experiments were made for the purpose of illustrating this theory. In these experiments, powdered chalk was, by the application of heat under pressure, converted into crystalline calcspar. Afterwards Dr. McCulloch's labors had an important influence in satisfying geologists of the reality of corresponding changes in nature. Dr. McCulloch, by his very lively and copious descriptions of volcanic regions, by his representations of them, by his classification of igneous rocks, and his comprehensive views of the phenomena which they exhibit, probably was the means of converting many geologists from the Wernerian opinions.

Rocks which have undergone changes since they were deposited are termed by Mr. Lyell *metamorphic*. The great extent of metamorphic rock changed by heat is now uncontested. The internal changes which are produced by the crystalline forces of mountain masses have been the subjects of important and comprehensive speculations by Professor Sedgwick.]

*Sect. 6.—Theories of Changes of Climate.*

As we have already stated, Geology offers to us strong evidence that the climate of the ancient periods of the earth's history was hotter than that which now exists in the same countries. This, and other circumstances, have led geologists to the investigation of the effects of any hypothetical causes of such changes of condition in respect of heat.

The love of the contemplation of geometrical symmetry, as well as other reasons, suggested the hypothesis that the earth's axis had originally no obliquity, but was perpendicular to the equator. Such a construction of the world had been thought of before the time of Milton,[11] as what might be supposed to have existed when man was expelled from Paradise; and Burnet, in his *Sacred Theory of the Earth* (1690), adopted this notion of the paradisiacal condition of the globe

> The spring
> Perpetual smiled on earth with verdant flowers,
> Equal in days and nights.

In modern times, too, some persons have been disposed to adopt this hypothesis, because they have conceived that the present polar distribution of light is inconsistent with the production of the fossil plants which are found in those regions,[12] even if we could, in some other way, account for the change of temperature. But this alteration in the axes of a revolution could not take place without a subversion of the equilibrium of the surface, such as does not appear to have occurred; and the change has of late been generally declared impossible by physical astronomers.

The effects of other astronomical changes have been calculated by Sir John Herschel. He has examined, for instance, the thermotical consequences of the diminution of the eccentricity of the earth's orbit, which has been going on for ages beyond the records of history. He finds[13] that, on this account, the annual effect of solar radiation would increase as we go back to remoter periods of the past; but (probably at least) not in a degree sufficient to account for the apparent past

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[11] Some said he bade his angels turn askance
The poles of earth twice ten degrees and more
From the sun's axle, &c.—*Paradise Lost*, x. 214.

[12] Lyell, i. 155. Lindley, *Fossil Flora*. [13] *Geol. Trans.* vol. iii. p. 295.

changes of climate. He finds, however, that though the effect of this change on the mean temperature of the year may be small, the effect on the extreme temperature of the seasons will be much more considerable; "so as to produce alternately, in the same latitude of either hemisphere, a perpetual spring, or the extreme vicissitudes of a burning summer and a rigorous winter."[14]

Mr. Lyell has traced the consequences of another hypothesis on this subject, which appears at first sight to promise no very striking results, but which yet is found, upon examination, to involve adequate causes of very great changes: I refer to the supposed various distribution of land and water at different periods of the earth's history. If the land were all gathered into the neighborhood of the poles, it would become the seat of constant ice and snow, and would thus very greatly reduce the temperature of the whole surface of the globe. If, on the other hand, the polar regions were principally water, while the tropics were occupied with a belt of land, there would be no part of the earth's surface on which the frost could fasten a firm hold, while the torrid zone would act like a furnace to heat the whole. And, supposing a cycle of terrestrial changes in which these conditions should succeed each other, the winter and summer of this "great year" might differ much more than the elevated temperature which we are led to ascribe to former periods of the globe, can be judged to have differed from the present state of things.

The ingenuity and plausibility of this theory cannot be doubted: and perhaps its results may hereafter be found not quite out of the reach of calculation. Some progress has already been made in calculating the movement of heat into, through, and out of the earth; but when we add to this the effects of the currents of the ocean and the atmosphere, the problem, thus involving so many thermotical and atmological laws, operating under complex conditions, is undoubtedly one of extreme difficulty. Still, it is something, in this as in all cases, to have the problem even stated; and none of the elements of the solution appears to be of such a nature that we need allow ourselves to yield to despair, respecting the possibility of dealing with it in a useful manner, as our knowledge becomes more complete and definite.

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[14] *Geol. Trans.* vol. iii. p. 298.

## CHAPTER VI.

### Progress of the Geological Dynamics of Organized Beings.

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*Sect.* 1.—*Objects of this Science.*

PERHAPS in extending the term *Geological Dynamics* to the causes of changes in organized beings, I shall be thought to be employing a forced and inconvenient phraseology. But it will be found that, in order to treat geology in a truly scientific manner, we must bring together all the classes of speculations concerning known causes of change; and the Organic Dynamics of Geology, or of Geography, if the reader prefers the word, appears not an inappropriate phrase for one part of this body of researches.

As has already been said, the species of plants and animals which are found embedded in the strata of the earth, are not only different from those which now live in the same regions, but, for the most part, different from any now existing on the face of the earth. The remains which we discover imply a past state of things different from that which now prevails; they imply also that the whole organic creation has been renewed, and that this renewal has taken place several times. Such extraordinary general facts have naturally put in activity very bold speculations.

But it has already been said, we cannot speculate upon such facts in the past history of the globe, without taking a large survey of its present condition. Does the present animal and vegetable population differ from the past, in the same way in which the products of one region of the existing earth differ from those of another? Can the creation and diffusion of the fossil species be explained in the same manner as the creation and diffusion of the creatures among which we live? And these questions lead us onwards another step, to ask,—What *are* the laws by which the plants and animals of different parts of the earth differ? What was the manner in which they were originally diffused?—Thus we have to include, as portions of our subject, the *Geography of Plants*, and of *Animals*, and the *History of their change and diffusion;* intending by the latter subject, of course, *palætiological* History,—the examination of the causes of what has occurred, and the inference of past events, from what we know of causes.

It is unnecessary for me to give at any length a statement of the problems which are included in these branches of science, or of the progress which has been made in them; since Mr. Lyell, in his *Principles of Geology*, has treated these subjects in a very able manner, and in the same point of view in which I am thus led to consider them. I will only briefly refer to some points, availing myself of his labors and his ideas.

### *Sect.* 2.—*Geography of Plants and Animals.*

WITH regard both to plants and animals, it appears,[1] that besides such differences in the products of different regions as we may naturally suppose to be occasioned by climate and other external causes; an examination of the whole organic population of the globe leads us to consider the earth as divided into *provinces*, each province being occupied by its own group of species, and these groups not being mixed or interfused among each other to any great extent. And thus, as the earth is occupied by various nations of men, each appearing at first sight to be of a different stock, so each other tribe of living things is scattered over the ground in a similar manner, and distributed into its separate *nations* in distant countries. The places where species are thus peculiarly found, are, in the case of plants, called their *stations.* Yet each species in its own region loves and selects some peculiar conditions of shade or exposure, soil or moisture: its place, defined by the general description of such conditions, is called its *habitation.*

Not only each species thus placed in its own province, has its position further fixed by its own habits, but more general groups and assemblages are found to be determined in their situation by more general conditions. Thus it is the character of the *flora* of a collection of islands, scattered through a wide ocean in a tropical and humid climate, to contain an immense preponderance of tree-ferns. In the same way, the situation and depth at which certain genera of shells are found have been tabulated[2] by Mr. Broderip. Such general inferences, if

[1] Lyell, *Principles*, B. iii. c. v.

[2] Greenough, *Add.* 1835, p. 20

they can be securely made, are of extreme interest in their bearing on geological speculations.

The means by which plants and animals are now diffused from one place to another, have been well described by Mr. Lyell.[3] And he has considered also, with due attention, the manner in which they become imbedded in mineral deposits of various kinds.[4] He has thus followed the history of organized bodies, from the germ to the tomb, and thence to the cabinet of the geologist.

But, besides the fortunes of individual plants and animals, there is another class of questions, of great interest, but of great difficulty;—the fortunes of each species. In what manner do species which were not, begin to be? as geology teaches us that they many times have done; and, as even our own reasonings convince us they must have done, at least in the case of the species among which we live.

We here obviously place before us, as a subject of research, the Creation of Living Things;—a subject shrouded in mystery, and not to be approached without reverence. But though we may conceive, that, on this subject, we are not to seek our belief from science alone, we shall find, it is asserted, within the limits of allowable and unavoidable speculation, many curious and important problems which may well employ our physiological skill. For example, we may ask:—how we are to recognize the species which were originally created distinct? —whether the population of the earth at one geological epoch could pass to the form which it has at a succeeding period, by the agency of natural causes alone?—and if not, what other account we can give of the succession which we find to have taken place?

The most remarkable point in the attempts to answer these and the like questions, is the controversy between the advocates and the opponents of the doctrine of the *transmutation of species*. This question is, even from its mere physiological import, one of great interest; and the interest is much enhanced by our geological researches, which again bring the question before us in a striking form, and on a gigantic scale. We shall, therefore, briefly state the point at issue.

### *Sect.* 3.—*Question of the Transmutation of Species.*

We see that animals and plants may, by the influence of breeding, and of external agents operating upon their constitution, be greatly

[3] Lyell, B. iii. c. v. vi. vii.

[4] B. iii. c. xiii. xiv. xv. xvi.

modified, so as to give rise to varieties and races different from what before existed. How different, for instance, is one kind and breed of dog from another! The question, then, is, whether organized beings can, by the mere working of natural causes, pass from the type of one species to that of another? whether the wolf may, by domestication, become the dog? whether the ourang-outang may, by the power of external circumstances, be brought within the circle of the human species? And the dilemma in which we are placed is this;—that if species are not thus interchangeable, we must suppose the fluctuations of which each species is capable, and which are apparently indefinite, to be bounded by rigorous limits; whereas, if we allow such a *transmutation of species*, we abandon that belief in the adaptation of the structure of every creature to its destined mode of being, which not only most persons would give up with repugnance, but which, as we have seen, has constantly and irresistibly impressed itself on the minds of the best naturalists, as the true view of the order of the world.

But the study of Geology opens to us the spectacle of many groups of species which have, in the course of the earth's history, succeeded each other at vast intervals of time; one set of animals and plants disappearing, as it would seem, from the face of our planet, and others, which did not before exist, becoming the only occupants of the globe. And the dilemma then presents itself to us anew:—either we must accept the doctrine of the transmutation of species, and must suppose that the organized species of one geological epoch were transmuted into those of another by some long-continued agency of natural causes; or else, we must believe in many successive acts of creation and extinction of species, out of the common course of nature; acts which, therefore, we may properly call miraculous.

This latter dilemma, however, is a question concerning the facts which have happened in the history of the world; the deliberation respecting it belongs to physical geology itself, and not to that subsidiary science which we are now describing, and which is concerned only with such causes as we know to be in constant and orderly action.

The former question, of the limited or unlimited extent of the modifications of animals and plants, has received full and careful consideration from eminent physiologists; and in their opinions we find, I think, an indisputable preponderance to that decision which rejects the transmutation of species, and which accepts the former side of the dilemma; namely, that the changes of which each species is susceptible, though difficult to define in words, are limited in fact. It is extremely interesting and satisfactory thus to receive an answer in which we can confide, to inquiries seemingly so wide and bold as those which this subject involves. I refer to Mr. Lyell, Dr. Prichard, Mr. Lawrence, and others, for the history of the discussion, and for the grounds of the decision; and I shall quote very briefly the main points and conclusions to which the inquiry has led.[5]

It may be considered, then, as determined by the over-balance of physiological authority, that there is a capacity in all species to accommodate themselves, to a certain extent, to a change of external circumstances; this extent varying greatly according to the species. There may thus arise changes of appearance or structure, and some of these changes are transmissible to the offspring: but the mutations thus superinduced are governed by constant laws, and confined within certain limits. Indefinite divergence from the original type is not possible; and the extreme limit of possible variation may usually be reached in a brief period of time: in short, *species have a real existence in nature*, and a transmutation from one to another does not exist.

Thus, for example, Cuvier remarks, that notwithstanding all the differences of size, appearance, and habits, which we find in the dogs of various races and countries, and though we have (in the Egyptian mummies) skeletons of this animal as it existed three thousand years ago, the relation of the bones to each other remains essentially the same; and, with all the varieties of their shape[6] and size, there are characters which resist all the influences both of external nature, of human intercourse, and of time.

### *Sect.* 4.—*Hypothesis of Progressive Tendencies.*

Within certain limits, however, as we have said, external circumstances produce changes in the forms of organized beings. The causes of change, and the laws and limits of their effects, as they obtain in the existing state of the organic creation, are in the highest degree interesting. And, as has been already intimated, the knowledge thus obtained, has been applied with a view to explain the origin of the existing population of the world, and the succession of its past conditions. But those who have attempted such an explanation, have found it necessary to assume certain additional laws, in order to enable themselves to de-

[5] Lyell, B. iii. c. iv.

[6] *Ossem. Foss.* Disc. Prél. p 61.

duce, from the tenet of the transmutability of the species of organized beings, such a state of things as we see about us, and such a succession of states as is evidenced by geological researches. And here, again, we are brought to questions of which we must seek the answers from the most profound physiologists. Now referring, as before, to those which appear to be the best authorities, it is found that these additional positive laws are still more inadmissible than the primary assumption of indefinite capacity of change. For example, in order to account, on this hypothesis, for the seeming adaptation of the endowments of animals to their wants, it is held that the endowments are the result of the wants; that the swiftness of the antelope, the claws and teeth of the lion, the trunk of the elephant, the long neck of the giraffe have been produced by a certain plastic character in the constitution of animals, operated upon, for a long course of ages, by the attempts which these animals made to attain objects which their previous organization did not place within their reach. In this way, it is maintained that the most striking attributes of animals, those which apparently imply most clearly the providing skill of their Creator, have been brought forth by the long-repeated efforts of the creatures to attain the object of their desire; thus animals with the highest endowments have been gradually developed from ancestral forms of the most limited organization: thus fish, bird, and beast, have grown from *small gelatinous bodies*, "petits corps gelatineux," possessing some obscure principle of life, and the capacity of development; and thus man himself with all his intellectual and moral, as well as physical privileges, has been derived from some creature of the ape or baboon tribe, urged by a constant tendency to improve, or at least to alter his condition.

As we have said, in order to arrive even hypothetically at this result, it is necessary to assume besides a mere capacity for change, other positive and active principles, some of which we may notice. Thus, we must have as the direct productions of nature on this hypothesis, certain monads or rough draughts, the primary *rudiments* of plants and animals. We must have, in these, a constant *tendency to progressive improvement*, to the attainment of higher powers and faculties than they possess; which tendency is again perpetually modified and controlled by the *force of external circumstances*. And in order to account for the simultaneous existence of animals in every stage of this imaginary progress, we must suppose that nature is compelled to be *constantly* producing those elementary beings, from which all animals are successively developed.

I need not stay to point out how extremely arbitrary every part of this scheme is; and how complex its machinery would be, even if it did account for the facts. It may be sufficient to observe, as others have done,[7] that the capacity of change, and of being influenced by external circumstances, such as we really find it in nature, and therefore such as in science we must represent it, is a tendency, not to improve, but to deteriorate. When species are modified by external causes, they usually degenerate, and do not advance. And there is no instance of a species acquiring an entirely new sense, faculty, or organ, in addition to, or in the place of, what it had before.

Not only, then, is the doctrine of the transmutation of species in itself disproved by the best physiological reasonings, but the additional assumptions which are requisite, to enable its advocates to apply it to the explanation of the geological and other phenomena of the earth, are altogether gratuitous and fantastical.

Such is the judgment to which we are led by the examination of the discussions which have taken place on this subject. Yet in certain speculations, occasioned by the discovery of the *Sivatherium*, a new fossil animal from the Sub-Himalaya mountains of India, M. Geoffroy Saint-Hilaire speaks of the belief in the immutability of species as a conviction which is fading away from men's minds. He speaks too of the termination of the age of Cuvier, "la clôture du siècle de Cuvier," and of the commencement of a better zoological philosophy.[8] But though he expresses himself with great animation, I do not perceive that he adduces, in support of his peculiar opinions, any arguments in addition to those which he urged during the lifetime of Cuvier. And the reader[9] may recollect that the consideration of that controversy led us to very different anticipations from his, respecting the probable future progress of physiology. The discovery of the Sivatherium supplies no particle of proof to the hypothesis, that the existing species of animals are descended from extinct creatures which are specifically distinct: and we cannot act more wisely than in listening to the advice of that eminent naturalist, M. de Blainville.[10] "Against this hypothesis, which, up to the present time, I regard as purely gratuitous, and likely to turn geologists out of the sound and excellent road in which they now are, I willingly raise my voice, with the most absolute conviction of being in the right."

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[7] Lyell, B. III. c. iv.

[8] *Compte Rendu de l'Acad. des Sc.* 1837, No. 3, p 81.

[9] See B. XVII. c. vii.

[10] *Compte Rendu*, 1837, No. 5, p. 168.

[2nd Ed.] [The hypothesis of the progressive developement of species has been urged recently, in connexion with the physiological tenet of Tiedemann and De Serres, noticed in B. XVII. c. vii. sect. 3;—namely, that the embryo of the higher forms of animals passes by gradations through those forms which are permanent in inferior animals. Assuming this tenet as exact, it has been maintained that the higher animals which are found in the more recent strata may have been produced by an ulterior development of the lower forms in the embryo state; the circumstances being such as to favor such a developement. But all the best physiologists agree in declaring that such an extraordinary developement of the embryo is inconsistent with physiological possibility. Even if the progression of the embryo in time have a general correspondence with the order of animal forms as more or less perfectly organized (which is true in an extremely incomplete and inexact degree), this correspondence must be considered, not as any indication of causality, but as one of those marks of universal analogy and symmetry which are stamped upon every part of the creation.

Mr. Lyell[11] notices this doctrine of Tiedemann and De Serres; and observes, that though nature presents us with cases of animal forms degraded by incomplete developement, she offers none of forms exalted by extraordinary developement. Mr. Lyell's own hypothesis of the introduction of new species upon the earth, not having any physiological basis, hardly belongs to this chapter.]

### *Sect. 5.—Question of Creation as related to Science.*

But since we reject the production of new species by means of external influence, do we then, it may be asked, accept the other side of the dilemma which we have stated; and admit a series of creations of species, by some power beyond that which we trace in the ordinary course of nature?

To this question, the history and analogy of science, I conceive, teach us to reply as follows:—All palætiological sciences, all speculations which attempt to ascend from the present to the remote past, by the chain of causation, do also, by an inevitable consequence, urge us to look for the beginning of the state of things which we thus contemplate; but in none of these cases have men been able, by the aid of science, to arrive at a beginning which is homogeneous with the

[11] *Principles*, B. III. c. iv.

known course of events. The first origin of language, of civilization, of law and government, cannot be clearly made out by reasoning and research; just as little, we may expect, will a knowledge of the origin of the existing and extinct species of plants and animals, be the result of physiological and geological investigation.

But, though philosophers have never yet demonstrated, and perhaps never will be able to demonstrate, what was that primitive state of things in the social and material worlds, from which the progressive state took its first departure; they can still, in all the lines of research to which we have referred, go very far back;—determine many of the remote circumstances of the past sequence of events;—ascend to a point which, from our position at least, seems to be near the origin; —and exclude many suppositions respecting the origin itself. Whether, by the light of reason alone, men will ever be able to do more than this, it is difficult to say. It is, I think, no irrational opinion, even on grounds of philosophical analogy alone, that in all those sciences which look back and seek a beginning of things, we may be unable to arrive at a consistent and definite belief, without having recourse to other grounds of truth, as well as to historical research and scientific reasoning. When our thoughts would apprehend steadily the creation of things, we find that we are obliged to summon up other ideas than those which regulate the pursuit of scientific truths;—to call in other powers than those to which we refer natural events: it cannot, then, be considered as very surprizing, if, in this part of our inquiry, we are compelled to look for other than the ordinary evidence of science.

Geology, forming one of the palætiological class of sciences, which trace back the history of the earth and its inhabitants on philosophical grounds, is thus associated with a number of other kinds of research, which are concerned about language, law, art, and consequently about the internal faculties of man, his thoughts, his social habits, his conception of right, his love of beauty. Geology being thus brought into the atmosphere of moral and mental speculations, it may be expected that her investigations of the probable past will share an influence common to them; and that she will not be allowed to point to an origin of her own, a merely physical beginning of things; but that, as she approaches towards such a goal, she will be led to see that it is the origin of many trains of events, the point of convergence of many lines. It may be, that instead of being allowed to travel up to this focus of being, we are only able to estimate its place and nature, and to form of it such a judgment as this;—that it is not only the source of mere vegetable and animal life, but also of rational and social life. language and arts, law and order; in short, of all the progressive tendencies by which the highest principles of the intellectual and moral world have been and are developed, as well as of the succession of organic forms, which we find scattered, dead or living, over the earth.

This reflection concerning the natural scientific view of creation, it will be observed, has not been sought for, from a wish to arrive at such conclusions; but it has flowed spontaneously from the manner in which we have had to introduce geology into our classification of the sciences; and this classification was framed from an unbiassed consideration of the general analogies and guiding ideas of the various portions of our knowledge. Such remarks as we have made may on this account be considered more worthy of attention.

But such a train of thought must be pursued with caution. Although it may not be possible to arrive at a right conviction respecting the origin of the world, without having recourse to other than physical considerations, and to other than geological evidence; yet extraneous considerations, and extraneous evidence, respecting the nature of the beginning of things, must never be allowed to influence our physics or our geology. Our geological dynamics, like our astronomical dynamics, may be inadequate to carry us back to an origin of that state of things, of which it explains the progress: but this deficiency must be supplied, not by adding supernatural to natural geological dynamics, but by accepting, in their proper place, the views supplied by a portion of knowledge of a different character and order. If we include in our Theology the speculations to which we have recourse for this purpose, we must exclude from them our Geology. The two sciences may conspire, not by having any part in common; but because, though widely diverse in their lines, both point to a mysterious and invisible origin of the world.

All that which claims our assent on those higher grounds of which theology takes cognizance, must claim such assent as is consistent with those grounds; that is, it must require belief in respect of all that bears upon the highest relations of our being, those on which depend our duties and our hopes. Doctrines of this kind may and must be conveyed and maintained, by means of information concerning the past history of man, and his social and material, as well as moral and spiritual fortunes. He who believes that a Providence has ruled the affairs of mankind, will also believe that a Providence has governed the material world. But any language in which the narrative of this government of the material world can be conveyed, must necessarily be very imperfect and inappropriate; being expressed in terms of those ideas which have been selected by men, in order to describe appearances and relations of created things as they affect one another. In all cases, therefore, where we have to attempt to interpret such a narrative, we must feel that we are extremely liable to err; and most of all, when our interpretation refers to those material objects and operations which are most foreign to the main purpose of a history of providence. If we have to consider a communication containing a view of such a government of the world, imparted to us, as we may suppose, in order to point out the right direction for our feelings of trust, and reverence, and hope, towards the Governor of the world, we may expect that we shall be in no danger of collecting from our authority erroneous notions with regard to the power, and wisdom, and goodness of His government; or with respect to our own place, duties, and prospects, and the history of our race so far as our duties and prospects are concerned. But that we shall rightly understand the detail of all events in the history of man, or of the skies, or of the earth, which are narrated for the purpose of thus giving a right direction to our minds, is by no means equally certain; and I do not think it would be too much to say, that an immunity from perplexity and error, in such matters, is, on general grounds, very improbable. It cannot then surprise us to find, that parts of such narrations which seem to refer to occurrences like those of which astronomers and geologists have attempted to determine the laws, have given rise to many interpretations, all inconsistent with one another, and most of them at variance with the best established principles of astronomy and geology.

It may be urged, that all truths must be consistent with all other truths, and that therefore the results of true geology or astronomy cannot be irreconcileable with the statements of true theology. And this universal consistency of truth with itself must be assented to; but it by no means follows that we must be able to obtain a full insight into the nature and manner of such a consistency. Such an insight would only be possible if we could obtain a clear view of that central body of truth, the source of the principles which appear in the separate lines of speculation. To expect that we should see clearly how the providential government of the world is consistent with the unvarying laws by which its motions and developements are regulated, is to expect to understand thoroughly the laws of motion, of developement, and of providence; it is to expect that we may ascend from geology and astronomy to the creative and legislative centre, from which proceeded earth and stars; and then descend again into the moral and spiritual world, because its source and centre are the same as those of the material creation. It is to say that reason, whether finite or infinite, must be consistent with itself; and that, therefore, the finite must be able to comprehend the infinite, to travel from any one province of the moral and material universe to any other, to trace their bearing, and to connect their boundaries.

One of the advantages of the study of the history and nature of science in which we are now engaged is, that it warns us of the hopeless and presumptuous character of such attempts to understand the government of the world by the aid of science, without throwing any discredit upon the reality of our knowledge;—that while it shows how solid and certain each science is, so long as it refers its own facts to its own ideas, it confines each science within its own limits, and condemns it as empty and helpless, when it pronounces upon those subjects which are extraneous to it. The error of persons who should seek a geological narrative in theological records, would be rather in the search itself than in their interpretation of what they might find; and in like manner the error of those who would conclude against a supernatural beginning, or a providential direction of the world, upon geological or physiological reasonings, would be, that they had expected those sciences alone to place the origin or the government of the world in its proper light.

Though these observations apply generally to all the palætiological sciences, they may be permitted here, because they have an especial bearing upon some of the difficulties which have embarrassed the progress of geological speculation; and though such difficulties are, I trust, nearly gone by, it is important for us to see them in their true bearing.

From what has been said, it follows that geology and astronomy are, of themselves, incapable of giving us any distinct and satisfactory account of the origin of the universe, or of its parts. We need not wonder, then, at any particular instance of this incapacity; as, for example, that of which we have been speaking, the impossibility of accounting by any natural means for the production of all the successive tribes of plants and animals which have peopled the world in the various stages of its progress, as geology teaches us. That they were, like our own animal and vegetable contemporaries, profoundly adapted to the condition in which they were placed, we have ample reason to believe; but when we inquire whence they came into this our world, geology is silent. The mystery of creation is not within the range of her legitimate territory; she says nothing, but she points upwards.

### *Sect. 6.—The Hypothesis of the regular Creation and Extinction of Species.*

1. *Creation of Species.*—We have already seen, how untenable, as a physiological doctrine, is the principle of the transmutability and progressive tendency of species; and therefore, when we come to apply to theoretical geology the principles of the present chapter, this portion of the subject will easily be disposed of. I hardly know whether I can state that there is any other principle which has been applied to the solution of the geological problem, and which, therefore, as a general truth, ought to be considered here. Mr. Lyell, indeed, has spoken[12] of an hypothesis that "the successive creation of species may constitute a regular part of the economy of nature:" but he has nowhere, I think, so described this process as to make it appear in what department of science we are to place the hypothesis. Are these new species created by the production, at long intervals, of an offspring different in species from the parents? Or are the species so created produced without parents? Are they gradually evolved from some embryo substance? or do they suddenly start from the ground, as in the creation of the poet?

> . . . . . . . . Perfect forms
> Limbed and full-grown: out of the ground up rose
> As from his lair, the wild beast where he wons
> In forest wild, in thicket, brake, or den; . . .
> The grassy clods now calved; now half appeared
> The tawny lion, pawing to get free
> His hinder parts; then springs as broke from bounds,
> And rampant shakes his brinded mane; &c. &c.
>
> *Paradise Lost*, B. vii.

Some selection of one of these forms of the hypothesis, rather than the others, with evidence for the selection, is requisite to entitle us to

[12] B. III. c. xi. p. 234.

place it among the known causes of change which in this chapter we are considering. The bare conviction that a creation of species has taken place, whether once or many times, so long as it is unconnected with our organical sciences, is a tenet of Natural Theology rather than of Physical Philosophy.

[2nd Ed.] [Mr. Lyell has explained his theory[13] by supposing man to people a great desert, introducing into it living plants and animals; and he has traced, in a very interesting manner, the results of such a hypothesis on the distribution of vegetable and animal species. But he supposes the agents who do this, before they import species into particular localities, to study attentively the climate and other physical conditions of each spot, and to use various precautions. It is on account of the notion of design thus introduced that I have, above, described this opinion as rather a tenet of Natural Theology than of Physical Philosophy.

Mr. Edward Forbes has published some highly interesting speculations on the distribution of existing species of animals and plants. It appears that the manner in which animal and vegetable forms are now diffused requires us to assume centres from which the diffusion took place by no means limited by the present divisions of continents and islands. The changes of land and water which have thus occurred since the existing species were placed on the earth must have been very extensive, and perhaps reach into the glacial period of which I have spoken above.[14]

According to Mr. Forbes's views, for which he has offered a great body of very striking and converging reasons, the present vegetable and animal population of the British Isles is to be accounted for by the following series of events. The marine deposits of the *meiocene* formation were elevated into a great Atlantic continent, yet separate from what is now America, and having its western shore where now the great semi-circular belt of gulf-weed ranges from the 15th to the 45th parallel of latitude. This continent then became stocked with life, and of its vegetable population, the flora of the west of Ireland, which has many points in common with the flora of Spain and the At-

[13] B. III. c. viii. p. 166.

[14] See, in *Memoirs of the Geological Survey of Great Britain*, vol. i. p. 336, Professor Forbes's Memoir "On the Connection between the Distribution of the existing Fauna and Flora of the British Isles, and the Geological Changes which have affected their area, especially during the epoch of the Northern Drift."

lantic islands (the *Asturian* flora), is the record. The region between Spain and Ireland, and the rest of this meiocene continent, was destroyed by some geological movement, but there were left traces of the connexion which still remain. Eastwards of the flora just mentioned, there is a flora common to Devon and Cornwall, to the south-east part of Ireland, the Channel Isles, and the adjacent provinces of France;—a flora passing to a southern character; and having its course marked by the remains of a great rocky barrier, the destruction of which probably took place anterior to the formation of the narrower part of the channel. Eastward from this *Devon* or *Norman* flora, again, we have the *Kentish* flora, which is an extension of the flora of North-western France, insulated by the breach which formed the straits of Dover. Then came the *Glacial period*, when the east of England and the north of Europe were submerged, the northern drift was distributed, and England was reduced to a chain of islands or ridges, formed by the mountains of Wales, Cumberland, and Scotland, which were connected with the land of Scandinavia. This was the period of glaciers, of the dispersion of boulders, of the grooving and scratching of rocks as they are now found. The climate being then much colder than it now is, the flora, even down to the water's edge, consisted of what are now Alpine plants; and this *Alpine* flora is common to Scandinavia and to our mountain-summits. And these plants kept their places, when, by the elevation of the land, the whole of the present German Ocean became a continent connecting Britain with central Europe. For the increased elevation of their stations counterbalanced the diminished cold of the succeeding period. Along the dry bed of the German Sea, thus elevated, the principal part of the existing flora of England, the *Germanic* flora, migrated. A large portion of our existing animal population also came over through the same region; and along with those, came hyenas, tigers, rhinoceros, aurochs, elk, wolves, beavers, which are extinct in Britain, and other animals which are extinct altogether, as the primigenian elephant or mammoth. But then, again, the German Ocean and the Irish Channel were scooped out; and the climate again changed. In our islands, so detached, many of the larger beasts perished, and their bones were covered up in peat-mosses and caves, where we find them. This distinguished naturalist has further shown that the population of the sea lends itself to the same view. Mr. Forbes says that the writings of Mr. Smith, of Jordan-hill, "On the last Changes in the relative Levels of the Land and Sea in the British Islands," published in the *Memoirs of the Wer* *nerian Society for* 1837-8, must be esteemed the foundation of a critical investigation of this subject in Britain.]

2. *Extinction of Species.*—With regard to the extinction of species, Mr. Lyell has propounded a doctrine which is deserving of great attention here. Brocchi, when he had satisfied himself, by examination of the Sub-Apennines, that about half the species which had lived at the period of their deposition, had since become extinct, suggested as a possible cause for this occurrence, that the vital energies of a species, like that of an individual, might gradually decay in the progress of time and of generations, till at last the prolific power might fail, and the species wither away. Such a property would be conceivable as a physiological fact; for we see something of the kind in fruit-trees propagated by cuttings: after some time, the stock appears to wear out, and loses its peculiar qualities. But we have no sufficient evidence that this is the case in generations of creatures continued by the reproductive powers. Mr. Lyell conceives, that, without admitting any inherent constitutional tendency to deteriorate, the misfortunes to which plants and animals are exposed by the change of the physical circumstances of the earth, by the alteration of land and water, and by the changes of climate, must very frequently occasion the loss of several species. We have historical evidence of the extinction of one conspicuous species, the Dodo, a bird of large size and singular form, which inhabited the Isle of France when that island was first discovered, and which now no longer exists. Several other species of animals and plants seem to be in the course of vanishing from the face of the earth, even under our own observation. And taking into account the greater changes of the surface of the globe which geology compels us to assume, we may imagine many or all the existing species of living things to be extirpated. If, for instance, that reduction of the climate of the earth which appears, from geological evidence, to have taken place already, be supposed to go on much further, the advancing snow and cold of the polar regions may destroy the greater part of our plants and animals, and drive the remainder, or those of them which possess the requisite faculties of migration and accommodation, to seek an asylum near the equator. And if we suppose the temperature of the earth to be still further reduced, this zone of now-existing life, having no further place of refuge, will perish, and the whole earth will be tenanted, if at all, by a new creation. Other causes might produce the same effect as a change of climate; and, without supposing such causes to affect the whole globe, it is easy to

imagine circumstances such as might entirely disturb the equilibrium which the powers of diffusion of different species have produced;—might give to some the opportunity of invading and conquering the domain of others; and in the end, the means of entirely suppressing them, and establishing themselves in their place.

That this extirpation of certain species, which, as we have seen, happens in a few cases under common circumstances, might happen upon a greater scale, if the range of external changes were to be much enlarged, cannot be doubted. The extent, therefore, to which natural causes may account for the extinction of species, will depend upon the amount of change which we suppose in the physical conditions of the earth. It must be a task of extreme difficulty to estimate the effect upon the organic world, even if the physical circumstances were given. To determine the physical condition to which a given state of the earth would give rise, I have already noted as another very difficult problem. Yet these two problems must be solved, in order to enable us to judge of the sufficiency of any hypothesis of the extinction of species; and in the mean time, for the mode in which new species come into the places of those which are extinguished, we have (as we have seen) no hypothesis which physiology can, for a moment, sanction.

### *Sect.* 7.—*The Imbedding of Organic Remains.*

There is still one portion of the Dynamics of Geology, a branch of great and manifest importance, which I have to notice, but upon which I need only speak very briefly. The mode in which the spoils of existing plants and animals are imbedded in the deposits now forming, is a subject which has naturally attracted the attention of geologists. During the controversy which took place in Italy respecting the fossils of the Sub-Apennine hills, Vitaliano Donati,[15] in 1750, undertook an examination of the Adriatic, and found that deposits containing shells and corals, extremely resembling the strata of the hills, were there in the act of formation. But without dwelling on other observations of like kind, I may state that Mr. Lyell has treated this subject, and all the topics connected with it, in a very full and satisfactory manner. He has explained,[16] by an excellent collection of illustrative facts, how deposits of various substance and contents are formed; how plants and animals become fossil in peat, in blown sand, in volcanic matter, in

[15] Lyell, B. I. c. iii. p. 67. (4th ed.)

[16] B. III. c. xiii. xiv. xv. xvi. xvii.

alluvial soil, in caves, and in the beds of lakes and seas. This exposition is of the most instructive character, as a means of obtaining right conclusions concerning the causes of geological phenomena. Indeed, in many cases, the similarity of past effects with operations now going on, is so complete, that they may be considered as identical; and the discussion of such cases belongs, at the same time, to Geological Dynamics and to Physical Geology; just as the problem of the fall of meteorolites may be considered as belonging alike to mechanics and to physical astronomy. The growth of modern peat-mosses, for example, fully explains the formation of the most ancient: objects are buried in the same manner in the ejections of active and of extinct volcanoes; within the limits of history, many estuaries have been filled up; and in the deposits which have occupied these places, are strata containing shells,[17] as in the older formations.

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[17] Lyell, B. III. c. xvii. p. 286. See also his Address to the Geological Society in 1837, for an account of the Researches of Mr. Stokes and of Professor Göppert, on the lapidification of vegetables.

# PHYSICAL GEOLOGY.

## CHAPTER VII.

### Progress of Physical Geology.

#### *Sect.* 1.—*Object and Distinctions of Physical Geology.*

BEING, in consequence of the steps which we have attempted to describe, in possession of two sciences, one of which traces the laws of action of known causes, and the other describes the phenomena which the earth's surface presents, we are now prepared to examine how far the attempts to refer the facts to their causes have been successful: we are ready to enter upon the consideration of Theoretical or *Physical* Geology, as, by analogy with Physical Astronomy, we may term this branch of speculation.

The distinction of this from other portions of our knowledge is sufficiently evident. In former times, Geology was always associated with Mineralogy, and sometimes confounded with it; but the mistake of such an arrangement must be clear, from what has been said. Geology is connected with Mineralogy, only so far as the latter science classifies a large portion of the objects which Geology employs as evidence of its statements. To confound the two is the same error as it would be to treat philosophical history as identical with the knowledge of medals. Geology procures evidence of her conclusions wherever she can; from minerals or from seas; from inorganic or from organic bodies; from the ground or from the skies. The geologist's business is to learn the past history of the earth; and he is no more limited to one or a few kinds of documents, as his sources of information, than is the historian of man, in the execution of a similar task.

Physical Geology, of which I now speak, may not be always easily separable from Descriptive Geology: in fact, they have generally been combined, for few have been content to describe, without attempting in some measure to explain. Indeed, if they had done so, it is probable that their labors would have been far less zealous, and their expositions far less impressive. We by no means regret, therefore, the mixture of these two kinds of knowledge, which has so often occurred; but still, it is our business to separate them. The works of astronomers before the rise of sound physical astronomy, were full of theories, but these were advantageous, not prejudicial, to the progress of the science.

Geological theories have been abundant and various; but yet our history of them must be brief. For our object is, as must be borne in mind, to exhibit these, only so far as they are steps discoverably tending to the *true* theory of the earth: and in most of them we do not trace this character. Or rather, the portions of the labors of geologists which do merit this praise, belong to the two preceding divisions of the subject, and have been treated of there.

The history of Physical Geology, considered as the advance towards a science as real and stable as those which we have already treated of (and this is the form in which we ought to trace it), hitherto consists of few steps. We hardly know whether the progress is begun. The history of Physical Astronomy almost commences with Newton, and few persons will venture to assert that the Newton of Geology has yet appeared.

Still, some examination of the attempts which have been made is requisite, in order to explain and justify the view which the analogy of scientific history leads us to take, of the state of the subject. Though far from intending to give even a sketch of all past geological speculations, I must notice some of the forms such speculations have at different times assumed.

### *Sect. 2.—Of Fanciful Geological Opinions.*

Real and permanent geological knowledge, like all other physical knowledge, can be obtained only by inductions of classification and law from many clearly seen phenomena. The labor of the most active, the talent of the most intelligent, are requisite for such a purpose. But far less than this is sufficient to put in busy operation the inventive and capricious fancy. A few appearances hastily seen, and arbitrarily interpreted, are enough to give rise to a wondrous tale of the past, full of strange events and supernatural agencies. The mythology and early poetry of nations afford sufficient evidence of man's love of the wonderful, and of his inventive powers, in early stages of intellectual development. The scientific faculty, on the other hand,

and especially that part of it which is requisite for the induction of laws from facts, emerges slowly and with difficulty from the crowd of adverse influences, even under the most favorable circumstances. We have seen that in the ancient world, the Greeks alone showed themselves to possess this talent; and what they thus attained to, amounted only to a few sound doctrines in astronomy, and one or two extremely imperfect truths in mechanics, optics, and music, which their successors were unable to retain. No other nation, till we come to the dawn of a better day in modern Europe, made any positive step at all in sound physical speculation. Empty dreams or useless exhibitions of ingenuity, formed the whole of their essays at such knowledge.

It must, therefore, independently of positive evidence, be considered as extremely improbable, that any of these nations should, at an early period, have arrived, by observation and induction, at wide general truths, such as the philosophers of modern times have only satisfied themselves of by long and patient labor and thought. If resemblances should be discovered between the assertions of ancient writers and the discoveries of modern science, the probability in all cases, the certainty in most, is that these are accidental coincidences;—that the ancient opinion is no anticipation of the modern discovery, but is one guess among many, not a whit the more valuable because its expression agrees with a truth. The author of the guess could not intend the truth, because his mind was not prepared to comprehend it. Those of the ancients who spoke of the *harmony* which binds all things together, could not mean the Newtonian gravitation, because they had never been led to conceive an attractive force, governed by definite mathematical laws in its quantity and operation.

In agreement with these views, we must, I conceive, estimate the opinions which we find among the ancients, respecting the changes which the earth's surface has undergone. These opinions, when they are at all of a general kind, are arbitrary fictions of the fancy, showing man's love of generality indeed, but indulging it without that expense of labor and thought which alone can render it legitimate.

We might, therefore, pass by all the traditions and speculations of Oriental, Egyptian, and Greek cosmogony, as extraneous to our subject. But since these have recently been spoken of, as conclusions collected, however vaguely, from observed facts,[1] we may make a remark or two upon them.

[1] Lyell, B. i. c. ii. p. 8. (4th ed.)

The notion of a series of creations and destructions of worlds, which appears in the sacred volume of the Hindoos, which formed part of the traditionary lore of Egypt, and which was afterwards adopted into the poetry and philosophy of Greece, must be considered as a mythological, not a physical, doctrine. When this doctrine was dwelt upon, men's thoughts were directed, not to the terrestrial facts which it seemed to explain, but to the attributes of the deities which it illustrated. The conception of a Supreme power, impelling and guiding the progress of events, which is permanent among all perpetual change, and regular among all seeming chance, was readily entertained by contemplative and enthusiastic minds; and when natural phenomena were referred to this doctrine, it was rather for the purpose of fastening its impressiveness upon the senses, than in the way of giving to it authority and support. Hence we perceive that in the exposition of this doctrine, an attempt was always made to fill and elevate the mind with the notions of marvellous events, and of infinite times, in which vast cycles of order recurred. The "great year," in which all celestial phenomena come round, offered itself as capable of being calculated; and a similar great year was readily assumed for terrestrial and human events. Hence there were to be brought round by great cycles, not only deluges and conflagrations which were to destroy and renovate the earth, but also the series of historical occurrences. Not only the sea and land were to recommence their alternations, but there was to be another Argo, which should carry warriors on the first sea-foray,[2] and another succession of heroic wars. Looking at the passages of ancient authors which refer to terrestrial changes in this view, we shall see that they are addressed almost entirely to the love of the marvellous and the infinite, and cannot with propriety be taken as indications of a spirit of physical philosophy. For example, if we turn to the celebrated passage in Ovid,[3] where Pythagoras is represented as asserting that land becomes sea, and sea land, and many other changes which geologists have verified, we find that these observations are associated with many fables, as being matter of exactly the same kind;—the fountain of Ammon which was cold by day and warm by night;[4]—the waters of Salmacis which effeminate men;—the Clitorian spring which makes them loathe wine;—the Simplegades islands which were once moveable;—the Tritonian lake which covered men's bodies with feathers;—and many similar marvels. And the general purport of

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[2] Virg. *Eclog.* 4. [3] *Met.* Lib. xv. [4] V. 309, &c.

the whole is, to countenance the doctrine of the metempsychosis, and the Pythagorean injunction of not eating animal food. It is clear, I think, that facts so introduced must be considered as having been contemplated rather in the spirit of poetry than of science.

We must estimate in the same manner, the very remarkable passage brought to light by M. Elie de Beaumont,[5] from the Arabian writer, Kazwiri; in which we have a representation of the same spot of ground, as being, at successive intervals of five hundred years, a city, a sea, a desert, and again a city. This invention is adduced, I conceive, rather to feed the appetite of wonder, than to fix it upon any reality: as the title of his book, *The Marvels of Nature*, obviously intimates.

The speculations of Aristotle, concerning the exchanges of land and sea which take place in long periods, are not formed in exactly the same spirit, but they are hardly more substantial; and seem to be quite as arbitrary, since they are not confirmed by any examples and proofs. After stating,[6] that the same spots of the earth are not always land and always water, he gives the reason. "The principle and cause of this is," he says, "that the inner parts of the earth, like the bodies of plants and animals, have their ages of vigor and of decline; but in plants and animals all the parts are in vigor, and all grow old, at once: in the earth different parts arrive at maturity at different times by the operation of cold and heat: they grow and decay on account of the sun and the revolution of the stars, and thus the parts of the earth acquire different power, so that for a certain time they remain moist, and then become dry and old: and then other places are revivified, and become partially watery." We are, I conceive, doing no injustice to such speculations by classing them among *fanciful* geological opinions.

We must also, I conceive, range in the same division another class of writers of much more modern times;—I mean those who have framed their geology by interpretations of Scripture. I have already endeavored to show that such an attempt is a perversion of the purpose of a divine communication, and cannot lead to any physical truth. I do not here speak of geological speculations in which the Mosaic account of the deluge has been referred to; for whatever errors may have been committed on that subject, it would be as absurd to disregard the most ancient historical record, in attempting to trace back the history of the earth, as it would be, gratuitously to reject any other

[5] *Ann. des Sc. Nat.* xxv. 380.

[6] *Meteorol.* i. 14.

source of information. But the interpretations of the account of the creation have gone further beyond the limits of sound philosophy: and when we look at the arbitrary and fantastical inventions by which a few phrases of the writings of Moses have been moulded into complete systems, we cannot doubt that these interpretations belong to the present Section.

I shall not attempt to criticize, nor even to enumerate, these Scriptural Geologies,—*Sacred Theories of the Earth*, as Burnet termed his. Ray, Woodward, Whiston, and many other persons to whom science has considerable obligations, were involved, by the speculative habits of their times, in these essays; and they have been resumed by persons of considerable talent and some knowledge, on various occasions up to the present day; but the more geology has been studied on its own proper evidence, the more have geologists seen the unprofitable character of such labors.

I proceed now to the next step in the progress of Theoretical Geology.

### *Sect. 3.—Of Premature Geological Theories.*

While we were giving our account of Descriptive Geology, the attentive reader would perceive that we did, in fact, state several steps in the advance towards general knowledge; but when, in those cases, the theoretical aspect of such discoveries softened into an appearance of mere classification, the occurrence was assigned to the history of Descriptive rather than of Theoretical Geology. Of such a kind was the establishment, by a long and vehement controversy, of the fact, that the impressions in rocks are really the traces of ancient living things; such, again, were the division of rocks into Primitive, Secondary, Tertiary; the ascertainment of the orderly succession of organic remains; the consequent fixation of a standard series of formations and strata; the establishment of the igneous nature of trap rocks; and the like. These are geological truths which are assumed and implied in the very language which geology uses; thus showing how in this, as in all other sciences, the succeeding steps involve the preceding. But in the history of geological theory, we have to consider the wider attempts to combine the facts, and to assign them to their causes.

The close of the last century produced two antagonist theories of this kind, which long maintained a fierce and doubtful struggle;—that of Werner and that of Hutton: the one termed *Neptunian*, from its ascribing the phenomena of the earth's surface mainly to aqueous agency; the other *Plutonian* or *Vulcanian*, because it employed the force of subterraneous fire as its principal machinery. The circumstance which is most worthy of notice in these remarkable essays is, the endeavor to give, by means of such materials as the authors possessed, a complete and simple account of all the facts of the earth's history. The Saxon professor, proceeding on the examination of a small district in Germany, maintained the existence of a chaotic fluid, from which a series of universal formations had been precipitated, the position of the strata being broken up by the falling in of subterraneous cavities, in the intervals between these depositions. The Scotch philosopher, who had observed in England and Scotland, thought himself justified in declaring that the existing causes were sufficient to spread new strata on the bottom of the ocean, and that they are consolidated, elevated, and fractured by volcanic heat, so as to give rise to new continents.

It will hardly be now denied that all that is to remain as permanent science in each of these systems must be proved by the examination of many cases and limited by many conditions and circumstances. Theories so wide and simple, were consistent only with a comparatively scanty collection of facts, and belong to the early stage of geological knowledge. In the progress of the science, the "theory" of each part of the earth must come out of the examination of that part, combined with all that is well established, concerning all the rest; and a general theory must result from the comparison of all such partial theoretical views. Any attempt to snatch it before its time must fail; and therefore we may venture at present to designate general theories, like those of Hutton and Werner, as *premature*.

This, indeed, is the sentiment of most of the good geologists of the present day. The time for such general systems, and for the fierce wars to which the opposition of such generalities gives rise, is probably now past for ever; and geology will not again witness such a controversy as that of the Wernerian and Huttonian schools.

. . . . . . As when two black clouds
With heaven's artillery fraught, come rattling on
Over the Caspian: then stand front to front,
Hovering a space, till winds the signal blow
To join their dark encounter in mid-air.
So frowned the mighty combatants, that hell
Grew darker at their frown; so matched they stood:
For never but once more was either like
To meet so great a foe.

The main points really affecting the progress of sound theoretical geology, will find a place in one of the two next Sections.

[2nd Ed.] [I think I do no injustice to Dr. Hutton in describing his theory of the earth as *premature*. Prof. Playfair's elegant work, *Illustrations of the Huttonian Theory*, (1802,) so justly admired, contains many doctrines which the more mature geology of modern times rejects; such as the igneous origin of chalk-flints, siliceous pudding stone, and the like; the universal formation of river-beds by the rivers themselves; and other points. With regard to this last-mentioned question, I think all who have read Deluc's *Geologie* (1810) will deem his refutation of Playfair complete.

But though Hutton's theory was premature, as well as Werner's, the former had a far greater value as an important step on the road to truth. Many of its boldest hypotheses and generalizations have become a part of the general creed of geologists; and its publication is perhaps the greatest event which has yet occurred in the progress of Physical Geology.]

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# CHAPTER VIII.

## The Two Antagonist Doctrines of Geology.

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### *Sect.* 1.—*Of the Doctrine of Geological Catastrophes.*

THAT great changes, of a kind and intensity quite different from the common course of events, and which may therefore properly be called *catastrophes*, have taken place upon the earth's surface, was an opinion which appeared to be forced upon men by obvious facts. Rejecting, as a mere play of fancy, the notions of the destruction of the earth by cataclysms or conflagrations, of which we have already spoken, we find that the first really scientific examination of the materials of the earth, that of the Sub-Apennine hills, led men to draw this inference. Leonardo da Vinci, whom we have already noticed for his early and strenuous assertion of the real marine origin of fossil impressions of shells, also maintained that the bottom of the sea had become the top of the mountain; yet his mode of explaining this may perhaps be claimed by the modern advocates of uniform causes as more allied to their opinion, than to the doctrine of catastrophes.[1] But Steno, in 1669, approached nearer to this doctrine; for he asserted that Tuscany must have changed its face at intervals, so as to acquire six different configurations, by the successive breaking down of the older strata into inclined positions, and the horizontal deposit of new ones upon them. Strabo, indeed, at an earlier period had recourse to earthquakes, to explain the occurrence of shells in mountains; and Hooke published the same opinion later. But the Italian geologists prosecuted their researches under the advantage of having, close at hand, large collections of conspicuous and consistent phenomena. Lazzaro Moro, in 1740, attempted to apply the theory of earthquakes to the Italian strata; but both he and his expositor, Cirillo Generelli, inclined rather to reduce the violence of these operations within the ordinary course of nature,[2] and thus leant to the doctrine of uniformity, of which we have afterwards to speak. Moro was encouraged in this line of speculation by the extraordinary occurrence, as it was deemed by most persons, of the rise of a new volcanic island from a deep part of the Mediterranean, near Santorino, in 1707.[3] But in other countries, as the geological facts were studied, the doctrine of catastrophes appeared to gain ground. Thus in England, where, through a large part of the country, the coal-measures are extremely inclined and contorted, and covered over by more horizontal fragmentary beds, the opinion that some violent catastrophe had occurred to dislocate them, before the superincumbent strata were deposited, was strongly held. It was conceived that a period of violent and destructive action must have succeeded to one of repose; and that, for a time, some unusual and paroxysmal forces must have been employed in elevating and breaking the pre-existing strata, and wearing their fragments into smooth pebbles, before nature subsided into a new age of tranquillity and vitality. In like manner Cuvier, from the alternations of fresh-water and salt-water species in the strata of Paris, collected the opinion of a series of great revolutions, in which "the thread of induction was broken." Deluc and others, to whom we owe the first steps in geological dynamics, attempted carefully to distinguish between causes now in action, and those which have ceased to act; in which latter class they reckoned the causes which have

[1] "Here is a part of the earth which has become more light, and which rises, while the opposite part approaches nearer to the centre, and what was the bottom of the sea is become the top of the mountain."—Venturi's *Léonardo da Vinci.*

[2] Lyell, i. 3. p. 64. (4th ed.) [3] Ib. p. 60.

elevated the existing continents. This distinction was assented to by many succeeding geologists. The forces which have raised into the clouds the vast chains of the Pyrenees, the Alps, the Andes, must have been, it was deemed, something very different from any agencies now operating.

This opinion was further confirmed by the appearance of a complete change in the forms of animal and vegetable life, in passing from one formation to another. The species of which the remains occurred, were entirely different, it was said, in two successive epochs: a new creation appears to have intervened; and it was readily believed that a transition, so entirely out of the common course of the world, might be accompanied by paroxysms of mechanical energy. Such views prevail extensively among geologists up to the present time: for instance, in the comprehensive theoretical generalizations of Elie de Beaumont and others, respecting mountain-chains, it is supposed that, at certain vast intervals, systems of mountains, which may be recognized by the parallelism of course of their inclined beds, have been disturbed and elevated, lifting up with them the aqueous strata which had been deposited among them in the intervening periods of tranquillity, and which are recognized and identified by means of their organic remains: and according to the adherents of this hypothesis, these sudden elevations of mountain-chains have been followed, again and again, by mighty waves, desolating whole regions of the earth.

The peculiar bearing of such opinions upon the progress of physical geology will be better understood by attending to the *doctrine of uniformity*, which is opposed to them, and with the consideration of which we shall close our survey of this science, the last branch of our present task.

### *Sect.* 2.—*Of the Doctrine of Geological Uniformity.*

The opinion that the history of the earth had involved a series of catastrophes, confirmed by the two great classes of facts, the symptoms of mechanical violence on a very large scale, and of complete changes in the living things by which the earth had been tenanted, took strong hold of the geologists of England, France, and Germany. Hutton, though he denied that there was evidence of a beginning of the present state of things, and referred many processes in the formation of strata to existing causes, did not assert that the elevatory forces which raise continents from the bottom of the ocean, were of the same order as well as of the same kind, with the volcanoes and earthquakes which now shake the surface. His doctrine of uniformity was founded rather on the supposed analogy of other lines of speculation, than on the examination of the amount of changes now going on. "The Author of nature," it was said, "has not permitted in His works any symptom of infancy or of old age, or any sign by which we may estimate either their future or their past duration:" and the example of the planetary system was referred to in illustration of this.[4] And a general persuasion that the champions of this theory were not disposed to accept the usual opinions on the subject of creation, was allowed, perhaps very unjustly, to weigh strongly against them in the public opinion.

While the rest of Europe had a decided bias towards the doctrine of geological catastrophes, the phenomena of Italy, which, as we have seen, had already tended to soften the rigor of that doctrine, in the progress of speculation from Steno to Generelli, were destined to mitigate it still more, by converting to the belief of uniformity transalpine geologists who had been bred up in the catastrophist creed. This effect was, indeed, gradual. For a time the distinction of the *recent* and the *tertiary* period was held to be marked and strong. Brocchi asserted that a large portion of the Sub-Apennine fossil shells belonged to a living species of the Mediterranean Sea: but the geologists of the rest of Europe turned an incredulous ear to this Italian tenet; and the persuasion of the distinction of the tertiary and the recent period was deeply impressed on most geologists by the memorable labors of Cuvier and Brongniart on the Paris basin. Still, as other tertiary deposits were examined, it was found that they could by no means be considered as contemporaneous, but that they formed a chain of posts, advancing nearer and nearer to the recent period. Above the strata of the basins of London and Paris,[5] lie the newer strata of Touraine, of Bourdeaux, of the valley of the Bormida and the Superga near Turin, and of the basin of Vienna, explored by M. Constant Prevost. Newer and higher still than these, are found the Sub-Apennine formations of Northern Italy, and probably of the same period, the English "crag" of Norfolk and Suffolk. And most of these marine formations are associated with volcanic products and fresh-water deposits, so as to imply apparently a long train of alternations of corresponding processes. It may easily be supposed that, when the subject had assumed this form, the boundary of the present and past condition of the earth

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[4] Lyell, i. 4, p. 94. [5] Lyell, 1st ed. vol. iii. p. 61.

was in some measure obscured. But it was not long before a very able attempt was made to obliterate it altogether. In 1828, Mr. Lyell set out on a geological tour through France and Italy.[6] He had already conceived the idea of classing the tertiary groups by reference to the number of recent species which were found in a fossil state. But as he passed from the north to the south of Italy, he found, by communication with the best fossil conchologists, Borelli at Turin, Guidotti at Parma, Costa at Naples, that the number of extinct species decreased; so that the last-mentioned naturalist, from an examination of the fossil shells of Otranto and Calabria, and of the neighboring seas, was of opinion that few of the tertiary shells were of extinct species. To complete the series of proof, Mr. Lyell himself explored the strata of Ischia, and found, 2000 feet above the level of the sea, shells, which were all pronounced to be of species now inhabiting the Mediterranean; and soon after, he made collections of a similar description on the flanks of Etna, in the Val di Noto, and in other places.

The impression produced by these researches is described by himself.[7] "In the course of my tour I had been frequently led to reflect on the precept of Descartes, that a philosopher should once in his life doubt everything he had been taught; but I still retained so much faith in my early geological creed as to feel the most lively surprize on visiting Sortino, Pentalica, Syracuse, and other parts of the Val di Noto, at beholding a limestone of enormous thickness, filled with recent shells, or sometimes with mere casts of shells, resting on marl in which shells of Mediterranean species were imbedded in a high state of preservation. All idea of [necessarily] attaching a high antiquity to a regularly-stratified limestone, in which the casts and impressions of shells alone were visible, vanished at once from my mind. At the same time, I was struck with the identity of the associated igneous rocks of the Val di Noto with well-known varieties of 'trap' in Scotland and other parts of Europe; varieties which I had also seen entering largely into the structure of Etna.

"I occasionally amused myself," Mr. Lyell adds, "with speculating on the different rate of progress which geology might have made, had it been first cultivated with success at Catania, where the phenomena above alluded to, and the great elevation of the modern tertiary beds in the Val di Noto, and the changes produced in the historical era by the Calabrian earthquakes, would have been familiarly known."

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[6] 1st ed. vol. iii. Pref.

[7] Lyell, 1st ed. Pref. x.

Before Mr. Lyell entered upon his journey, he had put into the hands of the printer the first volume of his "Principles of Geology, being an attempt to explain the former Changes of the Earth's Surface *by reference to the Causes now in Operation*." And after viewing such phenomena as we have spoken of, he, no doubt, judged that the doctrine of catastrophes of a kind entirely different from the existing course of events, would never have been generally received, if geologists had at first formed their opinions upon the Sicilian strata. The boundary separating the present from the anterior state of things crumbled away; the difference of fossil and recent species had disappeared, and, at the same time, the changes of position which marine strata had undergone, although not inferior to those of earlier geological periods, might be ascribed, it was thought, to the same kind of earthquakes as those which still agitate that region. Both the supposed proofs of catastrophic transition, the organical and the mechanical changes, failed at the same time; the one by the removal of the fact, the other by the exhibition of the cause. The powers of earthquakes, even such as they now exist, were, it was supposed, if allowed to operate for an illimitable time, adequate to produce all the mechanical effects which the strata of all ages display. And it was declared that all evidence of a beginning of the present state of the earth, or of any material alteration in the energy of the forces by which it has been modified at various epochs, was entirely wanting.

Other circumstances in the progress of geology tended the same way. Thus, in cases where there had appeared in one country a sudden and violent transition from one stratum to the next, it was found, that by tracing the formations into other countries, the chasm between them was filled up by intermediate strata; so that the passage became as gradual and gentle as any other step in the series. For example, though the conglomerates, which in some parts of England overlie the coal-measures, appear to have been produced by a complete discontinuity in the series of changes; yet in the coal-fields of Yorkshire, Durham, and Cumberland, the transition is smoothed down in such a way that the two formations pass into each other. A similar passage is observed in Central-Germany, and in Thuringia is so complete, that the coal-measures have sometimes been considered as subordinate to the *todtliegendes*.[8]

Upon such evidence and such arguments, the doctrine of catastro-

[8] De la Beche, p. 414, *Manual.*

phes was rejected with some contempt and ridicule; and it was maintained, that the operation of the causes of geological change may properly and philosophically be held to have been uniform through all ages and periods. On this opinion, and the grounds on which it has been urged, we shall make a few concluding remarks.

It must be granted at once, to the advocates of this geological uniformity, that we are not arbitrarily to assume the existence of catastrophes. The degree of uniformity and continuity with which terremotive forces have acted, must be collected, not from any gratuitous hypothesis, but from the facts of the case. We must suppose the causes which have produced geological phenomena, to have been as similar to existing causes, and as dissimilar, as the effects teach us. We are to avoid all bias in favor of powers deviating in kind and degree from those which act at present; a bias which, Mr. Lyell asserts, has extensively prevailed among geologists.

But when Mr. Lyell goes further, and considers it a merit in a course of geological speculation that it *rejects* any difference between the intensity of existing and of past causes, we conceive that he errs no less than those whom he censures. "An *earnest and patient endeavor to reconcile* the former indication of change,"[9] with *any* restricted class of causes,—a habit which he enjoins,—is not, we may suggest, the temper in which science ought to be pursued. The effects must themselves teach us the nature and intensity of the causes which have operated; and we are in danger of error, if we seek for slow and shun violent agencies further than the facts naturally direct us, no less than if we were parsimonious of time and prodigal of violence. *Time*, inexhaustible and ever accumulating his efficacy, can undoubtedly do much for the theorist in geology; but *Force*, whose limits we cannot measure, and whose nature we cannot fathom, is also a power never to be slighted: and to call in the one to protect us from the other, is equally presumptuous, to whichever of the two our superstition leans. To invoke Time, with ten thousand earthquakes, to overturn and set on edge a mountain-chain, should the phenomena indicate the change to have been sudden and not successive, would be ill excused by pleading the obligation of first appealing to known causes.[10]

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[9] Lyell, B. iv. c. i. p. 328, 4th ed.

[10] [2nd Ed.] [I have, in the text, quoted the fourth edition of Mr. Lyell's *Principles*, in which he recommends "an earnest and patient endeavor to reconcile the former indications of change with the evidence of gradual mutation

In truth, we know causes only by their effects; and in order to learn the nature of the causes which modify the earth, we must study them through all ages of their action, and not select arbitrarily the period in which we live as the standard for all other epochs. The forces which have produced the Alps and Andes are known to us by experience, no less than the forces which have raised Etna to its present height; for we learn their amount in both cases by their results. Why, then, do we make a merit of using the latter case as a measure for the former? Or how can we know the true scale of such force, except by comprehending in our view all the facts which we can bring together?

In reality when we speak of the *uniformity* of nature, are we not obliged to use the term in a very large sense, in order to make the doctrine at all tenable? It includes catastrophes and convulsions of a very extensive and intense kind; what is the limit to the violence which we must allow to these changes? In order to enable ourselves to represent geological causes as operating with uniform energy through all time, we must measure our time by long cycles, in which repose and violence alternate; how long may we extend this cycle of change, the repetition of which we express by the word *uniformity?*

And why must we suppose that all our experience, geological as well as historical, includes more than *one* such cycle? Why must we insist upon it, that man has been long enough an observer to obtain the *average* of forces which are changing through immeasurable time?

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now in progress." In the sixth edition, in that which is, I presume, the corresponding passage, although it is transferred from the fourth to the first Book (B. i. c. xiii. p. 325) he recommends, instead, "an earnest and patient inquiry how far geological appearances are reconcileable with the effect of changes now in progress." But while Mr. Lyell has thus softened the advocate's character in his language in this passage, the transposition which I have noticed appears to me to have an opposite tendency. For in the former edition, the causes now in action were first described in the second and third Books, and the great problem of Geology, stated in the first Book, was attempted to be solved in the fourth. But by incorporating this fourth Book with the first, and thus prefixing to the study of existing causes arguments against the belief of their geological insufficiency, there is an appearance as if the author wished his reader to be prepared by a previous pleading against the doctrine of catastrophes, before he went to the study of existing causes. The Doctrines of Catastrophes and of Uniformity, and the other leading questions of the Palætiological Sciences, are further discussed in the *Philosophy of the Inductive Sciences* Book x.]

The analogy of other sciences has been referred to, as sanctioning this attempt to refer the whole train of facts to known causes. To have done this, it has been said, is the glory of Astronomy: she seeks no hidden virtues, but explains all by the force of gravitation, which we witness operating at every moment. ' But let us ask, whether it would really have been a merit in the founders of Physical Astronomy, to assume that the celestial revolutions resulted from any selected class of known causes? When Newton first attempted to explain the motions of the moon by the force of gravity, and failed because the measures to which he referred were erroneous, would it have been philosophical in him, to insist that the difference which he found ought to be overlooked, since otherwise we should be compelled to go to causes other than those which we usually witness in action? Or was there any praise due to those who assumed the celestial forces to be the same with gravity, rather than to those who assimilated them with any other known force, as magnetism, till the calculation of the laws and amount of these forces, from the celestial phenomena, had clearly sanctioned such an identification? We are not to select a conclusion now well proved, to persuade ourselves that it would have been wise to assume it anterior to proof, and to attempt to philosophize in the method thus recommended.

Again, the analogy of Astronomy has been referred to, as confirming the assumption of perpetual uniformity. The analysis of the heavenly motions, it has been said, supplies no trace of a beginning, no promise of an end. But here, also, this analogy is erroneously applied. Astronomy, as the science of cyclical motions, has nothing in common with Geology. But look at Astronomy where she has an analogy with Geology; consider our knowledge of the heavens as a palætiological science;—as the study of a past condition, from which the present is derived by causes acting in time. Is there then no evidence of a beginning, or of a progress? What is the import of the Nebular Hypothesis? A luminous matter is condensing, solid bodies are forming, are arranging themselves into systems of cyclical motion; in short, we have exactly what we are told, on this analogy, we ought not to have;—the beginning of a world. I will not, to justify this argument, maintain the truth of the nebular hypothesis; but if geologists wish to borrow maxims of philosophizing from astronomy, such speculations as have led to that hypothesis must be their model.

Or, let them look at any of the other provinces of palætiological speculation; at the history of states, of civilization, of languages. We may assume some *resemblance* or connexion between the principles which determined the progress of government, or of society, or of literature, in the earliest ages, and those which now operate; but who has speculated successfully, assuming an *identity* of such causes? Where do we now find a language in the process of formation, unfolding itself in inflexions, terminations, changes of vowels by grammatical relations, such as characterize the oldest known languages? Where do we see a nation, by its natural faculties, inventing writing, or the arts of life, as we find them in the most ancient civilized nations? We may assume hypothetically, that man's faculties develop themselves in these ways; but we see no such effects produced by these faculties, in our own time, and now in progress, without the influence of foreigners.

Is it not clear, in all these cases, that history does not exhibit a series of cycles, the aggregate of which may be represented as a uniform state, without indication of origin or termination? Does it not rather seem evident that, in reality, the whole course of the world, from the earliest to the present times, is but *one* cycle, yet unfinished;—offering, indeed, no clear evidence of the mode of its beginning; but still less entitling us to consider it as a repetition or series of repetitions of what had gone before?

Thus we find, in the analogy of the sciences, no confirmation of the doctrine of uniformity, as it has been maintained in Geology. Yet we discern, in this analogy, no ground for resigning our hope, that future researches, both in Geology and in other palætiological sciences, may throw much additional light on the question of the uniform or catastrophic progress of things, and on the earliest history of the earth and of man. But when we see how wide and complex is the range of speculation to which our analogy has referred us, we may well be disposed to pause in our review of science;—to survey from our present position the ground that we have passed over;—and thus to collect, so far as we may, guidance and encouragement to enable us to advance in the track which lies before us.

Before we quit the subject now under consideration, we may, however, observe, that what the analogy of science really teaches us, as the most promising means of promoting this science, is the strenuous cultivation of the two subordinate sciences, Geological Knowledge of Facts, and Geological Dynamics. These are the two provinces of knowledge—corresponding to Phenomenal Astronomy, and Mathematical Mechanics—which may lead on to the epoch of the Newton of geology. We may, indeed, readily believe that we have much to do in both these departments. While so large a portion of the globe is geologically unexplored;—while all the general views which are to extend our classifications satisfactorily from one hemisphere to another, from one zone to another, are still unformed; while the organic fossils of the tropics are almost unknown, and their general relation to the existing state of things has not even been conjectured;—how can we expect to speculate rightly and securely, respecting the history of the whole of our globe? And if Geological Classification and Description are thus imperfect, the knowledge of Geological Causes is still more so. As we have seen, the necessity and the method of constructing a science of such causes, are only just beginning to be perceived. Here, then, is the point where the labors of geologists may be usefully applied; and not in premature attempts to decide the widest and abstrusest questions which the human mind can propose to itself.

It has been stated,[11] that when the Geological Society of London was formed, their professed object was to multiply and record observations, and patiently to await the result at some future time; and their favorite maxim was, it is added, that the time was not yet come for a General System of Geology. This was a wise and philosophical temper, and a due appreciation of their position. And even now, their task is not yet finished; their mission is not yet accomplished. They have still much to do, in the way of collecting Facts; and in entering upon the exact estimation of Causes, they have only just thrown open the door of a vast Labyrinth, which it may employ many generations to traverse, but which they must needs explore, before they can penetrate to the Oracular Chamber of Truth.

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I REJOICE, on many accounts, to find myself arriving at the termination of the task which I have attempted. One reason why I am glad to close my history is, that in it I have been compelled, especially in the latter part of my labors, to speak as a judge respecting eminent philosophers whom I reverence as my Teachers in those very sciences on which I have had to pronounce a judgment;—if, indeed, even the appellation of Pupil be not too presumptuous. But I doubt not that such men are as full of candor and tolerance, as they are of knowledge and thought. And if they deem, as I did, that such a history of

[11] Lyell, B. i. c. iv. p. 103.

science ought to be attempted, they will know that it was not only the historian's privilege, but his duty, to estimate the import and amount of the advances which he had to narrate; and if they judge, as I trust they will, that the attempt has been made with full integrity of intention and no want of labor, they will look upon the inevitable imperfections of the execution of my work with indulgence and hope.

There is another source of satisfaction in arriving at this point of my labors. If, after our long wandering through the region of physical science, we were left with minds unsatisfied and unraised, to ask, "Whether this be all?"—our employment might well be deemed weary and idle. If it appeared that all the vast labor and intense thought which has passed under our review had produced nothing but a barren Knowledge of the external world, or a few Arts ministering merely to our gratification; or if it seemed that the methods of arriving at truth, so successfully applied in these cases, aid us not when we come to the higher aims and prospects of our being;—this History might well be estimated as no less melancholy and unprofitable than those which narrate the wars of states and the wiles of statesmen. But such, I trust, is not the impression which our survey has tended to produce. At various points, the researches which we have followed out, have offered to lead us from matter to mind, from the external to the internal world; and it was not because the thread of investigation snapped in our hands, but rather because we were resolved to confine ourselves, for the present, to the material sciences, that we did not proceed onwards to subjects of a closer interest. It will appear, also, I trust, that the most perfect method of obtaining speculative truth,—that of which I have had to relate the result,—is by no means confined to the least worthy subjects; but that the Methods of learning what is really true, though they must assume different aspects in cases where a mere contemplation of external objects is concerned, and where our own internal world of thought, feeling, and will, supplies the matter of our speculations, have yet a unity and harmony throughout all the possible employments of our minds. To be able to trace such connexions as this, is the proper sequel, and would be the high reward, of the labor which has been bestowed on the present work. And if a persuasion of the reality of such connexions, and a preparation for studying them, have been conveyed to the reader's mind while he has been accompanying me through our long survey, his time may not have been employed on these pages in vain. However vague and hesitating and obscure may be such a persuasion, it belongs, I doubt not, to the dawning of a better Philosophy, which it may be my lot, perhaps, to develop more fully hereafter, if permitted by that Superior Power to whom all sound philosophy directs our thoughts.

# ADDITIONS TO THE THIRD EDITION.

## BOOK VIII.

### ACOUSTICS.

### CHAPTER III.

SOUND.

*The Velocity of Sound in Water.*

THE Science of which the history is narrated in this Book has for its objects, the minute Vibrations of the parts of bodies such as those by which Sounds are produced, and the properties of Sounds. The Vibrations of bodies are the result of a certain tension of their structure which we term *Elasticity*. The Elasticity determines the rate of Vibration: the rate of Vibration determines the audible note: the Elasticity determines also the velocity with which the vibration travels through the substance. These points of the subject, Elasticity, Rate of Vibration, Velocity of Propagation, Audible Note, are connected in each substance, and are different in different substances.

In the history of this Science, considered as tending to a satisfactory general theory, the Problems which have obviously offered themselves were, to explain the properties of Sounds by the relations of their constituent vibrations; and to explain the existence of vibrations by the elasticity of the substances in which they occurred: as in Optics, philosophers have explained the phenomenon of light and colors by the Undulatory Theory, and are still engaged in explaining the requisite modulations by means of the elasticity of the Ether. But the *Undulatory Theory of Sound* was seen to be true at an early period of the Science: and the explanation, in a general way at least, of all kinds of such undulations by means of the elasticity of the vibrating substances has been performed by a series of mathematicians of whom I have given an account in this Book. Hence the points of the subject already mentioned (Elasticity, Vibrations and their Propagations, and Note), have a known material dependence, and each may be employed in determining the other: for instance, the Note may be employed in determining the velocity of sound and the elasticity of the vibrating substance.

Chladni,[1] and the Webers,[2] had made valuable experimental inquiries on such subjects. But more complete investigations of this kind have been conducted with care and skill by M. Wertheim.[3] For instance, he has determined the velocity with which sound travels in water, by making an organ-pipe to sound by the passage of water through it. This is a matter of some difficulty; for the mouthpiece of an organ-pipe, if it be not properly and carefully constructed, produces sounds of its own, which are not the genuine musical note of the pipe. And though the note depends mainly upon the length of the pipe, it depends also, in a small degree, on the breadth of the pipe and the size of the mouthpiece.

If the pipe were a mere line, the time of a vibration would be the time in which a vibration travels from one end of the pipe to the other; and thus the note for a given length (which is determined by the time of vibration), is connected with the velocity of vibration. He thus found that the velocity of a vibration along the pipe in sea-water is 1157 *mètres* per second.

But M. Wertheim conceived that he had previously shown, by general mathematical reasoning, that the velocity with which sound travels in an unlimited expanse of any substance, is to the velocity with which it travels along a pipe or linear strip of the same substance as the square root of 3 to the square root of 2. Hence the velocity of sound in sea-water would be 1454 *mètres* a second. The velocity of sound in air is 332 *mètres*.

M. Wertheim also employed the vibrations of rods of steel and other metals in order to determine their *modulus of elasticity*—that is, the quantity which determines for each substance, the extent to which, in virtue of its elasticity, it is compressed and expanded by given pressures or tensions. For this purpose he caused the rod to vibrate near to a tuning-fork of given pitch, so that both the rod and the tuning-fork by their vibrations traced undulating curves on a revolving disk. The curves traced by the two could be compared so as to give their relative rate, and thus to determine the elasticity of the substance.

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[1] *Traité d'Acoustique*, 1809. [2] *Wellenlehre*, 1852.

[3] *Mémoires de Physique Mécanique.* Paris, 1848.

# BOOK IX.

## PHYSICAL OPTICS.

### *Photography.*

I HAVE, at the end of Chapter xi., stated that the theory of which I have endeavored to sketch the history professes to explain only the phenomena of radiant visible light; and that though we know that light has other properties—for instance, that it produces chemical effects—these are not contemplated as included within the domain of the theory. The chemical effects of light cannot as yet be included in exact and general truths, such as those which constitute the undulatory theory of radiant visible light. But though the present age has not yet attained to a *Science* of the chemistry of Light, it has been enriched with a most exquisite *Art*, which involves the principles of such a science, and may hereafter be made the instrument of bringing them into the view of the philosopher. I speak of the Art of *Photography*, in which chemistry has discovered the means of producing surfaces almost as sensitive to the modifications of light as the most sensitive of organic textures, the retina of the eye: and has given permanence to images which in the eye are only momentary impressions. Hereafter, when the laws shall have been theoretically established, which connect the chemical constitution of bodies with the action of light upon them, the prominent names in the Prelude to such an Epoch must be those who by their insight, invention, and perseverance, discovered and carried to their present marvellous perfection the processes of photographic Art:—Niepce and Daguerre in France, and our own accomplished countryman, Mr. Fox Talbot.

### *Fluorescence.*

As already remarked, it is not within the province of the undulatory theory to explain the phenomena of the absorption of light which take place in various ways when the light is transmitted through various mediums. I have, at the end of Chapter iii., given the reasons which prevent my assenting to the assertion of a special analysis of light by absorption. In the same manner, with regard to other effects produced by media upon light, it is sufficient for the defence of the theory that it should be consistent with the possibility of the laws of phenomena which are observed, not that it should explain those laws; for they belong, apparently, to another province of philosophy.

Some of the optical properties of bodies which have recently attracted notice appear to be of this kind. It was noticed by Sir John Herschel,[1] that a certain liquid, sulphate of quinine, which is under common circumstances colorless, exhibits in certain aspects and under certain incidences of light, a beautiful celestial blue color. It appeared that this color proceeded from the surface on which the light first fell; and color thus produced Sir J. Herschel called *epipolic* colors, and spoke of the light as *epipolized*. Sir David Brewster had previously noted effects of color in transparent bodies which he ascribed to internal dispersion:[2] and he conceived that the colors observed by Sir J. Herschel were of the same class. Professor Stokes[3] of Cambridge applied himself to the examination of these phenomena, and was led to the conviction that they arise from a power which certain bodies possess, of changing the color, and with it, the refrangibility of the rays of light which fall upon them: and he traced this property in various substances, into various remarkable consequences. As this change of refrangibility always makes the rays *less* refrangible, it was proposed to call it a *degradation* of the light; or again, *dependent emission*, because the light is emitted in the manner of self-luminous bodies, but only in dependence upon the active rays, and so long as the body is under their influence. In this respect it differs from *phosphorescence*, in which light is emitted without such dependence. The phenomenon occurs in a conspicuous and beautiful manner in certain kinds of fluor spar: and the term *fluorescence*, suggested by Professor Stokes, has the advantage of inserting no hypothesis, and will probably be found the most generally acceptable.[4]

It may be remarked that Professor Stokes rejects altogether the doctrine that light of definite refrangibility may still be compound, and may be analysed by absorption. He says, "I have not overlooked the remarkable effect of absorbing media in causing apparent changes

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[1] *Phil. Trans.* 1845.
[2] *Edinb. Trans.* 1833.
[3] *Phil. Trans.* 1852 and 1854.
[4] See *Phil. Trans.* 1852.

of color in a pure spectrum; but this I believe to be a subjective phenomenon depending upon contrast."

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# CHAPTER XIII.

## Undulatory Theory.

### *Direction of the Transverse Vibrations in Polarization.*

IN the conclusion of Chapter xiii. I have stated that there is a point in the undulatory theory which was regarded as left undecided by Young and Fresnel, and on which the two different opinions have been maintained by different mathematicians; namely, whether the vibrations of polarized light are perpendicular to the plane of polarization or in that plane. Professor Stokes of Cambridge has attempted to solve this question in a manner which is, theoretically, exceedingly ingenious, though it is difficult to make the requisite experiments in a decisive manner. The method may be briefly described.

If polarized light be *diffracted* (see Chap. xi. sect. 2), each ray will be bent from its position, but will still be polarized. The original ray and the diffracted ray, thus forming a broken line, may be supposed to be connected at the angle by a universal joint (called a *Hooke's Joint*), such that when the original ray turns about its axis, the diffracted ray also turns about its axis; as in the case of the long handle of a telescope and the screw which is turned by it. Now if the motion of the original ray round its axis be uniform, the motion of the diffracted ray round its axis is not uniform: and hence if, in a series of cases, the planes of polarization of the original ray differ by equal angles, in the diffracted ray the planes of polarization will differ by unequal angles. Then if vibrations be perpendicular to the plane of polarization, the planes of polarization in the diffracted rays will be crowded together in the neighborhood of the plane in which the diffraction takes place, and will be more rarely distributed in the neighborhood of the plane perpendicular to this, in which is the diffracting thread or groove.

On making the experiment, Prof. Stokes conceived that he found, in his experiments, such a crowding of the planes of diffracted polarization towards the plane of diffraction; and thus he held that the hypothesis that the transverse vibrations which constitute polarization are perpendicularly transverse to the plane of polarization was confirmed.[1]

But Mr. Holtzmann,[2] who, assenting to the reasoning, has made the experiment in a somewhat different manner, has obtained an opposite result; so that the point may be regarded as still doubtful.

### *Final Disproof of the Emission Theory.*

As I have stated in the History, we cannot properly say that there ever was an Emission Theory of Light which was the *rival* of the Undulatory Theory: for while the undulatory theory provided explanations of new classes of phenomena as fast as they arose, and exhibited a *consilience* of theories in these explanations, the hypothesis of emitted particles required new machinery for every new set of facts, and soon ceased to be capable even of expressing the facts. The simple cases of the ordinary reflexion and refraction of light were explained by Newton on the supposition that the transmission of light is the motion of particles: and though his explanation includes a somewhat harsh assumption (that a refracting surface exercises an attractive force through a *fixed finite* space), the authority of his great name gave it a sort of permanent notoriety, and made it to be regarded as a standard point of comparison between a supposed "Emission Theory" and the undulation theory. And the way in which the theories were to be tested in this case was obvious: in the Newtonian theory, the velocity of light is increased by the refracting medium; in the undulatory theory, it is diminished. On the former hypothesis the velocity of light in air and in water is as 3 to 4; in the latter, as 4 to 3.

But the immense velocity of light made it appear impossible to measure it, within the limits of any finite space which we can occupy with refracting matter. The velocity of light is known from astronomical phenomena;—from the eclipses of Jupiter's satellites, by which it appears that light occupies 8 minutes in coming from the sun to the earth; and from the aberration of light, by which its velocity is shown to be 10,000 times the velocity of the earth in its orbit. Is it, then, possible to make apparent so small a difference as that between its passing through a few yards of air and of water?

Mr. Wheatstone, in 1831, invented a machine by which this could

[1] *Camb. Trans.*, vol. IX. part i. 1849. [2] *Phil. Mag.*, Feb. 1857

be done. His object was to determine the velocity of the electric shock. His apparatus consisted in a small mirror, turning with great velocity about an axis which is in its own plane, like a coin spinning on its edge. The velocity of spinning may be made so great, that an object reflected shall change its place perceptibly after an almost inconceivably small fraction of a second. The application of this contrivance to measure the velocity of light, was, at the suggestion of Arago, who had seen the times of the rival theories of light, undertaken by younger men at Paris, his eyesight not allowing him to prosecute such a task himself. It was necessary that the mirrors should turn more than 1000 times in a second, in order that the two images, produced, one by light coming through air, and the other by light coming through an equal length of water, should have places perceptibly different. The mechanical difficulties of the experiment consisted in keeping up this great velocity by the machinery without destroying the machinery, and in transmitting the light without too much enfeebling it. These difficulties were overcome in 1850, by M. Fizeau and M. Léon Foucault separately: and the result was, that the velocity of light was found to be less in water than in air. And thus the Newtonian explanation of refraction, the last remnant of the Emission Theory, was proved to be false.

# BOOK X.

## THERMOTICS.—ATMOLOGY.

## CHAPTER III.

### The Relation of Vapour and Air.

#### *Sect.* 4.—*Force of Steam.*

THE experiments on the elastic force of steam made by the French Academy are fitted in an especial manner to decide the question between rival formulæ, in consequence of the great amount of force to which they extend; namely, 60 feet of mercury, or 24 atmospheres: for formulæ which give results almost indistinguishable in the lower part of the scale diverge widely at those elevated points. Mr. Waterston[1] has reduced both these and other experiments to a rule in the following manner:—He takes the zero of gaseous tension, determined by other experimenters (Rudberg, Magnus, and Regnault,) to be 461° below the zero of Fahrenheit, or 274° below the zero of the centigrade scale: and temperatures reckoned from this zero he calls "G temperatures." The square root of the G temperatures is the element to which the elastic force is referred (for certain theoretical reasons), and it is found that the density of steam is as the *sixth power* of this element. The agreement of this rule with the special results is strikingly close. A like rule was found by him to apply generally to many other gases in contact with their liquids.

But M. Regnault has recently investigated the subject in the most complete and ample manner, and has obtained results somewhat different.[2] He is led to the conclusion that no formula proceeding by

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[1] *Phil. Trans.* 1852.

[2] *Mém. de l'Institut,* vol. xxi. (1847). M. Regnault's Memoir occupies 767 pages.

a *power* of the temperature can represent the experiments. He also finds that the rule of Dalton (that as the temperatures increase in arithmetical progression, the elastic force increases in geometric progression) deviates from the observations, especially at high temperatures. Dalton's rule would be expressed by saying that the variable part of the elastic force is as $a^t$, where $t$ is the temperature. This failing, M. Regnault makes trial of a formula suggested by M. Biot, consisting of a sum of two terms, one of which is as $a^t$, and the other as $b^t$: and in this way satisfies the experiments very closely. But this can only be considered as a formula of interpolation, and has no theoretical basis. M. Roche had proposed a formula in which the force is as $a^z$, z depending upon the temperature by an equation[3] to which he had been led by theoretical considerations. This agrees better with observation than any other formula which includes only the same number of coefficients.

Among the experimental thermotical laws referred to by M. Regnault are, the Law of Watt,[4] that "the quantity of heat which is required to convert a pint of water at a temperature of zero into steam, is the same whatever be the pressure." Also, the Law of Southern, that "the latent heat of vaporization, that is the heat absorbed in the passage from the liquid to the gaseous consistence, is constant for all purposes: and that we obtain the total heat in adding to the constant latent heat the number which represents the latent heat of steam." Southern found the latent heat of the steam of water to be represented by about 950 degrees of Fahrenheit.[5]

### *Sect. 5.—Temperature of the Atmosphere.*

I MAY notice, as important additions to our knowledge on this subject, the results of four balloon ascents made in 1852,[6] by the Committee of the Meteorological Observatory established at Kew by the British Association for the Advancement of Science. In these ascents the observers mounted to more than 13,000, 18,000, and 19,000 feet, and in the last to 22,370; by which ascent the temperature fell from 49 degrees to nearly 10 degrees below zero; and the dew-point fell from 37° to 12°. Perhaps the most marked result of these observations is the following:—

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[3] The equation $z = \frac{t}{1 + mt}$.

[4] See Robison's *Mechanical Philosophy*, vol. ii. p. 8.

[5] Ib. p. 160.

[6] *Phil. Trans.* 1853.

The temperature of the air decreases uniformly as we ascend above the earth's surface; but this decrease does not go on continuously. At a certain elevation, varying on different days, the decrease is arrested; and for a depth of two or three thousand feet of air, the temperature decreases little, or even increases in ascending. Above this, the diminution again takes place at nearly the same rate as in the lower regions. This intermediate region of undecreasing temperature extended in the various ascents, from about altitude 4000 to 6000 feet, 6500 to 10,000, 2000 to 4500, and 4000 to 8000. This interruption in the decrease of temperature is accompanied by a large and abrupt fall in the temperature of the dew-point, or by an actual condensation of vapor. Thus, this region is the *region of the clouds*, and the increase of heat appears to arise from the latent heat liberated when aqueous vapor is formed into clouds.

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## CHAPTER IV.

### Theories of Heat.

#### *The Dynamical Theory of Heat.*

THAT the transmission of *radiant* Heat takes place by means of the vibrations of a medium, as the transmission of Sound certainly does, and the transmission of Light most probably, is a theory which, as I have endeavored to explain, has strong arguments and analogies in its favor. But that Heat itself, in its essence and quantity, is Motion, is a hypothesis of quite another kind. This hypothesis has been recently asserted and maintained with great ability. The doctrine thus asserted is, that Motion may be converted into Heat, and Heat into Motion; that Heat and Motion may produce each other, as we see in the rarefaction and condensation of air, in steam-engines, and the like: and that in all such cases the Motion produced and the Heat expended exactly measure each other. The foundation of this theory is conceived to have been laid by Mr. Joule of Manchester, in 1844; and it has since been prosecuted by him and by Professor Thomson of Glasgow, by experimental investigations of various kinds. It is difficult to make these experiments so as to be quite satisfactory; for it is difficult to measure *all* the heat gained or lost in any of the changes here contemplated. That friction, agitation of fluids, condensation of gases, conversion of gases into fluids and liquids into solids, produce heat, is undoubted: and that the quantity of such heat may be measured by the mechanical force which produces it, or which it produces, is a generalization which will very likely be found a fertile source of new propositions, and probably of important consequences.

As an example of the conclusions which Professor Thomson draws from this doctrine of the mutual conversion of motion and heat, I may mention his speculations concerning the cause which produces and sustains the heat of the sun.[1] He conceives that the support of the solar heat must be meteoric matter which is perpetually falling towards the globe of the sun, and has its motion converted into heat. He inclines to think that the meteors containing the stores of energy for future Sun-light must be principally within the earth's orbit; and that we actually see them there as the "Zodiacal Light," an illuminated shower, or rather tornado, of stones. The inner parts of this tornado are always getting caught in the Sun's atmosphere, and drawn to his mass by gravitation.

[1] On the Mechanical Energies of the Solar System. *Edinb. Trans.* vol. xxi. part i. (1854), p. 67.

# BOOK XI

## ELECTRICITY.

### GENERAL REMARKS.

ELECTRICITY in the form in which it was originally studied—Franklinic, frictional, or statical electricity—has been so completely identified with electricity in its more comprehensive form—Voltaic, chemical, or dynamical electricity—that any additions we might have to make to the history of the earlier form of the subject are included in the later science.

There are, however, several subjects which may still be regarded rather as branches of Electricity than of the Cognate Sciences. Such are, for instance, Atmospheric Electricity, with all that belongs to Thunderstorms and Lightning Conductors. The observation of Atmospheric Electricity has been prosecuted with great zeal at various meteorological observatories; and especially at the Observatory established by the British Association at Kew. The Aurora Borealis, again, is plainly an electrical phenomenon; but probably belonging rather to dynamical than to statical electricity. For it strongly affects the magnetic needle, and its position has reference to the direction of magnetism; but it has not been observed to affect the electroscope. The general features of this phenomenon have been described by M. de Humboldt, and more recently by M. de Bravais; and theories of the mode of its production have been propounded by MM. Biot, De la Rive, Kaemtz, and others.

Again, there are several fishes which have the power of giving an electrical shock:—the torpedo, the gymnotus, and the silurus. The agency of these creatures has been identified with electricity in the most general sense. The peculiar energy of the animal has been made to produce the effects which are produced by an electrical discharge or a voltaic current:—not only to destroy life in small animals, but to deflect a magnet, to make a magnet, to decompose water, and to produce a spark.

*Dr. Faraday's Views of Statical Electric Induction.*

According to the theories of electricity of Æpinus and Coulomb, which in this Book of our History are regarded as constituting a main part of the progress of this portion of science, the particles of the electric fluid or fluids exert forces, attractive and repulsive, upon each other in straight lines at a distance, in the same way in which, in the Newtonian theory of the universe, the particles of matter are conceived as exerting attractive forces upon each other. An electrized body presented a conducting body of any form, determines a new arrangement of the electric fluids in the conductor, attracting the like fluid to its own side, and repelling the opposite fluid to the opposite side. This is Electrical *Induction.* And as, by the theory, the attraction is greater at the smaller distances, the distribution of the fluid upon the conductor in virtue of this Induction will not be symmetrical, but will be governed by laws which it will require a complex and difficult calculation to determine—as we have seen was the case in the investigations of Coulomb, Poisson, and others.

Instead of this action at a distance, Dr. Faraday has been led to conceive Electrical Induction to be the result of an action taking place between the electrized body and the conductor through lines of contiguous particles in the mass of the intermediate body, which he calls the *Dielectric.* And the irregularities of the distribution of the electricity in these cases of Induction, and indeed the existence of an action in points protected from direct action by the protuberant sides of the conductor, are the causes, I conceive, which lead him to the conclusion that Induction takes place in *curved lines*[1] of such contiguous particles.

With reference to this, I may remark that, as I have said, the distribution of electricity on a conductor in the presence of an electrized body is so complex a mathematical problem that I do not conceive any merely popular way of regarding the result can entitle us to say, that the distribution which we find cannot be explained by the Coulombian theory, and must force us upon the assumption of an action in curved lines :—which is, indeed, itself a theory, and so vague a one

[1] *Researches*, 1165, &c.

that it requires to be made much more precise before we can say what consequences it does or does not lead to. Professor W. Thomson has arrived at a mathematical proof that the effect of induction on the view of Coulomb and of Faraday must, under certain conditions, be necessarily and universally the same.

With regard to the influence of different *Dielectrics* upon Induction, the inquiry appears to be of the highest importance; and may certainly necessitate some addition to the theory.

# BOOK XII.

## MAGNETISM.

### *Recent Progress of Terrestrial Magnetism.*

IN Chapter II., I have noticed the history of Terrestrial Magnetism; Hansteen's map published in 1819; the discovery of "magnetic storms" about 1825; the chain of associated magnetic observations, suggested by M. de Humboldt, and promoted by the British Association and the Royal Society; the demand for the continuation of these till 1848; the magnetic observations made in several voyages; the magnetic surveys of various countries. And I have spoken also of Gauss's theory of Terrestrial Magnetism, and his directions and requirements concerning the observations to be made. I may add a few words with regard to the more recent progress of the subject.

The magnetic observations made over large portions of the Earth's surface by various persons, and on the Ocean by British officers, have been transmitted to Woolwich, where they have been employed by General Sabine in constructing magnetic maps of the Earth for the year 1840.[1] Following the course of inquiry described in the part of the history referred to, these maps exhibit the declination, inclination, and intensity of the magnetic force at every point of the earth's surface. The curves which mark equal amounts of each of these three elements (the *lines of equal declination*, *inclination*, and *force*:—the *isogonal*, the *isoclinal*, and the *isodynamic* lines,) are, in their general form, complex and irregular; and it has been made a matter of question (the facts being agreed upon) whether it be more proper to say that they indicate four poles, as Halley and as Hansteen said, or only two poles, as Gauss asserts. The matter appears to become more clear if we draw magnetic *meridians;* that is, lines obtained by following the directions, or pointings, of the magnetic needle to the north or to

[1] These maps are published in Mr. Keith Johnstone's *Physical Atlas.*

the south, till we arrive at the points of convergence of all their directions; for there are only two *such* poles, one in the Arctic and one in the Antarctic region. But in consequence of the irregularity of the magnetic constitution of the earth, if we follow the inclination of the magnetic force round the earth on any parallel of latitude, we find that it has two *maxima* and two *minima*, as if there were four magnetic poles. The isodynamic map is a new presentation of the facts of this subject; the first having been constructed by Colonel Sabine in 1837.

I have stated also that the magnetic clements at each place are to be observed in such a manner as to bring into view both their *periodical*, their *secular*, and their *irregular* or *occasional* changes. The observations made at Toronto in Canada, and at Hobart Town in Van Diemen's Land, two stations at equal distances from the two poles of the earth, and also at St. Helena, a station within the tropics, have been discussed by General Sabine with great care, and with an amount of labor approaching to that employed upon reductions of astronomical observations. And the results have been curious and unexpected.

The declination was first examined.[2] This magnetical element is, as we have already seen (p. 232), liable both to a diurnal and to an annual inequality; and also to irregular perturbations which have been termed magnetic storms. Now it was found that all these inequalities went on increasing gradually and steadily from 1843 to 1848, so as to become, at the end of that time, above twice as large as they were at the beginning of it. A new periodical change in all these elements appeared to be clearly established by this examination. M. Lamont, of Munich, had already remarked indications of a decennial period in the diurnal variation of the declination of the needle. The duration of the period from minimum to maximum being about five years, and therefore the whole period about ten years. The same conclusion was found to follow still more decidedly from the observations of the dip and intensity.

This period of ten years had no familiar meaning in astronomy; and if none such had been found for it, its occurrence as a magnetic period must have been regarded, as General Sabine says,[3] in the light of a fragmentary fact. But it happened about this time that the scientific world was made aware of the existence of a like period in a pheno-

[2] *Phil. Trans.* 1852 and 1856. [3] *Phil. Trans.* 1856, p 382.

menon which no one would have guessed to be connected with terrestrial magnetism, namely, the spots in the Sun. M. Schwabe, of Dessau, had observed the Sun's disk with immense perseverance for 24 years:—often examining it more than 300 days in the year; and had found that the spots had, as to their quantity and frequency, a periodical character. The years of maximum are 1828, 1838, 1848, in which there were respectively 225,[4] 282, 330 groups of spots. The minimum years, 1833, 1843, had only 33 and 34 such groups. This curious fact[5] was first made public by M. de Humboldt, in the third volume of his *Kosmos* (1850). The coincidence of the periods and epochs of these two classes of facts was pointed out by General Sabine in a Memoir presented to the Royal Society in March, 1852.

Of course it was natural to suppose, even before this discovery, that the diurnal and annual inequalities of the magnetic element at each place depend upon the action of the sun, in some way or other.

Dr. Faraday had endeavored to point out how the effect of the solar heat upon the atmosphere would, according to the known relations of heat and magnetism, explain many of the phenomena. But this new feature of the phenomena, their quinquennial increase and decrease, makes us doubt whether such an explanation can really be the true one.

Of the *secular* changes in the magnetic elements, not much more is known than was known some years ago. These changes go on, but their laws are imperfectly known, and their causes not even conjectured. M. Hansteen, in a recent memoir,[6] says that the decrease of the inclination goes on progressively diminishing. With us this rate of decrease appears to be at present nearly uniform. We cannot help conjecturing that the sun, which has so plain a connexion with the diurnal, annual, and occasional movements of the needle, must also have some connexion with its secular movements.

In 1840 the observations made at various places had to a great extent enabled Gauss, in connexion with W. Weber, to apply his Theory to the actual condition of the Earth;[7] and he calculated the Declination, Inclination, and Intensity at above 100 places, and found

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[4] In 1837 there were 333.

[5] The observations up to 1844 were published in Poggendorf's *Annalen.*

[6] See K. Johnstone's *Physical Atlas.*

[7] *Atlas des Erdmagnetismus nach den Elementen der Theorie Entworfen.* See Preface.

the agreement, as he says, far beyond his hopes. They show, he says, that the Theory comes near to the Truth.

### *Correction of Ships' Compasses.*

The magnetic needle had become of importance when it was found that it always pointed to the North. Since that time the history of magnetism has had its events reflected in the history of navigation. The change of the declination arising from a change of place terrified the companions of Columbus. The determination of the laws of this change was the object of the voyage of Halley; and has been pursued with the utmost energy in the Arctic and Antarctic regions by navigators up to the present time. Probably the dependence of the magnetic declination upon place is now known well enough for the purposes of navigation. But a new source of difficulty has in the meantime come into view; the effect of the iron in the ship upon the Compass. And this has gone on increasing as guns, cables, stays, knees, have been made of iron; then steam-engines with funnels, wheels, and screws, have been added; and finally the whole ship has been made of iron. How can the compass be trusted in such cases?

I have already said in the history that Mr. Barlow proposed to correct the error of the compass by placing near to the compass an iron plate, which from its proximity to the compass might counterbalance magnetically the whole effect of the ship's iron upon the compass. This correction was not effectual, because the magnetic forces of the plate and of the ship do not change their direction and value according to the same law, with the change of position. I have further stated that Mr. Airy devised other means of correcting the error. I may add a few words on the subject; for the subject has been further examined by Mr. Airy[8] and by others.

It appears, by mathematical reasoning, that the magnetic effect of the iron in a ship may be regarded as producing two kinds of deviation which are added together;—a "polar-magnet deviation," which changes from positive to negative as the direction of the ship's keel, in a horizontal revolution, passes from semicircle to semicircle; and a "quadrantal deviation," which changes from positive to negative as the keel turns from quadrant to quadrant. The latter deviation may be remedied completely by a mass of unmagnetized iron placed on a level

[8] *Phil. Trans.* 1856.

with the compass, either in the athwartship line or in the fore-and-aft line, according to circumstances. "The polar-magnet-deviation" may be corrected at *any given place* by a magnet or magnets, but the magnets thus applied at one place will not always correct the deviation in another magnetic latitude. For it appears that this deviation arises partly from a magnetism inherent in the materials of the ship, not changing with the change of magnetic position, and partly from the effect of terrestrial magnetism upon the ship's iron. But the errors arising from both sources may be remedied by adjusting, at a new locality, the positions of the corrective magnets.

The inherent magnetism of the ship, of which I have spoken, may be much affected by the position in which the ship was built; and may change from time to time; for instance, by the effect of the battering of the waves, and other causes. Hence it is called by Mr. Airy "sub-permanent magnetism."

Another method of correcting the errors of a ship's compass has been proposed, and is used to some extent; namely, by *swinging* the ship round (in harbor) to all points of azimuth, and thus constructing a *Table of Compass Errors* for that particular ship. But to this method it is objected that the Table loses its value in a new magnetic latitude much more than the correction by magnets does; besides the inconveniences of steering a ship by a Table.

# BOOK XIII.

## VOLTAIC ELECTRICITY

### CHAPTER VIII.

#### Magneto-electric Induction.

FARADAY'S discovery that, in combinations like those in which a voltaic current was known to produce motion, motion would produce a voltaic current, naturally excited great attention among the scientific men of Europe. The general nature of his discovery was communicated by letter[1] to M. Hachette at Paris, in December, 1831; and experiments having the like results were forthwith made by MM. Becquerel and Ampère at Paris, and MM. Nobili and Antinori at Florence.

It was natural also that in a case in which the relations of space which determine the results are so complicated, different philosophers should look at them in different ways. There had been, from the first discovery by Oersted of the effect of a voltaic current upon a magnet, two rival methods of regarding the facts. Electric and magnetic lines exert an effort to place themselves transverse to each other (see chapter iv. of this Book), and (as I have already said) two ways offered themselves of simplifying this general truth :—to suppose an electric current made up of transverse magnetic lines; or to suppose magnetic lines made up of transverse electric currents. On either of these assumptions, the result was expressed by saying that *like* currents or lines (electric or magnetic) tend to place themselves parallel; which is a law more generally intelligible than the law of transverse position. Faraday had adopted the former view; had taken the lines of magnetic force for the fundamental lines of his system, and defined the direction of the magneto-electric current of induction by the relation

[1] *Ann. de Chimie*, vol. xlviii. (1831), p. 402.

of the motion to these lines. Ampère, on the other hand, supposed the magnet to be made up of transverse electric currents (chap. vi.); and had deduced all the facts of electro-dynamical action, with great felicity, from this conception. The question naturally arose, in what manner, on this view, were the new facts of magneto-electric induction by motion to be explained, or even expressed?

Various philosophers attempted to answer this question. Perhaps the form in which the answer has obtained most general acceptance is that in which it was put by Lenz, who discoursed on the subject to the Academy of St. Petersburg in 1833.[2] His general rule is to this effect: when a wire moves in the neighborhood of an electric current or a magnet, a current takes place in it, such as, existing independently, would have produced a motion opposite to the actual motion. Thus two parallel *forward* currents move towards each other:—hence if a current move towards a parallel wire, it produces in it a *backward* current. A moveable wire conducting a current *downwards* will move round the north pole of a magnet in the direction N., W., S., E:—hence if, when the wire have in it no current, we move it in the direction N., W., S., E., we produce in the wire an *upward* current. And thus, as M. de la Rive remarks,[3] in cases in which the mutual action of two currents produces a limited motion, as attraction or repulsion, or a deviation right or left, the corresponding magneto-electric induction produces an instantaneous current only; but when the electrodynamic action produces a continued motion, the corresponding motion produces, by induction, a continued current.

Looking at this mode of stating the law, it is impossible not to regard this effect as a sort of reaction; and accordingly, this view was at once taken of it. Professor Ritchie said, in 1833, "The law is founded on the universal principle that action and reaction are equal." Thus, if voltaic electricity induce magnetism under certain arrangements, magnetism will, by similar arrangements, react on a conductor and induce voltaic electricity.[4]

There are still other ways of looking at this matter. I have elsewhere pointed out that where polar properties co-exist, they are gene-

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[2] *Acad. Petrop.* Nov. 29, 1833. *Pogg. Ann.* vol. xxxi. p. 483.

[3] *Traité de l'Electricité,* vol. i. p. 441 (1854).

[4] On the Reduction of Mr. Faraday's discoveries in Magneto-electric Induction to a General Law. *Trans.* of R. S. in *Phil. Mag.* N.S. vol. iii. 37, and vol. iv. p. 11. In the second edition of this history I used the like expressions

rally found to be connected,[5] and have illustrated this law in the case of electrical, magnetical, and chemical polarities. If we regard motion backwards and forwards, to the right and the left, and the like, as *polar* relations, we see that magneto-electric induction gives us a new manifestation of connected polarities.

### *Diamagnetic Polarity.*

But the manifestation of co-existent polarities which are brought into view in this most curious department of nature is not yet exhausted by those which we have described. I have already spoken (chap. iv.) of Dr. Faraday's discovery that there are diamagnetic as well as magnetic bodies; bodies which are repelled by the pole of a magnet, as well as bodies which are attracted. Here is a new opposition of properties. What is the exact definition of this opposition in connexion with other polarities? To this, at present, different philosophers give different answers. Some say that diamagnetism is completely the opposite of ordinary magnetism, or, as Dr. Faraday has termed it for the sake of distinction, of *paramagnetism*. They say that as a north pole of a magnet gives to the neighboring extremity of a piece of soft iron a south pole, so it gives to the neighboring extremity of a piece of bismuth a north pole, and that the bismuth becomes for the time an inverted magnet; and hence, arranges itself across the line of magnetised force, instead of along it. Dr. Faraday himself at first adopted this view;[6] but he now conceives that the bismuth is not made polar, but is simply repelled by the magnet; and that the transverse position which it assumes, arises merely from its elongated form, each end trying to recede as far as possible from the repulsive pole of the magnet.

Several philosophers of great eminence, however, who have examined the subject with great care, adhere to Dr. Faraday's first view of the nature of Diamagnetism—as W. Weber,[7] Plücker, and Mr. Tyndall among ourselves. If we translate this view into the language of Ampère's theory, it comes to this:—that as currents are induced in iron and magnetics parallel to those existing in the inducing magnet or battery wire; so in bismuth, heavy glass, and other diamagnetic bodies, the currents induced are in the contrary directions:—

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[5] *Phil. Ind. Sc.* B. v. c. ii.

[6] Faraday's *Researches*, Art. 2429, 2430.

[7] Poggendorf's *Ann. Jou.* 1848.

these hypothetical currents being in non-conducting diamagnetic, as in magnetic bodies, not in the mass, but round the particles of the matter.

### *Magneto-optic Effects and Magnecrystallic Polarity.*

Not even yet have we terminated the enumeration of the co-existent polarities which in this province of nature have been brought into view. Light has polar properties; the very term *polarization* is the record of the discovery of these. The forces which determine the crystalline forms of bodies are of a polar nature: crystalline forms, when complete, may be defined as those forms which have a certain degree of symmetry in reference to opposite poles. Now has this optical and crystalline polarity any relation to the electrical polarity of which we have been speaking?

However much we might be disposed beforehand to conjecture that there is some relation between these two groups of polar properties, yet in this as in the other parts of this history of discoveries respecting polarities, no conjecture hits the nature of the relation, such as experiment showed it to be. In November, 1846, Faraday announced the discovery of what he then called "the action of magnets on light." But this action was manifested, not on light directly, but on light passing through certain kinds of glass.[8] When this glass, subjected to the action of the powerful magnets which he used, transmitted a ray of light parallel to the line of magnetic force, an effect was produced upon the light. But of what nature was this effect? When light was ordinary light, no change in its condition was discoverable. But if the light were light polarized in any plane, the plane of polarization was turned round through a certain angle while the ray passed through the glass:—a greater angle, in proportion as the magnetic force was greater, and the thickness of the glass greater.

A power in some respects of this kind, namely, a power to rotate the plane of polarization of a ray passing through them, is possessed by some bodies in their natural state; for instance, quartz crystals, and oil of turpentine. But yet, as Dr. Faraday remarks,[9] there is a great difference in the two cases. When polarized rays pass through oil of turpentine, in whatever direction they pass, they all of them have their

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[8] Silicated borate of lead. See *Researches*, § 2151, &c. Also flint glass, rock salt, water (2215).

[9] *Researches*, Art. 2231.

plane of polarization rotated in the same direction; that is, all to the right or all to the left; but when a ray passes through the heavy glass, the power of rotation exists only in a plane perpendicular to the magnetic line, and its direction as right or left-handed is reversed by reversing the magnetic polarity.

In this case, we have optical properties, which do not depend on crystalline form, affected by the magnetic force. But it has also been found that crystalline form, which is so fertile a source of optical properties, affords indications of magnetic forces. In 1847, M. Plücker,[10] of the University of Bonn, using a powerful magnetic apparatus, similar to Faraday's, found that crystals in general are magnetic, in this sense, that the axes of crystalline form tend to assume a certain position with reference to the magnetic lines of force. The possession of one optic axis or of two is one of the broad distinctions of the different crystalline forms: and using this distinction, M. Plücker found that a crystal having a single optic axis tends to place itself with this axis transverse to the magnetic line of force, as if its optic axis were repelled by each magnetic pole; and crystals with two axes act as if each of these axes were repelled by the magnetic poles. This force is independent of the magnetic or diamagnetic character of the crystal; and is a directive, more properly than an attractive or repulsive force.

Soon afterwards (in 1848) Faraday also discovered[11] an effect of magnetism depending on crystalline form, which at first sight appeared to be different from the effects observed by M. Plücker. He found that a crystal of bismuth, of which the form is nearly a cube, but more truly a rhombohedron with one diagonal a little longer than the others, tends to place itself with this diagonal in the direction of the lines of magnetic force. At first he conceived[12] the properties thus detected to be different from those observed by M. Plücker; since in this case the force of a crystalline axis is axial, whereas in those, it was equatorial. But a further consideration of the subject, led him[13] to a conviction that these forces must be fundamentally identical: for it was easy to conceive a combination of bismuth crystals which would behave in the magnetic field as a crystal of calcspar does; or a combination of calcspar crystals which would behave as a crystal of bismuth does.

And thus we have fresh examples to show that the Connexion of coexistent Polarities is a thought deeply seated in the minds of the pro-

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[10] Taylor's *Scientific Memoirs*, vol. v.

[11] *Researches*, Art. 2454, &c.

[12] Art. 2469.

[13] Art. 2593, 2601.

foundest and most sagacious philosophers, and perpetually verified and illustrated, by unforeseen discoveries in unguessed forms, through the labors of the most skilful experimenters.

### *Magneto-electric Machines.*

The discovery that a voltaic wire moved in presence of a magnet, has a current generated in it, was employed as the ground of the construction of machines to produce electrical effects. In Saxton's machine two coils of wire including a core of soft iron revolved opposite to the ends of a horseshoe magnet, and thus, as the two coils came opposite to the N. and S., and to the S. and N. poles of the magnet, currents were generated alternately in the wires in opposite directions. But by arranging the connexions of the ends of the wires, the successive currents might be made to pass in corresponding directions. The alternations or successions of currents in such machines are governed by a contrivance which alternately interrupts and permits the action; this contrivance has been called a *rheotome*. Clarke gave a new form to a machine of the same nature as Saxton's. But the like effect may be produced by using an electro-magnet instead of a common magnet. When this is done, a current is produced which by induction produces a current in another wire, and the action is alternately excited and interrupted. When the inducing current is interrupted, a momentary current *in an opposite direction* is produced in the induced wire; and when this current stops, it produces in the inducing wire a current *in the original direction*, which may be adjusted so as to reinforce the resumed action of the original current. This was pointed out by M. De la Rive in 1843.[14] Machines have been constructed on such principles by him and others. Of such machines the most powerful hitherto known is that constructed by M. Ruhmkorff. The effects of this instrument are exceedingly energetic.

### *Applications of Electrodynamic Discoveries.*

The great series of discoveries of which I have had to speak have been applied in many important ways to the uses of life. The *Electric Telegraph* is one of the most remarkable of these. By wires extended to the most distant places, the electric current is transmitted

[14] *Traité de l'Elect.* i. 391.

thither in an imperceptible time; and by means of well-devised systems of operation, is made to convey from man to man words, which are now most emphatically "winged words." In the most civilized states such wires now form a net-work across the land, which is familiar to our thoughts as the highway is to our feet; and wide seas have such pathways of human thought buried deep in their waves from shore to shore. Again, by using the chemical effects of electrodynamic action, of which we shall have to speak in the next Book, a new means has been obtained of copying, with an exactness unattainable before, any forms which art or nature has produced, and of covering them with a surface of metal. The *Electrotype Process* is now one of the great powers which manufacturing art employs.

But these discoveries have also been employed in explaining natural phenomena, the causes of which had before been altogether inscrutable. This is the case with regard to the diurnal variation of the magnetic needle; a fact which as to its existence is universal in all places, and which yet is so curiously diverse in its course at different places. Dr. Faraday has shown that some of the most remarkable of these diversities, and probably all, seem to be accounted for by the different magnetic effects of air at different temperatures: although, as I have already said, [Book xii.) the discovery of a decennial period in the diurnal changes of magnetic declination shows that any explanation of those changes which refers them to causes existing in the atmosphere must be very incomplete.[15]

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[15] *Researches*, Art. 2892.

# BOOK XIV.

## CHEMISTRY.

### CHAPTER IX.

#### The Electro-chemical Theory.

AMONG the consequences of the Electro-chemical Theory, must be ranged the various improvements which have been made in the voltaic battery. Daniel introduced between the two metals a partition permeable by chemical action, but such as to allow of two different acid solutions being in contact with the two metals. Mr. Grove's battery, in which the partition is of porous porcelain, and the metals are platinum and amalgamated zinc, is one of the most powerful hitherto known. Another has been constructed by Dr. Callan, in which the negative or conducting plate is a cylinder of cast iron, and the positive element a cylinder of amalgamated zinc placed in a porous cell. This also has great energy.

#### *The Number of Elementary Substances.*

There have not been, I believe, any well-established additions to the list of the simple substances recognized by chemists. Indeed the tendency at present appears to be rather to deny the separate elementary character of some already announced as such substances. Pelopium and Niobium were, as I have said, two of the new metals. But Naumann, in his *Elemente der Mineralogie* (4th ed. 1855), says, in a foot note (page 25): "*Pelopium* is happily again got rid of; for Pelopic Acid and Niobic Acid possess the same Radical.. *Donarium* had a still shorter existence."

In the same way, when Hermann imagined that he had discovered a new simple metallic substance in the mineral Samarskite from Miask. the discovery was disproved by H. Rose (*Pogg. Ann.* B. 73, s. 449).

In general the insulation of the new simple substances, the metallic bases of the earths, and the like,—their separation from their combinations, and the exhibition of them in a metallic form—has been a difficult chemical process, and has rarely been executed on any considerable scale. But in the case of *Aluminium*, the basis of the earth Alumina, the process of its extraction has recently been so much facilitated, that the metal can be produced in abundance. This being the case, it will probably soon be applied to special economical uses, for which it is fitted by possessing special properties.

# BOOK XV

## MINERALOGY.

BY the kindness of W. H. Miller, Esq., Professor of Mineralogy in the University of Cambridge, I am able to add to this part the following notices of books and memoirs.

### 1. *Crystallography.*

*Elemente der Krystallographie, nebst einer tabellarischen Uebersicht der Mineralien nach der Krystallformen*, von Gustav Rose. 2. Auflage. Berlin, 1838. The crystallographic method here adopted is, for the most part, that of Weiss. The method of this work has been followed in

*A System of Crystallography, with its Applications to Mineralogy.* By John Joseph Griffin. Glasgow, 1841. Mr. Griffin has, however, modified the notation of Rose. He has constructed a series of models of crystalline forms.

Frankenheim's *System der Krystalle.* 1842. This work adopts nearly the Mohsian systems of crystallization. It contains Tables of the chemical constitution, inclinations of the axis, and magnitude of the axes of all the crystals of which a description was to be found, including those formed in the laboratory, as well as those usually called minerals; 713 in all.

Fr. Aug. Quenstedt, *Methode der Krystallographie*, 1840, employs a fanciful method of representing a crystal by projecting upon one face of the crystal all the other faces. This invention appears to be more curious than useful.

Dr. Karl Naumann, who is spoken of in Chap ix. of this Book, as the author of the best of the Mixed Systems of Classification, published also *Grundriss der Krystallographie.* Leipzig, 1826. In this and other works he modifies the notation of Mohs in a very advantageous manner.

Professor Dana, in his *System of Mineralogy*, New Haven (U.S.), 1837, follows Naumann for the most part, both in crystallography and in mineral classification. In the latter part of the subject, he has made the attempt, which in all cases is a source of confusion and of failure, to introduce a whole system of new names of the members of his classification.

The geometry of crystallography has been investigated in a very original manner by M. Bravais, in papers published in the Journal of the Ecole Polytechnique, entitled *Mémoires sur les Systèmes formés par des Points*. 1850. *Etudes Crystallographiques*. 1851.

Hermann Kopp (*Einleitung in die Krystallographie*, Braunschweig, 1849) has given the description and measurement of the angles of a large number of laboratory crystals.

Rammelsberg (*Krystallographische Chemie*, Berlin, 1855) has collected an account of the systems, simple forms and angles of all the laboratory crystals of which he could obtain descriptions.

Schabus of Vienna (*Bestimmung der Krystallgestalten* in *Chemischen Laboratorien erzeugten Producte*, Wien, 1855; a successful Prize Essay) has given a description, accompanied by measurements, of 90 crystalline species from his own observations.

To these attempts made in other countries to simplify and improve crystallography, I may add a remarkable Essay very recently made here by Mr. Brooke, and suggested to him by his exact and familiar knowledge of Mineralogy. It is to this effect. All the crystalline forms of any given mineral species are derived from the *primitive form* of that species; and the degree of symmetry, and the *parameters*, of this form determine the angles of all derivative forms. But how is this primitive form selected and its parameters determined? The selection of the kind of the primitive form depends upon the *degree of symmetry* which appears in all the derivative forms; according to which they belong to the *rhombohedral*, *prismatic*, *square pyramidal*, or some other *system:* and this determination is commonly clear. But the parameters, or the angles, of the primitive form, are commonly determined by the *cleavage* of the mineral. Is this a sufficient and necessary ground of such determination? May not a simplification be effected, in some cases, by taking some other parameters? by taking a primitive form which belongs to the proper system, but which has some other angles than those given by cleavage? Mr. Brooke has tried whether, for instance, crystals of the rhombohedral system may not be referred with advantage to primitive rhombohedrons which have, in all the species, nearly the same angles. The advantage to be obtained by such a change would be the simplification of the laws of derivation in the derivative forms: and therefore we have to ask, whether the indices of derivation are smaller numbers in this way or with the hitherto accepted fundamental angles. It appears to me, from the examples given, that the advantage of simplicity in the indices is on the side of the old system: but whether this be so or not, it was a great benefit to crystallography to have the two methods compared. Mr. Brooke's Essay is a Memoir presented to the Royal Society in 1856.

## 2. *Optical Properties of Minerals.*

The *Handbuch der Optik*, von F. W. G. Radicke, Berlin, 1839, contains a chapter on the optical properties of crystals. The author's chief authority is Sir D. Brewster, as might be expected.

M. Haidinger has devoted much attention to experiments on the *pleochroism* of minerals. He has invented an instrument which makes the dichroism of minerals more evident by exhibiting the two colors side by side.

The pleochroism of minerals, and especially the remarkable clouds that in the cases of Iolite, Andalusite, Augite, Epidote, and Axinite, border the positions of either optical axis, have been most successfully imitated by M. de Senarmont by means of artificial crystallizations. (*Ann. de Chim.* 3 *Ser.* xli. p. 319.)

M. Pasteur has found that Racemic Acid consists of two different acids, having the same density and composition. The salts of these acids, with bases of Ammonia and of Potassa, are hemihedral, the hemihedral faces which occur in the one being wanting in the other. The acids of these different crystals have circular polarization of opposite kinds. (*Ann. de Chim.* 3 *Ser.* xxviii. 56, 99.) This discovery was marked by the assignation of the Rumford Medal to M. Pasteur in 1856.

M. Marbach has discovered that crystals of chlorate of soda, which apparently belongs to the cubic or tessular system, exhibit hemihedral faces of a peculiar character; and that the crystals have circular polarization of opposite kinds in accordance with the differences of the plagihedral faces. (*Poggendorf's Annalen*, xci. 482.)

M. Seybolt of Vienna has found a means of detecting plagihedral faces in quartz crystals which do not reveal them externally. (*Akad. d. Wissenschaft zu Wien*, B. xv. s. 59.)

### 3. *Classification of Minerals.*

In the *Philosophy of the Inductive Sciences*, B. VIII. C. iii., I have treated of the Application of the Natural-history Method of Classification to Mineralogy, and have spoken of the Systems of this kind which have been proposed. I have there especially discussed the system proposed in the treatise of M. Necker, *Le Règne Mineral ramené aux Méthodes d'Histoire Naturelle* (Paris, 1835). More recently have been published M. Beudant's *Cours élémentaire d'Histoire Naturelle, Minéralogie* (Paris, 1841); and M. A. Dufresnoy's *Traité de Minéralogie* (Paris, 1845). Both these works are so far governed by mere chemical views that they lapse into the inconveniences and defects which are avoided in the best systems of German mineralogists.

The last mineral system of Berzelius has been developed by M. Rammelsberg (Nürnberg, 1847). It is in principle such as we have described it in the history.

M. Nordenskiold's system (3rd Ed. 1849,) has been criticised by G. Rose, who observes that it removes the defects of the system of Berzelius only in part. He himself proposes what he calls a "Krystallo-Chemisches System," in which the crystalline form determines the genus and the chemical composition the species. His classes are—

1. Simple Substances.
2. Combinations of Sulphur, Selenium, Titanium, Arsenic, Antimony.
3. Chlorides, Fluorides, Bromides, Iodides.
4. Combinations with Oxygen.

We have already said that for us, all chemical compounds are *minerals*, in so far that they are included in our classifications. The propriety of this mode of dealing with the subject is confirmed by our finding that there is really no tenable distinction between native minerals and the products of the laboratory. A great number of eminent chemists have been employed in producing, by artificial means, crystals which had before been known only as native products.

# BOOK XVI

## CLASSIFICATORY SCIENCES.

### Botany.

FOR the purpose of giving to my reader some indication of the present tendency of Botanical Science, I conceive that I cannot do better than direct his attention to the reflections, procedure, and reasonings which have been suggested by the most recent extensions of man's knowledge of the vegetable world. And as a specimen of these, I may take the labors of Dr. Joseph Hooker, on the Flora of the Antarctic Regions,[1] and especially of New Zealand. Dr. Hooker was the Botanist to an expedition commanded by Sir James Ross, sent out mainly for the purpose of investigating the phenomena of Terrestrial Magnetism near the South Pole; but directed also to the improvement of Natural History. The extension of botanical descriptions and classifications to a large mass of new objects necessarily suggests wider views of the value of classes (genera, species, &c.,) and the conclusions to be drawn from their constancy or inconstancy. A few of Dr. Hooker's remarks may show the nature of the views taken under such circumstances.

I may notice, in the first place, (since this work is intended for general rather than for scientific readers,) Dr. Hooker's testimony to the value of a technical descriptive language for a classificatory science—a Terminology, as it is called. He says, "It is impossible to write Botanical descriptions which a person ignorant of Botany can understand, although it is supposed by many unacquainted with science that this can and should be done." And hence, he says, the state of botanical science demands Latin descriptions of the plants; and this is a lesson which he especially urges upon the Colonists who study the indigenous plants.

[1] *The Botany of the Antarctic Voyage of H. M. Discovery Ships Erebus and Terror, in the years* 1839–40. Published 1847. *Flora Novæ Zelandiæ.* 1853.

Dr. Hooker's remarks on the limits of species, their dispersion and variation, are striking and instructive. He is of opinion that species vary more, and are more widely diffused, than is usually supposed. Hence he conceives that the number of species has been needlessly and erroneously multiplied, by distinguishing the specimens which occur in different places, and vary in unessential features. He says that though, according to the lowest estimate of compilers, 100,000 is the commonly received number of known plants, he thinks that half that number is much nearer the truth. "This," he says, "may be well conceived, when it is notorious that nineteen species have been made of the Common Potatoe, and many more of *Solanum nigrum* alone. *Pteris aquilina* has given rise to numerous book species; *Vernonia cinerea* of India to fifteen at least. . . . . . . Many more plants are common to most countries than is supposed; I have found 60 New Zealand flowering plants and 9 Ferns to be European ones, besides inhabiting numerous intermediate countries. . . . . . So long ago as 1814, Mr. Brown drew attention to the importance of such considerations, and gave a list of 150 European plants common to Australia."

As an example of the extent to which unessential differences may go, he says (p. xvii.,) "The few remaining native Cedars of Lebanon may be abnormal states of the tree which was once spread over the whole of the Lebanon; for there are now growing in England varieties of it which have no existence in a wild state. Some of them closely resemble the Cedars of Atlas and of the Himalayas (*Deodar;*) and the absence of any valid botanical differences tends to prove that all, though generally supposed to be different species, are one."

Still the great majority of the species of plants in those Southern regions are peculiar. "There are upwards of 100 genera, subgenera, or other well marked groups of plants, entirely or nearly confined to New Zealand, Australia, and extra-tropical South America. They are represented by one or more species in two or more of those countries, and thus effect a botanical relationship or affinity between them all which every botanist appreciates."

In reference to the History of Botany, I have received corrections and remarks from Dr. Hooker, with which I am allowed to enrich my pages.

"P. 359. Note [3]. *Nelumbium speciosum*, the Lotus of India. The *Nelumbium* does not float, but raises both leaf and flower several feet above the water: the *Nymphæa Lotus* has floating leaves. Both enter largely into the symbolism of the Hindoos, and are often confounded

"P. 362. Note [5]. For *Arachnis* read *Arachis*. The *Arachidna* of Theophrastus cannot, however, be the *Arachis* or ground-nut.

"Pp. 388 and 394. For *Harlecamp* read *Hartecamp*.

"P. 394. For *Kerlen* read *Kalm*.

"P. 394. For *Asbech* read *Osbeck*.

"P. 386. *John Ray*. Ray was further the author of the present Natural System in its most comprehensive sense. He first divided plants into Flowerless and Flowering; and the latter into Monocotyledonous and Dicotyledonous:—'Floriferas dividemus in DICOTYLEDONES, quarum semina sata binis foliis, seminalibus dictis, quæ cotyledonorum usum præstant, e terra exeunt, vel in binos saltem lobos dividuntur, quamvis eos supra terram foliorum specie non efferant; et MONOCOTYLEDONES, quæ nec folia bina seminalia efferunt nec lobos binos condunt. Hæc divisio ad arbores etiam extendi potest; siquidem Palmæ et congeneres hoc respectu eodem modo a reliquis arboribus differunt quo Monocotyledones a reliquis herbis.'

"P. 408. *Endogenous and Exogenous Growth*. The exact course of the wood fibres which traverse the stems of both Monocotyledonous and Dicotyledonous plants has been only lately discovered. In the Monocotyledons, those fibres are collected in bundles, which follow a very peculiar course:—from the base of each leaf they may be followed downwards and inwards, towards the axis of the trunk, when they form an arch with the convexity to the centre; and curving outwards again reach the circumference, where they are lost amongst the previously deposited fibres. The intrusion of the bases of these bundles amongst those already deposited, causes the circumference of the stem to be harder than the centre; and as all these arcs have a short course (their chords being nearly equal), the trunk does not increase in girth, and grows at the apex only. The wood-bundles are here definite. In the Dicotyledonous trunks, the layers of wood run in parallel courses from the base to the top of the trunk, each externally to that last formed, and the trunk increases both in height and girth; the wood-bundles are here indefinite.

"With regard to the Cotyledons, though it is often difficult to distinguish a Monocotyledonous Embryo from a Dicotyledonous, they may always be discriminated when germinating. The Cotyledons, when two or more, and primordial leaves (when no Cotyledons are visible) of a Monocotyledon, are alternate; those of a Dicotyledon are opposite.

"A further physiological distinction between Monocotyledons and

Dicotyledons is observed in germination, when the Dicotyledonous radicle elongates and forms the root of the young plant; the Monocotyledonous radicle does not elongate, but pushes out rootlets from itself at once. Hence the not very good terms, *exorhizal* for Dicotyledonous, and *endorhizal* for Monocotyledonous.

"The highest physiological generalization in the vegetable kingdom is between *Phænogama* and *Cryptogama*. In the former, fertilization is effected by a pollen-tube touching the nucleus of an ovule; in Cryptogams, the same process is effected by the contact of a sperm-cell, usually ciliated (*antherozoid*), upon another kind of cell called a germ-cell. In Phænogams, further, the organs of fructification are all modified leaves; those of Cryptogams are not homologous." (J. D. H.)

## Zoology.

I have exemplified the considerations which govern zoological classification by quoting the reflexions which Cuvier gives us, as having led him to his own classification of Fishes. Since the varieties of Quadrupeds, or *Mammals* (omitting whales, &c.), are more familiar to the common reader than those of Fishes, I may notice some of the steps in their classification; the more so as some curious questions have recently arisen thereupon.

Linnæus first divides Mammals into two groups, as they have Claws, or Hoofs (*unguiculata*, *ungulata*.) But he then again divides them into six orders (omitting whales, &c.), according to their number of *incisor*, *laniary*, and *molar* teeth; namely:—

*Primates.* (Man, Monkey, &c.)

*Bruta.* (Rhinoceros, Elephant, &c.)

*Feræ.* (Dog, Cat, Bear, Mole, &c.)

*Glires.* (Mouse, Squirrel, Hare, &c.)

*Pecora.* (Camel, Giraffe, Stag, Goat, Sheep, Ox, &c.)

*Belluæ.* (Horse, Hippopotamus, Tapir, Sow, &c.)

In the place of these, Cuvier, as I have stated in the *Philosophy* (*On the Language of Sciences*, Aphorism xvi.), introduced the following orders: *Bimanes*, *Quadrumanes*, *Carnassiers*, *Rongeurs*, *Edentés*, *Pachyderms*, *Ruminans*. Of these, the *Carnassiers* correspond to the *Feræ* of Linnæus; the *Rongeurs* to his *Glires*; the *Edentés* are a new order, taking the Sloths, Ant-eaters, &c., from the *Bruta* of Linnæus, the Megatherium from extinct animals, and the Ornithorhynchus, &c., from the new animals of Australia; the *Ruminans* agree with the

*Pecora*; the *Pachyderms* include some of the *Bruta* and the *Belluæ*, comprehending also extinct animals, as *Anoplotherium* and *Palæotherium*.

But the two orders of Hoofed Animals, the Pachyderms and the Ruminants, form a group which is held by Mr. Owen to admit of a better separation, on the ground of a character already pointed out by Cuvier; namely, as to whether they are *two-toed* or *three-toed*. According to this view, the Horse is connected with the Tapir, the Palæotherium, and the Rhinoceros, not only by his teeth, but by his feet, for he has really three digits. And Cuvier notices that in the two-toed or even-toed Pachyderms, the astragalus bone has its face divided into two equal parts by a ridge; while in the uneven-toed pachyderms it has a narrow cuboid face. Mr. Owen has adopted this division of Pachyderms and Ruminants, giving the names *artiodactyla* and *perissodactyla* to the two groups; the former including the Ox, Hog, Peccary, Hippopotamus, &c.; the latter comprehending the Horse, Tapir, Rhinoceros, Hyrax, &c. And thus the *Ruminants* take their place as a subordinate group of the great natural even-toed Division of the Hoofed Section of Mammals; and the Horse is widely separated from them, inasmuch as he belongs to the odd-toed division.[2]

As we have seen, these modern classifications are so constructed as to include extinct as well as living species of animals; and indeed the species which have been discovered in a fossil state have tended to fill up the gaps in the series of zoological forms which had marred the systems of modern zoologists. This has been the case with the division of which we are speaking.

Mr. Owen had established two genera of extinct Herbivorous Animals, on the strength of fossil remains brought from South America: —*Toxodon*, and *Nesodon*. In a recent communication to the Royal Society[3] he has considered the bearing of these genera upon the divisions of odd-toed and even-toed animals. He had already been led to the opinion that the three sections, *Proboscidea*, *Perissodactyla*, and *Artiodactyla*, formed a natural division of Ungulata; and he is now led to think that this division implies another group, "a distinct division of the *Ungulata*, of equal value, if not with the *Perissodactyla* and *Artiodactyla*, at least with the *Proboscidea*." This group he proposes to call *Toxodonta*.

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[2] Owen, *Odontography*.

[3] *Phil. Trans.*, 1853.

# BOOK XVII.

## PHYSIOLOGY AND COMPARATIVE ANATOMY

### Vegetable Morphology.

#### *Morphology in Linnæus.*

I HAVE stated that Linnæus had some views on this subject. Dr. Hooker conceives these views to be more complete and correct than is generally allowed, though unhappily clothed in metaphorical language and mixed with speculative matter. By his permission I insert some remarks which I have received from him.

The fundamental passage on this subject is in the *Systema Naturæ;* in the Introduction to which work the following passage occurs:—

"Prolepsis (Anticipation) exhibits the mystery of the metamorphosis of plants, by which the herb, which is the *larva* or imperfect condition, is changed into the declared fructification: for the plant is capable of producing either a leafy herb or a fructification. . . . . .

"When a tree produces a flower, nature anticipates the produce of five years where these come out all at once; forming of the bud-leaves of the next year *bracts;* of those of the following year, the *calyx;* of the following, the *corolla;* of the next, the *stamina;* of the subsequent, the *pistils*, filled with the granulated marrow of the seed, the terminus of the life of a vegetable."

Dr. Hooker says, "I derive my idea of his having a better knowledge of the subject than most Botanists admit, not only from the *Prolepsis*, but from his paper called *Reformatio Botanices* (*Amœn. Acad.* vol. vi.); a remarkable work, in respect of his candor in speaking of his predecessors' labors, and the sagacity he shows in indicating researches to be undertaken or completed. Amongst the latter is, V. 'Prolepsis plantarum, ulterius extendenda per earum metamorphoses.' The last word occurs rarely in his *Prolepsis;* but when it does it seems to me that he uses it as indicating a normal change and not an accidental one.

"In the *Prolepsis* the speculative matter, which Linnæus himself carefully distinguishes as such, must be separated from the rest, and this may I think be done in most of the sections. He starts with explaining clearly and well the origin and position of buds, and their constant presence, whether developed or not, in the axil of the leaf: adding abundance of acute observations and experiments to prove his statements. The leaf he declares to be the first effort of the plant in spring: he proceeds to show, successively, that bracts, calyx, corolla, stamens, and pistil are each of them metamorphosed leaves, in every case giving MANY EXAMPLES, both from monsters and from characters presented by those organs in their normal condition.

"The (to me) obscure and critical part of the *Prolepsis* was that relating to the change of the style of *Carduus* into two leaves. Mr. Brown has explained this. He says it was a puzzle to him, till he went to Upsala and consulted Fries and Wahlenberg, who informed him that such monstrous *Cardui* grew in the neighborhood, and procured him some. Considering how minute and masked the organs of *Compositæ* are, it shows no little skill in Linnæus, and a very clear view of the whole matter, to have traced the metamorphosis of all their floral organs into leaves, except their stamens, of which he says, 'Sexti anni folia e staminibus me non in compositis vidisse fateor, sed illorum loco folia pistillacea, quæ in compositis aut plenis sunt frequentissima.' I must say that nothing could well be clearer to my mind than the full and accurate appreciation which Linnæus shows of the whole series of phenomena, and their *rationale.* He over and over again asserts that these organs are leaves, every one of them,—I do not understand him to say that the prolepsis is an accidental change of leaves into bracts, of bracts into calyx, and so forth. Even were the language more obscure, much might be inferred from the wide range and accuracy of the observations he details so scientifically. It is inconceivable that a man should have traced the sequence of the phenomena under so many varied aspects, and shown such skill, knowledge, ingenuity, and accuracy in his methods of observing and describing, and yet missed the *rationale* of the whole. Eliminate the speculative parts, and there is not a single error of observation or judgment; whilst his history of the developement of buds, leaves, and floral organs, and of various other obscure matters of equal interest and importance, are of a very high order of merit, are, in fact, for the time profound.

"There is nothing in all this that detracts from the merit of Goethe's re-discovery. With Goethe it was, I think, a deductive process,—with Linnæus an inductive. Analyse Linnæus's observations and method, and I think it will prove a good example of inductive reasoning.

"P. 473. Perhaps Professor Auguste St. Hilaire of Montpellier should share with De Candolle the honor of contributing largely to establish the metamorphic doctrine;—their labors were cotemporaneous.

"P. 390. Linnæus pointed out that the pappus was calyx: 'Et *pappum* gigni ex quarti anni foliis, in jam nominatis Carduis.'—*Prol. Plant.* 338." (*J. D. H.*)

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## CHAPTER VII.

### Animal Morphology.

THE subject of Animal Morphology has recently been expanded into a form strikingly comprehensive and systematic by Mr. Owen; and supplied by him with a copious and carefully-chosen language; which in his hands facilitates vastly the comparison and appreciation of the previous labors of physiologists, and opens the way to new truths and philosophical generalizations. Though the steps which have been made had been prepared by previous anatomists, I will borrow my view of them mainly from him; with the less scruple, inasmuch as he has brought into full view the labors of his predecessors.

I have stated in the History that the skeletons of all vertebrate animals are conceived to be reducible to a single Type, and the skull reducible to a series of vertebræ. But inasmuch as this reduction includes not only a detailed correspondence of the bones of man with those of beasts, but also with those of birds, fishes, and reptiles, it may easily be conceived that the similarities and connexions are of a various and often remote kind. The views of such relations, held by previous Comparative Anatomists, have led to the designations of the bones of animals which have been employed in anatomical descriptions; and these designations having been framed and adopted by anatomists looking at the subject from different sides, and having different views of analogies and relations, have been very various and unstable; besides being often of cumbrous length and inconvenient form.

The corresponding parts in different animals are called *homologues*.

a term first applied to anatomy by the philosophers of Germany; and this term Mr. Owen adopts, to the exclusion of terms more loosely denoting identity or similarity. And the Homology of the various bones of vertebrates having been in a great degree determined by the labors of previous anatomists, Mr. Owen has proposed names for each of the bones: the condition of such names being, that the homologues in all vertebrates shall be called by the same name, and that these names shall be founded upon the terms and phrases in which the great anatomists of the 16th, 17th, and 18th centuries expressed the results of their researches respecting the human skeleton. These names, thus selected, so far as concerned the bones of the Head of Fishes, one of the most difficult cases of this Special Homology, he published in a Table,[1] in which they were compared, in parallel columns, with the names or phrases used for the like purpose by Cuvier, Agassiz, Geoffroy, Hallman, Sœmmering, Meckel, and Wagner. As an example of the considerations by which this selection of names was determined, I may quote what he says with regard to one of these bones of the skull.

"With regard to the 'squamosal' (*squamosum.* Lat. pars squamosa ossis temporis.—Sœmmering), it might be asked why the term 'temporal' might not be retained for this bone. I reply, because that term has long been, and is now universally, understood in human anatomy to signify a peculiarly anthropotomical coalesced congeries of bones, which includes the 'squamosal' together with the 'petrosal,' the 'tympanic,' the 'mastoid,' and the 'stylohyal.' It seems preferable, therefore, to restrict the signification of the term 'temporal' to the whole (in Man) of which the 'squamosal' is a part. To this part Cuvier has unfortunately applied the term 'temporal' in one class, and 'jugal' in another; and he has also transferred the term 'temporal' to a third equally distinct bone in fishes; while to increase the confusion M. Agassiz has shifted the name to a fourth different bone in the skull of fishes. Whatever, therefore, may be the value assigned to the arguments which will be presently set forth, as to the special homologies of the 'pars squamosa ossis temporis,' I have felt compelled to express the conclusion by a definite term, and in the present instance, have selected that which recalls the best accepted anthropomorphical designation of the part; although 'squamosal' must be understood and applied in an arbitrary sense; and not as descriptive of a scale-like

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[1] *Lectures on Vertebrates.* 1846, p. 158. And *On the Archetype and Homologies of the Vertebrate Skeleton.* 1848, p. 172.

form; which in reference to the bone so called, is rather its exceptional than normal figure in the vertebrate series."

The principles which Mr. Owen here adopts in the selection of names for the parts of the skeleton are wise and temperate. They agree with the aphorisms concerning the language of science which I published in the *Philosophy of the Inductive Sciences;* and Mr. Owen does me the great honor of quoting with approval some of those Aphorisms. I may perhaps take the liberty of remarking that the system of terms which he has constructed, may, according to our principles, be called rather a *Terminology* then a *Nomenclature:* that is, they are analogous more nearly to the *terms* by which botanists describe the parts and organs of plants, than to the *names* by which they denote genera and species. As we have seen in the History, plants as well as animals are subject to morphological laws; and the names which are given to organs in consequence of those laws are a part of the Terminology of the science. Nor is this distinction between Terminology and Nomenclature without its use; for the rules of prudence and propriety in the selection of words in the two cases are different. The Nomenclature of genera and species may be arbitrary and casual, as is the case to a great extent in Botany and in Zoology, especially of fossil remains; names being given, for instance, simply as marks of honor to individuals. But in a Terminology, such a mode of derivation is not admissible: some significant analogy or idea must be adopted, at least as the origin of the name, though not necessarily true in all its applications, as we have seen in the case of the "squamosal" just quoted. This difference in the rules respecting two classes of scientific words is stated in the *Aphorisms* xiii. and xiv. *concerning the Language of Science.*

Such a Terminology of the bones of the skeletons of all vertebrates as Mr. Owen has thus propounded, cannot be otherwise than an immense acquisition to science, and a means of ascending from what we know already to wider truths and new morphological doctrines.

With regard to one of these doctrines, the resolution of the human head into vertebræ, Mr. Owen now regards it as a great truth, and replies to the objections of Cuvier and M. Agassiz, in detail.[2] He gives a Table in which the Bones of the Head are resolved into four vertebræ, which he terms the Occipital, Parietal, Frontal, and Nasal Vertebra, respectively. These four vertebræ agree in general with what Oken called the Ear-vertebra, the Jaw-vertebra, the Eye-vertebra, and

[2] *Archetype and Homologies of the Vertebrate Skeleton.* 1848, p. 141.

the Nose-vertebra, in his work *On the Signification of the Bones of the Skull*, published in 1807: and in various degrees, with similar views promulgated by Spix (1815), Bojanus (1818), Geoffroy (1824), Carus 1828. And I believe that these views, bold and fanciful as they at first appeared, have now been accepted by most of the principal physiologists of our time.

But another aspect of this generalization has been propounded among physiologists; and has, like the others, been extended, systematized, and provided with a convenient language by Mr. Owen. Since animal skeletons are thus made up of vertebræ, and their parts are to be understood as developements of the parts of vertebræ, Geoffroy (1822), Carus (1828), Müller (1834), Cuvier (1835), had employed certain terms while speaking of such developements; Mr. Owen in the *Geological Transactions* in 1838, while discussing the osteology of certain fossil Saurians, used terms of this kind, which are more systematic than those of his predecessors, and to which he has given currency by the quantity of valuable knowledge and thought which he has embodied in them.

According to his Terminology,[3] a vertebra, in its typical completeness, consists of a central part or *centrum;* at the back of this, two plates (the *neural apophyses*) and a third outward projecting piece (the *neural spine*), which three, with the centrum, form a canal for the spinal marrow; at the front of the centrum two other plates (the *hœmal apophyses*) and a projecting piece, forming a canal for a vascular trunk. Further lateral elements (*pleuro-apophyses*) and other projections, are in a certain sense dependent on these principal bones; besides which the vertebra may support *diverging appendages.* These parts of the vertebra are fixed together, so that a vertebra is by some anatomists described as a single bone; but the parts now mentioned are usually developed from distinct and independent centres, and are therefore called by Mr. Owen "autogenous" elements.

The *General* Homology of the vertebral skeleton is the reference of all the parts of a skeleton to their true types in a series of vertebræ: and thus, as *special* homology refers all the parts of skeletons to a given type of skeleton, say that of Man, *general* homology refers all the parts of every skeleton, say that of Man, to the parts of a series of Vertebræ. And thus as Oken propounded his views of the Head as a resolution of the Problem of *the Signification of the Bones of the Head,*

[3] *Archetype and Homologies of the Vertebrate Skeleton.* 1848, p. 81.

so have we in like manner, for the purposes of General Homology, to solve the Problem of *the Signification of Limbs*. The whole of the animal being a string of vertebræ, what are arms and legs, hands and paws, claws and fingers, wings and fins, and the like? This inquiry Mr. Owen has pursued as a necessary part of his inquiries. In giving a public lecture upon the subject in 1849,[4] he conceived that the phrase which I have just employed would not be clearly apprehended by an English Audience, and entitled his Discourse "On the *Nature* of Limbs:" and in this discourse he explained the modifications by which the various kinds of limbs are derived from their rudiments in an archetypal skeleton, that is, a mere series of vertebræ without head, arms, legs, wings, or fins.

*Final Causes.*

It has been mentioned in the History that in the discussions which took place concerning the Unity of Plan of animal structure, this principle was in some measure put in opposition to the principle of Final Causes: Morphology was opposed to Teleology. It is natural to ask whether the recent study of Morphology has affected this antithesis.

If there be advocates of Final Causes in Physiology who would push their doctrines so far as to assert that every feature and every relation in the structure of animals have a purpose discoverable by man, such reasoners are liable to be perpetually thwarted and embarrassed by the progress of anatomical knowledge; for this progress often shows that an arrangement which had been explained and admired with reference to some purpose, exists also in cases where the purpose disappears; and again, that what had been noted as a special teleological arrangement is the result of a general morphological law. Thus to take an example given by Mr. Owen: that the ossification of the head originates in several centres, and thus in its early stages admits of compression, has been pointed out as a provision to facilitate the birth of viviparous animals; but our view of this provision is disturbed, when we find that the same mode of the formation of the bony framework takes place in animals which are born from an egg. And the number of points from which ossification begins, depends in a wider sense on the general homology of the animal frame, according to which each part is composed of a certain number of autogenous vertebral elements. In this

[4] *On the Nature of Limbs*, a discourse delivered at a Meeting of the Royal Institution. 1849.

way, the admission of a new view as to Unity of Plan will almost necessarily displace or modify some of the old views respecting Final Causes.

But though the view of Final Causes is displaced, it is not obliterated; and especially if the advocate of Purpose is also ready to admit visible correspondences which have not a discoverable object, as well as contrivances which have. And in truth, how is it possible for the student of anatomy to shut his eyes to either of these two evident aspects of nature? The arm and hand of man are made for taking and holding, the wing of the sparrow is made for flying; and each is adapted to its end with subtle and manifest contrivance. There is plainly Design. But the arm of man and the wing of the sparrow correspond to each other in the most exact manner, bone for bone. Where is the Use or the Purpose of this correspondence? If it be said that there may be a purpose though we do not see it, that is granted. But Final Causes *for us* are contrivances of which *we see* the end; and nothing is added to the evidence of Design by the perception of a unity of plan which in no way tends to promote the design.

It may be said that the design appears in the modification of the plan in special ways for special purposes;—that the vertebral plan of an animal being given, the fore limbs are modified in Man and in Sparrow, as the nature and life of each require. And this is truly said; and is indeed the truth which we are endeavoring to bring into view: —that there are in such speculations, two elements; one given, the other to be worked out from our examination of the case; the *datum* and the *problem;* the homology and the teleology.

Mr. Owen, who has done so much for the former of these portions of our knowledge, has also been constantly at the same time contributing to the other. While he has been aiding our advances towards the Unity of Nature, he has been ever alive to the perception of an Intelligence which pervades Nature. While his morphological doctrines have moved the point of view from which he sees Design, they have never obscured his view of it, but, on the contrary, have led him to present it to his readers in new and striking aspects. Thus he has pointed out the final purposes in the different centres of ossification of the long bones of the limbs of mammals, and shown how and why they differ in this respect from reptiles (*Archetype*, p. 104). And in this way he has been able to point out the insufficiency of the rule laid down both by Geoffroy St. Hilaire and Cuvier, for ascertaining the true number of bones in each species.

Final Causes, or Evidences of Design, appear, as we have said, not merely as contrivances for evident purposes, but as modifications of a given general Plan for special given ends. If the general Plan be discovered after the contrivance has been noticed, the discovery may at first seem to obscure our perception of Purpose; but it will soon be found that it merely transfers us to a higher point of view. The adaptation of the Means to the End remains, though the Means are parts of a more general scheme than we were aware of. No generalization of the Means can or ought permanently to shake our conviction of the End; because we must needs suppose that the Intelligence which contemplates the End is an intelligence which can see at a glance along a vista of Means, however long and complex. And on the other hand, no special contrivance, however clear be its arrangement, can be unconnected with the general correspondences and harmonies by which all parts of nature are pervaded and bound together. And thus no luminous teleological point can be extinguished by homology; nor, on the other hand, can it be detached from the general expanse of homological light.

The reference to Final Causes is sometimes spoken of as unphilosophical, in consequence of Francis Bacon's comparison of Final Causes in Physics to Vestal Virgins devoted to God, and barren. I have repeatedly shown that, in Physiology, almost all the great discoveries which have been made, have been made by the assumption of a purpose in animal structures. With reference to Bacon's simile, I have elsewhere said that if he had had occasion to develope its bearings, full of latent meaning as his similes so often are, he would probably have said that to those Final Causes barrenness was no reproach, seeing they ought to be not the Mothers but the Daughters of our Natural Sciences; and that they were barren, not by imperfection of their nature, but in order that they might be kept pure and undefiled, and so fit ministers in the temple of God. I might add that in Physiology, if they are not Mothers, they are admirable Nurses; skilful and sagacious in perceiving the signs of pregnancy, and helpful in bringing the Infant Truth into the light of day.

There is another aspect of the doctrine of the Archetypal Unity of Composition of Animals, by which it points to an Intelligence from which the frame of nature proceeds; namely this:—that the Archetype of the Animal Structure being of the nature of an *Idea*, implies a mind in which this Idea existed; and that thus Homology itself points the way to the Divine Mind. But while we acknowledge the full value of this view of theological bearing of physiology, we may venture to say that it is a view quite different from that which is described by speaking of "Final Causes," and one much more difficult to present in a lucid manner to ordinary minds.

# BOOK XVIII.

## GEOLOGY.

WITH regard to Geology, as a Palætiological Science, I do not know that any new light of an important kind has been thrown upon the general doctrines of the science. Surveys and examinations of special phenomena and special districts have been carried on with activity and intelligence; and the animals of which the remains people the strata, have been reconstructed by the skill and knowledge of zoologists:—of such reconstructions we have, for instance, a fine assemblage in the publications of the Palæontological Society. But the great questions of the manner of the creation and succession of animal and vegetable species upon the earth remain, I think, at the point at which they were when I published the last edition of the History.

I may notice the views propounded by some chemists of certain bearings of Mineralogy upon Geology. As we have, in mineral masses, organic remains of former organized beings, so have we crystalline remains of former crystals; namely, what are commonly called *pseudomorphoses*—the shape of one crystal in the substance of another. M. G. Bischoff[1] considers the study of pseudomorphs as important in geology, and as frequently the only means of tracing processes which have taken place and are still going on in the mineral kingdom.

I may notice also Professor Breithaupt's researches on the order of succession of different minerals, by observing the mode in which they occur and the order in which different crystals have been deposited, promise to be of great use in following out the geological changes which the crust of the globe has undergone. (*Die Paragenesis der Mineralien.* Freiberg. 1849.)

In conjunction with these may be taken M. de Senarmont's experiments on the formation of minerals in veins; and besides Bischoff's

[1] *Chemical and Physical Geology.*

*Chemical Geology*, Sartorius von Walterhausen's Observations on the occurrence of minerals in Amygdaloid.

As a recent example of speculations concerning Botanical Palætiology, I may give Dr. Hooker's views of the probable history of the Flora of the Pacific.

In speculating upon this question, Dr. Hooker is led to the discussion of geological doctrines concerning the former continuity of tracts of land which are now separate, the elevation of low lands into mountain ranges in the course of ages, and the like. We have already seen, in the speculations of the late lamented Edward Forbes, (see Book xviii. chap. vi. of this History,) an example of a hypothesis propounded to account for the existing Flora of England: a hypothesis, namely, of a former Connexion of the West of the British Isles with Portugal, of the Alps of Scotland with those of Scandinavia, and of the plains of East Anglia with those of Holland. In like manner Dr. Hooker says (p. xxi.) that he was led to speculate on the possibility of the plants of the Southern Ocean being the remains of a Flora that had once spread over a larger and more continuous tract of land than now exists in the ocean; and that the peculiar Antarctic genera and species may be the vestiges of a Flora characterized by the predominance of plants which are now scattered throughout the Southern islands. He conceives this hypothesis to be greatly supported by the observations and reasonings of Mr. Darwin, tending to show that such risings and sinkings are in active progress over large portions of the continents and islands of the Southern hemisphere: and by the speculations of Sir C. Lyell respecting the influence of climate on the migrations of plants and animals, and the influence of geological changes upon climate.

In Zoology I may notice (following Mr. Owen)[2] recent discoveries of the remains of the animals which come nearest to man in their structure. At the time of Cuvier's death, in 1832, no evidence had been obtained of fossil Quadrumana; and he supposed that these, as well as Bimana, were of very recent introduction. Soon after, in the oldest (eocene) tertiary deposits of Suffolk, remains were found proving the existence of a monkey of the genus Macacus. In the Himalayan tertiaries were found petrified bones of a Semnopithecus; in Brazil, remains of an extinct platyrhine monkey of great size; and lastly, in the middle tertiary series of the South of France, was discovered a fragment of the jaw of the long-armed ape (*Hylobates*). But no fossil human

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[2] *Brit. Asso.* 1854, p. 112.

remains have been discovered in the regularly deposited layers of any of the divisions (not even the pliocene) of the tertiary series; and thus we have evidence that the placing of man on the earth was the last and peculiar act of Creation.

THE END.