MEDICO-PHYSICAL WORKS

OF

JOHN MAYOW, LL.D., M.D.

(1674)

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Johannes Mayow

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BEING A TRANSLATION OF
TRACTATUS QUINQUE MEDICO-PHYSICI

BY

JOHN MAYOW, LL.D., M.D.

(1674)

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PREFACE

"JOHN MAYOW, descended from a genteel family of his name, living at Bree, in Cornwall, was born in the parish of St Dunstan's in the West, in Fleet Street, London," in May 1643. His father's name was William and his mother's Elizabeth. He was received as a commoner of Wadham College, Oxford, on 3rd April 1658, and admitted a scholar on 23rd September 1659. Upon the recommendation of Henry Coventry, one of the Secretaries of State and a former fellow of the college, he was elected to a fellowship at All Souls' College on 3rd November 1660. He graduated B.C.L. on 30th May 1665 and D.C.L. on 5th July 1670. He also "studied physic, and became noted for his practice therein, especially in the summer time, in the city of Bath." He died "in an apothecary's house, bearing the sign of the Anchor, in York Street, near Covent Garden, within the liberty of Westminster . . . in the month of September 1679, and was buried in the Church of St Paul, Covent Garden."

These particulars, derived from Wood's *Athenæ Oxonienses*, 1722, and the notice by Hartog in the *Dictionary of National Biography*, convey practically all we have been able to find as to the personal history of Mayow. On 30th November 1678 he was elected a Fellow of the Royal Society of London, having been proposed by Hooke.

His philosophical writings consist of five treatises, written in Latin. Two of these—

De respiratione; and
De rachitide

—were first published at Oxford in 1668. A revised edition of these two, together with the other three—

De sal-nitro et spiritu nitro-aereo;
De respiratione foetus in utero et ovo; and
De motu musculari et spiritibus animalibus

—was published at Oxford in 1674.

Mayow was thus twenty-five years old when he published his tracts on respiration and on rickets, and he died at the early age of thirty-six.

Mayow's works were not much noticed in his own time, and speedily fell into almost total oblivion, Hales, in his *Vegetable Staticks* (London, 1727), being the only author who refers to his writings in the earlier part of the eighteenth century. They were reprinted in Latin at the Hague in 1681 and at Geneva in 1685. A German translation appeared at Jena in 1799 and a French translation at Paris in 1840.

After the great revolution in chemical theory which followed the discovery of oxygen, Mayow's book was discovered in old libraries, where it had remained disregarded for a hundred years; and those who discovered it were astonished to see that the new chemistry, which was rapidly conquering the scientific world, was to be found in this old book. As far as we know, Dr Thomas Beddoes was the first distinctly to recognise Mayow's claim. Dr Beddoes published his discovery of Mayow in a letter to Dr Edmund Goodwyn, with an *Analysis of Mayow's Chemical*

Opinions. It is dated Oxford, 12th February 1790, two years before his resignation of the readership in chemistry. Besides Beddoes we have Dr J. B. A. Scherer, physician in Vienna, who in 1793 published *Beweis, dass J. Mayow vor 100 Jahren den Grund zur antiphlogistischen Chemie und Physiologie gelegt hat*, and also G. D. Yeats, M.B. of Hertford College, Oxford, physician at Bedford, who in 1798 published *Observations on the Claims of the Moderns to some Discoveries in Chemistry and Physiology*.

Beddoes quotes from Blumenbach's *Institutiones Physiologicæ*, 1787, the following remarkable passage: "Magna jam pars memorabilium horum phænomenorum," says he, speaking of respiration, "quibus nuperis lustris et physica de aeribus factitiis disciplina et physiologia negotii respirationis tam egregie ditata et illustrata est, jam ante centum et quod excurrit annos innotuit acutissimi ingenii medico Joanni Mayow, cujus de *sal-nitro et spiritu nitro-aereo* (quo nempe nomine dephlogisticatum aerem insignivit) tractatum, Oxon. 8vo editum, magna cum voluptate legi et relegi."

But these attempts to make Mayow and his work known to the scientific and medical world were not crowned with much success.

Mayow is indeed mentioned, and his work is discussed, in most books on the History of Chemistry; but as far as we have been able to discover, not many chemists or physiologists have made anything like an intimate personal acquaintance with his writings. Quite recently a considerable part of the treatise on nitre has been translated into German and published by Prof. Donnan (Ostwald's *Klassiker der exakten Wissenschaften*, Nr. 125, 1901). We had begun the translation some time previously, but soon saw that it was not possible to obtain a clear idea of Mayow's scientific position without reading the whole of the five treatises. We therefore now present to the reader a translation of Mayow's *Opera omnia.* Our rule has been to translate as literally as possible, avoiding the use of any words or phrases which have, since Mayow's time, acquired a special scientific meaning. This has led to the retention of expressions not now familiar to scientific readers. Some of these we may here note. Sulphur is often used for what may be called the combustible principle, and sulphureous matter almost always means combustible matter, without any suggestion that it contains sulphur in the sense we should mean if we used the phrase now. There is no difficulty in seeing quite clearly when Mayow uses the word sulphur in the general and when in the special sense.

By "purely saline salt" he means an alkali, fixed or volatile, usually a carbonate; and "fixed salt" means potash or soda, usually as carbonate. It is scarcely necessary to say that the nature of the difference between the caustic and the mild alkalis was not discovered till nearly a hundred years after Mayow's time.

We have confined ourselves to the work of translation and have added nothing in the way of commentary or criticism; but it may be well to remind the reader that Hooke's *Micrographia* had been published shortly before Mayow wrote, and that most of Boyle's treatises appeared shortly after.

The edition of Mayow's works printed at Oxford in 1674 has been used in the preparation of the translation.

A. C. B.
L. D.

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ERRATUM

p. 206, line 15 from bottom—*for* "beat" *read* "beats."

ON SAL NITRUM AND NITRO-AËRIAL SPIRIT

CHAPTER I

ON SAL NITRUM

THAT this air surrounding us, which from its tenuity escapes the glance of the eye, and appears as a void to those who survey it, is impregnated with a universal salt of a nitro-saline nature, that is to say, with a vital, igneous, and highly fermentative spirit, will be obvious, I conceive, from what follows. But in order that this nitro-aërial salt may be more clearly understood, I think that we should begin with a history of nitre.

I am not unaware that there are already many treatises on nitre, and that there is scarcely one of our recent authors who has not written something about it; as if, indeed, it were ruled by fate that this wonderful salt should make no less noise in philosophy than in war, and fill the universe with its sound. Yet, meanwhile, the truth seems to be quite obscured by the multitude of writers, and even now nitre lies hid in darkness. But in order to set forth our views on a matter so recondite, let us examine briefly, according to our custom, of what elements nitre is composed, and, indeed, from what family it originates.

With regard then to the constituent elements of nitre, *sal nitrum* seems to be composed of an extremely fiery acid salt, and, in addition, of an alkali or of purely saline volatile salt taking the place of the *sal alkali*. And this may be ascertained not only from its analysis but also from the way in which it is produced. As regards sulphur, which nitre is commonly supposed to contain, it is our opinion that although nitre deflagrates readily enough when thrown into the fire, still a combustible sulphur is in no wise present, as will be shown below.

If nitre is analysed by distillation, acid spirit will pass into the receiver, while fixed nitre, closely resembling *sal alkali*, will be left in the retort. Moreover, when nitre is melted in a crucible, and sulphur is thrown in repeatedly until the nitre will deflagrate no longer, the substance left at the bottom is improperly called fixed nitre, since there is fixed only one element of the nitre, namely, its salt; the other element, namely, the acid spirit, escaping in vapour during deflagration. Still, this spirit may be retained if deflagration takes place in a tubulated retort with a receiver attached, or under a bell-jar.

Further, if nitre and tartar, mixed in equal quantities, are kindled by ignited iron or charcoal, fixed salt equal in amount to the whole of the tartar will be found after the deflagration, and of this at least some part is derived from the nitre, and is generally but improperly called salt of tartar; for, since this fixed salt is equal to the whole of the tartar, while the tartar is not all fixed salt, but consists to a large extent of spirit and a fetid oil which passes off during the deflagration, some part of the remaining salt must accordingly be due to the nitre.

In the same way, if we look at the mode in which nitre is produced, we shall recognise clearly the same constituent elements. For if the acid spirit of nitre is poured upon any alkali, or, in place of the alkali, upon purely saline volatile salt, from the mutual strife of these two things coming together and the intense action, *sal nitrum* is generated, which will readily deflagrate when thrown into the fire. So that nitre would seem to be born fit for fights and hostile encounters, since it derives its origin from the mutual conflict of opposing elements and from enmity itself.

The constituents of nitre having been in this way considered, let us next inquire how *sal nitrum* is produced in the earth. For from almost any soil impregnated by the air and the weather, but especially from such as abounds in sulphur and fixed or volatile salt, as that from stables, dovecots, and slaughter-houses, *sal nitrum* is abundantly derived, and from its source is well called *sal terræ*.

As to the mode in which nitre originates in the earth, the generally received opinion is that the earth as its proper matrix draws *sal nitrum* from the air in virtue of its own attractive force. And, indeed, there can be no doubt whatever that the air contributes in no small degree to the generation of nitre, since nitre is only evolved from soil which is impregnated with air. Moreover, if earth from which all the nitre has been lixiviated be exposed to the air, it will after some lapse of time abound once more in nitre.

But, assuredly, one can scarcely suppose that the nitre itself is all derived from the air, but merely its more volatile and subtle part, the rest of the nitre being due to the earth, for of the nitre obtained from the earth, by no means the least part is a fixed salt which is not volatilised by the very fiercest fire. For in the distillation of nitre, only the acid spirit of the nitre is volatilised, the fixed salt being left in the retort. And there is no ground for saying that the nitre of the air is of a more volatile nature than ordinary nitre. For if earth from which all the nitrous salt has been lixiviated is exposed to the air, *sal nitrum*, not volatile, I say, but fixed and of the ordinary sort, will after some lapse of time be produced. Consequently, if that nitre were wholly derived from the air, then nitre of the more fixed sort, that is, common nitre, must reside in the air, but that such should fly about in the very rare air is not to be supposed. But you may say that the same nitre which is volatile when flying about in the air, becomes fixed in the earth. But I would ask how that can take place, unless something fixed from the earth be added to the volatile nitre of the air? And what else is this, but saying that the volatile part of the nitre is derived from the air, while its more fixed part is derived from the earth?

To this we further add that if nitre, such as is extracted from the earth, resided in the air, then as the aërial nitre, whatever it be, mixes with kindled fires (for the aërial nitre becomes food for the fire), it would necessarily follow that every flame, even the mild flame of a lamp, would constantly detonate on account of the nitrous particles mixed with it; yet this does not take place.

From these considerations it seems to be established that *sal nitrum* is derived partly from the air and partly from the earth, and this will appear still clearer from what follows.

Let us consider then, in the next place, what part of the nitre is contributed by the earth, and, also, what is contributed by the air. With regard to this, it is our opinion that the *fixed salt* of which nitre in part consists, is derived from the earth—and for this reason, that it cannot, as we have already indicated, reside in the very rare air on account of its highly fixed nature. It favours this view that from earth impregnated with fixed or volatile salt, as from stables and also from soil containing quicklime or ashes, *sal nitrum* is lixiviated in greater abundance than from any other soil, because these salts, united in course of time with nitro-aërial spirit in a way to be explained below, are converted into nitre. And, indeed, it is probable that ashes, quicklime, and the like, fertilise the soil, for this reason only, that they afford fixed salt for the production of nitre, as will be shown below.

Here, perhaps, some one will object that if earth from which all the salts have been lixiviated is exposed to the air, *sal nitrum* will, after some time, be produced in it anew.

I reply that seeds of fixed salts exist, although obscurely, in all soil, even in that which has been lixiviated, and that these, by the force of a sort of aërial ferment, are digested in course of time into fixed salt, as I shall endeavour to show below. That the earth is impregnated with a certain universal seed, fecundating all things, has long been a received opinion. Why, then, not suppose that this macrocosmic seed is either itself fixed salt or, at least, the seeds of fixed salts hidden in the bosom of the earth ; and that these when brought in progress of time to maturity are, together with nitro-aërial spirit, changed into *sal nitrum*. And it is a proof of this that nitre generated in the bowels of the earth contributes in no small degree to the growth of plants, as will be shown below. For as metallic seeds here and there dispersed through the mass of the earth are in the course of time converted into perfect metals, it is in like manner probable that seeds of fixed salts lie deep hidden in every fertile soil as in a suitable matrix, and that they by long digestion and the influx of air are changed into fixed salts. For in no other way can we conceive whence there should arise such an abundance of fixed salts as is usually obtained by lixiviation from the ashes of burned plants. For certainly none of these salts can proceed from another source than the earth. Indeed, it is probable that earth, pure and simple, is nothing else than sulphur and fixed salt united together in the closest bonds, and that both are held together in so firm a union that it is only after a long period of fermentation, set up by the air and the weather, that they reach a state of activity. But this will be discussed more fully below. And, at any rate, if sulphur and fixed salt are melted by a gentle heat, there results from their union a mixture of a dark purple colour, very like a clod of earth—the only difference, perhaps, being that earth is composed of sulphur and fixed salt, both immature, and united together by a closer bond.

CHAPTER II

OF THE AËRIAL AND IGNEOUS PART OF THE SPIRIT OF NITRE

It is shown that the Spirit of Nitre is a compound body, and that it is derived partly from the air and partly from terrestrial matter. First, of its aërial part.

In the previous chapter we treated generally of the elements of which nitre is composed, and specially of its more fixed part, the *sal alkali*, to wit. Let us look, in the next place, at the source of the other element of the nitre—viz., its acid spirit. Regarding this, I was for some time in doubt whether nitrous spirit in a state of the finest subdivision did not reside in the air and, fleeting to and fro and permeating all things in virtue of its most penetrating nature, instantly assail, whether from hate or rather from some conjugal affection, whatever fixed or volatile salt it at any time encountered in its wandering path ; and whether these two, closely united together, did not appear to coalesce in a *tertium quid*, to wit, common nitre. And assuredly there are arguments of no little weight which can be advanced in support of this hypothesis. For any other source than the air for the acid spirit of nitre is scarcely conceivable, since *sal nitrum* is generated in earth which is in no way impregnated with an acid salt. It is also to be noted that all salts whatever, fixed and volatile, as also vitriols, if calcined to the complete expulsion of the acid spirits, acquire when exposed for some time to the air a certain acidity, and become to some extent nitrous. Moreover, steel filings if exposed to moist air, are corroded just as they would be by acid fluids, and are changed into aperitive *crocus martis*. So that apparently a certain acid and nitrous spirit resides in the air.

But when I had seriously considered the matter, the acid spirit of nitre seemed to be too ponderous and fixed to circulate as a whole through the very thin air. Besides, the nitro-aërial salt, whatever it may be, becomes food for fires, and also passes into the blood of animals by means of respiration, as will be shown below. But the acid spirit of nitre, being humid and extremely corrosive, is fitted rather for extinguishing flame and the life of animals, than for sustaining them.

But although the spirit of nitre does not proceed altogether from the air, still we must believe that some part of it originates from the air. For, since some part of the nitre is derived from the air, as has been shown above, while the fixed salt, of which nitre in part consists, proceeds from the earth, the remainder of the nitre, that is to say, its acid and fiery spirit, must be derived, in part at least, from the air. But in order that the aërial part of the spirit of nitre may be better understood, we must briefly premise the following.

First, it is, I think, to be admitted that something aërial, whatever it may be, is necessary to the production of any flame—a fact which the experiments of Boyle have placed beyond doubt, since it is established by these experiments that a lighted lamp goes out much sooner in a glass that contains no air than it does in the same when filled with air—a clear proof that the flame enclosed in the glass goes out, not so much because it is choked, as some have supposed, by its own soot, as because it is deprived of its aërial food. For since there is more room for receiving the smoke in the empty glass than in the glass that is full of air, the lamp would go out in the latter sooner than in the former, if its extinction were due to the smoke. Besides, no sulphureous matter, if placed in a glass from which the air has been pumped, can be kindled either by ignited charcoal or iron, or by the solar rays collected by means of a burning-glass; so that there can be no doubt whatever that certain aërial particles are quite indispensable to the production of fire, and, indeed, it is our opinion that these are mainly instrumental in the production of fire, and that the shape of the flame is mainly dependent upon these, thrown into extremely brisk motion, as will be explained at greater length below. But it is not to be supposed that the air itself, but only that its more active and subtle part is the igneo-aërial food, since a lamp enclosed in a glass goes out when there is still an ample enough supply of air in it, for neither is it to be believed that the particles of air which existed in the said glass are annihilated by the burning of the lamp, nor yet that they are dissipated, since they are unable to penetrate the glass. Further, it is impossible that these igneo-aërial particles are any perfect nitre, as is generally supposed—for it was already pointed out that not the very nitre as a whole, but only a certain part of it, resides in the air.

In the second place, it would be reasonable to suppose that the igneous particles of air necessary to the support of all flame reside in *sal nitrum* and constitute its more active and fiery part, for it is to be noted that nitre mixed with sulphur deflagrates readily enough in a glass which does not contain air, and also under water, as will be established by the following experiment; for let gunpowder, very finely ground, be made into a hardish mass with a little water, and let a small tube, closed at one end, be densely filled with it by forcibly ramming the stuff in with a stick. Next, let that gunpowder be set on fire at the open end of the tube, and the tube be inverted and plunged into water, and kept there. Then the gunpowder will deflagrate under water until it is all gone. Moreover, that powder, arranged in the manner aforesaid, will burn in a glass containing no air, although other fires are presently extinguished because the aërial food is withdrawn—a sufficiently clear proof that *sal nitrum* contains in itself the igneo-aërial particles necessary to the production of flame; so that for its deflagration there is no need for a supply of igneous particles from the air.

That igneo-aërial particles exist in nitre is further evident from this, that flame produced by deflagrating nitre is caused by the igneo-aërial particles residing in it and bursting out in a compact body with fiery motion, but not by its sulphureous particles. For it is probable that nitre has no sulphureous particles as ingredients; for I cannot agree with the famous Dr Willis, who has stated in his treatise on Fermentation that there is a great deal of sulphur in nitre. His principal arguments are these—that if nitre is thrown upon the fire it will immediately produce a flame, and that it is especially generated in places where there are sulphureous animal excrements. But, with all due respect to so eminent a man, I should have thought that nitre, pure and simple, is in no wise impregnated with sulphureous particles. For neither in the rectified spirit of nitre nor in pure *sal alkali* is any combustible sulphur to be found; and yet, from the combination of these two, nitre will be produced. But, because nitre produced in this manner will deflagrate, we cannot believe that this results from sulphureous particles, for it contains none, but from the igneo-aërial particles contained in it and thrown into very rapid motion. And this will be made still clearer by what follows.

For it is to be noted that for the production of any flame, it is absolutely necessary, as has been already pointed out, that there should be not only sulphureous particles, but also igneo-aërial particles. To kindle any sulphureous matter, igneo-aërial particles must be supplied, either from the air or from nitre previously added. And this is the reason why sulphur will not take fire in a vacuum unless nitre has been mixed with it. But, on the other hand, for the kindling of nitre there is no need for igneo-aërial particles to be supplied from without, because it will deflagrate readily enough in places from which air is excluded. But for its kindling it is quite indispensable that some sulphureous matter be mixed with it. For if nitre be thrown into a heated crucible it will not take fire. If, however, any sulphureous matter be previously mixed with it, then the nitre, when thrown into the said crucible, will immediately burst into flame. Nay, nitre can in no wise be kindled by the flame of a candle, or by the solar rays, unless sulphur has been previously mixed with it. And yet the same nitre if thrown upon charcoal will be easily set on fire; but this happens because the sulphureous particles of the charcoal ignite it. From these considerations it is undoubtedly established that nitre has no sulphureous particles contained in it, and this is why, for its deflagration, sulphureous particles must be supplied from without. And hence it follows that nitre supplies in the flame excited by it the igneo-aërial particles only, but by no means the sulphureous particles, of which it is altogether destitute. And hence it is that the flame of nitre is very different from that which is produced by any deflagrating sulphureous matter. For sulphureous matter burns when igneo-aërial particles are supplied from the air, but nitre from the igneo-aërial particles closely packed in itself and breaking forth in densest array. Hence it is that the flame of nitre is exceedingly impetuous. That the form of flame depends mainly on the igneo-aërial particles, I shall attempt to show later.

But the reason why nitre is chiefly produced in places that are largely imbued with sulphur will be evident from what follows.

Since this treatise was written, Boyle's experiments recently published have come into our hands. In the second of these it is shown that when gunpowder is kindled, by means of a burning-glass, in a glass freed from air, the flame is not propagated, as in other cases, through the whole of its mass, but that only those grains on which the collected solar rays fall take fire. So that it appears that the access of air is necessary even for the kindling of gunpowder. I reply that although that powder will deflagrate by means of the igneo-aërial particles residing in it, in a place where there is no air, and under water, still the access of external air contributes in no small degree to its kindling. For it is to be noted that air, on account of its great elastic power, lies in the closest proximity to the sulphureous particles of the gunpowder and even presses powerfully against them, whence it is that when the powder is once ignited in free air nitro-aërial particles of the air are never lacking to keep up the flame; while, on the other hand, the igneo-aërial particles in gunpowder are intimately blended with the sulphureous particles only in the individual grains, and are not carried to those that are kindled; so that the flame of the powder is speedily extinguished in a vacuum, owing to a break in the continuity of the igneo-nitrous particles. But how greatly the elasticity of the air helps to produce fire will be more fully established by what will be said below.

From what has been already said, it is, I think, to some extent proved that nitre contains in itself the igneo-aërial particles required for the production of flame. Wherefore, since some part of nitre is derived from the air and igneo-aërial particles exist in it, it seems we should affirm the proposition *that the aërial part of nitre is nothing else than its igneo-aërial particles.*

But now since the aërial part of nitre exists in its acid spirit, but not in the fixed salt, which, as we have already shown, forms the rest of the nitre, we may conclude that *the igneo-aërial particles of nitre*, which are identical with its aërial part, *are hidden in the spirit of nitre*, *and constitute its aërial part.*

Indeed, it is probable that the spirit of nitre is a compound, and that some of its particles are flexile, humid, and of a grosser nature, being apparently derived from terrestrial matter—as I shall endeavour to show below—but that other particles are rigid, dry, and extremely subtle, agile, ethereal, and really igneous, and yet, being united with saline particles in a fluid and moist condition, are unfitted for entering on a fiery movement, and that these at any rate are derived from the air.

With regard then to the aërial part of nitrous spirit, we maintain that it is nothing else than the igneo-aërial particles which are quite necessary for the production of any flame. Wherefore, let me hence-forth call the fiery particles, which occur also in air, nitro-aërial particles or nitro-aërial spirit.

For, indeed, the spirit of nitre seems to derive its caustic and very potent fiery nature from the nitro-aërial and fiery particles which reside in it. So that it is commonly, and not improperly, called potential fire. And, indeed, when nitre mixed with sulphur is set on fire, it is probable that the nitro-aërial particles of this impetuous flame proceed from the nitrous spirit, since the fixed salt of nitre, with which the acid spirit is combined, is very alien to the nature of flame, and remains to a large extent at the bottom of the crucible after the nitre has been deflagrated in it. And, indeed, I do not know anything in nature approaching nearer to fire than the red spirit of nitre which passes into the receiver in the course of distillation, with a ruddy colour; but the ruddiness of nitrous spirit, rivalling flame, seems to be due to the igneo-aërial particles of the spirit, which are agitated with an almost fiery movement. It is corroborative of this view that the spirit of nitre is extremely corrosive, destructive, and caustic, and possesses a very flame-like nature. And, indeed, it is probable that the form of flame depends largely, if not exclusively, upon nitro-aërial particles such as are contained in the spirit of nitre, as will be shown below.

One will readily object here that the acid spirit of nitre is by no means combustible, for it will not, like sulphureous matter, blaze if thrown into the fire, but, on the contrary, it will put the fire out. I reply that the igneo-aërial particles existing in the spirit of nitre are in a humid condition, and that they are prevented from beginning a fiery movement on account of being covered over with particles of acid fluid, as we have already intimated. And, indeed, humidity is a very great hindrance to fire, since it is exceedingly well adapted for extinguishing fiery particles. But although the spirit of nitre will not deflagrate if put into a flame, yet if it is poured upon salt of tartar, nitre will be generated from the combination, and if this be thrown into the fire it will immediately produce flame. But we must suppose that the flame of this deflagrating nitre is caused by the igneo-aërial particles of the nitrous spirit being thrown into agitation. For the salt of tartar of which the rest of the nitre is composed seems to be very alien to the nature of flame, as we have shown above. But the reason why the igneo-aërial particles of nitrous spirit are well adapted, when combined with fixed salt, for producing flame, seems to be this, that when the spirit of nitre unites with fixed salt to form nitre, its humidity being lost, it changes into a dry and rigid substance; so that as its igneo-aërial particles exist now in a dry condition, there is no obstacle to their commencing a fiery motion.

Further, nitro-aërial particles must, it appears, if they are to take the form of flame, enter into close combination with a fixed salt, or with something else to take the place of the fixed salt; so that they may be torn violently and with elastic force from their partner and thrown into a state of the most rapid motion, as I shall endeavour to show below.

CHAPTER III

OF THE NATURE OF NITRO-AËRIAL AND IGNEOUS SPIRIT

FROM what has been already said, it is, I think, to some extent certain what the nature of the nitro-aërial and fiery spirit is. For since the aërial and igneous part of nitre, or what is the same thing, nitro-aërial spirit, exists in the acid spirit of nitre and constitutes its more active part, it follows that the nitro-aërial and fiery spirit is of a nitro-saline nature, and has the character rather of an acid, than of a fixed, salt. And, assuredly, the effects of fire seem to agree well with an extremely subtle and highly corrosive salt, as will be shown immediately. It must, however, be remarked that this igneous salt is in no way hostile to fixed salts, but, on the contrary, rather intensifies their power than diminishes it as acid liquids do. For fixed salts when heated in the fire become more acrid and caustic in their nature. Certainly the acid spirit of nitre seems to be opposed to fixed salts only as regards its terrestrial and humid part, but not as to its dry and fiery part. Nay, there is not so much contrariety, I think, as is commonly supposed between fixed salt and any acid, as will be more fully expounded below.

But let us consider in the next place the part which nitro-aërial spirit, or what is the same thing, the aërial part of nitre, plays in producing fire. On this point my opinion is that *the form of flame is chiefly due to the nitro-aërial spirit set in motion.* For I do not think we ought to agree with recent philosophers, who believe that fire can be produced by the subtle particles of any kind of matter if they are thrown into violent agitation. In fact, while the Peripatetics formerly assigned a distinct quality for almost every natural operation and multiplied *entia* unnecessarily, the Neoterics on the other hand maintain that all natural effects result from the same matter, its form and its state of motion or of rest alone being changed, and that consequently any thing whatever may be obtained from any thing. But in truth this new philosophy seems to depart too far from the doctrine of the ancients, and I have thought it better to take an intermediate path. It would certainly be a reasonable supposition that certain particles of matter which are unlike in no other respect than in the form and extremely solid and compact contexture of their parts, differ so much that by no natural power can they be changed one into another, and that the *Elements* consist of primary, and in this way peculiar, particles. Hence, I conceive that fire can be produced only by particles of a certain kind, and this is obvious from the very fact that it cannot be kindled without nitro-aërial particles.

As regards the sulphureous particles which are also indispensable for the production of fire, the necessity for them seems to arise merely from this that they are naturally fit to throw nitro-aërial particles into a state of rapid and fiery commotion. And I think it is not impossible that fire may be produced without the presence of sulphureous particles. The fire from the solar rays when condensed by a burning-glass, and the other celestial fires appear to be of this sort. For although sulphureous particles are absolutely necessary for kindling the kitchen fire, yet I do not think they exist in celestial fires, as I shall endeavour to show below.

For whether we consider flame as sharp, caustic, and in the highest degree corrosive, or as possessing an extremely penetrating and dissolving power, or finally as being ruddy and bright, in all of which qualities the true essence of fire consists, all these, I say, seem to proceed from its nitro-aërial spirit, since the particles of the latter are in the highest degree subtle, sharp, and caustic. For it has been shown above that the extremely corrosive and acrid nature of nitrous spirit is due to the nitro-aërial and fiery particles which reside in it. And, indeed, fire and the spirit of nitre are so like in respect to their caustic virtue, that it can scarcely be doubted that their extremely corrosive nature is due to particles of the same kind, namely, to the nitro-aërial and fiery spirit which resides in both.

Besides, nitro-aërial particles when in very great commotion become red like fire and glisten, as is clearly seen in the spirit of nitre which is ruddy during distillation. Nay, that every kind of light proceeds from the motion of the nitro-aërial particles will be shown below; while, on the other hand, the gentler sulphureous particles, however violently agitated, appear less fitted for assuming the keen and eminently destructive nature of fire.

If we consider attentively the nature of flame and reflect upon the character of the change which the fiery particles undergo on being ignited, we can form no other conception than that the kindling of the igneous particles consists in their extremely rapid motion. Why then should we not suppose that saline particles are specially fit for the production of fire? For since they are extremely solid, subtle, and agile, they seem to be much better suited for executing a swift and fiery movement than the crasser and very soft sulphureous particles.

But the reason for the notion that it is the sulphureous rather than the nitro-aërial particles which take fire is, that the grosser sulphureous nutriment of fire is always in view, while the nitro-aërial particles are so fine and subtle that they quite escape observation, and yet it is certain that nitro-aërial particles are not less necessary than sulphureous particles for the production of fire.

The following experiment confirms what has been said, viz., if nitre be put into a hot crucible it will soon liquefy but will not take fire, although oil will immediately burn if thrown into the crucible. The inference from this is that the fiery particles which penetrate the glowing crucible are not of a sulphureous nature, for otherwise the nitre would be kindled by the fiery particles mixed with it, for sulphur particles when mixed with melted nitre immediately ignite it. But the proof that the igneous particles collected in the heated crucible are of a nitro-saline nature is this, that any sulphureous matter cast into the said crucible is ignited by those particles; but sulphureous particles are not thrown into a state of extremely rapid and fiery motion without the aid of nitro-aërial particles.

We remark further that sulphureous particles are of so crass a nature that we can scarcely imagine, however heated they may be and however minutely divided, that they will become so subtle and nimble as to be able to penetrate, like fiery particles, metals, glass, and such like very solid things, and this seems to be confirmed by the following experiment. For let a polished metal plate be kept for some time in the flame of a candle so that the igneous particles deeply penetrating the said plate make it hot. But that the igneous particles entering the plate are the nitro-aërial particles of fire and not sulphureous is evident from this, that the sulphureous particles adhere to the outer surface of the plate in the form of soot and do not at all penetrate the plate. And yet we cannot doubt but that the sulphureous particles adhering to the plate were on fire, as far as their nature allows. For it must be supposed that the sulphureous particles which ascend from the wick into the flame are on fire from their first entrance into the flame, since the flame could not be produced without the burning of sulphureous particles. Nay, the black colour of these particles indicates a burning of some sort. But this will be made clearer by what will be said below.

Finally, the nitro-aërial particles in *the flame produced by solar rays* collected by a burning-glass are particularly bright. This celestial flame appears to be due merely to the nitro-aërial particles of the atmosphere set in fiery motion by the action and intense impulse of light. And this we must suppose is the reason that antimony, when calcined by the solar beams, is fixed and made diaphoretic, just as if it were changed into *Bezoardicum minerale* by spirit of nitre poured upon it and drawn off again and again. Indeed, it is probable that it is the nitro-aërial particles with which that spirit abounds, and in some motion of which the solar rays consist, that fix antimony and render it diaphoretic. It favours this view that antimony acquires a diaphoretic virtue, not only from the spirit of nitre and the solar rays, but also from the flame of nitre in which nitro-aërial particles are more densely collected. Nor should it be overlooked that antimony, calcined by the solar rays, is considerably increased in weight, as has been ascertained by experiment. Indeed, we can scarcely imagine any other source for this increase of the antimony than the nitro-aërial and igneous particles fixed in it during calcination.

I am aware that it is the common opinion that the diaphoretic virtue of antimony is due to the loss of its extraneous and combustible sulphur in its calcination. But I am not sure that this view is quite consistent with truth. For it is well known that if antimony and nitre are mixed and thrown into a heated crucible, a very impetuous flame will arise from them, since the sulphur of the antimony ignites the nitre mixed with it. If, however, the antimony has detonated (as the chemists phrase it) with about a double quantity of nitre, then nitre mixed with it will no longer produce a flame, since the combustible sulphur of the antimony has been entirely removed in the first detonation. And still the antimony has not yet acquired the diaphoretic virtue. Hence, for its further fixation, charcoal or some sulphureous matter should be put from time to time into the crucible in which the antimony, along with the nitre last added to it, has been fused, so that the nitre may ignite and the antimony be fixed by its long-enduring flame. Clearly, then, the fixation of antimony appears to be caused, not so much by the removal of its extraneous sulphur, as by the fixation in it of the nitro-aërial particles in which the flame of nitre abounds.

The reason why an addition of tartar to nitre contributes greatly to the fixation of antimony is obvious from what has been said. For I think it must be attributed to the tartar being imbued with such sulphur as is suitable for gradually and thoroughly burning the nitre. For tartar mixed with nitre effects its calcination in the best way, as has been shown above. And hence it is that antimony kept in the flame of nitre, kindled by the sulphur of tartar and long burning, is fixed by the nitro-aërial particles of the nitre and becomes diaphoretic. Nor is it probable that salt of tartar contributes anything to the fixation of antimony. For a fixed salt such as that of tartar is quite unsuitable for exalting the emetic property of antimony. Otherwise salt of tartar, but not tartar itself, would have to be used for the calcination of antimony. We remark, lastly, with respect to the fixation of antimony, that it appears advisable to begin its calcination in the first instance with nitre alone, so that the nitre may kindle and remove the impure sulphur of the antimony, and then to mix tartar with the nitre, that the remainder of the nitre, now that the sulphur of the antimony has been removed, may be burned by the sulphur of the tartar, and the fixation of the antimony completed.

By this hypothesis of ours, it is not difficult to explain why fires that burn with a bright flame purify the air from pestilential miasma, and are consequently so beneficial in contagious diseases. For no doubt the nitro-aërial particles which are inhaled by animals in respiration (as will be shown below) approach from all sides for the production of the flame, and are hurried along in it with a motion of the greatest velocity. And the result is that these particles are purged by the motion and the fire from their poisonous taint. But the subject of fire will be treated more fully in Chapter VII.

CHAPTER IV

OF THE SOURCE OF ACID LIQUIDS; ALSO OF THE TERRESTRIAL PART OF THE SPIRIT OF NITRE

THAT the spirit of nitre is a compound, and that it is derived partly from the air and partly from the earth, has been shown above. We have already treated of its aërial part; so that its terrestrial and acid part remains for discussion.

It is extremely difficult to understand how the spirit of nitre originates in the earth. For earth appears to possess the nature of a fixed, rather than of an acid, salt. And yet it is undoubtedly true that if exposed to the air it will, after some lapse of time, be impregnated with nitre. But it has been shown above that the acid salt of which the nitre in part consists originates in the earth. But that it may be understood how the acid spirit of nitre is generated in the earth, let me be allowed to prefix some observations regarding the spirit of sulphur and other acid liquids, because there exists among all acid spirits a very great likeness and affinity.

Hitherto the opinion has prevailed that an acid salt of a vitriolic nature lies concealed in the structure of sulphur, and that from this, exhaling in the deflagration of sulphur and collected in a superimposed glass bell-jar, the acid spirit of sulphur is composed. But it seems scarcely probable that a spirit so corrosive should reside in common sulphur, which has a sweetish and by no means acid taste. Nay, sulphur seems to have rather the nature of an alkaline than of an acid salt, as is proved by the fact that common sulphur will enter very readily into combination with the fixed salts allied to it. For it must not be said here, that the combination of fixed salts with sulphur arises from the secret presence of an acid salt in the sulphur with which the fixed salts seek a union. For if such were the case, effervescence and heat would be produced by the union of the sulphur and the fixed salt, as happens in an encounter between opposite salts. Moreover, when contending salts are mixed together, they destroy each other and are changed into a *tertium quid* which is altogether different from what existed before. But, in fact, fixed salt and sulphur if melted at a low temperature unite without any effervescence whatever; and neither of them is destroyed. On the contrary, their powers are mutually increased, as if they had united in a friendly league.

Wherefore since it is improbable that so acid a spirit is contained in the mass of sulphur and is not elicited unless the sulphur is burned, why should we not suppose that the spirit is produced, by the burning of the sulphur, in the following way? For I suppose that common sulphur contains in addition to its sulphureous particles pure and simple, a salt of a fixed or rather metallic nature in the closest union with its sulphureous particles, which saline part sometimes crystallises when sulphur is dissolved by the spirit of turpentine.

Further, it should be noted that the flame of kindled sulphur, as indeed flame of every kind, consists in this that the sulphureous particles of the deflagrating substance and the nitro-aërial particles mutually excite themselves to a very rapid motion, as we have shown above. But as the minutely divided saline particles of the sulphur are very closely united to its sulphureous particles, it happens in the deflagration of sulphur (when the sulphureous and nitro-aërial particles throw each other into fiery motion) that the saline particles of the sulphur, adhering to its sulphureous particles, are by frequent impacts of the nitro-aërial particles struck, rubbed, and comminuted, so that the saline particles from being often rubbed and pounded, are at last sharpened like small swords and are moreover so attenuated as to be changed from rigid and solid into flexible and fluid particles. The saline particles of the sulphur in fact which were previously of a fixed nature change, after they are thus sharpened and made fluid, into an acrid and acid liquid, and probably constitute the common spirit of sulphur.

That the facts of the case are as stated may be inferred from a careful study of the flame of sulphur since it is very different from other flames. For nitro-aërial particles do not shine ruddily and glow in the flame of burning sulphur as in other cases, but owing to their diminished motion appear blue, so that it would appear that some third substance is interposed between the nitro-aërial and sulphureous particles, and that by it these fiery particles are hindered in their motion. For as the nitro-aërial and fiery particles, when in violent agitation, glow, so when their motion is retarded they appear of a blue colour. And this is the reason that the flame of an expiring lamp is wont to be blue. But of this more elsewhere.

It serves to confirm what has been said that the flame of sulphur in consequence of the somewhat sluggish movement of the fiery particles is less caustic and almost harmless. For if a finger or anything combustible is thrust into the blue flame of sulphur, but not into the sulphur mass, it will not be burned as by other fires, but will remain for some time uninjured. To this we add further that the flame of sulphur does not expand like other flames but bursts forth from time to time and seems as it were to effervesce. From this it is to be inferred that there is a third substance mixed with it on which the fiery particles act. And that these particles, of a saline or metallic nature, mixed with the flame of the sulphur and sharpened by the rubbing of the nitro-aërial particles and brought at last to a fluid state, constitute the acid and corrosive spirit of sulphur, we take to be at least a probable conjecture; for otherwise I have no notion how the acid spirit is produced, for it is improbable that it exists in the structure of the sulphur before its combustion, as has been shown above.

To this we further add that the oil of vitriol expelled after several days' distillation seems to be produced in nearly the same way. For it is certain from experience that if the distillation of vitriol is continued with the strongest fire for ten or even more days, acid spirit will still all the while pass into the receiver. But it is scarcely to be believed that any acid spirit is so fixed and ponderous as to be able to remain so long in the hottest fire. We must rather suppose that nitro-aërial particles of the fire, in the course of the long continued distillation of the vitriol, encounter the metallic sulphur of the colcothar and effervesce—the result being that the saline particles of that sulphur which are placed among the mutually rubbing fiery particles are pounded and comminuted so as at last to be sharpened and brought into a fluid state. And these at last carried up by the force of the fire compose the oil of vitriol very much in the same way as we showed above that the spirit of sulphur is produced when sulphur is burned. And indeed it is probable that the distillation of vitriol will go on as long as any of the saline particles of the colcothar remain, these being brought into the fluid state under the action of heat in the manner aforesaid.

Further, I do not know but that acid spirits distilled from heavy woods, such as Guaiacum wood and the like, are formed in a similar way by the action of fire during distillation. Indeed, Guaiacum wood before distillation does not seem to be endowed with an acid but rather with a fixed salt. For its powder or decoction effervesces when spirit of vitriol (but not when fixed salt) is poured on it. It corroborates this view that the saline particles of that wood are in close combination with the sulphureous particles, as will be shown below, whence it is that the nitro-aërial particles of the fire, encountering the sulphureous particles of the wood in the course of distillation, rub the saline particles and bring them into a fluid state in the manner aforesaid. We observe also in passing that acid spirits distilled from sugar and honey appear to be produced in a not very dissimilar way by the action of the nitro-aërial spirit of fire. For such plants as have no acid taste and yet yield an acid spirit on distillation are composed of sulphureous in intimate union with saline particles, and are therefore suited for yielding an acid liquid in the manner aforesaid.

As the nitro-aërial spirit of fire, encountering saline-sulphureous particles with very brisk motion and fiery effervescence, rubs down in a moment and reduces to a fluid state the saline particles which are closely involved with the sulphureous, so the same nitro-aërial spirit effervescing in a slower motion with saline-sulphureous particles, changes the saline particles into an acid liquor only after some time has elapsed. And an example of this is to be sought in vitriol when calcined to the entire removal of the acid spirit. For if that vitriol has been exposed for some time to moist air it will be impregnated anew with acid spirit. Indeed, nitro-aërial spirit encounters the metallic sulphur of colcothar in a gentle manner and effervesces with it in an obscure way, whence it is that the saline or metallic particles of the sulphur are brought in the manner aforesaid to a state of fluidity. Certainly we can scarcely imagine any other mode for the formation of the vitriolic spirit in colcothar, for it does not arise in the colcothar immediately after distillation, and we cannot suppose (as has been elsewhere shown) that it is entirely derived from the air.

Further, the acid spirit of which vitriols are composed seems obviously to be produced in the same way. For vitriols are produced from the stone or rather the saline-sulphureous earth usually called Marchasite, and from it on the application of fire the flowers of common sulphur are elicited in considerable abundance. But after this earth has been exposed for some time to the air and wet weather and then (as its nature is) has fermented spontaneously, it will be found to be richly impregnated with vitriol. No doubt the nitro-aërial spirit, effervescing with the metallic sulphur of these Marchasites, converts their more fixed part into an acid liquid which, directly it is produced, attacks the metallic particles of the said stone and draws them out and at last coalesces with them to form vitriol.

But indeed iron rust also, which has a vitriolic nature, seems to be produced by the action of nitro-aërial particles meeting with the metallic sulphur of iron, for the saline particles of the iron when brought into a fluid condition in the manner aforesaid corrode and dissolve its metallic particles ; and from these combined, rust or a sort of imperfect vitriol is produced—very much as if the iron had been smeared with some acid liquid.

It should also be noticed that acid salt or sourness is produced by the action of nitro-aërial spirit not only in solids but also in liquids. For it is not enough to say that the acidification or the fluidity of the salts arises from this, that saline particles which before had been mutually hidden by the intervention of the other particles, afterwards, the bond of the mixture being loosed, flow together and spread themselves out through the whole structure of the substance, and that when these gain dominion sourness is produced in the mixture as some have imagined. For we must suppose that all the salts of the liquid were even from the first diffused through its whole mass, since they were dissolved in the liquid. It should rather be maintained that the souring of liquids is caused by the change of their fixed salt into an acid salt, a result which is probably due to the action of nitro-aërial spirit. For liquids abounding in fixed salt and sulphur, such as French wine and strong ale, acquire acidity from lengthy fermentation. Moreover, the fermentation of the liquids consists in the effervescence of nitro-aërial particles, whether contained in the liquid or entering from without, with the saline-sulphureous particles of the liquid, as I shall endeavour to show below. And hence it is that the saline particles of the liquid closely combined with the sulphureous particles are beaten and rubbed by the nitro-aërial particles and at last liquefied in the manner aforesaid. It corroborates this view that wines or strong ale long exposed to the solar rays or kept in a warm place turn in the course of time into vinegar. In fact the nitro-aërial particles communicated to these liquids by the solar rays or by fire (for I intend to show in another place that every kind of heat is due to nitro-aërial particles put in motion) effervesce with the saline-sulphureous particles of these liquids, with the result that the saline particles are sharpened by the action of the nitro-aërial particles and converted into acid salts. And whatever in fine aids the fermentation of liquids and throws their particles into violent commotion, as, for example, very warm weather and thunder, accelerates the souring of the liquids. To this I add that if common sulphur is dissolved in water in which quicklime has been slaked, or in lye, this solution which was imbued at first with fixed salt will in course of time become acid, so that the sulphur will not any longer remain dissolved in it.

It is also to be noted that saline and sulphureous particles exist in a fluid state in liquids—the consequence being that the saline particles in liquids cannot be so rubbed and comminuted by the action of nitro-aërial particles as in the case of solids. And this seems to be the reason why the acid salt of soured liquids is less sharp and corrosive than the spirit of sulphur and other eminently corrosive liquids of the same kind.

In the light of what has been said it will not be difficult to understand how the acid spirit of nitre is generated in the earth. For it was pointed out in another place that fertile earth is nothing but sulphur and fixed salt both immature, in a state of the closest combination, and indeed a dark purple clod of earth appears not very unlike colcothar, except that in the latter sulphur is combined with a metallic salt but in the former with fixed salt. As then the nitro-aërial spirit effervescing in a fiery motion with the particles of common sulphur, or again encountering with more gentle heat the saline-sulphureous particles of colcothar, sharpens more quickly or more slowly their saline-metallic particles and brings them to a fluid condition ; so also the same nitro-aërial spirit, descending in virtue of its most penetrating nature into the depths of the earth, there attacks the terrestrial sulphur and fermenting with it in an obscure motion rubs, attenuates, and sharpens the saline particles which are firmly clasped in its bosom, so that they at last became flexile, liquid, and in the highest degree acrid. The saline particles of the earth when made fluid in this manner become a suitable abode in which nitro-aërial particles may be hidden and detained. And in my opinion the spirit of nitre, of the sort obtained by distillation, is composed of these two firmly united.

And so at last I have endeavoured to show that all acid salts are produced from saline particles brought to a state of fluidity or fusion by means of nitro-aërial spirit, and also how this is done. With respect to the difference of acid liquids—this must be supposed to result from diversity of the salts out of which they are formed, as also from this, that the fixed salts are rubbed and sharpened now in a greater and now in a less degree by the nitro-aërial spirit. And yet there is a great affinity and likeness among all acid salts, and in them all, as in an appropriate medium, nitro-aërial and igneous particles reside, as will be shown below.

The particles of nitrous spirit generated in the earth in the manner aforesaid, as soon as they are produced, approach the seeds of the fixed salts which, as has been elsewhere shown, are hidden in the bosom of the earth, and solicit and call them forth into conjugal union as a suitable consort and of their own kin; and, lastly, from them, combined in the closest alliance, *sal nitrum* is produced, much as in colcothar or vitriolic earth, other saline particles that have been brought to a fluid condition by nitro-aërial spirit, meeting other saline particles of a nature akin to their own, coalesce to form vitriol as we have indicated above.

It follows from what has been said that there is no such great repugnance between fixed and acid salts as is commonly supposed. That they indeed boil up, when mixed together, with remarkable violence should not be imputed to any enmity between them but rather to a sort of conjugal affection. These salts, in fact, grind each other in order that they, being divided to the utmost extent possible, may be united in a closer bond.

From what has been said the reason is clear why nitrous but not purely saline salts are extracted by lixiviation from the earth, for particles of nitrous spirit generated in the earth take into union with them and render nitrous all the fixed salt which has attained to perfect maturity. And, indeed, it is probable that the seeds of the fixed salts existing in the bosom of the earth cannot be lixiviated and extracted without the aid of nitrous spirit.

That nitre is generated in the earth in the manner aforesaid may be inferred also from the fact that it is principally produced in such soil as is imbued with saline - sulphureous particles—as, for instance, in slaughter-houses, stalls, stables, and the like. In fact, fixed or volatile salts provide suitable material for the production of nitre, but sulphureous particles contribute in no other way to the generation of nitre than by exciting, when they effervesce with nitro-aërial particles, that heat in the earth's bosom by which first nitrous spirit is produced and then nitre itself. For, as has been shown above, the sulphureous particles of terrestrial matter must not be supposed to constitute nitre in part.

It is in some measure established I think from what has been said what the elements are of which *sal nitrum* is composed. For it seems to consist of salt of three kinds, of which one, the most active, is derived from the air, and it has an ethereal and fiery nature. This salt, as an architect, forges for itself from terrestrial matter a saline vehicle in which, as in a fitting subject, it resides. The saline vehicle along with the fiery salt which occupies it forms the spirit of nitre, which from the moment of its production meets the fixed salts of the earth which have attained to proper maturity, and coalesces with them to form common nitre. So much then for *sal nitrum*.

CHAPTER V

OF FERMENTATION

Of Nitro-Aërial Spirit, so far as fermentations leading to the birth or death of things are caused by it.

When nitro-aërial spirit effervesces with the saline-sulphureous particles of the earth in the manner described in the previous chapter, then the nitrous spirit produced by its action meets the fixed salts of the earth, and there is kindled in the earth's bosom that mild and enduring warmth by which the common mother fosters and quickens the seeds hidden in her womb. But that this fermentation may be more clearly understood, let me first speak shortly of the fermentative principles of things.

Among the elements of natural things nitro-aërial spirit holds the first place, so that it may rightly be called Mercury, since it is a substance exceedingly subtle, agile, and ethereal, and is also the primary instrument of life and motion not only in plants but also in animals, as I shall try to show below. Among the elements of the Peripatetics the two chief are Fire and Air, but for these two our nitro-aërial mercury might justly be substituted, since it possesses a really fiery nature and constitutes also the most active and fermentative part of the air, as will afterwards be shown. With regard to the spirit of the chemists, which usually leads their band of elements, I am quite unable to understand what they mean by the very grand word spirit. For with respect to the spirits of fermented liquids—that is, those which blaze when thrown into the fire—they are to be referred to the second element of the chemists, to wit, sulphur. But corrosive and saline spirits which alone remain ought to be entered in the register of the salts. So that clearly nitro-aërial particles should alone be distinguished by the name of spirit. Nitro-aërial spirit appears in very different conditions according as it is at rest or in motion, and that slower or very nimble, as will be shown more fully below.

In the list of elements sulphur has a claim to the next place because after nitro-aërial mercury it is the most fermentative. And indeed, except these two, there do not seem to be any active elements. Sulphur is seen in various states for now it lies inert and lulled to sleep, now it is raised to proper vigour and maturity ; sometimes indeed it is extremely fierce and irrepressible, as will be shown below.

Nitro-aërial spirit and sulphur are engaged in perpetual hostilities with each other, and indeed from their mutual struggle when they meet and from their diverse state when they succumb by turns all the changes of things seem to arise.

Salt, which has a passive nature, should be reckoned as the third of the elements. For in whatever way it may be volatilised there never, I think, begins in it a dance of internal movements. Salt is either fixed or volatile, both are however of nearly the same nature : but there is a greater variation in the condition of salt when it is changed from purely saline into acid. Salt has great affinity and relationship with nitro-aërial spirit and also with sulphur ; for these very active elements are by turns married to salt as to a fitting bride, and are fixed in its embrace, as will presently be shown.

Besides the elements already mentioned, water and *terra damnata* are to be found in almost every thing. Water seems to be a suitable vehicle for nitro-aërial spirit and sulphur, and together with *terra damnata* contributes to the building up of the frame of things in due strength and consistency.

Thus far of the elements viewed in themselves ; we have next to consider them in regard to how far they act on each other and mutually enter into various combinations. Hence proceed the internal movements of things and their rise and destruction.

First then, in the birth of plants, the nitro-aërial spirit or Mercury when set in motion by the impulse of solar rays descends in virtue of its very penetrating nature into the depths of the earth and attacks there its most bitter enemy terrestrial sulphur, firmly united with fixed salt and nearly hidden and buried in its embrace ; and by its very frequent vibrations lashes and wears it. The sulphur thus aroused and to some extent liberated from its terrestrial yoke by the frequent blows of the nitro-aërial particles begins a contest with its nitro-aërial foe, and from their mutual disturbance a rather notable effervescence is excited in the bosom of the earth, as we have indicated above. Meanwhile the particles of fixed salt held in the embrace of the sulphur are so worn by numerous strokes of the nitro-aërial spirit that they are brought at last into a fluid condition in the way described above. And by these, when liquefied, any fixed salt still adhering to the terrestrial sulphur is lixiviated (as was shown above). And so at last the nitro-aërial Mercury, having according to his furtive nature secretly entered the territory of his enemy sulphur and robbed him of his saline consort, wedded to her as to a suitable spouse succumbs, fixed by fate of an unhappy marriage, and almost buried in her embrace. For it has been shown above that nitro-aërial spirit is detained as in a suitable hospice in fixed salt which has passed into the fluid state. But when held in the embrace of a salt and coalescing with it, which is partly acid partly saline, to form nitre, it exists in a condition of the greatest fixity.

But meanwhile the terrestrial sulphur which, not so long ago, wrapped in wedlock with fixed salt, was fixed indeed, now, the saline yoke thrown off by help of the nitro-aërial spirit, attains a condition of volatility and gets the mastery, the nitro-aërial spirit being depressed. And in this way, in fine, extremely volatile and inflammable sulphureous particles (such as the oils which are obtained by distillation from plants) are very likely produced from the rude mass of the earth, which seems to behave like a *caput mortuum*. For the volatilisation of sulphur consists in its being liberated from a union with fixed salt. And it is therefore clear that the fixation of sulphur arises from its intimate union with a fixed salt. For if sulphureous oils are combined with fixed salt, as is the case in soap, they lose their inflammability altogether. For soap, which is partly composed of oil, will not blaze if thrown into the fire.

When in this way nitro-aërial spirit, effervescing obscurely with terrestrial matter, raises its sulphureous part to the requisite volatility and coalesces also with its saline part to form nitre, the elements of natural things are brought into the condition required for the production of plants. For all plants seem to be composed of terrestrial sulphur in a sufficiently volatile and inflammable condition, and of nitro-aërial spirit held in the embrace of salt and subdued, that is to say of nitrous salt, as will be shown presently.

It serves as a proof of the origin of vegetables in this way that at the time when vegetables chiefly spring from the earth, the nitro-aërial spirit ferments most actively with terrestrial matter, and *sal nitrum* is chiefly produced in the earth: in fact experience shows that nitre (which we suppose to be formed by nitro-aërial spirit effervescing with the saline-sulphureous particles of the earth) is produced in the earth in greater abundance in the beginning of spring than in the other seasons of the year. In winter, indeed, nitro-aërial particles and terrestrial sulphur are compressed by frost and are fettered as it were and hindered almost from moving at all. But at the beginning of spring nitro-aërial spirit is set in motion by the greater heat of the sun, and the structure of the earth is laid open now that the ice has melted. And then the nitro-aërial spirit set in motion descends deep into the earth, and meeting there with its saline-sulphureous particles, minutely broken, gives rise to an intense enough effervescence, with the result that nitre is generated in abundance, and plants grow up luxuriantly.

From what has been said we can see why animal excreta, salts of lye and also quicklime, and similar substances imbued with fixed salts fertilise the soil. Indeed the saline-sulphureous excreta of animals, as also fixed salts in union with terrestrial sulphur, are specially adapted for effervescing with nitro-aërial spirit, and they also supply appropriate material for the production of nitre and consequently contribute not a little to the production of plants.

Thus then the so much talked of fermentation by which the numerous family of plants is produced from the bosom of the earth, appears to be nothing else but the internal motion of nitro-aërial particles when they meet with the sulphur and salt of the earth, in virtue of which terrestrial nitre is produced and the sulphur brought to a suitable volatility.

It follows from what has been said that the salts of which plants are composed are to some extent nitrous and not purely saline, as we intimated above. For all vegetable salts are derived either from the air or from the earth. As regards the air it is by no means to be supposed that an alkaline and fixed salt resides in it; nor is the earth impregnated with a purely saline salt, for only nitrous salts can be extracted from it by lixiviation. And hence we may conclude that the salts of plants are nitrous and not purely saline. Hence in soil on which plants grow abundantly no nitrous salt is to be found, the reason being that all the nitre of the soil is sucked out by the plants. But when plants are calcined to ashes, the acid spirit of the nitre of which they are composed goes off as vapours, while the other element of the nitre—to wit, the alkaline salt—is left in the ashes. And hence it is that plants yield a greater quantity of fixed salt when burned fresh and with the least possible flame; but this does not result as some suppose because the alkaline salt goes off as vapours when the plants are slightly dried, for it has an exceedingly fixed nature and remains undiminished and intact in the hottest fire. But when plants containing much sulphur are dried and then burned in a bright flame, the sulphureous parts, burning with a fiercer flame, kindle the nitrous salt, and carry the whole of it away with them as vapours, very much as when gunpowder is ignited. If, however, green herbs are calcined with the flame kept down, their volatile sulphur, together with the original moisture, passes into smoke and goes away, while the nitrous salt remains behind; but if calcined in a hotter fire, the spirit of nitre is expelled as by distillation, the alkaline salt being left with the *terra damnata* in the ashes; and in proportion to the violence with which the calcination is effected, in that proportion are the salts alkalised when the nitrous spirit is expelled. Hence we may gather that it is not advisable violently to calcine diuretic salts (so named from promoting urine), for, by doing so, those salts are deprived of their nitrous and diuretic spirit. And hence it is that the lye, say of the ashes of *Genista*, is more efficacious in dropsy than its fixed salt thoroughly alkalised by violent calcination.

The nitrous spirit of plants seems to be clearly present in a fire of burning charcoal, for the smoke from that fire assails the nostrils when brought near to it, very much in the same way as the vapour that proceeds from the spirit of nitre. And indeed it is likely that the nitrous spirit which exhales from burning charcoal in the form of smoke, is the reason for that smoke being so acrid, and for its sometimes causing suffocation.

And further the nitrous spirit of plants manifests itself strikingly in their fermenting juices, when these have been kept for some time in a glass vessel carefully closed. For when these liquids are drunk the nitrous particles irritate the nervous parts of the throat with a quite striking pungency and bring on an almost convulsive choking. So that, when liquids of this sort are drunk, they are commonly and not improperly said to cut. Moreover, the nitrous particles in which these liquids abound seem to be the cause of their being so cold. For nitre mixed with liquids makes them very cold, and almost freezes them, as will be shown more fully elsewhere.

In some plants the oily parts are so heavy and so closely united with nitrous salt that they are not separated from each other by calcination; but the burning sulphureous particles carry the nitrous particles away with them as vapours. And hence it is that plants of this sort yield only a small quantity of fixed salt, as is the case with resinous woods, such as *Lignum sanctum* and the like.

Nor should it be overlooked that the nitre innate in plants contributes not a little to their burning, and that those which abound the most in nitrous particles take fire at once, even when they are green and full of moisture. Among these the ash is especially remarkable, for be it ever so green it yet burns with a bright flame. But, indeed, its richness in nitre may be inferred from the fact that while burning it gives out, from time to time, cracks like kindled nitre.

Thus far, we have considered the fermentation tending to the production of plants. We have still to inquire shortly as to the internal motion by which plants rush to their destruction.

In regard to this, it is our opinion that the fermentation which tends to the destruction of plants is also caused by the mutual agitation of the nitro-aërial and saline-sulphureous particles—with this distinction, however, that in the origin of vegetables, nitro-aërial spirit, when put in vigorous movement, attacks sulphur existing in a fixed state, and when the sulphur has been brought to volatility, the nitro-aërial spirit is fixed, imprisoned in saline bonds, as has been shown above. But, on the other hand, in the destruction of things, the internal movement is for the most part set up by the sulphur being too highly exalted. In fact, the sulphureous particles in a state of too vigorous movement attack the nitro-aërial spirit when lying asleep in the bosom of the fixed salt, and mindful as it were of former wrongs, thrust it forth from its saline shelter and set it in motion; but the nitro-aërial spirit, when violently torn from its saline partner, throws everything into disorder by its impetuous motion and loosens the union of the compound. But in order that what has been said may be better understood, let us consider in how many ways things rush to their destruction, for in all these we shall find that internal movement is caused by sulphureous particles and nitro-aërial spirit whether the latter is derived from without or not.

In the first place, the mode in which the structure of things is most speedily dissolved is Fire. But this is nothing else than an exceedingly impetuous fermentation of nitro-aërial and sulphureous particles in mutual agitation, as has been shown above. Thus, in combustion, sulphureous particles, moving with extreme velocity, throw into a most violent and fiery motion the nitro-aërial particles which exist in a state of fixation. This is evident when nitre is burned, for in its burning, nitro-aërial particles which were previously fixed and inert in the embrace of the fixed salt, are thrown into fiery motion by the agency of the sulphureous particles. And indeed it is probable that even the nitro-aërial particles of the air are in a fixed state previous to their being roused into fiery motion, as I shall endeavour to show elsewhere.

As the destructive power of fire is due to nitro-aërial particles, so also every internal movement which things undergo seems to depend upon a less violent agitation of the same particles. And it is a proof of this that in putrefaction and in nearly all fermentative movements some heat is excited, and this must be supposed to result from the motion of nitro-aërial particles, as will be shown immediately. How great moreover is the resemblance and affinity between fire and all other fermentations will appear from what follows.

With regard to fire, it is to be noted that for the burning of things, it is necessary that nitro-aërial particles should either be already in the burning substance or be supplied from the air. Gunpowder burns very readily on account of the nitro-aërial particles it contains; plants burn partly from the nitro-aërial particles they contain, and partly from such as come from the air; but sulphureous matter, pure and simple, can only be ignited by nitro-aërial particles supplied by the air.

And, just as for the production of fire, so also for exciting fermentations in plants, both sulphureous and nitro-aërial particles must either exist in the things to be fermented or be supplied from without. The juice expressed from plants, such as the must of wine or of apples and the like, effervesces on account of the nitro-aërial and sulphureous particles which it contains. For we have shown above that nitrous salts and therefore also nitro-aërial particles are contained in most plants, though, at the same time, the nitro-aërial spirit supplied by the air contributes much to the fermentation of these liquids, for very warm weather intensifies the action in no small degree. Further, that the fermentation of the aforesaid liquids, as also of all things whatsoever, is due to the mutual agitation of nitro-aërial and saline-sulphureous particles, is evident from the fact that liquids of this kind, and indeed nearly every thing, become sour in fermenting; for it has been shown above that acidity is caused by the action of nitro-aërial spirit. Should any one be inclined to think that the fermentation of the said liquids ought not to be classed among effervescences with a destructive tendency, I reply that although the juices expressed from plants become more perfect by fermentation so far as their use to man is concerned, yet, in respect to the compound whose structure it impairs, the aforesaid effervescence is rightly called destructive.

But when the decay of things is caused by extraneous heat and moisture, the internal movement is mainly effected by nitro-aërial particles supplied by the air. For nitro-aërial particles abound in a moist warmth; for we must suppose that heat of all kinds is due to their motion. When therefore nitro-aërial particles enter any substance along with extraneous moisture, they engage in conflict with the saline-sulphureous particles which they meet, and in consequence of their mutual agitation the structure of the compound is dissolved. Hence such things as exclude nitro-aërial spirit protect substances from corruption. And this is the reason why vegetable fruits, and even flesh, when covered with butter are preserved for a long time from putrefying, also iron smeared with oil is not corroded by rust. And indeed oil and other things containing sulphur appear to be extremely well adapted for excluding nitro-aërial spirit. For sulphureous and nitro-aërial particles, from their mutual enmity, keep off and repel each other, as I have attempted to show in another place; and for a similar reason, spices which are full of sulphur keep dead bodies for a long time from putrefying.

It is an additional proof of the foregoing that those things which consist of a combination of sulphur and salt, fixed or volatile, are particularly adapted for producing fermentation. Of this kind are yolk of egg and all sorts of gall of animals, as also soap—all of which when mixed with any farinaceous mass make it swell and ferment in cooking. But the fermentative nature of the things named seems to be due to this, that in cooking, the nitro-aërial particles of the fire meet their saline-sulphureous particles and effervesce with them.

I add further, that the glow or warmth which arises in nearly everything when fermenting, appears to be caused by nitro-aërial spirit, as has been already indicated. For I think it is clear, from what has been said elsewhere, that a most intense and fiery heat proceeds from nitro-aërial particles when thrown into very rapid motion. And indeed we must suppose that heat of every kind depends upon the same particles when briskly agitated, for heat and fire must be supposed to result from the agitation with different degrees of motion of the same kind of particles. For in glowing substances of all sorts the presence of nitro-aërial particles is shown by sufficiently clear signs.

As to the heat resulting from fire and from the fermentation of things, it has now been shown that it is caused by the motion of nitro-aërial particles. As to the heat of antagonistic salts when mixed together, we must imagine that it too is caused by nitro-aërial spirit. For it has already been shown that acid and corrosive liquids are produced by the action of nitro-aërial particles, and that nitro-aërial spirit resides in them as in a fitting subject ; although I shall endeavour to show below that the heat produced by the fermentation of contrary salts depends also upon nitro-aërial particles supplied by the air.

Further, with respect to the heat which arises in solid bodies when rubbed together, it is probably due to nitro-aërial particles residing in those bodies and thrown into motion by vehement rubbing; for the more solid kinds of wood appear to be rich in nitrous particles as has been shown above. Nay, that solidity and even rigidity are caused by nitro-aërial particles I shall endeavour to show below. Lastly, with respect to blood, and quicklime, and other things of the same kind, it will be established below that their heat is produced by nitro-aërial spirit.

Lastly, we remark with regard to fermentation in general that nitro-aërial spirit will not ferment with sulphur unless the sulphur is to some extent fixed. For liquids which contain sulphureous particles highly exalted, such as spirit of wine, never effervesce even though exposed to the warmest air, while juice expressed from grapes and new ale (in which the sulphureous particles have not yet attained to vigour and inflammability) ferment of themselves. And indeed it is the saline particles in union with the sulphureous which seem to be the cause of everything becoming acid in fermentation as we have indicated above. For the saline particles which were previously wrapped up in the sulphureous particles, are afterwards by fermentation set free from their fellowship, and besides brought into a fluid condition as described above.

But although nitro-aërial spirit does not attack highly exalted sulphureous particles, yet sulphureous particles, when in the greatest vigour and agitation, attack nitro-aërial spirit and throw it into a very swift and fiery motion. For, as it is necessary that sulphur should be to some extent in a state of fixation in order to produce in substances the more sluggish movement of fermentation, so, on the other hand, for an effervescence exceedingly intense and igneous, it appears to be necessary that nitro-aërial spirit should exist in a somewhat fixed condition, but the sulphureous particles in a state of vigour and motion, as we have indicated above.

CHAPTER VI

OF NITRO-AËRIAL SPIRIT IN SO FAR AS IT PRODUCES RIGIDITY IN BODIES, AND THE POWER OF RESILIENCE. ALSO OF THE CAUSE OF ELASTICITY. INCIDENTALLY OF THE BREAKING OF GLASS DROPS

Thus far we have treated of nitro-aërial spirit in its state of motion and vigour ; it remains to contemplate it in a state of rest. As the nitro-aërial particles in a state of motion are the cause of nearly all natural movements—so, on the other hand, they are the cause, I think, of rigidity and the power of resilience, when quiescent and securely fixed in the pores of bodies.

For in order that glass or iron and the like may become rigid and acquire the power of resilience they must be made to glow in a very hot fire, and then be quickly cooled by being plunged at once into cold water, that so the nitro-aërial particles conveyed by the fire to the said substances may be obstructed in their movement by encountering cold, and secured more firmly in the structure of these substances. For the same nitro-aërial particles which, when whirled round and hot, separated from each other the particles of these glowing substances and opened up their structure, now, when they cease to move in consequence of encountering cold, are fixed like wedges or very solid spikelets in their pores. Things are hardened by them when fixed in this manner, and indeed cold seems to close the pores of things in this way only.

But that nitro-aërial particles reside in iron made rigid in this way, is evident from the fact, that this iron acquires the property of giving out fire when struck by a flint. For we must suppose that the sparks struck out from steel are caused by igneo-nitrous particles of the steel bursting forth with extreme velocity on account of the violence of the blow. Indeed we perceive that the fire of these sparks is very like burning nitre, and that they burn very readily though struck out from the steel in a place where there is no air—which is certainly a clear proof that there are nitro-aërial particles in steel. So that to strike fire from it there is no need—as in other cases where fire is produced—for nitro-aërial particles to be supplied from the air. It corroborates this view that if heated iron cools slowly, the igneo-nitrous particles gradually extricate themselves and escape through the open pores of the iron (for these are not, as in the previous case, contracted by the cold); so that the iron, from want of igneo-nitrous particles, becomes less rigid, and unfit to give out fire when struck. And what the ingenious Dr R. Hooke has set down in his *Micrographia* is not opposed to this—to wit, that the sparks of steel, after their extinction, are nothing but small globules or minute vitrified bits of steel. For it must not on that account be thought that little morsels of metal take the form of flame when greatly agitated by a very violent blow, but rather that the igneo-aërial particles hidden in the pores of the iron are excited by the violence of the blow into a really fiery motion, and that a little bit of the iron is melted by them and changed into a sort of glass.

As the rigidity of iron and glass and similar substances arises from nitro-aërial particles imparted to them by fire, so probably the rigidity of frozen water is also caused by nitro-aërial particles which, from being fixed like pegs between the aqueous particles, arrest their fluid movement and press them together. For as in fire nitro-aërial particles whirled round with swiftest motion disturb the particles of the substances in which they exist and break them into minute parts, so on the contrary in the cold, they, set up as spikes, fasten like wedges among the particles of bodies and cause them to become rigid, as has been said above.

That nitro-aërial and igneous particles in a state of rest produce rigidity and cold may be inferred from the case of nitre itself in which the nitro-aërial particles become extremely cold, and when mixed with vinous liquids almost freeze them, and yet if they are agitated by sulphureous particles they become in fact fiery, as happens in the ignition of nitre. And hence it is that if the hand or other member when stiff with cold be brought near the fire it is hurt as if by fire and even destroyed, for the nitro-aërial particles which freeze as it were the chilly part in which they are fixed, assume also a fiery nature when agitated by the heat of the fire and burn it. So that without doing violence to language cold may properly be said to burn.

In the foregoing we must seek for the reason why water that has been boiled freezes sooner, as some suppose, on being exposed to cold. For nitro-aërial particles derived from the fire abound in boiling water, and these when they cease to move on exposure to cold no longer agitate the aqueous particles but fix and freeze them. For the case here does not seem to be very different from that of glowing iron being plunged into cold water, for the iron, cooling rapidly, becomes more rigid and so to say firmly frozen. And indeed in my opinion frozen water differs from hardened iron chiefly in this, that the branching particles of the iron adhere firmly to each other as though they were joined by clasping hooks, so that the nitro-aërial spicules are more closely interlaced with them.

Hence too the reason is obvious why soil that has been bound fast in the ice of winter becomes more fertile in the following spring. Doubtless the nitro-aërial particles from which when closely fixed in its structure the freezing of the soil results, produce when set in motion afterwards by the warmth of spring that effervescence in the bosom of the earth to which the generation of all-fertilising nitre and the growth of plants are due, as is shown elsewhere.

Further, that water is frozen by nitro-aërial particles fixed in it seems also to be confirmed by the fact that when frozen it is rarified and expanded. I am aware that the ingenious Descartes gives a different explanation of the rarefaction of frozen water. In fact that eminent man supposes that the aqueous particles when less disturbed by subtle matter cease to move and become somewhat curved, from which it results that they cannot then contract themselves into so narrow a space as before, when the subtle matter having power enough to bend them as it pleased was always adapting their forms to the measure of the places in which they were. But in fact what should force the aqueous particles set in motion by the subtle matter into other situations, once they have adapted their forms to the dimensions of the places in which they are? For they should rather retain the same situation by ceasing to move. For particles adapted to any space would by no means change their position and recede from each other with conspicuous force (as happens with frozen water, which breaks through the strongest glass), unless some force were applied to those particles.

It is probable then that nitro-aërial particles enter the pores of the water pointwise and like wedges draw them somewhat apart from one another so that the mass of that water has to dilate and swell, the aqueous particles meanwhile passing from a flexible to a rigid condition owing to the nitro-aërial particles which are fixed in them, so that they no longer flow hither and thither but are joined and, by cohering firmly together, constitute a solid body.

We note here in passing that as nitro-aërial particles arrest the movement of aqueous particles and freeze them, so nitro-aërial particles when secured in their turn among aqueous particles are as it were fettered and fixed. And this seems to be the reason why water is specially adapted for extinguishing flame; while yet, on the other hand, sulphureous and nitro-aërial particles mutually agitate and repel each other. And hence it is that spirituous liquids which contain volatile sulphur are never frozen. For if wine be exposed to the cold of winter all the spirituous and sulphureous particles of the wine will be driven into the mid-liquid and only the aqueous particles circulating round them will be frozen. Indeed the nitro-aërial particles ward off and repel as much as possible the sulphureous particles and surround them, when driven into the mid-liquid, as it were with a hostile blockade.

OF THE RATIONAL CAUSE OF ELASTICITY

So much then for the cause of rigidity. It remains to inquire why rigid bodies when bent spring back of themselves to their original shape—for in this lies the explanation of elasticity. But to pave the way to our view of the subject the following must be premised.

In the first place, let us consider in how many ways rigid bodies can be bent, and what sort of change as to their shape they undergo when bent. Suppose then a rigid body with its sides equal and parallel such as that delineated in Plate I., Fig. 4.

In the first place, this rigid body can be bent by elongating its convex surface while the concave surface remains of the same length as before inflexion, as is shown in Plate I., Fig. 5, where let *a*, *c*, *b*, *n*, *d*, be the bent rigid body whose concave surface *a*, *c*, is supposed to be equal to the length of the rigid body before inflexion but whose convex surface *b*, *n*, *d*, is elongated by as much as the line *b*, *n*, *d*, is longer than the line *a*, *c*.

The second mode of bending a rigid body is that in which the convex surface is drawn inwards towards the concave surface—all the surfaces of the rigid body retaining meanwhile their original length as is shown in Plate I., Fig. 5, where let *a*, *c*, – *b*, *e*, *d*, be the bent rigid body whose convex surface *b*, *e*, *d*, we suppose, while the rigid body is bent, to be brought just so far inwards as to be equal to the concave surface *a*, *c*, that is to the length of the rigid body before inflexion. And indeed if all the sides of rigid bodies are to retain when bent their original length, they can be bent in no other way than by approximating their convex and concave surfaces.

The third mode of bending a rigid body is that in which the planes at its extremities are turned towards each other and also elongated, while the concave and convex surfaces retain their original length as in Plate I., Fig. 6, in which let *a*, *c*, *b*, *d*, be the bent rigid body whose convex surface *b*, *d*, I suppose to be equal to the line *i*, *i*, or what is the same thing to the length of the rigid body before it was bent. Then the planes at its extremities *a*, *b*, and *c*, *d*, must be turned towards each other and elongated as is clear from the figure. For these end planes are inclined at the angle *b*, *i*, *e*, and are elongated by as much as the plane *b*, *i*, is longer than the plane *e*, *i*.

Lastly, a rigid body can be bent by shortening its concave surface while its convex surface and also end planes remain of the same length as before; as may be seen in Plate I., Fig. 7, where let *a*, *c*, *b*, *d*, be the bent rigid body whose concave side *a*, *c*, I suppose before the inflexion equal to the line between the extremities *e*, *e*. But now when the rigid body is bent that surface is shortened by the difference between *e*, *a*, and *e*, *c*. But we suppose the convex surface *b*, *d*, to retain its original length, or what is the same thing to be equal to the line between the extremities *e*, *e*.

But these observations regarding the various modes of bending a rigid body will be better understood from the following example. At the ends of a flexible rod, let two other shorter rods also flexible be fixed perpendicularly, as in Plate I., Fig. 8. Then let a string attached to the end of one of the rods be passed through a hole in the end of the other, as is seen in the same figure. Then the rod with the two small rods and the attached string will represent the sides of the rigid body delineated in Fig. 4. But now if the rod be bent so as to represent the concave surface of the rigid body while the string represents the convex, and the string meanwhile has been loosened at one of its ends so that it can be lengthened as the rod bends so as to remain parallel to it, then the bent rod with the string parallel to it and the rods at its ends will represent the rigid body bent in the first manner and delineated in Fig 5.

But if the string is fastened at each end so that it cannot be lengthened when the rod is bent, you will see that the string which represents the convex surface of the rigid body passes inwards and is drawn towards the stick or concave surface exactly like the rigid body bent in the second manner and delineated in Plate I., Fig. 5.

Further, if the string be hindered by any force from being drawn inwardly when the rod is bent, then the two little rods which represent the end planes of the rigid body will be drawn inwards towards each other, as is the case in a rigid body bent in the third manner and delineated in Fig. 6. But in order that the rigid body may be exhibited as it is bent in the said figure, the rods at the end should not only be drawn towards each other but also lengthened.

Finally, if the rod be bent towards the string so as to represent the convex side of the bent rigid body, and the cord which now represents the concave side be meanwhile shortened, but in such a way as to remain parallel to the rod, you will have a representation of a rigid body as it is bent in the last mode in Fig. 7.

We have next to remark with respect to rigid bodies that their branching parts are so mutually interlaced and so firmly compacted that the rigid bodies can neither be lengthened nor shortened without being ruptured. Indeed rigid bodies seem to acquire a nature of this kind because their pores are crammed and wholly filled up with certain particles fixed in them, so that the shape of their pores can by no force be altered, for in order that anything may be elongated by extension or shortened by compression its pores must change to a more oblong shape. For example, let us put before our eyes a row of parts or branchlets linked together such as is delineated in Plate I., Fig. 1. If this is to be lengthened or shortened it will be necessary for the pores of the said branchlets to become more oblong, as is manifest in Plate I., Figs. 2 and 3, the first of which represents the pores of the elongated and the second of the shortened branchlets changed in both cases, though in a different manner, into a more oblong shape. If these pores, however, be completely filled with any solid particles, then in this case neither the branchlets nor their pores could have their length extended. And indeed it is probable that nitro-aërial particles are fixed like little pegs in the pores of rigid bodies and fill them up, since it is by these being fixed in the pores of bodies that rigidity is produced, as was previously shown. And hence it comes about that neither the pores of rigid bodies nor consequently the rigid bodies themselves can be either elongated or shortened. And from this we may infer that when perfectly rigid bodies are bent they remain on every side of the same length as before.

These things being assumed, it follows that the convex surface of a perfectly rigid body will in bending be drawn towards the concave surface as is the case when a rigid body is bent in the second manner. For if it were bent in any other way some one of its surfaces would have to be either elongated or contracted, as is evident from what has been said. But it is implied in our idea of a perfectly rigid body that none of its sides can be lengthened or shortened.

We may gather from what has been said that the matter of a rigid body undergoes when bent a notable compression—and that, especially, at the middle, as is obvious from Plate I., Fig. 5. For when the convex surface *b*, *e*, *d*, of the bent rigid body has been brought inwards towards the concave surface *a*, *c*, the matter of the rigid body at *e* must suffer a notable compression. Hence the reason is obvious why rigid bodies when bent too much usually break near the middle. Just as in making bows this alone is attended to that their middle part be thick and strong enough, while it is of no consequence if the ends are thinner.

Indeed if a rigid body be so compact and solid that there are no interstices between its parts, and it cannot, in consequence, be compressed into less space, then clearly such a rigid body cannot be bent but will rather break. For it should be observed that rigid bodies which can be bent, although their outer surfaces are extremely solid and firmly compacted, have very many little spaces in their interior, as is clearly the case in iron and glass, which are specially rigid. For when glowing iron or glass is plunged into water, that they may cool quickly and become rigid, their outer parts cool sooner than their inner, and on that account become more rigid and solid. For the nitro-aërial particles which are in the still heated interior open little spaces here and there, in order to continue their motions, and when they gradually extricate themselves outwards they are detained in the now cooled surface and render it exceedingly solid, although very many empty little spaces are left in the interior.

Thus far then of bodies of the greatest rigidity—that is bodies whose sides can neither be lengthened nor shortened—but since, as is probable, there is nothing so perfectly rigid, we must suppose that, in the bending of such rigid bodies as we know, not only does the convex side come nearer to the concave, but that also the planes at the ends incline to each other in the manner already described, also that the convex side is a little elongated, and lastly that the concave side is just such a little shortened. For the force by which a rigid body is bent tends to produce all these results. For I think it is the case always that if a force seeks to effect something and there are various ways in which it may attain the result, while yet there is in all of them great resistance and difficulty, the force I say endeavours to perform the thing in these several ways. Hence since a rigid body can be bent, as was previously shown, either by the approximation of the convex to the concave side, or by the elongation of the one or the shortening of the other, or lastly by the inclination to each other of the end planes; and since there is at the same time great resistance to the bending of the rigid body by any of the methods (for the matter of a rigid body is so dense and compact, that its sides cannot without difficulty be drawn towards each other; its sides also are so firm and solid that they cannot be lengthened or shortened without great pressure), hence I say the force by which one tries to bend a rigid body has enough to do in effecting its purpose in those several ways. Still, however, while the rigid body is bending at the same time in all of these ways, its matter suffers a notable compression, especially at the middle, as is evident from what has been said. It should also be remarked here that when the force by which a rigid body is bent tends to draw the convex surface towards the concave, it tends also by compressing the matter of the rigid body to protrude it towards the sides. Whence it results that as rigid bodies on bending are attenuated as to their thickness, so on the other hand they are somewhat increased in breadth.

Since in bending rigid bodies the convex side comes thus to be lengthened and the concave to be shortened, the result is that the thinner rigid bodies are, the more and the more easily can they be bent, for although glass is very fragile and can scarcely be bent, yet fine threads of it can be wound round a bobbin and tied in a knot. But that the reason of this difference may be understood, let *a*, *c*, *b*, *d*, in Plate I., Fig. 9, be a very slender rigid body whose convex and concave surfaces were equal before inflexion—but now that it is bent, the convex surface *b*, *d*, is a little elongated. Let us suppose that two points *e*, *e*, are so placed in the convex surface that the line between the limits *e*, *e*, is equal to the concave surface *a*, *c*, which we suppose to be equal to the length of the rigid body before inflexion. But now if the convex surface of the bent rigid body be lengthened out at both ends, at one end from *e* to *b* at the other from *e* to *d* as is done in the figure, then there is no need for this surface being drawn inwardly when the rigid body is bent—nor consequently that its matter should be compressed. And yet this is inevitable in rigid bodies whose surfaces cannot be elongated, as we have already shown.

Further, if we suppose also that the concave surface of the rigid body *a*, *c*, is shortened as much proportionally as the convex surface is lengthened (for it should be observed that the force by which a rigid body is bent tends as has been shown above not only to draw out the convex surface but also to contract the concave)—say that the concave surface at each end is contracted to *n*, and the convex lengthened from *e* to *i*—while the rigid body is thus bending there is no need that the convex surface should move inwards, and so the matter of a rigid body thus bent will suffer no compression. And yet the convex surface is diminished a half less in this case than in the preceding.

And now we remark that in the case of a very slender rigid body such as glass threads, whose surfaces are much nearer each other than in the figure, the contraction of the concave surface and the elongation of the convex are extremely small. And hence it is that the more slender rigid bodies are very easily bent; for if their sides, as is the case here, be but a little lengthened or shortened their matter will scarcely be compressed at all. On the other hand, let *a*, *c*, *f*, *g*, be a bent rigid body twice as thick as the former. If in bending this body, its convex surface is not to be drawn inwards, nor its matter compressed, the elongation of the convex surface and the shortening of the concave must be much greater than in the previous case. For the elongation of the convex surface has to be as great at each end as is the distance *f*, *o*, and *g*, *o*, or at least half that distance. For we suppose the line between the limits *o*, *o*, to be equal to the line *a*, *c*, or what is the same thing to the length of the rigid body before inflexion. Now this can be easily illustrated by means of the instrument delineated in Fig. 8. For if the string of that instrument be placed near the rod, so that the apparatus may represent a somewhat slender rigid body, then if the string be lengthened out a little, while the rod is bent, it will remain always parallel to the rod and will not be constrained to move inwards, and yet if the string be at a greater distance from the rod you will see that the string, unless it be drawn out much more while the rod is bending, will be drawn downwards towards the rod. But indeed since the rigid body is as solid and compact in respect to its external parts as the more slender rigid body or even more so (for the exterior parts of a thick rigid body, be it glass or iron, are more quickly cooled than the interior, so that it is extremely solid as respects its external surfaces—in so far as the nitro-aërial particles in endeavouring to escape are detained in the exterior parts which have now cooled down, and are fixed there as we have already shown—but this does not take place in a more slender rigid body, since all its parts are cooled at nearly the same moment), hence I say it results that the convex surface of a thicker rigid body cannot bear to be drawn out as far as is necessary for bending it without compression of its parts, so that it is now necessary that the convex surface of the shortened rigid body should also go inwards and make, say the line *h*, *m*, *h*, in the figure; and that cannot be done without notable compression. And the further the two surfaces are from one another, so much the more must the convex surface pass inward during the bending, and consequently the matter of the rigid body will suffer the greater compression; so that very thick rigid bodies cannot be bent. And thus it is that a broad and thin plate is easily bent so far as regards the broad surfaces which are near each other, while as regards the lateral surfaces which are much farther apart it cannot be bent.

While we thus maintain that the power of recoil in rigid bodies should be ascribed to the compression of their matter, I would not be understood as thinking that matter thus compressed endeavoured to extend itself, for that would be to assume elasticity but not to explain it, and any one would be ready to ask: whence arises the power of recoil in the compressed matter? But with a view to a further conjecture on this extremely recondite subject I think it should first be maintained in regard to motion in general that it can be produced in no other way than by impulse. For as regards a natural inclination of inanimate things by which (in popular belief) they begin this or that movement spontaneously, I simply cannot understand it. For I do not know how an elective movement of that kind can exist without intelligence or at least sensation. Our opinion indeed is that inanimate things have no inclination, but that instead of it there is that power merely by which every thing remains always as far as may be in the same state. But a power of this kind implies nothing more than that inanimate things are unable to dispose of themselves or to alter their state, but are altogether dependent upon other things. Whenever then a body is at rest nothing else can be imagined than that it will remain for ever in a state of rest, unless as the ingenious Descartes has remarked a force is introduced from some other thing. Wherefore elasticity and gravity from which spontaneous movements, as they are usually called, arise, must be supposed to be due to impact of something invisible. But since such things as are possessed of elastic force and gravity are always ready to move, provided there is nothing to hinder their motion, it seems that we should certainly conclude that there is some kind of matter which being in constant agitation always strikes the said things in its motion and tries to move them. It is long since Descartes drew attention to such perpetually moving matter, and indeed there can be no doubt whatever regarding its existence. For I cannot conceive how sound, and light, and the like, are propagated where there is no air unless there exists some fine matter by means of which impulses and movements of such kind are kept up. Should any one ask here what it is which perpetually agitates that subtle matter, I answer that it was set in motion when first created and that there is nothing that can hinder its motion. For we must imagine that it has no weight at all to stop its motion, but that it is rather probable that the weight of bodies is due to its impulse. Nor is this subtle matter impeded in its motion by meeting with other bodies, since it must be supposed so thin, and smooth, and solid as either to pass with ease through the pores of bodies or to be reflected with its motion unaffected when it impinges upon their solid particles. For it is impossible that this very subtle matter should strike against any soft body. For softness implies a great number of particles in a loose state of union, but this matter is so fine that it cannot at a time touch several particles, and so can only strike one, and that is hard.

Let us then suppose that this subtle matter occupies little spaces here and there interspersed among the particles of rigid bodies and sets up its motion of circumgyration in these pores without hindrance of any kind. For it is to be observed that the matter of rigid bodies, of whatever sort it be, was at one time soft, tender, and to some extent fluid, so that the subtle matter was able from the beginning to open out little spaces in which to set up its motion. But now when the rigid bodies are bent and their matter suffers compression, the pores and little spaces of the bent rigid body are necessarily somewhat contracted, so that the subtle matter is unable to describe its circles in these now contracted little spaces, and therefore it strikes and impels any particles of the rigid body which have been pushed into the spaces where it moves. Meanwhile the subtle matter is reflected from the parts which it strikes, and dashes instantly against the particles opposite, to be repelled anew. And so the subtle matter strikes incessantly the parts of the rigid body, and its particles which are almost infinite in number and distributed through the whole mass of the rigid body all co-operate to restore to their original shape the pores of the rigid body and consequently the rigid body itself. And in this effort the explanation of elasticity seems to lie.

We remark here in passing that the force with which a string that has been violently stretched contracts to its original length arises from the same cause. For although the string itself may be a loose body, yet its minute fibres are somewhat rigid and their shape alters with the extension of the string. But the movement of the contracting string is caused by the effort of its fibres to revert to their original form.

From what has been said, we may conjecture why the glass globules with a sharp beak attached to them, commonly called glass drops, which are formed by dropping a little molten glass into cold water, burst with remarkable and almost explosive violence into the minutest fragments if the fine end of the beak is broken. Some imagine that the violence with which the drops burst is caused by the bending and tension of the parts of the glass. But it is hard to conceive how the parts of the said glass can be in a state of tension, since to produce the tension from which elastic force results it is an indispensable requisite that the rigid body after it has cooled should be bent by some force or, what amounts to the same thing, should be stretched. For in whatever way the heated rigid body or its parts may be bent or distended while cooling, they will never be under tension unless their form be altered by some force after they have cooled.

But although I do not think that the parts of the rigid body are under tension, still it is probable that that violence with which the glass drops burst is truly elastic, and that it results as elastic force does from the impulse of the subtle matter. And it is probable that this elastic force arises in the following way. When the small portion of glass glows and is in a sense fused, its structure is opened to such an extent by the rapid movement of nitro-aërial and fiery particles, that space enough exists in it for the nitro-aërial particles and besides for the subtle matter to execute their movements. But when the molten glass is dropped into cold water the fiery particles crowded at its surface are immediately arrested in their motion when they meet with the water particles, and those in the interior also soon desist from moving. But it should now be noticed that when the glass is cooled in this manner its parts settle down and it becomes itself contracted, not because these parts of the glass spontaneously approach each other (for the parts not yet cooled are not under tension and therefore do not possess a power of recoil or a motion of restitution, as we have shown above), but it is rather to be supposed that the subsidence of the vitreous parts arises from this, that the nitro-aërial particles occupy less space when they cease from their fiery motion and no longer push the parts of the glass away from each other, so that the particles of the glass are forced towards each other by the pressure of the atmosphere.

But, now, since the outer surface of the said glass immediately becomes rigid from being rapidly cooled by the water, it becomes so solid by reason of the nitro-aërial particles infixed in it, that the nitro-aërial particles and the subtle matter existing in the interior cannot as in other cases extricate themselves. Hence it is that in glass contracted in this way the subtle matter imprisoned among the minutest particles of the glass has not room as before for freely continuing its movements. For resistance is made to the movement of the subtle matter, from two causes; first, because the particles of the glass being at rest do not yield to the movement of the subtle matter as before; and secondly, because the particles of the glass in subsiding under the pressure of the atmosphere are driven into the little spaces in which the perpetually agitated matter carries out its motion. Hence that matter strikes the particles of glass which oppose its motion and endeavours to part them from each other. Indeed the case is very much as if the glass were bent almost to breaking; in fact it is to be observed that the pulsation of the subtle matter is almost capable of breaking the said glasses, as is evident from the fact that when drops of molten glass are dropped into water most of them immediately burst asunder; and indeed we must suppose that all the rest just escape being broken.

Wherefore when the beak of this kind of glass is broken, the force of the subtle matter, aided somewhat by the concussion of the whole glass caused by the fracture of the beak, is now able to draw the particles of the glass apart and to thrust them out with violence. I add further that when the glass is broken in any part, the particles of glass which were previously confined by its extremely solid surface, rush out by the open door at the very moment when the effort of the subtle matter begins to take effect, and by this no small addition is made to its force. And, finally, let us further consider that the pointed beak of the glass must be bent before it is broken ; whence it follows that the matter contained in the beak undergoes compression anew, as was shown above to happen when rigid bodies are bent. Hence the subtle matter, compressed by the bending of the beak, strikes against all the adjoining particles of the glass ; but since the external surface of the glass is more compact and solid than its interior parts (for when these globules are formed, the heated and melted glass is dropped into cold water, so that the external surface is cooled quickly by the water while the internal parts cool more slowly), it comes to pass that the compressed subtle matter can more easily make a way for itself into the globular part of the glass, as being less compact, than break through the more solid surface of the glass. And this may also be inferred from the fact that the glass beak can be bent much more and is broken with greater difficulty than glass under other conditions. And the reason of this seems to be that the subtle matter which, when compressed under other conditions and about to make its escape, bursts through the particles of the bent glass and drives them out with violence, now takes its way into the globular part of the glass (the pyramidal shape of the glass contributing not a little to this) : but the particles of the subtle matter when pushed from the beak into the globular part of the glass, effect a greater compression there, and in consequence the whole glass is violently and most minutely fractured. For as under other conditions, if glass or any rigid body is broken, the parts about the middle, where the matter is most compressed, are broken into small pieces and fly asunder, so the matter in the glasses here discussed, being everywhere compressed, bursts all over.

CHAPTER VII

THAT THE ELASTIC POWER OF AIR IS DUE TO NITRO-AËRIAL SPIRIT; ALSO OF THE MANNER IN WHICH AIR IS IMPREGNATED ANEW WITH NITRO-AËRIAL PARTICLES; INCIDENTALLY OF THE ELEMENTS OF FIRE AND OF COLD

The experiments of Boyle have proved beyond doubt that air is eminently elastic and therefore spreads and expands immensely when relieved from the pressure of the atmosphere. But it is not so clear to what cause the elastic force of the air is due. I shall, however, state briefly the result of my reflections on this recondite subject.

In the first place, then, I take it for granted that the air contains certain particles termed by us elsewhere nitro-aërial which are absolutely indispensable for the production of fire, and that these in the burning of flame are drawn from the air and removed, so that the latter when deprived of these particles ceases to be fit for supporting fire, as has been shown above.

It must also be admitted that the elastic force of the air is due to the same aërial particles as those by which flame is supported—an inference which we deduce from the fact that air deprived of these nitro-aërial particles loses elastic force, as will be established by what follows.

For firstly we have to note, what almost everybody knows, that if a cupping-glass filled with flame be applied to the skin, the flame will soon go out and the space within the cupping-glass will be almost empty, and as a consequence the skin will be driven into the hollow of the cupping-glass by the pressure of the surrounding air. But now let us inquire why the space within the cupping-glass becomes almost empty immediately upon the extinction of the flame. One might readily say here that the fiery and aërial particles are agitated in the flame with a very rapid motion and are much rarefied, but that after the extinction of the flame they cease from their movement and are condensed, so that these particles are no longer able to resist the pressure of the surrounding air. But this answer does not seem quite satisfactory, for it is probable that air is largely mixed with the flame, since it supplies it with nutriment, so that not even the smallest part of the flame is altogether destitute of air. But if the air were distributed abundantly enough in the flame, it does not appear that it would be condensed after the extinction of the flame to the extent required for rendering the space in the cupping-glass so empty.

Wherefore I think it should be maintained that the air mixed with the flame is, by the burning of the flame, quickly deprived of its nitro-aërial and elastic particles, so that this air not only becomes unfit for sustaining fire but also loses in part its elasticity. Hence when a flame enclosed in a glass vessel has exhausted the nitro-aërial particles of the air, it soon goes out and the space contained within is like a vacuum, not only on account of the diminished motion of the igneous particles, but partly also from the lack of elastic particles, as will appear more evident from the following experiments.

For instance, let a burning candle be placed in water so that the wick may stand about six finger-breadths above the water, and then let an inverted cupping-glass of sufficient height be put over the light and plunged immediately into the water surrounding the light, as is shown in Plate V., Fig. 1. Care, however, must be taken that the surface of the water enclosed within the glass be at the same level as the water without. But that this may be attained in the present experiment, and also in those that follow, let one leg of an inverted syphon be enclosed within the cavity of the cupping-glass before it is put into the water while the other leg projects outside, yet so that the end of each leg may be above the surface of the water, as is seen in the said figure. The use of the syphon is to enable the air enclosed in the alembic, and compressed by the underlying water while the glass is being let down into the water, to pass out through the cavity of the syphon, so that the water within may not be depressed below the level of the water outside, as it would otherwise be. But when the air ceases to pass through the syphon (which will happen almost in an instant) the syphon should be at once withdrawn, that the air may not afterwards rush through it into the glass. When these arrangements are made let the cupping-glass be firmly fixed so that it may descend no further into the water, and you will presently see, while the light still burns, the water rising gradually into the cavity of the cupping-glass.

I will not deny that the ascent of the water arises in part from the circumstance that when the light is about to expire, the air enclosed in the cupping-glass is less agitated and rarefied by the igneous particles than formerly. But the rise of the water into the glass must not be ascribed to this cause alone, since it is partly due to this, that the lamp enclosed in the glass is, by its own burning, deprived of nitro-aërial and elastic particles so that the air there is not able as before to resist the pressure of the atmosphere. And this will be further confirmed by the following experiment.

For let any combustible material which will readily take fire be suspended in an inverted cupping-glass as large as can be had, as is shown in Plate V., Fig. 1 (I am myself in the habit of suspending a bit of camphor to which a small piece of linen, charred in the usual way into tinder and dipped in melted sulphur, is attached). When this has been done, let the inverted cupping-glass be immersed in the water about ten finger-breadths so that the water enclosed in the glass may be at the same level as the water outside, which can be done, easily enough, by means of the bent syphon already described ; and lastly, let the water outside be drawn off until the level of the water within is higher than that of the water outside, so that it may be more distinctly seen, or better, let the cupping-glass be transferred to another and shallower vessel by placing under it a small vessel, big enough, however, to receive the mouth of the cupping-glass, and then transferring the small vessel filled with water, together with the cupping-glass resting upon it, into a suitable vessel almost full of water. And let the cupping-glass remain there until the air heated by the hands of the operator has been condensed to its original state. And then, lastly, let the height of the water within be noted by papers affixed here and there to the sides of the glass by means of a paste made of barley-meal boiled in water. Now let the cupping-glass be exposed to the rays of the sun and let the camphor or other combustible matter enclosed in it be kindled by means of a burning-glass, by first lighting the aforesaid sulphured linen placed under the combustible matter. When this has been done you will see the water within descend on account of the agitation of the fiery particles, and the rarefaction of the air inside. When the light has gone out let the cupping-glass and the small vessel on which it rests be removed from the sun's rays, that the air enclosed in it may cool again and return to its former condition, and then you will find that the water within has risen above the point marked at first. And indeed I have found by calculation that the air has been reduced in volume by about one-thirtieth by the burning of the light.

After the smoke of the burning light with which the cupping-glass was filled had entirely disappeared and the glass had become as bright within as at first, I tried to kindle the light in it a second time by throwing the solar rays upon another piece of camphor, suspended in the glass in the same way as before, but the experiment did not succeed—a sufficiently clear proof that the air had been, by the burning of the light, deprived of its igneo-aërial particles, so as to be quite unfit for sustaining flame anew. But lest any one should think that the light could not be kindled a second time in the glass, because the inner sides of the glass had been dimmed so much by the smoke of the light previously burned in it that the rays of light could not be transmitted through the glass with sufficient intensity, I fasten a piece of paper about a hand-breadth broad, with its margins all round coated with the aforesaid paste, to the inner side of the cupping-glass at the place where the solar rays are to be transmitted. When the fumes have entirely vanished this paper is to be pulled off, by a thread attached to it and extending outside the vessel, so that the solar rays may pass through the part of the glass which has been protected from the soot.

It is a further confirmation of our hypothesis that the air given out from the lungs of animals has its elastic force diminished in consequence of the loss of its nitro-aërial particles, as will be manifest from the following.

Let a moistened bladder be stretched over the circular orifice of any vessel and tied to it just as the skin of a drum is stretched; then let a small bell-jar in which a little animal, say a mouse, has been put, be accurately applied to the said bladder by placing a weight upon the jar lest the animal inside should upset it (as is shown in Plate V., Fig. 2). When things have been arranged in this manner it will in a short time be seen that the jar is firmly fixed to the bladder; and the bladder also, at the place where it lies under the jar, is forced upwards into the cavity of the glass just as if the jar had been applied with a flame enclosed in it. And this will take place while the animal is still breathing. Nay, if the jar be grasped by the hand and raised, the bladder, along with the vessel, will still adhere firmly to it unless the vessel is very heavy. And indeed a little animal placed in a cupping-glass which is to be fixed to the skin can supply to a small extent the place of the flame. And from this it is clear that the elastic power of the air enclosed in the aforesaid jar has been diminished by the breathing of the animal, so that it is no longer able to resist the pressure of the surrounding air.

But in order that this matter may be better understood, let me submit yet another experiment to the same effect—an experiment moreover from which it will be easy to perceive in what proportion the air is diminished as to its volume when deprived of vital particles by the breathing of the animal. Thus, let a small animal placed on a suitable support be enclosed in an inverted glass, or better, let the animal be put into a suitable cage and suspended in a glass jar just as the vessel is suspended in Plate V., Fig. 4. Then let the inverted glass be sunk a little into the water so that the water enclosed in the glass may stand at the same level as the water outside, as may be done by means of the bent syphon already described. When this is done let the water outside be drawn off a little in order that the height of the water within may be better observed. And let it be indicated by papers attached here and there to the sides of the glass. And so you will soon see the water sensibly rising into the cavity of the glass, although the heat produced by the presence of the animal in the glass, and also the breath proceeding from it, might be expected rather to produce an opposite effect.

But we can perceive in the following way the extent to which the air enclosed in the glass undergoes contraction before it becomes unsuitable for sustaining animal life. For let the space in the glass occupied by the air when the animal was at first placed in it and also the space occupied by the same air when the water has risen in the glass after the suffocation of the animal be measured, as can be done by pouring water into those spaces so as to fill them and measuring it—but warning should be given here in passing that when these spaces are thus measured everything should remain in the glass the same as before. And now let it be ascertained by calculation how much the first space is greater than the second. For to that extent the air is lessened as to its elastic force and volume by the breathing of the animal. And in fact I have ascertained from experiments with various animals that the air is reduced in volume by about one-fourteenth by the breathing of the animals. But care should be taken in making this experiment that the animal be placed only a little above the surface of the water, for a reason to be afterwards given.

From what has been said it is quite certain that animals in breathing draw from the air certain vital particles which are also elastic. So that there should be no doubt at all now that an aërial something absolutely necessary to life enters the blood of animals by means of respiration. And indeed if the necessity for breathing arose, as some have imagined, merely from this that the mass of the blood should be churned and divided into the most minute parts by the movement of the lungs, there would certainly be no reason why an animal, enclosed in a glass vessel in the manner described, should die so soon, because the air there avails as much after the death of the animal as before to inflate the lungs and consequently to churn the mass of the blood. For as that air is impelled by the pressure of nearly the whole atmosphere, there is nothing to hinder it from being urged into the dilated thorax of the animal, and on this the inflation of the lungs depends, as we have shown elsewhere.

There is now no reason therefore for denying the entrance of air into the blood because on account of the dulness of our senses the vessels by which it enters cannot be seen. For other ducts which serve to convey thicker liquids are not seen by the eye until their different capillaries, after a passage of some length, unite in a noticeable canal. For what keenness of vision has ever beheld the sources of the lymphatic or lacteal vessels or even of the veins? How much less may one discern these aërial ducts which must be very short and extremely small, for these ducts do not, like the others, run any considerable distance and at last join one another, but merely pass separately by a very short and obscure route through the membranes of the lungs; for that the aërial particles should be mixed with the blood in the minutest and most intimate way, it is necessary that they enter the blood by vessels or rather pores almost infinite in number, distributed, here and there, through the whole mass of the lungs. And yet in the lungs, when boiled and dissected, an almost infinite number of openings resembling most minute points are seen by the aid of the microscope. But whether these points are the mouths of capillary tracheæ, or of vessels opening into the blood, I cannot state with certainty.

Hence it is manifest that air is deprived of its elastic force by the breathing of animals very much in the same way as by the burning of flame. And indeed we must believe that animals and fire draw particles of the same kind from the air, as is further confirmed by the following experiment.

For let any animal be enclosed in a glass vessel along with a lamp so that the entrance of air from without is prevented, which is easily done if the orifice of the inverted glass be immersed in water in the manner already described. When this is done we shall soon see the lamp go out and the animal will not long survive the fatal torch. For I have ascertained by experiment that an animal enclosed in a glass vessel along with a lamp will not breathe much longer than half the time it would otherwise have lived.

Nor is there any reason for supposing that the animal is suffocated by the smoke of the lamp, for scarcely any smoke will emanate from it if spirit of wine is used, and indeed the animal will live in the glass for some time after the extinction of the lamp—that is, after the fumes have entirely disappeared—so that it is by no means to be supposed that it has been suffocated by the fumes of the lamp. But since the air enclosed in the glass is in part deprived of its nitro-aërial particles by the burning of the lamp, as has already been pointed out, it cannot support long the breathing of the animal, hence not only the lamp but also the animal soon expires for want of nitro-aërial particles.

But the reason why an animal can live for some time after the extinction of the lamp seems to be this. It is only by a continuous and moreover an abundant and rapid stream of nitro-aërial particles that a lamp is sustained. Consequently if the succession of nitro-aërial particles be but for a moment interrupted, or if they are not supplied in due abundance, the flame will immediately sink down and expire. Hence as soon as the nitro-aërial particles begin to come but sparsely and slowly to the flame it presently goes out. But a smaller ration of aërial nourishment and that introduced at intervals will suffice for animals; so that an animal can be sustained by the aërial particles remaining after the extinction of the flame. It supports this view that the movement of the subsiding lungs conduces not a little to draw in the aërial particles if any remain in the said glass and to carry them into the blood of the breathing animal. Hence it results that the animal does not die until the aërial particles have been entirely exhausted. And hence it is that the air in which an animal is suffocated is contracted in volume by more than twice as much as that in which a lamp goes out, as was formerly pointed out.

Further, having suspended combustible matter in a glass vessel beside an animal, I tried to ignite it by means of a burning-glass after the animal was suffocated, and that that might if possible succeed, I protected from the breath of the animal the side of the glass through which the solar rays were to be transmitted, by means of a piece of paper fixed to it as already described. But the experiment did not succeed. I shall not, however, make any certain pronouncement in regard to this, because wintry weather and a sky almost constantly wrapped in clouds prevented me from repeating the experiment. It is probable, however, that air which is unsuitable for supporting life is also incapable of producing flame, since a greater quantity of aërial particles is needed for the burning of a lamp than for sustaining life. But it is to be noted here that although flame and life are sustained by the same particles it is not on that account to be supposed that the mass of the blood is really on fire, as will be shown in the next chapter.

But assuredly difficulties by no means slight occur in connection with what has been said. For, in the first place, how should it be that an animal or a lamp enclosed in these glass vessels is unable to survive while a sufficient abundance of air is contained in them? For the water underneath ascends into a part only of these glasses and the remaining space is filled with air, and that air although diminished in volume is yet able to resist the pressure of the surrounding air.

Further, in what way shall we suppose that the air in the aforesaid glasses loses its elastic force? For we must believe that air is contained in them in undiminished quantity after the extinction of the flame and the death of the animal. For the aërial particles are not annihilated by the burning of the flame or the breathing of the animal. Nor are they driven out of the glass, for neither air nor any other elastic matter mixed with it is able to penetrate glass, as we have indicated above; for otherwise the pressure of the air in the glass could not be removed or diminished by any suction, inasmuch as the air or the elastic matter would immediately enter the glass from which the air had been exhausted and fill the space left by the air, especially since the pressure of the surrounding air assists towards their entrance.

Since then the air still remains in these vessels, shall we suppose that it has been condensed and that the ascent of the water results from this ? But neither is this the case, for we note that the water in the glass in which the light has gone out rises above its former level while the air is not yet completely cooled. And indeed the heat caused by the presence of the animal is fitted to produce rarefaction rather than condensation. Besides if the air underwent no other change than condensation only, there is no reason why the lamp or the animal should not have been sustained by it. Should any one happen to say that the elastic force of the air was diminished by the respiration of the animal because some part of the air entered the blood of the animal, I reply that the blood of the animal, when it was enclosed in the glass at first, contained an equal and even a larger supply than afterwards of aërial and elastic particles, and it therefore follows that some elastic particles must pass out from the blood of the animal into the glass *pari passu* with others that enter in, and consequently that elastic particles must be contained in the glasses after the animal has breathed for some time in it in no less abundance than before. Nay, even although more aërial matter should enter the blood of the animal than is given out from it, still it would continue to exist in the glasses, and in accordance with its elastic nature would occupy as much space as otherwise.

But, to make a conjecture on this difficult subject, let us consider in how many ways the elastic force of bodies may originate. And, in the first place, we notice that the particles of all bodies whatever, when set in motion, open out and seek to expand into a larger volume, inasmuch as they require more space than before for executing their motions. And indeed it is probable that some subtle and nimble matter interspersed with aërial particles and continually agitating them conduces not a little to the expansive force of the air. And in this way the elastic force of the air seems to be increased when heat is communicated to it, for the nitro-aërial particles (from whose agitation we have concluded elsewhere that heat arises) strike the aërial particles and keep them in motion; but these when moved tend to unfold. But indeed it is scarcely credible that the elasticity of air depends on this cause alone; because when the lamp or the animal is shut up in the aforesaid glasses, the aërial particles there, being heated by the flame or by the presence of the animal, would necessarily be set in motion, and therefore the elastic force of that air would be rather increased than diminished, if it resulted merely from the movements of aërial particles.

2. Elastic force, or the power of recoil, arises from the bending of rigid bodies inasmuch as these when bent strive to return to their original form. And indeed it is probable that the elasticity of the air results mainly from this very cause. It would certainly be reasonable to suppose that nitro-aërial and fiery particles are fixed in the aërial particles themselves and constitute the more active part of them. For although aërial particles are very minute and are commonly regarded as most simple and elementary, still it seems to me necessary to suppose that they are compound and that some of their parts are branchy and adhere firmly to each other as if by mutually clasping hooks; while others are extremely subtle, solid, smooth, agile, fiery and truly elementary, and that these when firmly fixed among the other particles make them rigid in much the same way as rigidity and elasticity are in-

duced in iron by nitro-aërial particles communicated to it from fire, as I previously endeavoured to show. I am also of opinion that the elastic force of the air consists in this that the particles of the air becoming rigid, and compressed and bent by the weight of the incumbent atmosphere, strive to spread themselves out.

Certainly the rigidity of the aërial particles seems to be the cause of their not entering the minute pores of bodies so readily as the grosser particles of watery liquids, as could be established by very many experiments. For although aërial particles are very minute, yet on account of their rigidity they cannot adapt their forms, like the flexible particles of watery liquids, to the tortuous passages of bodies. Hence also it seems to come about that water ascends in very minute glass tubes and also into the pores of a sponge and other things of that kind. For although rigid particles of air cannot enter extremely fine little pores of that sort, yet water is forced up into them as into an empty space by the pressure of the remaining air.

To this I add further that the rigidity of aërial particles appears to contribute not a little to the kindling of fire, inasmuch as the nitro-aërial particles on being violently torn from the particles of the air in which they were firmly fixed are thrown into very rapid motion, for otherwise I do not see how the nitro-aërial particles could begin so rapid a movement. But of this more will be said afterwards.

But now it is probable that aërial particles when mixed with flame lose their elasticity in the following manner. Thus we must suppose that the sulphureous particles of fire, when thrown into violent agitation, approach all the particles of air which are nearest them, and impinge on the nitro-aërial particles which the air contains and by their collision drive them forcibly out, and that at last from these, violently ejected and in vehement commotion, fire is produced, as will be shown more fully below.

Further, it is a reasonable supposition that the aërial particles, deprived in the manner aforesaid of nitro-aërial particles, become not only unfit for sustaining fire but also change from rigid to flexible and in consequence are deprived of their elasticity, for that the rigidity of aërial particles is due to nitro-aërial particles fixed in them, while their elasticity results from their rigidity, I have already endeavoured to show. Indeed aërial particles when passing out from flame appear to be in a condition very similar to that of a steel plate which is slowly cooled after it has been heated, for this also loses its elasticity as the fiery particles extricate themselves from its structure, and becomes moreover incapable of having, as before, sparks struck out of it by flint. And in fact fire seems to be nothing else than a collection of very minute sparks very densely struck out from aërial particles by the collision of sulphureous particles. For the case is very much as if we were to suppose that innumerable little particles of flint and steel collide at the same instant with each other. For as aërial particles are solid bodies and are rigid like steel plates, they seem to be fit enough for having fire struck out of them. Hence if the sulphureous particles are too volatile and fine the flame produced by them is very sluggish, such as is the flame of burning spirit of wine or the very feeble and almost harmless fires which are produced by the sulphureous effluviæ of animals, for since these extremely fine and volatile sulphureous particles only collide in a feeble and gentle manner with the aërial particles, they are scarcely able to strike effectively against the nitro-aërial particles and set them in igneous motion.

I add further in confirmation of what has been said, that the nitro-aërial particles to which the elastic force of the air is due are fixed in the aërial particles themselves and are torn from them by the burning of a lamp or by the breathing of animals; for that the nitro-aërial and elastic particles which are lacking in the afore-mentioned glass vessels are neither air itself nor some material interspersed among its particles, has been shown above, and therefore it must be concluded that the elastic particles are implanted in the particles of the air themselves and constitute their more active part, and that it is in fine because these are driven out from the aërial particles by the burning of fire or by the breathing of animals that air becomes quite effete and destitute of elastic force.

That the igneo-aërial particles are not air itself pure and simple, but only its more subtle part, may be inferred besides from this, that nitro-aërial particles, whatever they be, exist in nitre and constitute its fiery and aërial part, as was shown above. But who can imagine that air itself resides in such abundance in nitre as is required for its burning in a place void of air? If spirit of nitre be poured upon any fixed salt when taken fresh from the fire, nitre will be produced by their union; but it is not to be supposed that air is present in such quantity in either of these principles, nor can we believe that air coalesces along with these principles in the generation of nitre. Nor is it probable that air without any force applied to it would condense to such an extent as would have to be supposed in the case of nitre if its burning resulted from air residing in it. For to the production of so impetuous a flame as is produced by a small morsel of nitre, no mean supply of air is required, but that so much should be imprisoned in a small piece of nitre is very unlikely, especially as it is only such air as is possessed of a very high degree of elasticity that is suitable for the production of flame. But these points will be still further established by the following experiment.

For instance, let spirit of nitre and also salt of tartar, or any other fixed salt dissolved in a small quantity of distilled water, be placed in separate glass vessels and enclosed in another sufficiently large glass vessel from which the air is afterwards exhausted, as far as possible, by means of an air-pump (indeed in the experiment made by me the air was almost entirely pumped out). When this is done, if any aërial or elastic substance be present mixed with the aforesaid liquids, it will escape in the form of bubbles when the pressure of the ambient air is withdrawn. When the bubbles, if there be any, no longer escape from the liquids (for it is to be observed that from nearly every liquid when in a place void of air small bubbles are wont to rise) let the aforesaid liquids be mixed, and an intense effervescence will immediately be produced. Let everything remain in this condition until the action has entirely ceased, and then, lastly, let the mixture be removed and evaporated at a mild heat to the dryness of salt, and so at the bottom of the glass we shall find nitre generated in an airless place, which in accordance with the nature of nitre will, if placed on a burning coal burst into flame; and yet it is by no means to be supposed that air is present in nitre produced in this way. Further, if nitre itself dissolved in distilled water be put in a place empty of air, air in the form of bubbles scarcely escapes at all from the solution—certainly in a less degree than from common water—

a clear enough proof that air is not so densely enclosed in nitre.

It is thus evident that the igneo-aërial particles common to nitre and air are not air itself, but only certain very subtle particles which fixed in air and in nitre constitute their more active and fiery part. Indeed it is probable that igneo-aërial spirit is fixed in the saline particles of nitre very much in the same way as in the aërial particles, and that it is in consequence of their being violently torn from both kinds of particles and thrown into violent agitation that fire is produced.

It will not be difficult to understand from this hypothesis of ours why the water ascends in a glass in which a lamp or an animal is enclosed, although air exists in it in the same abundance as before, and there is no reason to suppose that it has condensed. For no other conception is possible than that the elastic force of the air has been diminished, and that this is due to a certain change wrought in the aërial particles themselves. But what that change should be, which diminishes the elastic force of the air, unless we suppose that the particles from being rigid become flexible, I confess that I do not understand.

Further, in what has been already said the reason is to be sought why lamp and animal when placed in the aforesaid glass vessels expire even when air in sufficient abundance seems to be contained in them. It must not be supposed here that of the air enclosed in those vessels a part has been entirely consumed while the rest remains unchanged, because if that were so there would be nothing to hinder the animal from still breathing in it. But it must rather be thought that nearly all the particles of the air have undergone some change, and that they have been deprived to such an extent of nitro-aërial particles that the air has become quite unfit to sustain life and flame. But then you will say that the air enclosed in the glasses is still possessed of sufficient elastic force to resist the pressure of the atmosphere, so that it would seem not to have been deprived of its nitro-aërial and elastic particles. And how then can it be that an animal or a lamp cannot be sustained by it? Nay, I have ascertained that the air in which an animal or a lamp has expired is possessed of no less elastic force than any other air, for when the pressure of the atmosphere is removed it expands with no less vigour than common air, as will be shown in Chapter X. But this seems flatly to contradict what has been said on this matter. The answer to be given to this difficulty is, I think, that the elastic force of the air referred to does not result from the elasticity of its aërial particles being as intense as that of unaltered air, but rather from this, that as aërial particles when deprived of nitro-aërial particles become less rigid, so they are also more bent by the pressure of the atmosphere; but a weaker rigid body, provided it has been greatly bent and stretched, will have no less elastic force than a stronger rigid body less bent by the very same force. It should be also noted that the elastic force of the said air results in part also from this, that as that air deprived of elastic particles is reduced to narrower space, aërial particles in air of this sort are aggregated in greater abundance and more densely than in common air.

Here we remark in passing that if the elastic force of the air resulted from certain agile particles interspersed in it, and if these were exhausted by the breathing of the animal or the burning of the lamp, then air in which an animal or a lamp has been enclosed would by no means expand with as much force as unaltered air. So that even by this it is clearly proved that aërial particles are altered in some way by the breathing of an animal or the burning of a lamp, and that they contract in consequence into less space, as was previously said.

From this it appears to be established that aërial particles are not fit to sustain fire and life unless they possess a certain degree of elasticity and rigidity, since, in so far as they are less rigid, they do not contain nitro-aërial particles in sufficient abundance nor can these be driven out or drawn out quickly enough.

It is also clear from the aforesaid hypothesis why air passes up in a continual stream to support combustion. For I do not think that this should be ascribed merely to the rarefaction of the air mixed with the flame : but because the aërial particles mixed with the flame are deprived of nitro-aërial particles, and therefore also of elasticity, it comes about that they are no longer able to resist the pressure of the ambient air. Hence such particles of air as are nearest press into the place of the ignited particles of the air and drive them upwards, since they have lost not only elasticity but also their former weight in consequence of the disruption from them of the extremely solid nitro-aërial particles. And thus one particle displaces another and the flame is renewed by a fresh access of air. The following experiment points also to the same conclusion, viz. :—If a small animal such as a mouse or a bird is enclosed in the manner aforesaid at the top of a glass vessel it will die much sooner, and the water underneath will rise much less than if the same animal had been placed in the lower part of the glass. This will be very manifest if two birds or two mice are enclosed at the same time, one in the upper and the other in the lower part of the glass ; for in this case the animal put in the lower part of the glass will for some time survive the other. It must not be supposed here that the vapours expired by the animal occupy the top of the glass and exclude the air from that part; for these vapours soon condense and adhere to the sides of the glass; for otherwise the underlying water would be depressed by them. But it would be reasonable to think that the particles of air expelled from the lungs of animals become lighter, because the nitro-aërial particles are in part removed from them, and that they rise in consequence to the top of the glass; and that, being more densely crowded there, they are capable of resisting the pressure of the air below and of excluding it, but are nevertheless unfit to sustain life; while at the same time the air at the bottom of the glass remains unchanged and the animal placed there is still able to breathe. It is also worthy of notice that when a small animal, say a mouse, is shut up in a glass and suffers from want of air, it turns its mouth hither and thither in an upward direction in quest of breath; but when it perceives that it suffers more there from want of breath it is wont to bring its mouth downwards, and when it gets a little refreshment there it pushes its mouth as far down as it can into the glass and keeps it there.

As an animal, so also a lamp expires sooner when placed in the upper than in the lower part of the glass, although this should perhaps be ascribed in part to the smoke which occupies the top of the glass. Nay, if an inverted bell-jar be suspended in the air and then a lamp from which scarcely any smoke proceeds be placed in it, you will soon observe the lamp going out, because the air contained in the glass is soon rendered incapable of sustaining fire on account of the burning of the lamp. But as it is lighter than the rest of the air, the surrounding air forces it upwards and does not easily permit it to descend out of the glass. So that it may be clearly inferred that air is deprived, by respiration and by the burning of fires, of certain solid and heavy particles, because it becomes lighter when it passes out from flame or from the lungs of animals.

Here one is led to admire the providence of the highest and best Artificer by whose most wise counsel it has been arranged that air, when deprived of its nitro-aërial particles and vital spirit, should lose at once its elasticity and its weight, so that it is borne aloft by the elastic force and pressure of the remaining air and fresh air comes in place of the effete; for otherwise there would be no society at all of men or even of animals, for we should be obliged to spend our lives single and separate, namely, where a ration of nitro-aërial spirit sufficient for sustaining life might be obtained for each. And indeed between mortals there would be perpetual strife about the acquisition and the determination of the boundaries not so much of fields as of tracts of air. Moreover the life of each would be a sort of perpetual pilgrimage, inasmuch as we should find it necessary to wander by night and by day, through the world and in desert places, not so much to gain wealth and foreign dainties as to hunt after aërial nourishment, and to banish ourselves far to avoid the popular breath. But how much better has our best Father consulted for us, who has fashioned this air which surrounds us with such skill, that nitro-aërial spirit, the most necessary Elixir of life, should come to us everywhere of its own accord—nay, even rush uninvited into our very mouths and inmost vitals.

HOW AIR WHEN DEPRIVED OF NITRO-AËRIAL PARTICLES IS SUPPLIED WITH THEM ANEW

Since aërial particles, whether by the burning of fires or the respiration of animals, are deprived in the manner aforesaid of nitro-aërial particles, let us consider how it is that air is not at last all consumed by the burning of fires and the breathing of animals, or at least rendered so effete as to be no longer able to sustain flame and life. As to this, it may be supposed that air when deprived of nitro-aërial particles and therefore also of its elasticity and weight (as was previously shown) is impelled upwards by the pressure of the rest of the air, and that when raised on high it is then impregnated anew with nitro-aërial particles. For indeed it is probable that nitro-aërial particles, being extremely small and volatile, float in the higher regions of the air, and that when collected there in sufficient abundance they constitute, in accordance with their diverse conditions, the elements either of fire or of cold.

With regard to the element of fire, it is probable that it dwells in the very body of the sun, which appears to be nothing but an immense chaos of nitro-aërial particles carried round in a perpetual whirl with the swiftest motion. Indeed, I think, there is very little difference between the light of the sun and its rays collected by means of a burning-glass, in which nitro-aërial particles are engaged in igneous motion without accompanying sulphureous particles, as we have elsewhere indicated. For although sulphureous particles are required at first to put nitro-aërial particles in motion and to kindle sublunary fires, still we must suppose that the nitro-aërial particles in the sun, not now entangled in terrestrial particles but free from every kind of impediment, will continue to eternity their motions, once started, without the aid of sulphureous particles; since, indeed, inanimate things will never suspend their movements unless they are hindered by some cause. Further, we must suppose that nitro-aërial and sulphureous particles never remain long in the same place, since they mutually ward off and repel each other in consequence of a natural antagonism; and hence it is that there must be a continual supply of nitro-aërial and of sulphureous particles for producing sublunary fire. Yet since the solar light has lasted for so many ages, it is probable that nitro-aërial particles, free from the presence of sulphureous particles, move with the greatest velocity in it. The rays of light will be considered later.

As nitro-aërial particles agitated with swiftest motion constitute the solar body and fiery chaos, so probably those pretty near the sun move with their velocity somewhat abated and are intensely hot without burning. But at a greater distance from the sun; namely, in that region of the air which is commonly called mid-air and is near the poles of the world, it is probable that they cease altogether from their whirling movement and are either altogether at rest or advance pointwise, erect like spears, and that in this state they constitute that other element of cold. For it appears to me that we must certainly maintain that cold is something positive and does not consist in this merely that the particles of bodies cease from all motion, as some have imagined; for the effects of cold are of such a kind as cannot result from mere privation of motion, as we have shown above. Moreover, that nitro-aërial particles are lodged in ample enough plenty in mid-air is evident from the fact that that region is extremely cold and that vapours to some extent freeze in it. For the air in the middle region is at least as cold as that which rests on the top of the loftier mountains, which in fact does not permit the snow there to melt even in mid-summer, as was remarked by the illustrious Descartes. But I have endeavoured to show above that intense cold and the congelation of vapours are due to nitro-aërial particles. Further, that nitro-aërial particles are crowded in the highest region of the air seems to be confirmed by the blue colour of the sky; for as nitro-aërial particles, when urged with swiftest motion, glow and flame, so when they move more slowly, or when their motion ceases altogether, they assume a blue colour. And it is an indication of this that the flame of sulphur is blue; for the nitro-aërial particles do not move so swiftly in it as in other flames, as has been elsewhere stated. And hence it would seem that when flame is about to expire in subterranean crypts, or even from the lack of sulphureous nutriment, the nitro-aërial particles in it do not shine brightly as in other circumstances, but take a blue colour on account of their diminished motion. To these I add, lastly, that iron and other very rigid substances of the same kind appear blue when polished on account of the nitro-aërial particles densely fixed in them.

These things being admitted, it is reasonable to suppose that aërial particles when deprived, whether by the burning of fire, or by the breathing of animals, or in any other way, of their nitro-aërial particles and consequently of their weight and elasticity, are driven upwards by the pressure of the rest of the air, and that they ascend until they arrive where nitro-aërial particles moving with the greatest rapidity constitute the element of fire: further, that the aërial particles on entering the fiery element in the manner described, immediately glow and are impregnated anew with nitro-aërial particles, and are moreover rendered heavier by the accession of the nitro-aërial particles (just as antimony when calcined by the solar rays is increased in weight on account of the nitro-aërial particles infixed in it, as has been elsewhere shown): and, finally, that the nitro-aërial particles when impregnated in that way and made heavier are by their own weight borne downwards to the coldest region of the air, and being rapidly cooled there become extremely rigid and acquire anew resilient force. For the case here seems not to differ much from what happens when soft and inelastic iron is made to glow by putting it into the fire, and is then immediately cooled by plunging it into cold water, so that it thus recovers its rigidity and resilient force.

It is besides probable that the aërial particles, when made rigid in the manner aforesaid and borne downwards by their weight, are likewise in some degree bent by the weight of the superincumbent air, so that they no longer move straight like arrows but begin to rotate. Hence it is that they at last, like the steel spring which sets automata in motion, are bent in multiple convolutions and crowded together. And thus, it seems, there ultimately arises in the aërial particles that conspicuous elasticity and tendency to expand indefinitely.

As regards the descent of aërial particles they are probably not borne straight down, but obliquely towards the poles. For the continual ascent of vapours and of air which goes on in the meridional region, on account of the very intense heat there and the rarefaction of the air, hinders their straight descent; so that, returning obliquely from the poles, they move towards the south till at last they reach the lower earth for the various needs of animals. And this seems to be the reason that the north wind is very dry and cold, since it brings with it air which is heavy with nitro-aërial particles and these extremely cold and dry. And as aërial particles are thus raised aloft when deprived of nitro-aërial spirit and being then anew impregnated with it return thence to the lower regions, the aërial particles seem to circulate like a macrocosmic blood in a perpetual circuit, and even the air itself, as in its circulation it takes in nitro-aërial spirit, in some sense breathes.

CHAPTER VIII

OF NITRO-AËRIAL SPIRIT IN SO FAR AS IT IS BREATHED BY ANIMALS

HITHERTO we have treated of nitro-aërial spirit and its effects upon plants and other natural things—it remains for us to inquire into the office it fulfils in the case of animals. In our treatise on Respiration published some time ago various considerations induced me to maintain that the chief use of respiration, that, namely, which makes it so necessary, is that particles of a certain kind, absolutely necessary for the support of animal life, may be separated from the air by means of the lungs and mixed most minutely with the mass of the blood ; and in confirmation of our opinion we brought forward in the previous chapter experiments by which it was shown that the air expelled from the lungs of animals is deprived of certain elastic particles and in consequence undergoes contraction.

Further, I attempted to show that the elastic force of inspired air is diminished because the nitro-aërial particles which are extremely subtle and nimble are extracted and in a sense struck out from the aërial particles. But let us now investigate how this is done.

In regard to this point I for some time suspected that nitro-aërial and elastic particles are struck out from the aërial particles by the special structure of the lungs. But on more full consideration of the matter I prefer the view that aërial particles enter the mass of the blood and are there deprived of their nitro-aërial particles, and in consequence partly lose their elastic force, a view which is confirmed by the following experiment.

Thus let a rod equal in length to the diameter of a glass bell-jar at its widest part be put inside it, and placed transversely and drawn downwards till both ends of the rod lean upon the sides of the glass and are supported by them, as is shown in Plate V., Fig. 4. Next let an earthenware vessel, glazed inside and capable of holding about four fluid ounces, be hung from the transverse rod by an iron hook attached to it, and let it be about half-filled with spirit of nitre. Further, let some small pieces of iron, tied together into a bundle and suspended by means of a string from the rod, be made to hang directly over the vessel (the string moreover ought to be of such a length that its other end may reach to the mouth of the glass and hang outside, in the manner shown in the figure). These arrangements made, the mouth of the inverted bell-jar should be sunk in the water about five finger-breadths, yet so that the water within the bell-jar may be at the same level as the water outside, as may be done by means of a syphon, the form and mode of use of which were described in the preceding chapter. Then let the water outside be drawn off until it is lower than the water inside by about three finger-breadths. And let everything remain thus until the air enclosed in the glass, heated by the hands of the operator, has returned to its former state. And then, lastly, let the height of the water within be noted by papers attached here and there to the outer surface of the glass, as is shown in the aforesaid figure.

And now let the aforesaid small pieces of iron be lowered by means of the string, the end of which hangs outside, into the vessel which contains the spirit of nitre. And so a very intense action will soon be excited and the water within will at once be depressed by the vapours thence arising.

After action of this sort has gone on for the third part of an hour more or less, or rather when the water within has been depressed about three finger-breadths by the vapours produced, let the pieces of iron be lifted out of the vessel by means of the aforesaid string. This done, after a short time you will see the water within gradually rising, and in the course of an hour or two you will see it far above the height first marked. For the water which was quickly depressed by the aforesaid vapours about three finger-breadths below the point first marked, now rises some three finger-breadths more or less above it ; so that about a fourth part of the space in the glass which was previously occupied by air is now occupied by the water rising within. And indeed the water which has risen in this way in the glass will not, even after a long time, fall to the original mark.

So that clearly we must conclude that the air contained in the glass has its elastic force diminished by about one-fourth part, in consequence of the said action produced by the spirit of nitre encountering the iron. Hence it is unable to make the same resistance as previously to the pressure of the atmosphere, and consequently the water underneath is impelled upwards into the glass.

Further, after the vapours in the said glass have been as far as possible condensed and the water inside does not rise any higher, let its height be indicated as before by pieces of paper. Then let the iron be lowered a second time into the vessel containing the spirit of nitre, that action may be excited anew. But that this may the better succeed, an ample supply of spirit of nitre ought to be put into the vessel, or rather two vessels containing spirit of nitre, and also two small portions of iron, should be suspended in the glass. When the water sinks anew about five finger-breadths in consequence of the vapours produced, let the iron as before be taken out of the vessel, and when this is done the water will gradually rise in the glass, yet not so quickly nor so far as the first time. For the water which rose after the first action about six finger-breadths, and far indeed above the height first marked, will only rise after the second, even should it be more intense, some two finger-breadths more or less. Nay, it will never rise to the mark from which it fell. If the action be repeated a third time the result will be just the same as in the second.

Now it seems extraordinary that the water which, after the effervescence first excited, was raised far above the height from which it sank, should after the second action, though brought about in the very same way, not even recover its former height. For what are clearly opposite effects seem to be produced from the same cause. Nor was any error committed in making the experiment, for when often repeated the result was always the same.

But now to submit our views in explanation of the phenomenon, it is in the first place probable I say that not only the air which was contained in that part of the glass in which the water rose after the first action, but that nearly all the air in the glass was impaired by the first action. For we notice that in about two hours after the said action the water had risen so as to occupy a fourth part of the glass more or less. But it is not to be supposed that in that space of time the exhalations or vapours produced by the action were entirely condensed. Nay, they will never entirely become liquid, as will be shown below. Let us suppose then that this exhalation is contracted to the extent of a half by condensation (for I have ascertained in a way to be described below that exhalations of that kind will only condense to the extent of about a half in so short a time as that in which the water rose in the glass), and it follows that the space in the glass, including that into which the water did not rise, is about half occupied by exhalations not yet contracted; since this has undergone a contraction equal to half the space which is left in the glass before the water could rise in it.

And in this a reason must be sought for the water not rising, after the action set up the second time, above the mark from which it was depressed. No doubt the effervescing particles which issued from the previous action being mixed in great profusion with the aërial particles, impaired them in the way to be described below; and the water in consequence rose into the space which was left not only by the condensation of the exhalations but also by the removal of the air. But since the air in the glass was for the most part impaired by the first fermentation, or rather had its elastic force diminished as far as possible, hence it is that the water, after the action brought on the second time, ascends in the glass only to the extent that the exhalations produced undergo condensation, and consequently much less than in the previous case. And since these exhalations will never be completely condensed, it follows that the water will never rise to the point from which it was depressed.

As to the manner in which the air contained in the aforesaid glass lost its elastic force, it is not to be supposed that it was condensed on account of being cooled by the saline exhalations, for the water rose in the glass while it was still warm from the action. Nay, if the glass be warmed by placing it near the fire so that the water contained in it may be rarefied, the water underneath can yet scarcely be depressed to the level first marked, even when the heat is intense; and when the glass cools it will rise again to its former height.

Nor is it probable that the air coalesces with the particles given off in the action and is as it were coagulated, since it will not submit to so much compression unless under a very intense force. Further, these effervescing particles are turned into a sort of vitriol which remains under the form of a liquid, but it is by no means probable that air in such quantity can be imprisoned in so small a portion of liquid, as was more amply stated in another place.

Wherefore it is reasonable to suppose that the aërial particles, inasmuch as they are rigid, are rubbed among the fermenting particles in the glass, and broken perhaps into very minute parts, so that the nitro-aërial and elastic particles are struck out of them, and the air consequently is deprived of its elasticity, and reduced to smaller volume, as was explained in the previous chapter.

Aërial particles indeed appear to lose their elastic force in the aforesaid action very much in the same way as in fire, which has been elsewhere shown to be nothing but a very impetuous fermentation. And indeed it is probable that the heat produced by the aforesaid action and also by others of the same sort, is due at least in part to nitro-aërial particles struck out in that way from the air. And this seems to be confirmed by the fact that if a corrosive liquid be mixed with a salt, or with a metal opposed to it, in a place almost destitute of air, though the two when mixed together effervesce in a conspicuous manner, yet the heat produced by them does not seem to be so intense as it would otherwise be—a fact which has been noted also by the illustrious Boyle.

I made also an experiment in an exactly similar way to ascertain whether fixed salts mixed with acid liquids and other actions of that kind diminish the elastic force of the air, and found as the result of observation that, if the elasticity of the air is to be manifestly diminished, it is absolutely necessary that the action should be of such a kind that the exhalations of the fermentation should last for some time and be disseminated through the air, or at least, that the exhalations if suddenly produced should undergo great condensation afterwards. For it is to be noted that the air in the aforesaid glass is impaired by the fumes dispersed through it even after the iron has been taken out of the spirit of nitre and the fermentation in the vessel has ceased. For otherwise if the air had been injuriously affected only while the action lasted, then the space left by the impaired air would have been filled by the vapours emitted, and consequently the water would not have risen in the glass except in so far as these underwent condensation. But the water rose in the glass much faster than vapours of that kind usually condense, so that we must believe that it ascended not only into the place of the condensed vapours but also into that of the air at that time impaired. Indeed we must suppose that particles of the liquid and of the metal, dispersed through the air and fermenting there, gradually rub its particles and diminish their elastic force. Hence if the vapours produced by any action are of such a sort as cannot last a good while in the air, the water will only ascend into the space left by them on their contraction by condensation. If therefore they are of such a kind that they do not undergo notable condensation, the water in the said glass will not rise above the limit first marked. Whenever, I say, the vapours generated will occupy more space than is left by the impaired air, then, however much the elastic force of the air is diminished, it will not be observed in the said glass. And hence it is that if action is caused in the glass by spirit of nitre and a fixed salt being mixed, as also by oil of vitriol and iron acting on each other, in the manner aforesaid, the water will not rise above its original level.

It is to be noticed in passing that although the exhalation produced by the spirit of nitre and iron, when acting on one another, will never pass into a liquid condition, as will be shown below, still it can scarcely be thought that it is really air. For when the fermentation was first produced in the aforesaid glass, the air in it was for the most part impaired—at least as regards elastic force—as was previously pointed out; yet when the glass was almost filled with the exhalation produced by the fumes, instead of with air, then the water did not rise in the glass after the second action as it did before, because that exhalation could not, like air, be diminished as to its volume by the fermenting particles, but was on the contrary increased by them. But this sort of exhalation will be discussed more fully later.

After this experiment we must suppose that air when breathed by animals loses its elastic force in the following manner. For I assume, in the first place, that the mass of the blood is a liquid conspicuously in a state of fermentation, as will be shown below. Since then through the action of the lungs aërial particles are mixed intimately and in the minutest parts with its fermenting particles, it comes to pass that the aërial particles have their elastic force diminished by the particles of the blood in the same way as by the vapours of fermentation in the aforesaid glass. Indeed it is probable that the fermenting particles of the blood rub the aërial particles interspersed among them and strike out from them the nitro-aërial spirits, and that at last the aërial particles, deprived of their nitro-aërial and elastic particles, become unfit for sustaining life and lose besides a part of their elasticity.

Now that we have introduced nitro-aërial particles into the mass of the blood, the question at once follows what use they serve. I discussed this subject in my treatise on Respiration published a good while ago. Let me be permitted, however, to add some things here. It is our opinion, then, that as in vegetables so also in animals, nitro-aërial particles are the principal instrument of life and motion.

For in the first place nitro-aërial spirit when mixed with the saline-sulphureous particles of the blood appears to excite in it vital fermentation. In fact, just as nitro-aërial particles when they slowly enter the pores of the earth encounter there saline-sulphureous particles, immature indeed, in an obscure fermentation on which, as has been shown elsewhere, the life of plants depends; so the same nitro-aërial particles when introduced more profusely into the mass of the blood by the action of the lungs, and mixed in their minutest parts with its saline-sulphureous particles, brought to a state of active vigour, produce a very marked fermentation such as is requisite for animal life. For it is to be noted that blood consists of the same particles as earth but in a more exalted state. For as immature sulphur, when closely combined with the seeds of a fixed salt, composes earthy matter, as has been said elsewhere, so the mass of the blood is made up of saline-sulphureous particles raised to a suitable volatility, and hence it is that both have the same colour—to wit, a dark purple. Indeed I attempted to show above that nearly all fermentations of natural things result from the motion of nitro-aërial particles; and in fact I have no doubt at all that the effervescence of the blood is due to the same cause: accordingly when respiration is arrested, the effervescence of the blood immediately ceases and animal life is extinguished.

And what confirms still more the view just stated is the fact that the blood which entered the lungs with a dark colour, returns from them more florid and ruddy, as arterial blood is, as was observed by the illustrious Lower in vivisections. He also showed that that change made in the mass of the blood is caused, not so much by its being triturated in the lungs as by the air being mixed with it. For when venous blood is placed in a vessel, the upper surface which is exposed to the air acquires a scarlet and florid colour, although the blood at the bottom of the vessel appears as a dark purple; and yet it too if exposed to the air will after a short time become ruddy. So that it is not surprising that the blood in the lungs, where the air diffused through all its particles mixes intimately with it, is rendered florid throughout.

And now we remark that air mixed with blood produces the ruddy colour in it, since it sets up fermentation in its mass. For arterial blood which is florid has its particles in motion and also effervesces conspicuously, while the darker and duskier venous blood is more grumous and is more quickly coagulated on account of its feebler effervescence.

The following experiment also corroborates the view here taken. If blood that has been kept for some time in a vessel be put into a glass from which the air is exhausted by an air-pump, the blood at the surface where it was of a florid colour will effervesce gently and rise in bubbles. But if arterial blood while still warm be put in a place void of air, it will expand in a remarkable way and rise in an almost infinite number of bubbles. And it is probable that this results partly from the effervescence of its particles and their being thrown into movement, and partly from its particles being mixed with air.

But to submit yet another experiment. If spirit of nitre be poured upon a liquid saturated with volatile salt and sulphur, such as the spirit of hartshorn impregnated with its own oil, a very marked effervescence and a very ruddy scarlet colour will be produced at once in the liquid, and yet this florid colour changes into a dark purple when the liquid ceases to effervesce. No doubt the nitro-aërial particles (and we have elsewhere shown that the spirit of nitre abounds with them) effervescing with the saline-sulphureous particles of the aforesaid liquid seem to cause the scarlet colour which rivals that of arterial blood. For it is the nature of nitro-aërial particles when set in motion to produce a ruddy colour in the substances in which they are, as happens in spirit of nitre, which is ruddy during distillation.

It is to be noted here that as nitro-aërial particles cause the fermentation of the blood, so this fermentation strikes out, in the way shown above, and draws from the air other nitro-aërial particles by which fermentation goes on anew in the blood. In fact nitro-aërial spirits, when mixed with the mass of the blood in the lungs, produce an intense enough effervescence in it; but they are soon separated for the most part from the blood in the system of the body, for purposes to be explained below; so that the fermentation of the blood on its return to the lungs is for want of them much diminished and more sluggish; but still it does not cease so completely as to be unable to draw nitro-aërial particles from the air for its renewal. And so in fine the movement of fermentation is perpetuated in the animal Automaton.

Just as the fermentation of the blood, so also its heat arises I think from the effervescence of nitro-aërial particles with the saline-sulphureous particles of the blood. For if any saline-sulphureous minerals, such as the vitriolic Marchasites and the like, be exposed, when recently dug up, to moist air, they will shortly effervesce and become intensely hot, inasmuch as aërial particles give rise to a very pronounced effervescence when they meet with the saline-sulphureous particles of the mineral. Further all substances, at least those which are endowed with some degree of consistency, grow somewhat warm while fermenting—a result which is due as I have tried to show elsewhere to the motion of nitro-aërial particles. How much greater then will be the effervescence and heat of the blood which abounds in saline-sulphureous particles duly exalted, and with which aërial particles are densely and in their minutest parts mixed by the action of the lungs? To this I add that the very intense heat which animals experience when urged to violent motion, arises partly because in violent movements there is very great need of increased respiration, and thus the nitro-aërial particles introduced into the blood in greater abundance will produce greater effervescence and heat than usual; for the friction of the limbs in the most violent movements is not so great as to be able to excite so fervid a heat. Nay, if any one breathes, even when at rest, but a little more intensely, he will soon feel himself in an unusual glow of warmth. However the heat excited in animals by violent exercise is in part also due to the effervescence of nitro-aërial particles and sulphureous particles, originating in the motor parts, as will be pointed out elsewhere.

I am not unaware that the learned Dr Willis in his treatise on the Heat of the Blood has advanced various arguments by which he endeavours to show that the heat of blood is not due to its fermentation. This eminent man also asserts *that liquids never acquire heat in fermenting*. But indeed it is evident from common experience that all the thicker and richer liquors, those namely which abound in saline-sulphureous particles, such as strong ale and the like, grow somewhat warm in the course of fermentation. However there is no kinship between any other liquids and the mass of the blood, since the latter is so thick that its particles do not exist in a fluid state except when fermenting. Hence blood when drawn off is soon coagulated and acquires a certain consistence. But such things as are possessed of consistence, even in the opinion of this learned man, generate heat in effervescing. Further, blood abounds beyond all other liquids in saline-sulphureous particles, and nitro-aërial spirits are densely and most minutely mixed with them, and when these effervesce together, as it is their nature to do, an intense enough heat must arise; while in other liquids, fermentation is only produced by a much more minute quantity of nitro-aërial and saline-sulphureous particles. Further, the nitro-aërial spirit by which fermentation is excited in other liquids is not supplied directly from the air with motion and vigour but is innate in these liquids themselves, wrapt in the embrace of the salt and fixed, as has been shown elsewhere.

But yet another difficulty brought forward by this eminent man opposes what has been said—viz., *that the action and heat of contrary salts effervescing together are increased in a vacuum, as is proved by Boyle's experiments. Wherefore, if the heat of the blood were caused by its fermentation, it would seem that it ought to become more intense when the air is withdrawn. But on the contrary, if by suppressing respiration air is prevented from entering the blood, fermentation soon ceases and the animal quickly dies.*

I reply that from the fact that the fermentation of the blood immediately ceases on account of want of air, it follows that it is caused by nitro-aërial spirit, as has just been said.

Further, although contrary salts when mixed together, and other things of a like kind when fermenting, expand and rise more in a place void of air than elsewhere, still it is by no means on that account to be granted that they effervesce more intensely. For the particles of any substance when effervescing in unconfined air are so much pressed by the weight and pressure of the incumbent atmosphere that they cannot expand freely ; while yet the same particles, being scarcely burdened at all in a place void of air, will in performing their movements spread out and swell up much more : but this does not come from their more intense action and motion, but from the removal of the hindrance. And hence it is that if water slightly warmed be put in a place void of air, its more agile particles will make the liquid swell and as it were boil, and yet it is not to be supposed that these particles are agitated with a greater force than before. So that clearly, even if the fermentation of a mixture of contrary liquids in a vacuum were to go on with less force than in free air, still their fermenting particles would spread out to a greater extent than otherwise owing to the withdrawal of the pressure of the atmosphere.

To this I add that contrary salts, if mixed together in a vacuum and then quickly removed after effervescence has gone on for some time, do not appear to have been heated as much as in other circumstances. So that it would appear that even the heat of contrary salts fermenting together, depends to some extent on nitro-aërial particles struck out from the air, as we have already hinted.

It is besides to be noted that there is a great difference between the fermentation of the blood and that of contrary salts, inasmuch as the effervescence of the latter is due to an internal principle, namely to nitro-aërial particles contained in them, as I already endeavoured to show, while the fermentation of the blood is excited by the nitro-aërial spirit supplied by the air meeting its saline-sulphureous particles. Whence it is, that the access of air is not so necessary for the action of contrary salts. But such things as have not nitro-aërial particles contained in them, such as the mass of the blood, all saline-sulphureous minerals, and likewise such things as ferment from extraneous moisture and heat, effervesce only when nitro-aërial particles reach them from the air. And this is the reason why the fermentation of the blood subsides immediately when the air is withdrawn.

But the reason why blood quickly coagulates when drawn off, although exposed to the air, is because it is necessary for the fermentation, and therefore also for the preservation of the fluidity of the blood, that nitro-aërial particles should be mixed densely and in very minute parts with its saline-sulphureous particles, as takes place in the lungs. And yet even blood that has been shed effervesces at its surface, that is, where it is mixed with the air, as was previously shown.

From what has been already said, it is I think in some degree made out that the fermentation of the blood, and hence also its heat, arises from nitro-aërial particles fermenting with its saline-sulphureous particles ; so that we do not need to have recourse to an imaginary Vital Flame that by its continual burning warms the mass of the blood, much less to affirm a degree of heat in the blood intense enough to produce light, from the rays of which, transmitted to the brain, the Sensitive Soul is supposed to be produced. I know not what the ancients dreamed about certain feral fires hidden in the urns of the dead, but now for the first time the vital flame, if such a thing can be, is kindled in the viscera of animals, so that we all now burn like Ucalegon, and there is no reason why we should any longer wonder at a Salamander living in the midst of flames. But really fire seems to be better adapted for the dissolution and destruction of things than for sustaining animal life. Nor indeed is the mass of the blood in any way suitable for producing flame ; for although it consists of sulphureous particles, yet these are held so firmly in the embrace of the saline ones that blood will not burn, even when thrown into the fire. But if any fire of this sort were kindled in the mass of the blood when the blood rushes forth from a divided artery, the flame would certainly reveal itself by its own light. For it is not to be thought that any flame is ever kindled which is not luminous and also somewhat caustic and destructive, unless one is disposed to palm off fumes for flame. Nor is it credible that the vital flame is extinguished in the blood as it rushes out of the vessels, for the air by blowing on it is fitted rather to excite than to extinguish fire. And lastly, what are we to think of the vital fire of aquatic animals? It must indeed be fierce and invincible to burn under water and be such as the whole ocean cannot quench. The existence of subterranean fires is not yet established ; it is much less probable that there are subaqueous fires. With respect to a lucid soul inhabiting the brains of animals, I ask how it is possible that this light which is supposed to enlighten with its rays the whole brain and nervous system, should never be seen by the eye. Truly, fires of this sort and new lights, no less in Anatomy than in Religion, have always seemed to me vain and fanatical.

With respect to the fermentation of the blood we note further, that if the saline-sulphureous particles in the mass of the blood have been too much elevated, the nitro-aërial particles mixed with them will give rise to a very impetuous effervescence and to febrile heat. And thus it is that an ulcer of the lungs produces a hectic fever ; for the nitro-aërial particles, mixed with the too highly exalted saline-sulphureous particles of purulent matter, excite a very intense effervescence and febrile heat.

When saline-sulphureous and nitro-aërial particles effervesce so much in long-continued fevers, it comes about that the volatile salts of the blood are so worn that they change at last, in the manner already described, into acid salts. And hence it is that the blood acquires an acid nature after long-continued fevers; indeed the case is not very different from that of strong ale, which after long fermentation is converted into vinegar. Nay, even when the fermentation of the blood goes on aright, its saline particles are in course of time sharpened and liquefied by the action of the nitro-aërial spirit, and in combination with other (volatile) salts constitute a certain acido-saline salt not very unlike Sal-Armoniac. And thus it is that urine is impregnated with a certain Sal-Armoniac, and the proof of this is that copper is corroded by urine in the same way as by Sal-Armoniac. Further, a solution of sulphur made in lye is precipitated by urine poured on it just as by any acid liquid. Hence if ashes have urine or even blood mixed with them, volatile salt will in distillation be abundantly derived from them, inasmuch as the fixed salt of the ashes absorbs whatever of acid there is in the urine, so that its volatile salt, freed from the acid salt, readily ascends, precisely as happens in distilling Sal-Armoniac mixed with fixed salt.

Besides the uses thus far assigned to nitro-aërial spirit, a very great many other offices are served by it. For when nitro-aërial particles effervesce with the mass of the blood in the manner aforesaid, its saline-sulphureous particles are brought to due volatility, just as the sulphureous particles from terrestrial matter are brought to due maturity by the aid of nitro-aërial spirit, as has been shown elsewhere.

Further, in all the internal movements which take place in the bodies of animals, as in the digestion of food, and also in that most intense effervescence which gives rise to muscular contraction, nitro-aërial particles play the chief part, as I shall attempt to show in the fourth treatise.

CHAPTER IX

WHETHER AIR CAN BE GENERATED ANEW

Now that we have shown above in how many ways air is impaired, it will not be out of place to inquire whether it can be generated anew. On this point I shall introduce an experiment not very unlike the one by the illustrious Boyle already referred to.

Let, then, spirit of nitre and spring water, mixed in equal quantities, be placed in a glass vessel of sufficient size. Then let a small glass be so placed under the mixture that it shall be completely filled with the liquid. This done, let two or three globules of iron be placed at the mouth of this glass, and let it lie inverted at the bottom of the other, as is shown in Plate V., Fig. 3, care being taken that these globules do not fall out of the glass, and to secure this let the mouth of this glass be closed with the finger or in any other way, until it rests at the bottom of the other glass. These preparations made, the acid menstruum will, after a short time, corrode the iron globules and effervesce conspicuously with them, and the exhalations caused by the effervescence will rise in the form of bubbles to the top of the glass and constitute the air there, which, gradually increasing, will depress the underlying water. Let the glass, when it is completely filled with air of this kind, be raised a little, that the iron globules, which are to be removed from the liquid, may escape from it, care however being taken that the mouth of the glass be not raised above the liquid. And so we shall see that air which occupied the whole glass, gradually condense, and the underlying liquid rise into its place. And yet this air will not all become liquid, for the glass will be always about one-fourth filled with it ; and this air, however long kept, even in the coldest weather, will never be condensed into a liquid. If the iron globules be placed under the mouth of the glass while it is still inverted and put a second time into it, air will be produced anew, a certain part of which will never become liquid. So that the glass will be about half-filled with exhalations that will never be condensed.

If oil of vitriol mixed with water be substituted for spirit of nitre, or if a very sluggish fermentation be excited in the manner described, or also if the iron globules be allowed to remain in the glass for a day or two, then the air generated in this way will scarcely suffer condensation at all. For the partial contraction of the air in the aforesaid glass was due to this, that the exhalations produced by violent fermentation were agitated with a very rapid motion, which, gradually abating, the air was reduced to smaller bulk ; while, if the fermentation has been mild and has gone on for a long time, the exhalations generated later will take up and fill the place left by those previously condensed, so that the air will scarcely suffer any condensation at all.

It is not easy to know whether air of this kind is really common air or not, but this is certain, that it will expand like air upon the application of a gentle heat, and when cooled again will contract. Nay, this air is endowed with elastic force no less than common air, as I ascertained by the following experiment.

Let a small glass tube of the diameter of a goose quill and about four inches long, be hermetically sealed (as chemists say) at one end; then let a single drop of water be dropped in by the other and open end, and let it be marked on a paper attached to the outside of the glass how much of the space of the glass the drop occupies; then let a second drop, and then others be dropped into the glass in the same way as the first, and let the space occupied by them be marked on the aforesaid paper. This done, let the open end of the glass be fitted into the narrower opening of another glass open at both ends, and then let that opening be carefully closed with suitable cement, as is shown in Plate V., Fig. 5. Let the glass, when prepared in this manner, be so submerged in water contained in a suitable vessel that, when its orifice is turned upwards, all the air may pass out and water enter in its place, great care being taken that the narrow glass be also filled with water. Then let the glass filled with water be inverted and let it rest on the bottom of the other vessel, and let things remain in this condition.

And now the aforesaid air is to be transferred to this glass in the following manner. Let a small dish, big enough however to receive the mouth of the vessel containing the air, be placed under that vessel. Next let the small dish filled with the aforesaid liquid, together with the inverted glass, containing the air, resting upon it, be transferred to the vessel in which the glass first described has been placed. And let the orifice of the glass in which the air is, be placed in the orifice of the other glass filled with water, as is seen in Plate V., Fig. 5 (care being taken that the mouth of neither of the glasses is raised above the surface of the water), and let the glass be inclined until the air contained in it escapes and ascends into the other glass, which can in this way be filled with that air, although it is enough that a little of the air be introduced into it.

And now the glass into which the air is transferred in this manner, is to be enclosed in another glass from which the air may afterwards be pumped by Boyle's air-pump, which may be done in this way. Let a vessel, not too large, but capable of admitting the orifice of the glass in which the air has now been collected, be put under it, and then let this vessel filled with water be removed, with the other glass inverted and resting upon it, and be put inside the glass from which the air is to be pumped out. After the air has been partly exhausted, the air enclosed in the said glass will expand beyond the cavity of the glass, and most of it will escape through the underlying water. When the air has been removed as far as possible by the pump, let it be permitted to enter anew. When this is done the water in contact with the glass in which the aforesaid air is, will be driven up into it on account of the pressure of the atmosphere and will almost entirely fill it. For the air which was left in that glass will occupy only a part of the narrower glass ; and yet that small portion of air, a moment ago, when the pressure of the surrounding air was almost withdrawn, occupied the whole glass, and was able to resist the pressure of the surrounding water, and also of the air which could not be all pumped out. Therefore if the volume of the whole glass be measured by means of water put into it drop by drop, and compared with the space in the narrower glass which the residual air had filled, the extent to which the said air had expanded will be ascertained. For by as much as the one space exceeds the other, so much was the expansion of that air. And from many repetitions of the experiment, I have ascertained that air of that kind expands to more than two hundred times its volume; and indeed if it had been relieved from the pressure of the surrounding water, it would have expanded about twice as much. Nor will common air, when treated in the same manner, expand more; but it must be observed that in making experiments of this kind, every precaution must be taken that the airs, whose elastic powers are to be compared with one another, are pressed by an equal weight of the surrounding water, and also that the pressure of the surrounding air be diminished to an equal extent in every experiment, by pumping.

I note here in passing that I made an experiment in a similar way, to find whether the air in which an animal or a lamp had expired, possessed elastic force in an equal degree with unimpaired air, and, in fact, it appears to me to expand no less than any other air, as was previously said. But in order that experiments of this kind may be made, it is sometimes necessary that the air whose elastic force is to be investigated should be drawn off from the glass which contains it, and transferred to the glass first described, and this can be done in the following manner. Let a glass, not too large, be submerged in the water in which the glass is which contains the air to be drawn off, so that it is filled with water; then let this glass be inverted and placed under the orifice of the other glass, and raised in it in the following manner. A transverse rod has to be attached, from the first, to the inner sides of the glass, as is seen in Plate V., Fig. 4. And let a string be so suspended from the rod that both its ends, drawn from under the orifice of the glass, hang outside. Then one end of the string is to be tied to the bottom of the glass that is to be raised, and the other end pulled till the glass rises above the surface of the water inside. When the water has fallen out of the glass which has thus been pulled up in an inverted position, and air has taken its place, let this glass be pulled down by means of another string previously fastened to its mouth, and taken out of the other glass, in such a way that its mouth may remain continuously inverted; and then, lastly, the air contained in it may be transferred to the glass first described, in the manner already shown.

Although air generated from the aforesaid fermentation possesses no less elastic force than common air, it does not on that account follow that it is really air —viz., such as possesses vital and igneous particles. For that air in which an animal or a light has expired possesses elastic force in an equal degree with inviolate air, and yet it is destitute of nitro-aërial and vital particles. But in order to determine whether this air generated anew is fit for sustaining life or not, let the following experiment be performed. But before this can be done, it is first of all necessary that air of that sort be generated in sufficient abundance, which can be done as follows without any great waste of the aforesaid liquid. Let a sufficiently large glass be immersed in water and filled with it, and let it remain inverted. And now let the air generated in a small glass, in the manner described, be transferred to this glass, as was shown above; then let another small portion of the air produced as before be conveyed in like manner to the glass, and let this process be repeated until the air is present in sufficient quantity.

And that we may now make our experiment, let a small animal, say a mouse placed in a small cage, be placed in the upper part of the cavity of an inverted glass with a suitable support below it, as is delineated in Plate V., Fig. 6. And let the glass with the animal inside be so immersed in the water, that the water inside may rise as high as the support on which the mouse rests, which can be done by means of the curved syphon described in Chapter VII.; and let everything remain in this condition till the animal dies, and let the length of time during which the animal breathes in the glass be carefully noted. Now let the dead animal be removed and another living animal be put in its place. And let it as before be shut in the glass immersed in the water, care being taken that the same quantity of air as before may be enclosed in the glass. This done, let the aforesaid air be transferred, as described, into the glass containing the animal, in such quantity that the air thus introduced shall by two or even three times exceed the quantity of ordinary air enclosed from the first in the glass. And then let the glass be raised till the water (which is depressed by the air introduced) reaches the support on which the animal rests, care being taken that the mouth of the glass be not lifted above the water. And let everything continue thus till the animal dies. When this has occurred we shall find that the animal has not lived much longer in the glass on the second occasion than the other did on the first; that is to say, before the aforesaid air was introduced into the glass. But if that air were truly air suitable for sustaining life, the animal put in on the second occasion would have survived twice as long as the previous one. And the reason that the animal, when the said air was put into the glass with it, lived a little longer than it would otherwise have done, appears to be that the air enclosed in the glass could be more gradually and less copiously breathed and corrupted by the animal in consequence of the admixed air.

Here, too, we might repeat our remarks in the previous chapter on the difference between this sort of air and common air. And yet it is probable that there is a great likeness between air of this kind and common air, and that the elastic force of both is due to no very different cause. For since iron consists of rigid particles, and the corrosive spirits consist of very elastic nitro-aërial particles, as has been elsewhere said, the air produced by their fermenting together will not be very different from common air, inasmuch as the latter appears to be formed of rigid particles, and these imbued with nitro-aërial spirit, as I have elsewhere endeavoured to show.

CHAPTER X

HOW FIRE IS PROPAGATED. ALSO WHY FLAME RISES TO A POINT

In the preceding chapters the nature of fire has been frequently discussed ; let us now inquire how it is that fire, so very small when first kindled, spreads itself so enormously if only it be supplied with sufficient abundance of sulphureous nutriment. For it is to be noted that nitro-aërial and sulphureous particles, when in fiery motion, agitate with their own velocity very many others, and these in their turn others of the same mass; so that indeed the smallest spark is sufficient to cause a wide conflagration, which, yet, is entirely contrary to the ordinary laws of nature. For in other cases when one body in motion impels and moves another at rest, the motion in both is diminished. For a moving body which strikes another body and moves it from its place, loses as much of its own motion as it communicates to the other; so that when a few particles set in motion a large number of the same mass, the motion in each will be greatly diminished.

That the burning particles in fire, then, should increase their motions to such an extent, it seems to me necessary that some other moving body should be added to the ignited particles, to promote and intensify their motion. And I think this is how it occurs. It is no doubt probable that sulphureous particles when divided most minutely, and violently agitated by the application of fire or in any other way, impinge upon the nitro-aërial particles residing in the particles of the air, or of common nitre, and drive them into those small spaces in which the subtle matter revolves with swiftest motion (as we showed above), but that the nitro-aërial and sulphureous particles, when driven within the spherules which are described by that revolving matter, are driven further and forced out by it with their motions greatly increased, and that at last the nitro-aërial particles, on being violently sundered in this way from the fixed salt of nitre, or from the aërial particles with which they were previously most firmly united, are thrown into a truly fiery motion. Assuredly we must suppose that the sulphureous and nitro-aërial particles, and also the subtle matter, are fashioned by the supreme Artificer with truly marvellous skill, so as to be naturally adapted for throwing each other into a motion of extreme velocity. Indeed, not only in the burning of fire but in producing most natural movements, we must assume as much skill and careful fitting of the mutually adapted particles, as in automata constructed with the most accurate human art. If any one should think that in explaining spontaneous movements of this kind I am having recourse to things too minute, I reply that it is nature's way to produce all the greatest things by means of the least, for how small and delicate are those spirituous particles which set the huge machinery of the elephant in truly stupendous motion? And what is to be said of the very small particles of insects? For as their whole bulk is merely a point in appearance, how minute must those portions be which we cannot suppose to be one-thousandth part of the whole? Indeed I make bold to affirm that natural movements of this kind are effected by particles too minute for human discernment.

It is to be concluded from the foregoing that the ignited particles in the burning of fire are agitated with elastic impulse, for just as we have shown above that the power of recoil in bent rigid bodies is due to the pulsation of subtle matter, so also we maintain that the motion of ignited particles results from impact of the same matter. Indeed the burning of fire does not seem to be very different from the bending to fracture of any rigid body, or rather of the aforementioned glass drops. For as in bent rigid bodies, their particles, thrust within the circles of the subtle matter, are struck by it, and at last, if the rigid body is bent to breaking, are violently driven out, so in fire the sulphureous and nitro-aërial particles, when they enter the minute pores of the air or of nitre itself, and are impelled there into the spherules of the subtle matter, produce the same effect in the aërial particles, or on anything else impregnated with nitro-aërial particles, as if, like rigid bodies, they were bent to breaking : so that the igneo-nitrous particles would appear to burst forth with elastic impetus from the air, not otherwise than the small bits of glass from the fractured glass. It is a corroboration of this view that aërial particles and the particles of nitre itself, in which, namely, igneo-nitrous spirit especially resides, become stiff like rigid bodies and are imbued with elasticity, and are consequently of a sort fit for having igneous particles shot out of them with elastic impetus.

It would be easy to show here that nearly all natural movements, such as the generation and dissolution of things and the internal motions of opposed liquids, are, similarly, to some extent due to the battering of the subtle matter. It is probable indeed that these motions are excited in the same way as fire is kindled ; for fire appears to be nothing but the most intense fermentation of nitro-aërial and sulphureous particles. It certainly does not seem to be sufficient to say with the eminent Willis in regard to these natural motions, that the more active particles expand, ascend, and finally fly away ; for, in a discussion of this sort, what should specially be shown is why particles at rest begin to move, for whatever is at rest will remain for ever at rest, if it is not set in motion by some cause. With respect to motion of this kind, it appears to me necessary to suppose some perpetually moving matter by the impulse of which these motions are effected. As then the very rapid motion of fiery particles seems to proceed from this, that the sulphureous particles coming up to the particles of nitre or of air and there impinging upon the subtle matter agitated with extreme velocity, are, by its impulse, thrust out with elastic violence along with the nitro-aërial particles which these substances contain ; so the milder fermentation of natural things must be supposed to arise from this, that nitro-aërial particles, along with extraneous moisture, penetrate a saline-sulphureous mass and enter the abode of the subtle matter, by which, as it is in violent agitation, the nitro-aërial together with the sulphureous particles are driven off. For fiery effervescence seems to differ only in this respect from the milder internal motions by which plants hasten to growth or decay, that in fire, nitro-aërial particles, in close union with fixed salt or with aërial particles, are, by the impulse of sulphureous particles and of the subtle matter, violently sundered from their partner and thrown into very brisk motion ; whereas, on the other hand, in the said fermentations, as the sulphureous particles are not held so firmly in the embrace of fixed salt, they are thrown into a milder motion by the impact of the nitro-aërial particles and of the subtle matter. But these matters have been more fully discussed elsewhere. Nor does the process seem to be different in the effervescence of opposed liquids, for when one of these intrudes into the pores of another, and therefore also into the abode of the subtle matter, and is expelled again, an internal movement and effervescence of the particles is set up.

WHY THE FORM OF FLAME IS ALWAYS POINTED

As to fire, let us consider lastly why flame of every sort rises to a point. On this it is to be remarked, in the first place, that the sulphureous particles, bursting out from the burning matter, pass through the whole blaze and kindle flame everywhere in their passage. For since the sulphureous particles, which burn at the top of a lamp, have proceeded from the wick, they must necessarily have passed through the whole of the intermediate flame. Further, it is not to be doubted that these particles were burning from their first entrance into the flame, since the lowest part of the flame is produced only by the kindling of sulphureous particles. And hence it is that when a lamp goes out in consequence of a failure of sulphureous nutriment, the flame is last seen at the top and at some distance from the wick. For it is the last group of sulphureous particles passing through the blaze, and everywhere in its transit kindling flame, that is seen at the top of the blaze. But since no sulphureous particles now remain to follow these last ones, there must be an interval without flame between the last burning particles at the top of the flame and the wick.

But now the question arises why the sulphureous particles at a certain distance from the wick no longer kindle flame. For since the sulphureous particles burn at the extreme verge of the flame, they must consequently be in more violent motion there than when, as yet unkindled, they were about to enter the flame. And therefore there seems to be more reason for these particles, once kindled, persevering in their fiery movement and flame, than for their entering upon a fiery movement at first. As to this, my opinion is that the fire is extinguished at a certain distance from the wick, not because the motion of the sulphureous particles is diminished, but because, in their passage through the flame, they are rubbed so much and made so small and subtle by their own combustion that they become at last incapable of throwing nitro-aërial particles into fiery movement. In fact, for the formation of a flame, it seems to be necessary that the sulphureous particles should neither be too fixed nor very volatile. For we notice that the sulphureous particles of spirit of wine and also the very subtle particles of camphor, are scarcely able to throw nitro-aërial particles into fiery motion. And this is the reason that the fire they make is but languid and almost harmless. And indeed it is probable that sulphureous particles, in passing through the flame, are rendered so subtle by their own burning that they become quite unfit to produce fire. And this seems to be confirmed by the fact that if any polished plate be placed in the flame of a lighted candle, we shall soon see it covered with soot; but if the plate be held a little above the flame, no soot will gather on it. For the soot adhering to the plate seems to be nothing else than the sulphureous particles which shortly before were burning; now indeed they are changed somewhat in consequence of their burning and rendered more subtle and scorched. But those sulphureous particles which have burned longer, become at last so fine that they are incapable either of constituting the grosser structure of soot or of producing flame. It is a confirmation of this that the lower part of any flame is very different from the top. For the lower part is usually blue, while the upper part burns more brightly. And this difference seems to result from the different state of the sulphureous particles. Now then, since the sulphureous particles carried through the flame get smaller during the whole course of their passage and are therefore to some extent consumed, the flame must gradually become smaller and end at last, as it does, in a point.

CHAPTER XI

OF THE AËRIAL VORTEX, OR ASCENT OF SEA-WATER. Anglice: A SPOUT

SEEING that we have treated of the elastic power of air and of its motion, let me be permitted to submit here some observations regarding the wonderful phenomenon familiar to sailors, which is in English called a Spout; that, namely, in which a huge mass of water rises up on high like a pillar, and besides a whirlpool of water is carried aloft. For I think that that phenomenon is caused by a whirling movement of the air and by a diminution of its elastic force and pressure. For I cannot agree with those who maintain that an ascent of water such as this, arises solely from the circumgyration of the water, as though on being driven into a circle, whether by winds rushing from opposite points or in any other way, it were thrust on high at the centre of its vortex. For it is scarcely credible that from this cause water could be raised to so great a height as in the aforementioned phenomenon, since there is nothing to hinder water raised above the surface in that manner from soon flowing over to the sides. But indeed it has been ascertained by experiment that water, when made to rotate, rather falls downwards at the middle of its vortex than ascends in that way.

But in order that the reason of the phenomenon may be known, I have thought it desirable to present a representation of it, as it was delineated by a clever sailor who had much experience of these things, in Plate VI., Fig. 1. In this figure:

a, a, is the huge mass of sea-water rising up like a pillar or a mountain. This mass is sometimes of greater, sometimes of less height, and assumes sometimes a pyramidal and occasionally an orbicular form.

c, c, is a somewhat dense fog produced by the ascent of fine water particles. This, which derives its origin from the aforementioned mass of water, expands in all directions, but it soon begins to contract gradually, and at last ends in a point at *e, e*.

e, e, f, f, is a gloomy tube which, descending from an overhanging cloud, directly overtops the aforesaid column of water. This tube is at first like a stream of smoke, which seems to descend gradually, yet so as to leave a gap between its lower end and the underlying water. Further, this tube is after a short time filled with a whirling mass of vapours densely crowded together, or it may be of water, which rushes upwards with a most furious motion, with spiral revolution, and a remarkable roaring noise, as is shown in the aforesaid figure. After about ten minutes, a part of the tube, a third say, or a half, is often broken off, and when this happens, the waters rush down in immense quantity—a fearful sight to behold—and ships are sometimes sunk by them.

d, d, is the cloud from which the said tube is seen to descend. It is at its first appearance small and thin, but it soon expands widely in all directions, and at last, when it has become exceedingly dense and gloomy, it dissolves in a violent storm of rain. So much then by way of description of the phenomenon. It now remains for me to show next the way in which it is produced by a whirling movement of the air and by its diminished pressure.

And here in the first place I take it for granted that the air is sometimes driven round in a circle, whether as the result of winds rushing from opposite directions, and at length dashed against each other and bent into a circular path, or as the result of their being suddenly thrown back by the resistance of lofty mountains or of dense clouds, or in the manner indicated in the previous chapter.

2. It is to be observed that aërial particles when revolving in a circle, try to recede from the centre of their motion, as was observed long ago by Descartes. Hence if we suppose that a cylindrical column of air, reaching from the summit of the atmosphere to the water beneath, is made to rotate, and if that aërial vortex is large enough and is carried round with a very rapid rotation, the total force by which all the particles of that aërial vortex strive to depart from the centre of their motion will be great enough to resist the pressure of the surrounding air and even to overcome it. But if this happens, and the aërial particles recede from the centre of the vortex, the middle of it will be like an empty tube: for the case will be exactly like that in which a large number of globules are placed in a concave circular vessel and made to rotate rapidly with it, when you will see these globules go away from the centre of the cavity and describe their circular orbits at its side.

But now when a vacuum is made in this way in the middle of the air-vortex, the water which lies beneath will be forced to ascend by atmospheric pressure, just as it would be into a vacuous tube. Nay, although the aërial cylinder may not rotate with so swift a motion as is needed for completely overcoming the pressure of the surrounding air and producing a vacuum in the middle of the vortex, still, with however feeble force it rotates, the weight and pressure of the air will certainly diminish gradually from the outer edge of the vortex to its centre. For let *a*, *a*, *b*, *b*, in the aforesaid figure be the aërial cylinder extending from the summit of the atmosphere to the subjacent water. While as yet it was not in rotation, the pressure of the air of which it was composed is certainly quite equal to that of the atmosphere, since their weights were in equilibrium ; but now when the aërial cylinder is made to revolve, the force with which each particle in rotation strives to recede from the centre of its motion is added to the original pressure of the cylinder. Hence it is that these forces in union will preponderate over the pressure of the surrounding atmosphere, and therefore the adjoining air will be pushed out by the revolving air, and will necessarily recede somewhat, say from *a* to *i*, and from *b* to *g*, and consequently the rotated air following it will spread out into a larger space than before and constitute the cylinder *i*, *i*, *g*, *g*. Hence the rotated air is not a little rarefied, and consequently the water beneath is less pressed by it than before.

That the pressure of the rotated air gradually diminishes from the outer edge to the centre of the vortex, I gather from the following. For when all the particles of the aërial whirlpool strive to recede from the centre, it results that they impel and press against the particles of air adjacent to them on the outside ; while, on the contrary, the air between them and the centre of the vortex is subjected to less pressure from them now than while as yet they had no circular motion and no tendency to recede from the centre of their motion. But since the rotated air, in proportion to its nearness to the centre of the vortex, suffers less pressure, it follows that the air particles, the further they are within, expand and rarefy the more, by virtue of their elastic force, and consequently press less on the water which lies under them. Whence it is that the water rises gradually from the inner edge to the centre of the vortex, the water rising more in that part of the vortex where the rotation is more rapid, as is shown in the figure.

I remark here that it is probable that at the base of the mass of rising water, the water, as shown in the figure, is somewhat depressed (although this, perhaps, can scarcely be seen by sailors at a distance). For when the air surrounding the first cylinder, *a*, *a*, *b*, *b*, is thrust from *a* to *i* and from *b* to *g*, in the manner aforesaid, the result is that the air at *i* and *g*, being much compressed, undergoes considerable condensation, and in consequence the water beneath will be somewhat depressed by the greater weight than usual of superincumbent air.

With regard to the fog that rests upon the pile of waters, I think it is caused in this way. Thus since the air about the surface of the rising water rotates very rapidly, the water, at the outer parts at least, is carried round along with the air ; whence it comes about that small particles of the water, receding from the centre of their motion, are dispersed in all directions and borne upwards, just as would happen if a top with its upper surface spherical were wetted with water and made to spin.

But the reason why those vapours, as shown in the figure, are bent and at last unite in the torrent at *e*, *e*, seems to be this, that the nearer the rotated air is to the centre of the aërial vortex the more it is rarefied and thinned, as has already been shown. For hence it is that while the aqueous particles driven away from the aforesaid mass are carried upwards and outwards, they are at every instant of their progress bent inwards by the air, which gradually becomes denser from the centre of the vortex to its outer edge, but where the air is rarer the aqueous particles can more easily continue their movements; and, turned back in this way, they arrive at last at the middle of the aërial vortex (which behaves like a vacuous tube), and, congregated densely there, are carried aloft in a swift whirl and spiral revolution, as is shown in the figure. Moreover the spiral motion of those vapours arises from the circular motion of the surrounding air.

To bring the said vapours to the middle of the aërial vortex, and thence to raise them on high, the pressure of the atmosphere seems to contribute not a little, in addition to the force by which they have been torn from the mass of water. For since the air at the summit of the atmosphere is much rarer than that which is nearer the earth, and its pressure less, and since, also, the aërial vortex rotates there more rapidly (for the force which constrains the air to rotate probably comes from above), it follows that the force by which the aërial particles strive to recede from the centre of their motion will take effect much more easily at the summit of the atmosphere than near the underlying water; so that high up in the air where, namely, the said tube is seen, the aërial particles recede from the centre of the vortex and are able to produce a vacuum there, whereas the air from the lower end of the tube to the underlying water, being rotated less rapidly, is merely able to diminish the pressure of the atmosphere, which is greater there, but not altogether to remove it; and hence, the air and vapours at *e*, *e* (where the vacuous tube begins), are driven forcibly into the tube by the pressure of the surrounding air. Further, as all the neighbouring particles of air and vapours come into the place of those that have been carried up, and others again follow them, it comes to pass that vapours are carried in a continuous stream towards the centre of the vortex.

About the said tube, it is noteworthy that at first, when, namely, the whirl of vapours is not yet observed in it, it looks like a rather thin smoke and also gradually descends, while yet it would seem that the vapours entering its lower end would cause it to be seen there first. I think the reason of these things is that when the air first began its rotating movement, only a few rather thin vapours—those, namely, which, already in the atmosphere, were carried upwards—had entered the said tube. And these are first seen at the top of the tube because they are assembled there in greater abundance, and are also somewhat condensed on account of their diminished velocity. However, I do not know whether or not these things depend also on another cause, for, since the air receding from the middle of the vortex leaves the tube there nearly vacuous, the aërial particles, and there are but few remaining in it, will as they expand, appear under the form of smoke, not otherwise than it happens in a glass vessel when the air is being exhausted by Boyle's pump, as will be explained more fully elsewhere. And it seems to be for this reason that the smoky tube is first seen high in the air, where the force by which the rotated air strives to recede from the centre of its motion first takes effect and produces a vacuum, as has already been shown.

It is besides to be noted that the nearer to the water the said tube descends, the higher does the water underneath rise. And the reason of this seems to be that a long descending tube cannot be formed unless there is a very swift whirl of the air, and the ascent of the water depends on this.

When the vapours driven up in the said tube have come to the top of the aërial vortex, they, receding from the centre of their spiral motion, are dispersed all around, and, heaped up in great abundance, form the dense and gloomy cloud widely spread for a short time (*d*, *d*). This, after the motion of the vapours of which it consists has ceased, breaks up in a storm of rain, and, pressing by its weight on the underlying air and pushing it out, causes the violent wind. But it is to be observed that although at some distance from the aforesaid mass of waters, boisterous winds blow and the sea is very rough, yet near the phenomenon all is calm, which I think may thus be explained. Since the air is very dense at the outer edge of the vortex, as is seen in the figure, and is thrust out all round from the aërial vortex by the pressure of the superincumbent cloud, the winds carried towards the vortex are arrested and turned back by the air, which is very dense and also rushes in the opposite direction, so that their impulse cannot reach the vortex, yet meanwhile these winds driven backwards cause furious whirlwinds at some distance from the said phenomenon.

It also makes for this, that the sea near the column of rising water is whirled round—a motion in water very much opposed to the propagation of waves, which advance only in straight lines.

After the mass of water has again fallen, a certain part of the said tube is usually broken off, and when this happens a vast quantity of water descends from on high, and if a ship happens to be under it she is instantly overwhelmed and sunk. There can be no doubt that the water is produced by the condensation and collection of the vapours which have risen in the tube. But such a heaping up of them comes from this that when the motion of the aërial whirlwind has abated, the vapours at the top of the tube, ceasing from their motion and being condensed into water, are carried down and encounter others which are still ascending at the bottom of the tube, and at last fall down along with them.

But here we must ask why, on the cessation of the circular motion of the air, the elevated vapours presently fall; because, although the motion of the air by whirling the water underneath was the cause of its fine particles being carried up, yet, after these vapours have been once set in motion, the whirling motion of the air seems to contribute nothing to their ascent, unless perchance the air is reflected upwards by the water underneath.

In regard to this, it is probable that when the circular motion of the air gradually abates, the aqueous particles do not, as before, leave the aforesaid mass of water with a force intense enough to raise them as high as the summit of the atmosphere, and therefore these vapours must be heaped up in the manner already described, and rush downwards; while yet, if the whirling motion of the air has suddenly ceased, the vapours, violently agitated, ascend beyond the top of the tube and are dispersed there, so that the tube (as is sometimes the case) will seem to ascend aloft. To this I add further, that so long as the circular motion of the air continues, the pressure of the atmosphere contributes somewhat to drive the vapours upwards into the tube, as was previously said.

About the said phenomenon we remark in fine that if it should happen that the aforesaid tube rise right above an island or sea-coast, its lower end will be driven back from the island or coast, sea-wards, as is delineated in Plate VI., Fig. 2. But that the reason of this may be understood, let *a*, *a*, be a section of the aërial cylinder in rotation, which is interrupted at its lower end by the interposition of an island, as is seen in the figure. For the air near the island, rotated from *b* towards *d*, when it has reached the island at *c*, can advance no further in that circle; since therefore the rotating air is pressed also by the external air adjacent to it (and this, as has been already pointed out, is much condensed), it will necessarily be reflected towards *e*, where on meeting the air that has been carried from *b* towards *d*, it forces it outwards and is turned round along with it towards *f*. And so, the first vortex being interrupted, a new vortex, *e*, *f*, emerges, in the centre of which the column of water rises, as is shown in the same figure. But that new vortex coming, at some height above the sea, say at *i*, against the former vortex which is rotating with the opposite motion, is by it, as being the more powerful, gradually turned back, so that both ultimately coincide at *a*, *a*.

CHAPTER XII

OF LIGHT AND COLOURS

We have already treated of nitro-aërial spirit so far as fire is kindled by it; it remains for us to subjoin some things about light, the peculiar and most wonderful offspring of fire. With regard to rays of light, it can scarcely be believed that certain effluvia of more delicate flame, shot out from the luminous body, reach the beholder's eye. For who can imagine that any fiery corpuscles can be brought, almost in a moment, from the sun to the earth? Much less is it probable that fiery particles emanate from a small lamp in quantity sufficient to illuminate the region round about. Nor again is it to be thought that finer sulphureous particles, flying away far from the original blaze, excite other (nitro-aërial) particles for the kindling as it were of a very meagre flame, namely Light, as the distinguished Willis and others have supposed. For if such were the case, why should the light not endure for a little after the extinction of the lamp? For it seems to me that the sulphureous particles proceeding from the lamp just before its extinction, would continue to produce light until they arrived at the extreme limit of the illuminated region, for the sulphureous particles which emanate from any burning body do not resign their fiery nature until they have ascended to the furthest limits of the flame. Hence the flame of a lamp usually continues for a short time after its sulphureous matter has been entirely consumed, as was shown above. Yet since the sphere of the blaze is but small and the fiery particles pass through it in an instant, hence it is that the flame quickly expires. As, however, the sphere of light is much wider, it would seem that the luminous particles cannot pass through it so quickly but that the light should continue for some time after the lamp has been removed or extinguished. Further, if light were a somewhat finer flame, what should prevent it from being deflected hither and thither by blasts of wind like flame, in proportion to its consistence, such as that may be? And how, lastly, could rays of light be transmitted instantaneously through the most solid bodies such as glass, if light were propagated by means of sulphureous particles? For even the extremely small and nimble nitro-aërial particles do not penetrate bodies so solid, without some interval of time, even when they move with the swiftest and most fiery motion. How much less then will sulphureous particles, which seem to be grosser than nitro-aërial spirit, penetrate such bodies in an instant?

I may therefore maintain with the distinguished Descartes that light consists in motion or impulse alone, which, because of the continuity of the luminous medium, is transmitted to the greatest distance without any delay. For certainly impulse or motion is eminently adapted to the laws which are followed in the propagation of light. For the nature of impulse is such that it will promptly cease when the impelling force is withdrawn, and it advances only in straight lines. Further the force of impulse is propagated instantaneously to the greatest distances through solid bodies. For the case here, owing to the continuity of the medium, is just as if one end of a rod being moved, the blow impressed on it were transmitted almost instantaneously to the other very remote end.

As to the medium by the impulse of which the rays of light are transmitted, it is not to be believed that it is air itself, since light can be propagated very intensely even in a glass vessel containing no air. And therefore it is probable that besides the nitro-aërial particles fixed in the aërial particles, other nitro-aërial particles are interspersed among them and fill all their interstices; which we infer from this, that solar rays, if collected by means of a burning-glass, actually ignite even in a glass from which the air is exhausted. For gunpowder can be ignited by them there, and sulphureous matter can also be sublimed by their heat; but I have already attempted to show that heat and fire do not arise except from nitro-aërial particles set in motion. Thus it would seem that even in a place deprived of air there are nitro-aërial particles, and that the fire produced there by the solar rays, concentrated by means of a speculum, consists in this, that the nitro-aërial particles are so much impelled at the point where the solar rays meet that they are thrown into a really fiery motion. So that the medium by whose impulse the rays of light are propagated seems clearly to be nothing else than nitro-aërial particles very densely distributed through the atmosphere. Indeed it is probable that nitro-aërial particles when moving in a luminous body with a very rapid and fiery motion, communicate to the other nitro-aërial particles, dispersed through the ether and of the same nature as themselves, the peculiar impulse by which the rays of light are propagated.

But you will say, if nitro-aërial particles exist in a place void of air, why cannot a lamp be kindled and burn there since no requisite is lacking for the production of flame. I answer that the sulphureous particles of a lamp contribute in no way to produce flame, except in so far as they strike out from aërial particles, the nitro-aërial particles which, sundered with violence, are thrown into fiery motion, as was pointed out above. But sulphureous matter seems to be by no means fit for throwing into fiery motion the nitro-aërial particles disseminated through the ether.

Like igneous particles, so also moving particles of all substances whatsoever which give an impulse to the luminous medium in the due way, are capable of producing light. Hence it is that a kind of feeble light is emitted by the glow-worm, by rotten wood, and the like.

Further that light is propagated by the impulse of nitro-aërial particles seems to be confirmed by its passing with greater ease through such bodies as are extremely rigid and crammed with nitro-aërial particles—glass, for instance, and similar substances, but above all, aërial particles whose rigidity is due to nitro-aërial particles densely infixed in them, as I previously endeavoured to show.

Here too we can appeal to an experiment referred to by the Hon. Robert Boyle ; to wit, when air is suddenly pumped from a glass vessel, the glass soon becomes dark inside and seems to be filled with nebulous fumes, and besides light, or rather a certain momentary whiteness, is sometimes produced in it. This I think is to be accounted for by the immediate expansion of the residuary particles of air, when the greater part of the air is pumped from the glass ; not otherwise than as steel springs which have been bent round coil upon coil, open out in a moment as soon as the force by which they were bent is withdrawn. But when aërial particles extend themelves in this way, their structure changes at each successive moment in which the movement of recoil takes place, (as is evident from what has been said on the subject of elasticity) ; whence it is that the rays of light are somewhat interrupted, for, namely, the nitro-aërial particles infixed in the aërial particles, by whose impulse moreover light is transmitted, move with a motion different from that by which the action of light is propagated ; so that the aërial particles cannot now transmit the impulse of light as they would otherwise do, but reflect it in the manner of a mirror. But as soon as the aërial particles cease from their motion of recoil, the glass becomes again pellucid.

OF COLOURS

But with a view to a clearer understanding of the nature of light, let us make a brief investigation of the quality of the colours which are produced by light.

With regard to colours and the visible forms of things, it is the most generally received opinion that they are produced by the rays of light reflected in various ways. But indeed I am not sure that this way of explaining colours is quite in accordance with truth. For let us suppose that a lamp is placed outside a chamber so that the rays of light, by means of two apertures made in opposite walls of the chamber, may pass through the intervening space. When this is done, if the eye be placed in any part of the chamber except that through which the bundle of rays passes, the chamber will appear completely dark and the rays of light passing through it will not be seen at all. But now, let us suppose any coloured plane *b*, Plate I., Fig. 11, to be placed obliquely to the rays passing thus through the chamber; when this is done, the plane will be illuminated and by an eye placed at *a* will be seen of some colour, suppose white; and yet it seems that the rays of light are not reflected to the said eye. For if that plane is polished in the manner of a mirror and suitable for the reflection of rays of light, the rays falling upon it will, from its oblique position, be diverted from the eye and reflected to the opposite side of the chamber, towards *c*, to which the line of reflection tends. One would naturally say that a coloured surface, especially a white one, has little swellings or molecules most densely crowded upon it, whose innumerable very small surfaces look in all directions around; and that the rays of light falling upon these very small surfaces, which are turned towards the eye wherever it is situated, are reflected by them, as by so many mirrors, to the eye, and impress upon it the sense of colour. And hence it is that if the aforesaid plane were extremely smooth, so that all the rays falling upon it were reflected towards *c*, the plane could not be seen at all by an eye situated at *a* ; so that it appears that an image of the plane is propagated by certain rays reflected to the eye. But this answer seems unsatisfactory, for if the rays of light falling upon the said plane were reflected in that manner in every direction, then almost the whole chamber would be illuminated by reflected rays, just as the chamber at *c* will be lighted up if the said plane is polished and capable of reflecting the rays of light. For since we suppose the plane to appear white, the rays of light ought to be reflected all round in no small amount, for white is supposed to be produced only when the rays of light are very densely reflected. But in fact, although the said plane is seen as white by the aforesaid eye, yet the eye will be all the while in darkness, and will not be able to discern what is nearest to it. And yet if the eye were placed at *c*, where, namely, the rays reflected from the polished surface tend, it would be dazzled with light and would see everything near it illuminated by reflected light.

Besides it would follow from this hypothesis that the rays of light so reflected from any white surface are more dense than those which emanate from fire or any flame of a red or less white colour. For it is supposed that the intermediate colours, such as red and others of that kind, are produced by light and darkness variously intermixed, while white results from light when most densely reflected. But this is by no means the case; for if a reddish fire, such as that of burning coals, were kindled in the said chamber, the whole chamber would be illuminated by its rays, while the colour white is propagated with scarcely a trace of light. Still I would not deny that a white surface reflects some rays of light, in so far as it is polished in some parts and fitted to reflect the rays of light. But it is scarcely credible that the rays of light are reflected from a white surface in such abundance as is requisite for producing a white colour all round. But the reason why the said plane cannot be seen if it is highly polished is to be looked for in what is said below.

Further, it appears that the luminous body itself is not seen by the rays of its own light, whether direct or reflected. For a small lamp burning in a high place can be seen clearly enough several miles off, far beyond the bounds of its illumination, by an eye in a dark place. But this would not be if an image of it were produced only by rays of its own light.

But to submit my own view on this question, I think it is to be held as at least a probable conjecture that the image of a small lamp, seen beyond the limits of its illumination, is propagated by a peculiar impulse which is quite different from the impulse of light. Indeed igneo-nitrous particles when violently agitated in a luminous body, in so far as they strike and move other nitro-aërial particles kindred with themselves and these in their turn strike and move others, constitute the action of light, as I tried to show above; but in so far as these igneous particles impel a peculiar medium, distinct from the luminous medium, and impress upon it a sort of special undulation or impulse, they seem to diffuse in all directions a visible image of themselves. For it is probable that particles, igneous and luminous *per se*, and with their own motion, do not affect so much the sense of sight as of touch. For fiery particles cause very great pain, and the eye when exposed to too fierce a light is injured as if it were struck by some blow. Further, the solar rays strike not only the eye but also the nostrils exposed to them, and, by tickling them, cause sneezing. But luminous particles seem to propagate the image of themselves by the very gentle impulse of a peculiar extremely fine medium. And this when conveyed by a continuous undulation of the medium of vision to the eye, and by means of it to the delicate origin of the optic nerves, impresses on them such strokes as are appointed by nature for producing the image and the perception of light. Nor is it necessary for the perception of light and colours, as the very acute Descartes pointed out long ago, that a material image resembling the ideas which we form in our minds of objects, should pass from them to the eye, since indeed, to produce the different sense perceptions in the mind it is only necessary that the fine nerve threads which proceed from the brain should be agitated with various motions. For in this way a blind man who uses a stick to guide his steps, feels and distinguishes well enough, by the various ways in which the stick is moved when it strikes against bodies of all sorts, whether it is a tree, or a stone, or anything else that opposes him.

As the likeness of a luminous body, so also the image and visible form of an illuminated body appear to be propagated by the motion of a special medium distinct from the action of light. For when the aforementioned plane, exposed to the rays of light, is seen by an eye placed in darkness, its visible form is conveyed to the eye not by reflected rays of light, since these do not reach the eye, but by the motion of a peculiar medium quite different from the action of light. But the mode in which this special medium is moved will be treated of later.

As to the medium by whose impulse the likenesses of things are propagated, it is probable that it consists of finer matter compacted with greater continuity than the medium by which the rays of light are transmitted. For we must suppose a very subtle and ethereal matter which, interwoven with the nitro-aërial particles, occupies and fills up all the interstices of the atmosphere, and that by its impulse visible forms are propagated. In fact, the structure of the eye is so tender and delicate that it is able to perceive those very gentle blows of ethereal matter, imperceptible to the other senses, and besides to distinguish the variation in its pressure and rhythm on which vision and colour depend. And indeed it seems to be owing to the extreme tenuity of the medium of vision that the image of a lamp is conveyed to such a great distance without any delay. For the visible form of a luminous body is transmitted far beyond the limit of its light, and probably with a swifter motion than the rays of light are transmitted through the air.

That the image of an illuminated body is transmitted by the impulse of this special medium, and by a motion different from the action of light, I have endeavoured to show above. Let us therefore now consider next, how it is that the medium of vision is struck. On this point I was for some time in a difficulty as to whether it is by the impulse of the illuminated body or of the luminous particles that the medium of vision is affected. For I think we must maintain either that the luminous particles impinging on the surface of the illuminated body, impress such an impulse upon it as is fitted, when conveyed through the medium of vision and by its instrumentality to the eye, for presenting an image of the illuminated body, or (as seems to me more probable) that the nitro-aërial and luminous particles falling upon the illuminated body, acquire themselves a certain new motion, by which the medium of vision is struck with an impulse distinct from the action of light. For we notice that the rays of light do not, like igneous particles, spread their image in all directions around. For when the rays of light pass through the aforesaid chamber, they are seen only by an eye which directly faces them but not, as we showed above, by an eye situated at the side. Whence we may infer that nitro-aërial particles in a burning body are somehow driven round in a circle, and in their circular motion strike the medium of vision all round, but that the luminous particles move only in straight lines, so that, whether by very frequent blows or by a kind of pressure, they impel the visible medium directly forward only, but not to the side. And indeed the action of light tends to move straight forward, precisely as if it were projected in a straight line ; for the rays of light, when they strike upon a plane, are reflected like a solid body at an angle of reflection equal to the angle of incidence. But when luminous particles impinge on an illuminated body they probably acquire a new tremulous motion and are moved with very short and frequent vibrations, precisely as happens to a dart when it is thrown and one end of it strikes a solid body. Hence it is that the medium of vision is struck by the vibration of luminous particles even on the sides and in all directions. But since this sort of vibration of the luminous corpuscles varies according to the diversity in the surface of the illuminated bodies, and since, in fine, the medium of vision is differently impelled by it, hence it is that the diverse perceptions of colours and of images are produced and propagated all round.

With respect to the glittering white colour which a brightly shining lamp shows, it seems to depend on this, that the particles of the shining body, agitated with the swiftest motion, and in densest array, strike with vivid action the very subtle medium of vision in individual points, with no interruption, and by this, transmitted to the eye, the perception of light is caused. Moreover the variation of this glittering whiteness results from the interruption of the impulse of the very subtle medium at certain points by opaque bodies interspersed in the medium.

The white colour, which most nearly approaches glittering white in brightness, does not seem to depend, as is commonly supposed, on an extremely dense reflection of the rays of light. For, as I have endeavoured to show, all colours are different from the action of light.

It seems, therefore, that we should maintain that the white colour arises from this, that the luminous particles impinging on the illuminated surface, encounter in its very many points such a resistance that these particles are, in consequence, excited to some new tremulous motion with very frequent vibrations, as we have just shown. And by these very numerous particles, made to vibrate in this manner, the medium of vision is struck very frequently and at very many points with an impulse different from light; and in this way the white colour seems to be produced. And hence, since a white surface has usually many very minute excrescences distributed thickly over it—not that these reflect the rays of light (for rays of light are scarcely reflected at all from a white surface such as paper, but on the contrary the impulse of light is quite destroyed by it, as was shown above) but so far as the very numerous luminous particles falling on these molecules acquire a certain new vibration by which the very subtle medium of vision is struck in very many places—the white colour is propagated. But on the other hand if the rays of light are entirely absorbed, without resistance, by the surface of the illuminated body, or if the solid body is of such a kind as offers little resistance to luminous motion, black colour or rather the absence of all colour is the result. For example, if an eye situated in darkness is turned towards an illuminated mirror, but in such a position that the rays falling on the mirror are not reflected to the eye, the mirror will appear of a black colour, or rather it will scarcely be seen at all. Nay, although the mirror be so placed that the rays of light reflected from it fall upon the eye of the beholder, the rays will indeed flash like lightning upon the eye, but the glass itself will scarcely be seen at all. No doubt the luminous particles impinging on the mirror, throw the nitro-aërial particles which the glass contains—and which are kindred with themselves—into their own luminous motion, for it has elsewhere been shown that very solid bodies, such as glass and the like, have nitro-aërial particles inserted in them. But since the particles of the glass yield to the motion of the luminous particles and in no way resist it, these luminous particles will acquire none of that motion of vibration by which colours and the images of things are propagated; hence it is that glass of that kind has no colour at all.

As to red and the other colours which differ from the glittering white, they seem to be due to various mixtures of the glittering white with darkness.

From what has been said, we must seek for an explanation why substances of a white colour can scarcely be kindled by solar rays collected by a burning-glass, while, on the other hand, such as are black easily take fire. For when nitro-aërial and luminous particles, impinging on a white surface, acquire the new motion by which the white colour is propagated, they lose entirely their luminous and fiery impulse; but it is otherwise when they fall on a black surface.

Finally, we note also here that the impulses of light and of colours follow almost the same laws; for colours and images of things (although they are propagated laterally and in all directions, as was shown above) proceed, like the rays of light, only in straight lines, and further, like light, they undergo reflection and refraction.

CHAPTER XIII

OF LIGHTNING

HAVING now treated of fire and light, let me add a few remarks on lightning, since it seems to waver between flame and light.

In the first place then as to lightning, it is not to be supposed that any flame discharged from a flashing cloud reaches the eye. For who can conceive a flame so vast and swift as to spread in a moment over almost the whole hemisphere. Nor can it be said that the sulphureous exhalations which are raised by the sun's heat and widely dispersed through the atmosphere are all kindled together by the flame of the flashing cloud, for if it were so these sulphureous exhalations, when once kindled, would burn till they were totally consumed; and, consequently, since the sulphureous particles would be used up the first time, there would be no second flash. And indeed the flash would continue for some time; but the opposite is the case.

Should any one here say that the sulphureous exhalations, which are imprisoned here and there among the clouds, are separately kindled in consequence of their being violently agitated, and that the flash of lightning is due to the wide expansion of their light; I reply that it is scarcely probable that a fire so immense as to be capable of propagating light to such vast distances, is produced by a succession of separate flashes, for it is to be noted that lightning is not only seen at a distance, but reaches to the eye of the spectator. Further, if lightning were nothing else than rays of light, how does it happen that it not infrequently sets fire to things which it meets? For this is never done, even by the solar rays, except when collected by a burning-glass. And from what source finally should the power come, which is required for the effects usually produced by lightning, if it consisted merely of light?

I confess for my part that sulphureous exhalations disseminated through the air are not infrequently kindled by lightning, but the flame produced by their burning is quite different from a flash of lightning, and is propagated hither and thither in a skirmishing way, as it is led by the exhalations, and it also lasts for some time. Such a flame is sometimes seen in a very intense flash.

But that it may be understood what I think about lightning, it is allowable for us to assume that thunderstorms are caused by this, that the clouds high up in the atmosphere, when condensed and frozen, descend on those beneath with a violent crash, as has been shown by the very ingenious Descartes. Further, when the clouds dash violently against one another in this way, the air around is necessarily thrown into a tremulous motion; and this agitation of the air is conveyed, with scarcely any loss of time, to a great distance, on account of the continuity of the atmosphere. Nay, in whatever manner thunderstorms are produced, the air must certainly be agitated with a very great commotion.

Now I certainly think it is probable that the aërial particles, since they are solid and rigid, strike forcibly against each other in consequence of the violent concussion of the air produced in a thunderstorm, and that they wear themselves by the powerful and sudden mutual shock. The consequence is, that the nitro-aërial particles, struck out from the aërial particles and thrown into a fiery motion, produce a certain light and momentary flame (such as that of lightning is) which extends over nearly the whole hemisphere. For it is very much as though an immense congeries of very small flints were struck with so violent a blow that an almost infinite number of sparks of fire should be struck out from the collision of its innumerable particles; for in this case a certain light flame, propagated through the whole aggregate of flints, would be suddenly kindled. Besides I do not know whether or not the nitro-aërial particles disseminated among the aërial particles (from some sort of impulse of which, I assume that light arises) can be thrown by the violent concussion of the atmosphere into such a motion as is requisite for the production of light. And indeed if lightning were light and nothing more, it would seem to be due sometimes to this cause.

As to the fervent and very intense sultriness, which often precedes thunderstorms, it must not be thought that it results from sulphureous exhalations carried aloft and dispersed through the air; for that kind of sulphureous matter can only become heated, or contribute to the production of heat, by first being kindled. Therefore I think it should be held that the said sultriness is not infrequently the result of a tremulous and unequal movement of the air; for the air, when agitated by a motion of that kind, becomes very warm as the distinguished Descartes has remarked. For if any one blows vigorously against the back of the hand the breath is felt to be very cold; while, on the contrary, if he blows it into the contracted and bent palm of the hand, it becomes not a little warm; the reason seems to be that when the aërial particles are thrown into a tremulous motion by being reflected hither and thither in the hollow hand, it happens that the nitro-aërial particles, by their striking against each other, are gently detached and thrown into the motion that is required for heat. But if a considerable tract of air is at any time agitated by such a motion, it will not only grow warm but will also be in a condition for entering upon a motion of the kind required for the production of lightning.

As regards the wonderful violence with which lightning sometimes overthrows and burns whatever stands in its way, the distinguished Gassendi very ingeniously supposes that *glomeres* (as he calls them) consisting of nitrous, vitriolic, and sulphureous exhalations, together with a small portion of cloud gathered round them, descend to the earth, and that, when they take fire at last, they burst into a very impetuous flame and destroy everything they come near. But, indeed, not to say that nitrous or vitriolic vapours do not exist in the air (as I endeavoured to show above), it is scarcely probable that a small portion of cloud (if it descended to the earth in that way) could imprison inflammable matter with force sufficient for the stroke of a thunderbolt.

Wherefore we may assume that the immense force of lightning is produced in the following way. For instance, if a tower or other obstacle stands, at any time, directly in the path of the rush of air (for it is to be observed that, in a thunderstorm, the air pressed by the descending cloud spreads out one way or other), it happens that the air is brought to a stand by the opposition of the said things and is greatly condensed, as is shown in Plate I., Fig. 10. And not only the aërial particles but the sulphureous also, which are raised into the air by the great heat of the sun, are densely collected near the obstacle. But in consequence of the dense collection of the particles of both kinds, and their igniting in the manner aforesaid on account of the violent concussion of the air, a globular fire, and that very impetuous and rivalling gunpowder, is produced. For it has been shown elsewhere that the force of gunpowder is caused by nitro-aërial particles bursting out in densest crowd from the ignited nitre. Accordingly, since aërial particles are charged with the same nitro-aërial particles, if they ignite when densely collected, the flame produced by them will be very impetuous for the very same reason as in the case of gunpowder; so that now it is no wonder that a thunderbolt sometimes overwhelms and prostrates whatever it meets.

In what has been said, we must look for the reason why a thunderbolt sometimes melts a sword, while the sheath is left intact. No doubt, since the motion of the air excited in thunder shakes even solid bodies, it sometimes happens that iron and other solid bodies against which the force of that motion is specially directed are so agitated that the nitro-aërial particles which they contain (for we have shown elsewhere that rigid bodies such as iron, abound in nitro-aërial particles) are greatly disturbed and thrown into a fiery motion, and in consequence of this movement of the particles the structure of the substances in which they reside is destroyed. And the more solid bodies are, and the more they abound in nitro-aërial particles, the more quickly are they consumed when struck by lightning ; for the particles of the more solid body strike more violently against one another, and agitate themselves the more. Hence it is that the iron is melted by lightning while the scabbard, on account of its loose texture and the want of nitro-aërial particles, remains uninjured. Thus the very strength of bodies tends sometimes to their destruction and the strongest things perish the sooner from internal discords and movements.

It is also noteworthy that animals are often killed by lightning without showing any trace of a blow. This seems to be due to the fact that aërial particles—not those alone which are disseminated through the air, but those also which exist in the mass of the blood—are thrown by the violent concussion of the air into a kind of flash, in consequence of which they immediately become effete and altogether unsuitable for keeping up the fermentation of the blood. To this I add that the animal spirits are also dissipated by lightning, as will be shown elsewhere.

Lastly, with regard to the impetuous and whirling winds which usually accompany thunderstorms, they seem to be caused not merely by the air being violently thrust forth by the pressure of the descending cloud, but also, to some extent, in consequence of a vast tract of air being deprived of its elastic force and pressure, whether owing to the nitro-aërial and elastic particles struck out of the aërial particles colliding with each other in thunder, or on account of sulphureous exhalations kindled here and there throughout the air; for aërial particles when deprived of nitro-aërial particles, whether by combustion or in any other way, lose their elasticity, as was previously pointed out. Hence it is that the air, rushing from every quarter, is borne with rapid motion to that place where the pressure of the air is diminished. But these aërial torrents rushing from opposite points produce, in their passage, a sudden and impetuous wind, and when at last they meet and dash against each other they are bent in all directions and driven round in a circle, since the air rushes from all directions and allows no outlet for their escape; and this seems to be the cause of the violent whirlwinds which suddenly arise.

CHAPTER XIV

OF THE HEAT OF QUICKLIME. INCIDENTALLY OF THE COMBINATION OF OPPOSITE SALTS

Now that we have discussed nitro-aërial spirit in so far as it is the cause of fire and heat, it will not be out of keeping with our plan to treat of quicklime, in which, when it is sprinkled with water, nitro-aërial and igneous particles manifest themselves with very intense heat. With regard to the heat of quicklime, the learned Willis has maintained in his treatise on Fermentation that in consequence of the long-continued calcination of the calcareous stone, igneous particles are fixed in it and are firmly detained in its structure which is even harder after calcination than before, and that afterwards these particles, driven out of their quarters by the water poured on the lime, burst forth and by their motion produce heat. But indeed it seems to me scarcely probable that igneous particles are fixed in quicklime in this way, for its structure seems to be too loose to detain the extremely agile nitro-aërial particles. But even supposing igneous particles to exist in quicklime, how should the pouring of water upon it rouse them to the motion requisite for heat? For aqueous particles are of a nature to arrest the motion of igneous particles and to extinguish them, but not to throw them into motion.

Wherefore we are at liberty to suppose that nitro-aërial and igneous particles exist in quicklime, not simply and by themselves, but closely combined with some salt, and that the heat which arises when water is sprinkled upon it is due to the existence in it of contrary salts—an acid, to wit, and an alkali—and to their action upon each other.

For, first, we must hold that a fixed salt is contained in quicklime. For if quicklime is thrown upon an acid liquid, such as water with which oil of vitriol has been mixed, the water will presently be deprived of all its acidity, since the fixed salt of the lime immediately absorbs and destroys the acid salt of the vitriol, being contrary to it.

Further, if spirit of vitriol be poured upon quicklime slaked by pouring water on it and not yet quite dry, heat and a pretty brisk fermentation will result from their action upon each other—a clear proof that an alkaline salt exists in quicklime. For in this case the heat in the previously slaked lime does not arise from the moisture of the vitriolic spirit but from the effervescence of its acid salt with the fixed salt of the lime.

Further, common sulphur, boiled in the water in which quicklime has been slaked, will dissolve exactly as it would in a liquid imbued with an alkaline salt. However, if spirit of vitriol or any acid liquid be poured into the solution, the sulphur will at once be precipitated with a fetid smell ; so that it is certainly established that the solution of sulphur in the water in which quicklime has been slaked, is due to its alkaline salt, for otherwise the sulphur would not be precipitated from that water when acid liquids are poured into it.

To this I further add that if quicklime be put into a solution of sal armoniac, whatever of acid there is in the sal armoniac will be absorbed by the fixed salt of the lime, while the volatile sal armoniac, liberated from the saline fetters, passes meanwhile into vapours, just as if fixed salt of tartar had been mixed with the sal armoniac. Now all these things clearly prove that quicklime and the water in which it has been slaked are impregnated with a fixed salt ; and this we may see for ourselves, for it is certain from common observation that an alkaline salt, or at least a nitrous salt partly composed of an alkali, exudes from walls which have been recently whitewashed and adheres to them.

Further, the existence of acid salt in lime may be inferred from what follows. For if a solution of any fixed salt be mixed with the water of quicklime, precipitation will immediately take place and the water will become milky, which would not, however, happen unless that water were imbued with some acid salt. Besides, the water of quicklime poured upon any volatile salt fixes it and changes it into an insoluble lime, as was observed by the learned Zwelfer. But it is well enough known that volatile salts are not fixed or changed in that manner except by an acid salt. Further, if water which has slaked quicklime be poured copiously into boiled milk, the milk will soon be curdled, just as if an acid liquid had been mixed with it.

From what has been said, it is evident I think that contrary salts lie hidden in lime, as may also be inferred from the very contexture of slaked lime. For whenever contrary salts in their encounter lay hold of any third matter, there is formed from the close combination of all a neutral body which is quite insipid, like *terra damnata*, and altogether insoluble in water. Of this sort are nearly all the Magisteries, such as those of hartshorn, of coral, and the like. Nor does slaked lime seem to be anything but a Magistery formed by a union of contrary salts with a stony earth. Indeed if salt of tartar be mixed with a solution of alum, a *tertium quid* will be formed that is somewhat sweet and astringent to the taste and not very different from lime, so that it is not at all wonderful that an alkali, pure and unmixed, is not drawn out from quicklime by pouring water upon it; for its contrary salts act on each other when water is poured on it and are turned into a neutral body. But since the acid salt of the lime is not united firmly to the fixed salt, as will be shown below, the fixed salt extricates itself in the course of time from the fetters of the acid salt and at last, thrust from the structure of the lime, adheres to the whitewashed walls.

We notice, lastly, here, in support of the foregoing, that quicklime will not become warm if sprinkled with highly rectified spirit of wine, or spirit of turpentine, or with other liquids of that kind. And the reason seems to be that spirit of wine and liquids of that sort, abounding in volatile sulphur, are quite incapable of dissolving fixed salts, so that neither can the alkaline salt of quicklime be dissolved by them, and yet this is an indispensable condition of the production of heat in lime, as will be shown later. So that it is abundantly clear that the heat of quicklime is not, as is commonly supposed, produced by the igneous particles which are simply fixed in it, being thrown, at last, into vigorous motion by the humidity which is antagonistic to them, but by the liberation and effervescence of its salts; inasmuch as quicklime does not become hot when moistened by any liquid indiscriminately, but only by such as are fitted for speedily dissolving its salts. Nay, the liquids which, in consequence of abounding in volatile sulphur, are best adapted to set igneous particles in motion and produce heat, when poured on quicklime produce no heat in it, although, on the other hand, the same lime becomes very hot when sprinkled with aqueous fluids, the chief extinguishers of fire and heat. But this can result from no other cause than the fitness of the latter, but not of the former, liquids to dissolve its salts.

It remains now to inquire whence these different salts of lime trace their descent. In the first place, then, with respect to the origin of the fixed salt, it is generated in the same way as in earth. For as the seeds of fixed salts lie hidden in the earth's bosom, as was elsewhere shown, so too, they may be found in shells, chalk, and stones; and the proof of this is that most of these effervesce with any acid spirit poured upon them.

As regards the origin of the acid salt of quicklime, we must believe that it is made by the action of nitro-aërial and igneous particles during the long calcination of the calcareous stone. For as the acid spirit of sulphur is produced from the more fixed particles of common sulphur, also the spirits of vitriol and of nitre from metallic or earthy sulphur, by the action of nitro-aërial and igneous spirit, as I have before attempted to show, so it is likely that the nitro-aërial particles of fire encounter, in the course of the long-continued calcination of the calcareous stone, the more fixed particles of the sulphur of the stone (for calcareous stone, like flints, contains much sulphur), and rub and sharpen them, and at last convert them into an acid salt in the manner described above.

Let us see next how it is that contrary salts—acid, to wit, and alkali—subsist together in quicklime and yet do not act upon each other until water is poured on the lime. As regards this, it is probable that the acid spirit of quicklime becomes so sharp and fiery in consequence of its long calcination, as to be altogether unfit for engaging with fixed salts until its more powerful igneous force is diluted by admixture with water and to some extent moderated; for saline solvents are sometimes so corrosive that they fail to dissolve or in any way affect metals, which in their nature closely resemble fixed salts, till their too keen force is diminished by admixture of water.

Further, the fixed, like the acid, salt of the quicklime becomes in the highest degree biting and fiery by reason of the fiery particles infixed in it during its long calcination. For it is to be noted that although nitro-aërial and igneous particles are of a saline nature, still they are opposed neither to acid salt nor to alkali, but, on the contrary, when combined with either, increase its power and render it fiery. But since nitro-aërial particles are fixed in dense number in the acid, and in the fixed, salt of lime, it comes to pass that these contrary salts are kept apart and, as it were, reconciled by the mediation of the nitro-aërial particles which are in harmony with both, so that they are unable mutually to attack or act upon each other. But when these salts are diluted with water, they lay aside, at least to some extent, their fiery particles and become less acrid, as will be apparent if fixed salts exposed to a hot fire are afterwards dissolved in water; for then the salts which the fire has made extremely acrid and caustic will lay aside their acridity and return to their original state. Hence it is that the contrary salts of lime, after they have been dissolved in water, are then fit for acting upon each other and for mutual effervescence.

And in this we have an explanation of the fact that quicklime does not become hot when sprinkled with spirit of wine or other sulphureous liquids of that sort, as has been said before. For since such liquids are incapable of dissolving the fixed salt of the lime, they cannot temper its too acrid and fiery force; yet this is absolutely necessary for producing its heat.

I add, lastly, that the acid salt of the lime seems to contract a somewhat dry nature on account of the very dry and solid nitro-aërial particles densely fixed in it; whence it is that that acid spirit can remain so long in the hottest fire. For the extremely solid nitro-aërial particles, densely fixed in the acid salt, make it somewhat rigid, so that its particles cannot be carried aloft by any force of fire. And hence it is that that acid salt of lime, being of a drier nature, will not engage with its fixed salt until it has been dissolved by pouring water upon it.

But yet another difficulty is here presented. For admitting that contrary salts exist in lime and in the water in which it has been slaked, how does it happen that these opposing salts, even after they have acted upon each other, should reside in the said water with their strength almost unimpaired? For neither is utterly destroyed, but each of the two performs the operations appropriate to its nature, as was shown above; while yet in other cases when opposite salts are mixed, either both succumb, after a struggle in which each engages with equal strength, or one of them gains the mastery while the other is completely conquered.

In regard to this, it is probable that although in the lime and in the water in which it is slaked, the acid salt and the fixed salt combined together are changed into some neutral substance, yet the acid salt and the fixed salt are of such a kind as to be by no means fit for mutually subjugating themselves and destroying their powers. But to make this more intelligible, it will be of advantage, I think, to premise some brief observations on the combination of contrary salts with each other and with other substances.

OF THE COMBINATION OF CONTRARY SALTS, AND PRECIPITATION

In the first place, then, it is to be noticed that although acid salts and alkalies pass into a neutral substance when they meet, yet they do not, as is generally supposed, entirely destroy each other. For example, when the acid spirit of salt is coagulated with a volatile salt (and the same explanation applies to sal alkali), although the mixed salts seem to be destroyed, yet they may be separated from each other with their forces unimpaired, as takes place when sal armoniac (or any volatile salt combined with an acid spirit) is distilled with salt of tartar. For in this case whatever of acid there is in the sal armoniac will be coagulated with the fixed salt of tartar, but the volatile salt, of which it also in part consists, ascends of the same nature as before. And the reason of this is, that the acid spirit of salt is capable of entering into closer union with any fixed salt than it is with a volatile salt, so that it immediately leaves the volatile salts that it may be combined more intimately with the fixed salt. But if oil of vitriol is united with salt of tartar, they can scarcely be separated from each other. And yet this is not because these salts have mutually destroyed each other, but because there is nothing in nature with which either of them can unite more firmly than they do with each other.

As acid salts leave volatile salts to form a closer union with the fixed salt of tartar, as being a more suitable partner, so doubtless fixed salts select some one acid in preference to others that they may combine with it in a closer union.

But to illustrate this by an example : if oil of vitriol is poured upon nitre, which consists of an alkaline and of a volatile acid salt (as was shown above), the fixed salt of the nitre will soon leave its own acid and will enter into union with the acid of the vitriol, which is more concordant with it ; so that nitrous acid, on account of the mixture with the vitriolic acid, is correctly said to be precipitated from the embraces of the alkaline salt. That the case is so, is clear, for if nitre mixed with oil of vitriol be distilled, the spirit or acid salt of the nitre will pass under a mild heat into the receiving vessel, while yet in other circumstances that spirit will not be carried up except by a very vehement fire. No doubt it is because the volatile acid salt of the nitre has been expelled from the society of the alkaline salt by the more fixed vitriolic acid that the acid of nitre, now liberated from union with the alkaline salt, ascends under a heat no greater than is required for the rectification of the spirit of nitre; while in other circumstances the same spirit of nitre is sundered only with difficulty from its union with fixed salt, and requires a very intense heat for its distillation.

It is a corroboration of this view that the mass left in the retort after a distillation of this kind, closely resembles vitriolated tartar, and can be properly substituted for it. For since the alkaline salt of which nitre is in part composed, differs scarcely at all from salt of tartar, a union of that salt with oil of vitriol will produce an acido-saline salt, differing not much from vitriolated tartar.

Nor is it mutually among themselves only that salts strive after union, but also with other things; and from them they part so as to combine with a salt more concordant with themselves. For example, any acid spirit at once attacks metals and combines with them to form vitriol. But if salt of tartar be poured upon these vitriols dissolved in water, the acid salt of the vitriols immediately combines with the salt of tartar, and the metal, freed from the fetters of the acid salt, will fall headlong to the bottom.

As an acid salt combines with metals, so also does an alkaline salt with sulphur. If, however, an alkaline salt in union with sulphur be dissolved in water, and then any acid spirit be poured into the solution, the fixed salt will instantly rush into union with the acid salt; and the sulphur, meanwhile, liberated from its union with the fixed salt, and rising in aërial form, will indicate its presence by its fetid odour—as happens when sulphur dissolved in lye is precipitated by the addition of an acid liquid.

Nor is it with an alkali only but also with the metals allied to it that sulphur seeks for union; yet in such wise that the metals, like the alkaline salt, will at once leave it to form a more intimate union with an acid poured upon it. For if an acid liquid such as aqua fortis be poured upon a metal rich in sulphur, say antimony, and then the mixture be heated, the sulphur will be sublimed to a considerable extent, since the acid salt of the liquid uniting with the metal expels its sulphur from its lodgment.

Lastly, as metals leave their own sulphur that they may combine with an acid salt, so sulphur will quit metals with which it is united, to coalesce in a closer union with a fixed salt. For if a metal abounding in sulphur, such as *stibium*, be boiled in strong lye, the metallic sulphur will dissolve in it, since the sulphur seeks to be united with the fixed salt of the lye rather than with the metal; but this metallic sulphur will be precipitated also from the fixed salt, if an acid salt is poured upon it.

It is also to be noticed here that although sulphur, like an acid salt, can combine with an alkaline salt and with metals, and can be precipitated from them, we must not therefore suppose that a certain acid salt (such as the *Oleum Sulphuris per Campanam*) lies hidden in a mass of sulphur, and that by its intervention the alkaline salt unites with the sulphur. For if alkaline salt and sulphur united together (as in liver of sulphur) be dissolved in water, and the acid oil of sulphur be then added, the sulphur immediately thrust out from its union with the fixed salt will be precipitated. And yet, if the combination of the sulphur with the fixed salt resulted from this, namely, that the acid salt contained in the sulphur unites with the alkaline salt, then the pouring of that acid or oil of sulphur upon them when combined with each other would by no means separate them from each other. Nay, if such an acid existed in sulphur, it would hinder altogether the union of the sulphur and the alkaline salt; since all acids (but especially one so corrosive as oil of sulphur) have the power of separating sulphur from fixed salt and precipitating it. I further remark that acid salts do not combine with alkaline salt or even with metals, without effervescence and a notable degree of heat. But such is not the case when sulphur combines with either of them; so that clearly the combination of sulphur with fixed salt would appear to result not so much from any antagonism as from their mutual affinity, as has already been shown in some detail.

I may here further remark, by the way, that salts of different kinds should not without the greatest caution be compounded together in the same medicine, lest one of them should entirely destroy the efficacy of another, and even change it into something quite different from what it was at first. For example, when obstructions or a diminished fermentation of the blood, point to the use of steel, it seems to me inadvisable to mix the salt of wormwood or any lixivial salt with the vitriol of Mars or with the aperitive *Crocus Martis*. For when that medicine dissolves in the stomach, the acid salt which the vitriol of Mars contains will immediately unite with the lixivial salt, and meanwhile the metallic part of the vitriol, driven out from its acid fellowship of the salt, will be precipitated as Colcothar or astringent *Crocus Martis*, which is by no means aperitive. For the case will be very much as if the acid salt of the said vitriol were expelled by fire, when nothing metallic will be left but Colcothar or astringent *Crocus Martis*. And indeed the lixivial salt also mixed with the said vitriol, will acquire a quite new nature because of the addition to it of the acid salt of the vitriol.

But now to adapt the foregoing to the subject in hand, it is seen that in quicklime and the water in which it has been slaked, the contrary salts are of a nature little suited to combine very closely with each other, and the proof of this is that either of them will immediately unite with a salt more suitable for it.

For first it is manifest that the acid salt of lime separates from the fixed salt with which it was united, that it may be more firmly combined with the salt of tartar. For if salt of tartar be mixed with water in which quicklime has been slaked, precipitation takes place at once and the water becomes turbid and milky. And the reason is, that although the acid salt of the lime is to some extent subdued by its partner the fixed salt, yet its powers are not so completely destroyed but that the acid is still able to dissolve a small portion of limestone, and besides to fix volatile salts as acids do. If, however, salt of tartar be mixed with the aforesaid water, the acid salt of the lime will combine most closely with it as being a more suitable partner, and their strengths will be utterly destroyed by each other; so that the limestone can now no longer be dissolved by the destroyed acid of the lime, but is precipitated to the bottom along with these combined salts.

And for the same reason the alkaline, or rather the fiery volatile salt of the lime, will at once desert its acid partner to which it is united, in order to form a closer union with the acid spirit of vitriol, which is more suitable for it. For if sulphur be dissolved in water in which lime has been slaked and spirit of vitriol be then added to the solution, the sulphur will presently be precipitated with fetid odour. For although already in the slaking of the lime the alkaline salt of the lime has united with its acid, not without effervescence and a quite notable heat, yet it will nevetheless immediately desert the acid betrothed to it to be married in closer wedlock with the vitriolic acid, by which however its powers are so completely overcome that the alkaline salt of the lime combined with the vitriolic acid is no longer able as before to dissolve sulphur ; but that a fixed salt is combined with an acid salt in the water in which quicklime has been slaked, and further that the heat of that water is caused by the union of contrary salts, will be made still clearer by what is to be said below.

Since the contrary salts in the water in which quicklime has been slaked are but little fit for entering into a very close union and for mutually destroying each others powers, each of them consequently can perform the operations appropriate to its own nature. And this is seen clearly in sal armoniac, in which the acid salt is combined with a volatile salt, and yet that acid salt is not so completely subdued by its unequal adversary, the volatile salt united with it, as to be unable to dissolve iron as acids do and to change it into vitriol. Yet if any fixed salt contends with the acid armoniac salt, then indeed its strength is completely destroyed so that it is quite incapable of dissolving iron any longer. And the case in fact seems to be similar in water of quicklime, for here the less opposing salts do not so completely destroy each other but that either of them can act according to its nature. For the fixed salt of that water is able to dissolve sulphur, and its acid salt can fix and destroy volatile salts, as was said above.

With respect to water which has slaked quicklime, we note further that oil of vitriol, when put into it, does not produce in it effervescence, or precipitation, or any degree of heat. And yet it is certain that the acid salt of vitriol unites with the fixed salt of the lime. For if spirit of vitriol be poured into a solution of sulphur made in water that has slaked quicklime, the sulphur will be immediately precipitated, as we have already remarked. And yet this would certainly not happen unless the acid of the vitriol uniting with the fixed salt of the lime drove the sulphur from its lodgment.

But to make the reason of this clear, it should be noted that if salts which are somewhat saturated and weakened by their opposites, afterwards meet a salt that is still more repugnant, there will yet be no effervescence or heat from their mutual action as there would be in other circumstances. For example, if oil of vitriol be united with any metal whatever with a distinct ebullition (as is the case when iron is dissolved in oil of vitriol) and then fixed salt of tartar is put into that solution, although the acid spirit of the vitriol combines with the salt of tartar, and the metal now liberated from the acid salt is precipitated, still no ebullition or heat will be produced in so far as the acid spirit of the vitriol was previously to some extent saturated by the metal joined to it. And the same thing also happens when salt of tartar is mixed with a solution of sal armoniac. For when this is done the salt of tartar absorbs, without any ebullition, whatever acid there is in the sal armoniac.

And for a perfectly similar reason the alkaline salt of the water in which quicklime has been slaked unites without any effervescence with oil of vitriol poured into it, because the alkaline salt of the lime has been previously saturated with its own acid. Consequently that the water in which quicklime has been slaked is impregnated with contrary salts and these combined with each other, is clearly evidenced by the fact that the alkaline salt of that water unites with any acid salt whatever, and its acid salt with volatile salt, without any effervescence or heat.

But further, that the precipitation may be seen in any liquid and that the liquid may become turbid, it is indispensably necessary that what is precipitated should be opaque and not transparent, in order that as it descends it may fill the pores of the liquid and so hinder rays of light from being transmitted through them, as happens in the precipitation of iron or of any other metal. For when iron dissolved by an acid liquid is driven from its embraces, it is no longer transparent but resumes its previous state, namely, that of a solid and opaque body, so that the iron, as it goes down, obstructs the pores of the liquid and renders it turbid.

But if that which is precipitated be nowise opaque, but transparent or soluble in the liquid, then the liquid in which the precipitation takes place will become in noway turbid. Hence when oil of tartar *per deliquium* is mixed with a solution of sal armoniac, although the fixed salt of tartar absorbs the acid of the sal armoniac and the volatile armoniac salt is precipitated from its union with its partner, still the liquid does not become in the least turbid, because that volatile salt, in the course of its descent, dissolves in the liquid and does not at all obstruct its pores. Similarly, when an acid salt is precipitated from its union with a fixed salt by the addition of a more appropriate acid (as happens when the alkali of which nitre partly consists leaves the nitrous acid and combines with oil of vitriol poured upon it, or also when the alkaline salt of quicklime leaves its own acid and combines with the acid of vitriol), although the acid salt is first precipitated from the embrace of the alkaline salt, still the liquid does not become in the least degree turbid, because that acid salt which sinks is in nowise opaque but soluble in the liquid, and consequently transparent and imperceptible.

Nor should we omit to state that quicklime mixed with the lye of ashes renders it more powerful and acrid. And yet in water which has slaked lime the acid salt, which is entirely opposed to the lixivial salt, seems to have the upper hand, since precipitation will take place in that water if an alkaline salt is put into it, as was said before.

In regard to this it is probable that the aqueous part of the lye, when poured upon quicklime, no sooner meets the very fiery and dry acid salt of the lime and renders it fit as it were for dissolving, so that it effervesces with the alkaline salt (for it was shown above that the acid of lime does not, unless diluted with water, effervesce with an opposite salt), than the alkaline salt with which the lye is imbued combines at once with the acid salt of the lime and so destroys its powers that the alkaline salt which belongs to the lime is not now, as in other cases, conquered and subjugated by the acid of the lime, but may be extracted from the water of the lye with its powers unimpaired. Since, therefore, the alkaline salt of quicklime is extremely acrid and fiery, it necessarily follows that the lixivial water impregnated with it becomes in the highest degree biting, caustic, and fiery.

CHAPTER XV

OF THE THERMAL WATERS OF BATH. INCIDENTALLY, OF THE SOURCE OF SPRINGS

Among the most famous thermal waters are to be reckoned those of Bath, for in these wonderful waters there dwells perpetually a vestal and sacred fire—a friendly treaty being as it were formed between elements the most discordant.

Before coming to the question of how these springs are warmed, it will be of advantage, I think, to make a brief inquiry into the ingredients of their waters.

In the first place, then, it is evident that the Bath thermal waters are impregnated with a salt of an acid nature; for if any alkaline or purely volatile salt is mixed with their waters, a precipitation will immediately be produced in them, and they will become turbid and milky. Further, warmed milk will be coagulated if the water of these thermal springs is poured into it, precisely as if any acid liquid were used.

And yet the salt of the springs does not appear to be acid pure and simple, but combined with some alkaline salt: for if that water be evaporated to dryness, a salt of a more fixed nature will be found at the bottom of the vessel, since it effervesces if any acid spirit is poured on it. Of this nature, too, are the mud and sand of the springs which are thrown out along with the bubbling stream of water, for if any acid liquid is poured upon them, a marked ebullition will immediately be produced. It is also to be observed that a salt in these waters, or rather a limy earth, adheres almost everywhere to the bottom of the channels by which the waters are carried off.

From what has been said, it may be inferred that the thermal waters of Bath are impregnated with a certain acido-saline salt. And indeed this salt of the thermal waters does not seem to be very unlike vitriolated tartar or aluminous salt. But the reason why these salts do not destroy each other, but that each of them effervesces with a salt opposed to it, will be to some extent intelligible from what was said in the previous chapter. For the said salts are so imperfect that they are not able to destroy each other entirely when they combine together. But there will be a fuller discussion of these salts afterwards.

As for nitre and sulphur, with which it has been hitherto supposed that the Bath thermal waters are impregnated, I think that neither of them exists dissolved in the water of these springs.

That nitre does not exist in them is evident, because if the salts which remain after evaporating the water of the baths are placed on burning charcoal, they in nowise deflagrate like nitre. However, I will not deny that the immature salts of an alkaline nature (with which the mud and sand of the baths are imbued) may, perhaps, if exposed for some time to the air, be changed by its influence into nitre.

As to sulphur, which is so often said to be present in nearly all thermal springs, my opinion is that it does not exist dissolved in these waters. For if a solution of alum, or of vitriol, or any other salt, whether acid or fixed, is mixed with the water of these thermal springs, the precipitation of sulphur is not at all indicated either by a fetid odour or by any other sign. And yet that always happens in solution of sulphur made in the water in which quicklime has been slaked, or in lye, when the sulphur is precipitated from them by the addition of any acid liquid.

I am aware that the water of these thermal springs immediately becomes white if salt of tartar, or a simply saline volatile salt, be put into it, as was already remarked. But this white colour is caused by the precipitation not of sulphur but of a certain stony or aluminous matter, just as happens to water which has slaked quicklime when any fixed salt is mixed with it, though it is not to be supposed that sulphur is dissolved in it. For if sulphur were boiled in the water in which quicklime has been slaked, the water would not become white, as before, on the addition of fixed salt, but would do so on the addition of acid. So that fixed salts appear manifestly to be fitted for dissolving sulphur but not for precipitating it. Wherefore if the waters of the thermal springs were imbued with sulphur, they would not be precipitated as they are by a simply saline salt but by an acid. And indeed the sulphur precipitated by them would manifest itself by a fetid odour, but this does not at all happen.

I further add that some acid or aluminous salt seems to preponderate in these thermal waters, so that they are quite incapable of dissolving sulphur. Nay, if common sulphur be boiled in the said waters, these waters will not be at all tinged with the yellow colour of sulphur, nor can sulphur be precipitated in any way from the said decoction, as I have found by repeated trials. And indeed I am greatly surprised that the distinguished Willis in his Treatise on the Heat of the Blood, has asserted that sulphur can be dissolved when boiled in the water of the said thermal springs, just as in water that has slaked quicklime. But if sulphur ever appears to be dissolved in the said waters, I think the source of the error lies in the fact that the decoction was made in a vessel, used for such purposes, in which perhaps at some time a fixed salt had been boiled ; so that the solution of the sulphur might be caused by a small portion of the fixed salt with which the vessel was imbued.

It is generally believed that if silver be immersed in the hot Bath waters it will receive a yellow tinge, just as if it were put into a solution of sulphur ; and hence the common belief that these thermal waters are impregnated with sulphur ; but the contrary of this has been ascertained by experiment. For silver put in the baths does not become at all red or yellow, but rather black. But the error seems to arise from this, that the keepers of the baths are in the habit of tinging and as it were gilding silver coins with a saline-sulphureous mud such as is found everywhere in sewers, and then selling them for a small sum to visitors, as coins coloured by the waters of the baths.

It should also be noticed here that a certain bituminous mud and a small quantity of common sulphur are usually thrown out along with the gushing waters of the thermal springs ; yet these eithe float on the surface of the waters or lie at the bottom, but are in nowise dissolved in the said waters.

Nor are the said thermal waters impregnated with armoniac salt, as some have supposed ; for if salt of tartar is put into a solution of sal armoniac, the simply saline volatile salt (of which sal armoniac partly consists), released from the fetters of the acid salt with which it was previously united, will instantly rise in vapours and will soon betray its presence by assailing the nostrils which approach it. But this in noway happens with the thermal waters.

Lastly, as regards vitriol, the bath usually called the Cross Bath, and also the one named the Hot Bath, appear to contain no vitriol whatever. For if pounded gall-nuts are infused in the waters of the said baths, the waters will by no means assume a purple or a black colour, and yet this would certainly be the case if these waters were imbued with vitriol. With respect to the so-called King's Bath, it seems to be impregnated with a little vitriol; for if pounded gall-nuts are put into its water, it will be slightly tinged with a blackish-purple.

But it should be noted that along with the gushing waters of the said hot springs, there rushes out from the earth a mineral of a metallic nature which can easily be changed into vitriol. For if any acid liquid is poured on the sand which escapes from the earth along with the water of the hot springs and is found at the bottom of the baths, this sand, when corroded with considerable effervescence by the acid menstruum, will in part be turned into vitriol, just as iron filings are when corroded by an acid liquid. For if that sand of the baths, when impregnated with an acid liquid, be added to an infusion of galls, the liquid will at once acquire a dark purple colour; while, on the other hand, if the infusion of galls be poured upon the sand when taken fresh from the baths (but not yet corroded by an acid liquid), it will not become purple at all—undoubtedly a clear proof that the metallic sand of the baths assumes the vitriolic nature only when corroded by an acid menstruum. It is to be noticed also that this sand of the hot springs will spontaneously change into vitriol if kept for a considerable time and exposed to the air; for if such sand is put into an infusion of galls, the water will at once take a dark purple colour. Nay, if it be put on the tongue, the taste of vitriol will be manifest enough. Indeed nitro-aërial spirit, after a lapse of time, unites and effervesces with the metallic mineral or saline-sulphureous marchasite (such as vitriol is usually made from) which is mixed in the said sand, and at last turns it into vitriol in the manner described in another place.

Let us next consider how not only the hot springs of Bath but all thermal springs acquire their heat. With regard to the heat of thermal springs, I cannot agree with those who maintain that their heat is due to warmth communicated by subterranean fire. For if any fire of this sort burned in the bowels of the earth, it would certainly betray itself by its flame bursting out somewhere; since indeed a continual influx of air is absolutely necessary for the maintenance of fire. But granting that there are subterranean fires, how is it, I ask, that they are not extinguished by the waters which they are supposed to heat? And whence should the material be supplied for maintaining a fire so vast and enduring?

Wherefore, since it is not probable that the hot springs are heated by subterranean fires, we may maintain with our learned countryman, Jordan, that the warming of the hot springs originates in fermentation excited somewhere in the bowels of the earth. But to understand how this fermentation is produced the following must be premised.

In the first place, then, it may reasonably be supposed that at any rate most fountains derive their origin from rain-water. For it is not probable that any of them are derived, as is generally believed, from the sea; for who can entertain the idea that the gushing waters which burst forth from the summits of the loftiest mountains have their origin in the depths of the sea? For it is by no means to be believed that sea-water ascends by filtration to the tops of mountains; for in whatever way any liquid may rise up through a filter, yet it will not flow out of the filter unless its other end is at a lower level than the liquid; and consequently sea-water obviously cannot gush out by means of filtration from places at a higher level than the ocean itself.

Whether sea-water, which, in consequence of being impregnated with salt, is heavier than spring-water, is comparatively so much heavier as to force spring-water to the tops of lofty mountains, as the ingenious Dr R. Hooke has suggested, I will not definitely say; yet it can scarcely be thought that any springs are due to this cause. For if any had their source in the sea, how are we to explain that most fountains follow the temperature of the air and almost fail in very warm weather, or also in frost? Should any one say that the aqueous particles are by reason of the greater heat converted into vapours and ascend, and that therefore the springs dry up in summer, I ask, how is it then that fountains are diminished when the earth outside is so bound by frost that the passage for vapours is entirely closed?

I think, then, that it should be held that at least the majority of springs have their origin in rain-water. And yet I would not be understood as supposing that rain-water is stored in certain subterranean caverns, as it were in special cisterns, whence it flows out into springs. For such a supposition is unnecessary when the thing can be otherwise explained. Now since the surface of the earth is like a sponge or filter, it can absorb rain-water in quantity sufficient to form springs that will endure for a long time. For we observe that the outer crust of the earth is always steeped in a sort of moisture to the depth of several feet, and that aqueous particles escape from it only very slowly. And hence it is that the ground wet with showers can be dried up only after a long time. It corroborates this view that if a pair of cuts, several feet deep and of sufficient length, are dug in almost any soil so that they meet at their lowest part, the drops of water flowing here and there from the banks and uniting at last at the bottom, will make an artificial spring or rather rivulet. And this shows why it is that spring-waters well forth with greater or less abundance according as the weather has been wet or dry. Now this would by no means be the case if springs were derived from the sea and not from the air.

Further, it must, I think, be granted that nearly all the mountains in every part of the world consist of saline-sulphureous matters such as vitriolic or aluminous marchasites and the like.

I remark, in the last place, that if saline-sulphureous earth of this sort be wetted with rain-water it will effervesce and grow warm. For if marchasites or saline-sulphureous masses, such as vitriolic and similar salts are produced from, be exposed to moist air and rainy weather they will soon effervesce markedly. Nay, if any saline-sulphureous mineral, recently dug up, is wetted with rain-water it will after a short time effervesce and grow warm.

Let us next inquire why saline-sulphureous minerals of this sort effervesce when wetted with rain-water, for it is to be supposed that the heat of the thermal springs results from the same cause. And we may indeed suppose that the aërial particles which are mingled with almost all water, but especially with rain-water, contribute in no small degree to produce the said effervescence. For that air is mixed with aqueous particles has been placed beyond all doubt by Boyle's experiments; for if spring or rain-water be put in a glass from which the air is exhausted by suction, innumerable little bubbles, formed of the substance of air, will burst from it. In fact, as soon as the pressure of the atmosphere is almost removed, the aërial particles dispersed through the mass of the water immediately open and stretch out in virtue of their elastic force; so that the little portions of air which previously lay hidden and quite inconspicuous in the pores of the water, now swell into little bubbles which are forced upwards on account of their lightness. Moreover, that air resides in the small pores of water will be further proved by the following experiment. For let a small glass be placed in such a way under water contained in a suitable vessel, that all the air may escape from it and water enter in its place. Then let that glass, filled with water, be inverted and placed at the bottom of the said vessel, and let it remain so, much as in Plate V., Fig. 3. After these preparations, let the vessel into which the inverted glass is put be placed on the fire so that the water contained in it may be heated, and then let the vessel be removed from the fire so that the water may again cool. When this is done, we shall find that the top of the inverted glass is occupied by a kind of air partly composed of the vapours raised from the boiled water and not yet completely condensed, but partly of aërial matter. For since the air mixed with the water contained in the glass is rarefied by the heat of the fire, and consequently occupies more space than formerly, it is raised in the form of little bubbles to the top of the glass by the pressure of the surrounding water. And that this aërial matter is in part air, I infer also from this, that a certain portion of it will never condense into a liquid. It possesses, moreover, as much elastic force as ordinary air, as I have ascertained by the method elsewhere described.

To this I add, lastly, that air interspersed in water is drawn in by fishes for respiratory purposes. And indeed the gills with which fishes are endowed seem to be formed for this very end, that air (which is absolutely necessary for animal life) may be separated from the water by their action and mixed most intimately with the mass of the blood. And the reason that fishes are always engaged in alternately drawing in and expelling water, as terrestrial animals do common air, is that something aërial which is necessary to life may be separated from the water, as in the other case from the air, and passed into the mass of the blood.

It corroborates this view that most fishes possess a swimming-bladder filled with air. For there can be no doubt that fishes draw this air from the pores of the water. Let me say also in passing that if a fish is placed in water contained in a suitable vessel from which the air is exhausted, it will no longer swim on its belly, but on the contrary on its back. Its belly will also rise to some extent above the surface of the water. And the reason seems to be that the air enclosed in the swimming-bladder expands in virtue of its elastic force as soon as the pressure of the external air is withdrawn, so that the said bladder and also the abdominal cavity in which it lies are inflated by the air, with the result that these swollen parts are forced upwards by the pressure of the water while the back of the fish descends. But if after the fish has died for want of air, external air is again admitted into the glass, the fish will immediately sink to the bottom and its belly will again become flaccid.

Whether the air contained in the said bladder can pass into the mass of the blood and supply fishes with material for respiration I shall not definitely say, although that such is the case seems to be indicated by the fact that fishes can live a little longer than other animals when deprived of air—unless indeed this should be accounted for by the circumstance that fishes, owing to the very languid fermentation of their blood, consume a comparatively small quantity of air, so that, unlike most animals, they do not need to have a perpetual supply of air.

And now, since the air is intermingled so largely with rain-water, it is probable that a saline-sulphureous mineral effervesces when wetted with rain-water because the aërial particles, which are conducted by the aqueous particles, enter deep into a mineral of that nature and effervesce with its saline-sulphureous part. For I have already endeavoured to show that air is possessed of a highly fermentative nature, and that nearly all heat results from the effervescence of something aërial with saline-sulphureous particles. In fact, a saline-sulphureous mineral of this sort behaves not very differently from the mass of the blood, the heat of which arises from this, that the aërial particles conveyed to it by respiration effervesce conspicuously with its saline-sulphureous particles, as has been elsewhere pointed out more fully.

With respect to the heat of the thermal waters, I think it should be held that the aërial particles which descend with rain-water into the depths of the earth and meet there with the saline-sulphureous mineral, excite in it a very intense effervescence and heat ; and that springs of water, flowing from the mineral thus effervescing, constitute the thermal springs.

Further, it may well be supposed that the earth is penetrated in some places by pores of a kind suitable for the passage in dense numbers of aërial particles, and for sucking them in, as it were. For since the aërial particles, carried to the saline-sulphureous mineral and effervescing with it, are swept away by the streams of water flowing thence, the result is that the nearest particles of air are forced by atmospheric pressure into the place of those carried away, while these also are soon absorbed and followed by others; and thus aërial particles come to the effervescing mineral in a continual stream.

Perhaps the lofty hills, with deep and abrupt valleys between, which engirdle Bath on all sides, contribute somewhat to the entrance of aërial particles into the ground and to the maintenance of the heat of the thermal waters. For, in consequence of this, the air thrown back from the hills, and reflected in various directions in the valleys, strikes forcibly against the ground and is thrust into it.

It is a further proof of the views set forth, that the Bath waters contain a saline-sulphureous mineral, which seems not very unlike those marchasites from which vitriol is made; and indeed the sand of the baths, if exposed for some time to the air, will effervesce and turn into vitriol. But the reason that the salts of the said thermal waters are of an acido-saline nature seems to be, that when the saline-sulphureous mineral effervesces in the manner already described, some of the saline particles are brought to a certain fluidity, as was previously remarked. But these salts, when thus liquefied, unite with the other salts which are of an alkaline nature, and from these, combined with one another, a certain acido-saline salt is composed. It is, however, only an immature salt, since it is swept away, while not yet duly fermented nor brought to proper maturity, by the waters which flow from the effervescing mineral.

Should any one now ask how any mineral can possibly suffice for so long-continued a fermentation, I reply that the earth is imbued in certain places with a mineral seed, which, like vegetable seed, grows and reaches maturity; and that owing to it the waste of the said mineral is constantly repaired.

Finally, we remark here that nearly all springs have a certain warmth at their first rise; and indeed the heat of spring-water just escaping from the earth can be quite sensibly felt in winter when the hand is immersed in it. No doubt the aërial particles which descend with rain-water into the earth effervesce in an obscure motion with the saline-sulphureous particles of which nearly every kind of earth is composed; and consequently a certain warmth is produced, on which the growth of vegetables depends, as has been elsewhere shown. And hence it is that spring-waters are for the most part impregnated with acido-saline salts. For if salt of tartar is mixed with spring-water, precipitation usually takes place in it, and, like the aforesaid thermal waters, it becomes whitish—a clear proof of the presence in the water of a salt of an acid nature. And this, too, is the reason that soap does not mix with spring-water; for the acid salt of such water contends with the fixed salt of the soap, and so destroys its powers that the sulphureous and oily part of the soap cannot be dissolved in the spring-water by the fixed salt, now subdued, but floats on the surface in accordance with its oily nature. Moreover the acid salt of spring-water seems to be combined with a certain alkaline salt, although the latter be immature; and therefore it is, that oil of vitriol, when mixed with this water, produces a kind of effervescence.

SECOND TREATISE

ON RESPIRATION

The lungs are placed in a recess so sacred and hidden that nature would seem to have specially withdrawn this part both from the eyes and from the intellect; for, beyond the wish, it has not as yet been granted to any one to fit a window to the breast and redeem from darkness the profounder secrets of nature. For of all the parts of the body, the lungs alone, as if shrinking from observation, cease from their movement and collapse at once on the first entrance of light and self-revelation. Hence such an ignorance of Respiration and a sort of holy wonder. Still, let me draw near to the inmost vitals, and, concerning so obscure a matter, make at least a guess.

In discussing on Respiration I shall follow the method pointed out by nature and begin therefore with inspiration.

Every one knows that when we inspire, air rushes into the expanded chest and inflates the lungs. But authorities are not equally agreed as to the cause of the air rushing in with such vehemence. Some account for it by a vacuum and an attraction of I know not what imaginary sort.

Others again suppose that the air about the chest, pushed forward by its expansion, propels that which is next it, and this again the next; and that so the propulsion goes on, and thus at last the air near the mouth is driven into the lungs.

But indeed this view assumes that every place is full and that this immense space cannot admit any additional air, however little. But there is no reason for having recourse to this fulness when the thing can be and ought to be explained otherwise; for we may believe that propulsion of that kind cannot take place in air, as it is a fluid and easily moved. But this will be made clearer by the following experiment. For let us suppose that the narrow neck of a large enough glass vessel is put into one's mouth; then, the nostrils being firmly closed, let the air be sucked from the glass and drawn into the lungs, which will certainly happen if inspiration begins, the chest being dilated. But certainly in this case, such a propulsion of the air from the thorax to the lungs cannot be propagated, because of the interposition of the glass. But perhaps you will say that some more subtle matter passes through the glass and is pushed into the thorax, and that this serves as material for respiration. But indeed if the case were so, how could it be explained that a small animal shut up in a completely closed glass will soon die, if particles suitable for respiration still pass through the glass?

With respect, then, to the entrance of the air into the lungs, I think it is to be maintained that it is caused in the following manner by the pressure of the atmosphere. For as the air, on account of the weight of the superincumbent atmosphere, not only rushes into all empty places, but also presses forcibly upon whatever is next it (as Boyle's experiments have put beyond doubt), it follows that the air, passed through the nostrils and the trachea, up to the bronchia or gates of the lungs, presses against the lungs from within and seeks an entrance into them. Hence it is that when the inner sides of the thorax (which by compressing the lungs from without were resisting the pressure of this air) are drawn outwards by muscles whose function it is to dilate the chest, and the space in the thorax is enlarged, the air which is nearest the bronchial inlets, now that every obstacle is removed, rushes under the full pressure of the atmosphere into the cavities of the lungs, and by inflating them occupies and fills the space of the expanded chest.

The structure of the lungs is adapted for their inflation as thus described, for their substance is composed, as the eminent Dr Malpighi has noted, of very fine membranes, which form an almost infinite number of spherical vesicles whose mutual connection is such that there is easy access from the trachea to those nearest to it, and from these again to others. Consequently when these vesicles are inflated by an inrush of air, the whole substance of the lungs must necessarily expand.

Nor is it only the pressure of the atmosphere, but also the elastic force of the air by which it tends to expand indefinitely, that serves to inflate the lungs and cause inspiration, as takes place when the air, on the nostrils being closed, is drawn into the lungs (in the manner already described) from a glass placed in the mouth, and also when a small animal breathes in a glass that has been completely closed. For though the weight of the atmosphere does not, owing to the interposition of the glass, press or impel the air which it contains, still the air, in virtue of its elastic force, rushes into the expanded chest and lungs of the breathing animal and inflates them.

And indeed the force with which air that has not yet expanded seeks to enlarge its volume, is exactly equal to the pressure of the atmosphere, inasmuch as it depends upon it, and increases or diminishes according as this pressure is greater or less. For the elastic force of the air seems to be due to this, that the air, especially that near the earth, is compressed and its volume diminished by the weight of the superincumbent air ; hence it is that it always strives to expand, just as a fleece, when the force which compressed it is withdrawn, instantly unfolds and expands with a certain motion of restitution. And this can be confirmed by a well enough known experiment.

For if a bladder with most of the air pressed out of it, and tied by a tight ligature round the sphincter, be placed in a glass from which the air is afterwards exhausted, we shall at once see the bladder swell and become greatly distended, a rather pretty sight by the way. For although a very little air was contained in the bladder, yet when the external air (by the pressure of which it was reduced to small volume) is removed, it immediately expands and inflates the bladder, indeed sometimes violently bursts it. And, in fact, the inflation of the lungs is effected in a not very different way. For as soon as the sides of the thorax (which by compressing the lungs make them shrink) are drawn outwards, the air at the entrance to the lungs is immediately driven into them, whether by atmospheric pressure or in consequence of its own elastic force, and distends them.

But we may illustrate the inflation of the lungs in this way by yet another example. Let us then suppose that a bladder is enclosed in the cavity of a pair of bellows, and that its neck is so fixed to the pipe of the bellows which is placed in it, that air blown into the pipe can pass only into the bladder, as is seen in Plate II., Fig. 6. (But to ensure this result, the bladder should be fitted in that manner to the pipe before the latter is attached to the bellows.) Further, let the hole be made not as is usual in the lower blade of the bellows, but in the upper one, and let it be of considerable size. Let it also be perfectly closed by a plate of glass, attached to the blade by a suitable cement, so that the bladder shut up within may be seen through the glass as through a window. When these preparations have been made, you will see, if the bellows are opened by drawing the blades apart, the bladder swell and extend into the enlarged cavity of the bellows, as is shown in the figure referred to. And indeed it is plainly in the same way that the inflation of the lungs in the dilated chest is produced.

From this we conclude that the lungs are distended by the air rushing in, and that they do not expand of themselves, as some have supposed; for the muscles and fibres which are requisite for any motion are absent in the lungs. And indeed although the lungs were provided with muscles, under their contractile action the lungs would not expand but rather contract, as happens to the bladder and stomach, and other organs of the kind. And further, when the thorax is pierced, the lungs immediately collapse at that place—a clear proof certainly that their movement is not spontaneous, but depends entirely on that of the chest. As for the nerves which are distributed in the trachea and bronchia, they do not serve for movement, but for sensation and the nutrition of these organs.

It will be urged by those who maintain that the lungs move of themselves, that when the thorax is wounded the lobes of the lungs usually burst from the cavity in the chest and protrude through the opening of the wound, which would by no means happen if the lungs merely followed the movement of the chest and did not expand of themselves. This difficulty is thus answered by the learned Dr Highmore. The air, he says, pressing violently into the expanded chest and the lungs, does not instantly cease from its motion, but rushes where the way lies open and carries the lungs with it, on account of their extreme lightness, beyond the cavity of the thorax. But with all respect to such a man, the lungs do not (as I have ascertained by vivisections) protrude through the opening made by a wound in the chest unless the thorax is contracted; when, namely, the air does not, as the eminent man supposes, rush into the lungs, but, on the contrary, is driven out of them. So that it should rather be said, I think, that the lungs are so compressed by the thorax, which is everywhere contracted, that they burst forth where there is an outlet, that is, through the aperture of the wound; just as we see a sponge tightly compressed by the hands, protrude between the fingers if they are kept a little apart. But afterwards, when the chest expands and the lungs are no longer compressed by the sides of the thorax, now drawn outwards, the lobe of the lungs, which protruded beyond the cavity of the chest, will immediately return to it, unless perhaps, in consequence of its being caught tightly in the lips of the wound, the outlet for the air is closed and the lobe is kept inflated outside the thorax.

Nor is there more force in the objection to what has been said, that, in trifling wounds of the chest, the lungs are yet observed to move somewhat. For although the air rushes into the thorax through the aperture of the wound, so that the lungs are externally compressed by it, still air, in quantity sufficient to fill the dilated chest, cannot enter immediately on account of the smallness of the wound, and therefore air to fill the chest must rush in partly through the wound and partly through the trachea. Hence the lungs only partially expand, in the space, namely, of the enlarged thorax, as yet unoccupied by the air entering through the wound. But when the chest contracts, most of the air which has entered through the wound is expelled through the aperture of the wound, since its volume is too great to be retained in the now diminished cavity. When the chest, however, once more dilates, the air, as before, rushes not only through the wound but also by the trachea, into the lungs and dilates them (although with greater difficulty on account of the mass it has to raise), and in this way some motion of the lungs is kept up in the wounded chest.

Here, by the way, surgeons should be warned not to close the wound if the chest has been perforated except when the thorax is contracted to the utmost; for, otherwise, if the opening made by the wound is closed when the chest is dilated (that is, when the air has filled the interior cavity of the thorax), it will be impossible for the chest to contract on account of the resistance of the air inside, or for the lungs to expand, except partially, and, in consequence, suffocation will necessarily follow.

Now that it has been shown that the entrance of air into the lungs depends upon the dilatation of the chest, it remains for investigation how the chest is expanded. And here, following not so much the authority of writers as the truth, I shall state briefly what the thing itself teaches.

The received opinion is that of the intercostal muscles, only the external serve to dilate the chest, while the internal, on the other hand, contract it. But it seems to me more reasonable to suppose that the chest is dilated simultaneously by both. And that this may be better understood I shall premise the following observations.

We may affirm that the raising of the ribs dilates the space within the chest and that their depression diminishes it. For we suppose here (what any one may see in a skeleton) that the ribs (especially the lower, which contribute most to the dilatation of the chest) are not articulated to the spine and sternum at right angles, but that the angles below the ribs are a little less than right angles; so that if a rib is raised, its articulations with the spine and sternum will approach to right angles. We assert further that the chest is dilated by the ribs when raised to right angles. For let us imagine a number of arches lying upon a plane, as represented in Plate II., Fig. 1; while they remain in this position there is no space at all between the arches and the plane, for we suppose that thev are in mutual contact. If, however, the said arches are raised somewhat above the plane, there is a certain intervening space, and, in proportion as they rise towards right angles, the spaces between them and the plane become greater. Now it is precisely the same in the case of the chest. For let *a*, in the figure be the spine; *b*, the sternum; and *c*, *c*, *c*, *c*, the ribs. Let the plane between the spine and the sternum be the mediastinum, or any imaginary plane dividing the thorax into equal parts: how much the more then the ribs arched over the said plane (or what is the same thing, over the spine and sternum, which are in that plane) approach to right angles, so much the greater will be the space which lies between the raised ribs and the mediastinum, as we have already shown. And thus it is clear that one half of the chest is expanded by the ribs being raised towards right angles, and it is evidently the same with the other side. As for the false ribs, although their extremities are connected not with the sternum but with the diaphragm, they have notwithstanding the same motion, and in like manner dilate the chest. But since the ribs when drawn upwards approach nearer to right angles with the spine, and the ribs when raised to right angles open up a space in the thorax, it follows that when the ribs are drawn upwards they dilate the chest, which is what we undertook to prove. Nay, any one can experience in himself that the ribs are drawn upwards in inspiration and the dilatation of the chest, but that they descend in expiration and the contraction of the chest.

This premised, if the ribs are raised by the intercostal muscles, even the internal ones (which has next to be proved), it follows necessarily that the chest is dilated by their contraction.

Whenever, I say, a muscle attached to two bones contracts, the bone which is less fixed moves towards the other which is more fixed. Wherefore, since every lower rib is less fixed than the one above it, each of the lower ribs must be elevated when the intercostal muscles, even the internal ones, contract. For a quite similar reason holds with the internal as with the external muscles; nor is it an objection to this that the former are attached to the ribs in a different position, as is obvious from Plate II., Fig. 2, where the interior muscle *a*, *a*, will raise the lower and more mobile rib, in contracting, as well as the exterior muscle, *b*, *b*. And here it is to be noted that the ribs are so articulated with the spine that when they are pulled by the said muscles they easily ascend and are raised with rotation.

Nay, the very position of the muscles makes this plain, for if the interior muscle which is placed between the two lowest ribs were to pull the upper of the two downwards, all the ribs, since they are firmly attached to each other, would necessarily be drawn downwards at the same time, a thing which that weak and membrane-like muscle cannot do. How much more probable is it that the lower ribs are all drawn upwards together by the internal muscles between the upper ribs, for these are sufficiently strong and broad, while the weaker muscles between the lower ribs contribute merely to elevate the lower ribs. These things will be more clearly seen from Fig. 3, which shows the ribs and the internal muscles.

And this view is also supported by the oblique and contrary position of the intercostal muscles. For Nature seems to have inserted these muscles obliquely in the ribs (although a direct insertion would have suited better for moving them up or down) because the intervals between the ribs are so small that if these muscles had been inserted at right angles, they would have been shorter than the nature of muscles admits. Wherefore, that these muscles should have a suitable length, they had to be inserted obliquely (as they are) to the ribs. Yet as this oblique position is less suitable for elevating the ribs, Nature, that most wise engineer, has arranged the muscles with divers aspects so that while they pull the ribs obliquely with equal force in this direction and in that, the ribs meanwhile rise straight upwards, as is shown in Plate II., Fig. 4, where, when the external muscle *a*, *a*, and the internal *c*, *c*, contract together, the lower and more mobile rib will rise not obliquely but straight upwards, just as though it were pulled by a muscle attached to it at right angles. So that clearly the external and internal muscles contract simultaneously, and by their united effort elevate the ribs and expand the chest. Moreover, that the internal muscles do not cause expiration, may be gathered from the fact that the thorax in a dead animal is always contracted, for to die and to expire mean the same thing; but in the dead, the action of the muscles altogether ceases; and so this contraction of the chest cannot be caused by the internal muscles, since they no longer contract. Should any one remark here that that contraction of the thorax is caused by the internal muscles immediately before the death of the animal, I ask, in reply, how then is it that the external muscles (since the two cases are similar) never contract in the dying, so that the chest should remain for some time dilated?

It is probable then that, in expiration, the parts of the thorax return, by a movement of restitution, to their natural position without any aid from the muscles. For it is difficult and contrary to their natural position for the ribs to be drawn upwards, so that for this there is indispensable need of the twofold and united action of both sets of muscles, the internal and the external. But the ribs sink down again, without any work, as is clear in the case of a dead animal or a skeleton. Wherefore there is no reason for saying that Nature has provided as much muscular power for the latter action, which is clearly no action, as for the former, which is indeed a very difficult one.

And here it should be noticed that the ribs are joined to the spine, not as is commonly believed with a single but with a double articulation, and that these joints are placed so obliquely, and shaped with such contrivance, that the ribs cannot be raised by the intercostal muscles without being at the same time drawn outwards for the greater dilatation of the chest, as is clearly seen in Plate II., Fig. 5: in which let *a*, *e*, *i*, be a portion of a rib whose round head, *a*, enters *c*, the socket hollowed in the spine; this articulation is superior and interior. On the contrary, in the other articulation, the lower and exterior, a cavity, but a less conspicuous one, is hollowed out in the rib at *e*, and is articulated with the protuberance of the spine at *b*. And now, if we suppose the head, *a*, of the rib to be placed in the socket, *c*, of the spine, and the hollow of the rib, *e*, to rest on the protuberance, *b*, of the spine, and then the rib connected with the spine by these two articulations to be moved upwards, it is easily understood that the rib will be carried to the left, or, what is the same thing, outwards, in respect to the chest.

These articulations, and also their obliquity, are more noticeable in the skeleton of a sheep or of a horse than in that of a man. For it is to be observed that these articulations are much more oblique in some animals than in others; in animals, namely, which, destined for more violent exercise, have need of more violent respiration, the joints of the ribs are very oblique, in order that their ribs may be drawn more outwards by the contraction of the intercostal muscles, and that space enough may be opened in the chest for a sufficiently large expansion of the lungs.

Nor should it be overlooked that the cartilages, by the interposition of which the ribs are joined to the sternum, are inserted into the ribs with very notable obliquity, as is shown in Plate II., Fig. 3, in which *a*, *c*, is a rib, *c*, *e*, the cartilage, by means of which the rib is united to the sternum, *e*, the angle formed by the junction of the two. The obliquity, moreover, has for its object that the ribs may be extended and drawn outwards, circularly.

It is, besides, to be noted that in inspiration, especially when violent, the extremities of the false ribs move a little inwards; and the reason is that the diaphragm is joined to both ends of the false ribs, with the result that when it contracts, those ends are drawn inwards, but when it is relaxed they rise to their natural position.

With regard to the serrate, the *longissimus dorsi*, and the *pectoralis* muscles, it is probable that they have nothing to do with the expansion of the chest. For if the hand be placed on these muscles when we suddenly apply all our strength to dilate the thorax, it will be found that they neither harden nor draw together at all; yet this would happen if these muscles underwent contraction.

Besides the aforesaid muscles, the diaphragm also contributes to the expansion of the chest, and indeed ordinary inspiration seems to be mainly caused by it. In expiration the diaphragm being in diastole, and released from constriction and in a flaccid condition, is pushed upwards into the region of the thorax by the stomach and the other viscera contained in the abdomen; whence it is that it compresses the lungs and diminishes the space in the chest. But in inspiration the diaphragm is in systole, and contracted, so that it no longer remains greatly curved but is carried downwards and outwards as it flattens. So that the viscera of the abdomen, which were previously contained in its concavity, are, now that it has contracted, forced both downwards and outwards, and the space in the thorax which was previously occupied by the diaphragm and the said viscera is now left free for the expansion of the lungs. And indeed any one can feel in himself that the ribs rise in inspiration, while the viscera of the abdomen move both downwards and outwards under the pressure of the diaphragm. But all this will be more evident from an autopsy, for if the chest of any animal is opened and the diaphragm pressed downwards by the hand while the ribs are pulled upwards (and this is not done without much exertion), you will see that the chest dilates and that its capacity is enlarged. But as soon as the supporting force is withdrawn, the chest will of itself contract anew. For the diaphragm, pushed by the viscera in the lower part of the belly, will soon rise into the cavity of the chest, and the ribs, by descending forcibly to their natural position, will still further diminish the cavity of the thorax.

Hence if the stomach be too full, or if the liver or the rest of the viscera are much enlarged, respiration cannot go on except with difficulty, inasmuch as the said viscera, from their mass, press so much on the diaphragm as to prevent it from descending and enlarging the cavity of the chest. Still if, owing to an urgent necessity for more vigorous breathing, the violent contraction of the diaphragm forces the abdominal viscera downwards, notwithstanding their resistance, it not infrequently happens that its fibres contract too much in consequence of the violent strain, so that their tone is almost destroyed and respiration goes on afterwards with difficulty. This happens not infrequently in the horse when driven furiously immediately after a rather full meal. For, since there is need of more vigorous breathing when great exertions are made, and the diaphragm, for the wide dilatation of the chest, strives to descend further than usual, while the much distended stomach resists its movement, it comes about that its nervous fibres suffer not a little and are sometimes even broken. And hence when the breathing of a horse is injured in this way it is often, and not improperly, said to be *broken*. But when the diaphragm is thus weakened, respiration is carried on exclusively by means of the intercostal muscles, for these, when violently contracted, raise the ribs in a notable manner and enlarge the space within the chest, so that the defect of the diaphragm is to some extent supplied.

Similarly in *Orthopnœa,* in which the patient can only breathe in an erect position, it is probable that the abdominal viscera press too much on the diaphragm and keep it up in the chest, so that space enough for breathing cannot be provided in the thorax. If, however, the patient be placed in an upright position, the diaphragm, feeble though it be, aided by the weight of the same viscera, can force them downwards. And so, as the abdomen sinks, the chest dilates and the patient is able to breathe.

And quite similarly in the hysteric passion, when the organs in the lower part of the belly, convulsed and swollen, rise in a mass, and keep the diaphragm up, respiration must necessarily cease and suffocation follow, as it does. And it is reasonable to think that this is the cause of uterine suffocation, although I would not deny that the sympathy between the viscera and the throat, arising from nerve communication may contribute something to this. But certainly such a constriction of the chest does not seem to be caused at all by the convulsed and elevated diaphragm (as the renowned Dr Willis has supposed), for we have shown above that when the diaphragm is convulsed in inordinate systole and violently contracted, it descends and dilates the chest.

As for asthmatic paroxysms, in which the raised and distended lungs almost cause suffocation, there is no reason to think that the lungs are inflated by convulsion of the pulmonary nerves and thrown into excessive diastole. For granting that the lungs sometimes suffer convulsions, the contraction and convulsion of their fibres (if they have any) would involve rather the collapse and contraction of the lungs, as was previously pointed out. Nor is it probable that the lungs are thus inflated by vapours, for flatus contained in the pulmonary veins and arteries cannot inflate them. For even though flatus be introduced by a tube attached to the pulmonary artery the lungs will not swell. And any vapours contained in the vesicles of the lungs could certainly be expelled with ease along with the air. And therefore, I think, we should maintain that this kind of suffocation is caused by the convulsed intercostal muscles and diaphragm, for by their convulsion the chest is kept dilated too long, so that the lungs remain inflated and respiration is interrupted.

Nor should that affection be overlooked here in which the patient draws breath with difficulty and with a whistling sound. This ailment does not seem to arise always from phlegmatic humours choking the bronchia, but sometimes also in the following manner. When the diaphragm, whether owing to the pressure of the swollen viscera of the lower part of the belly or in consequence of its own weakness, is unable to contract and descend, and the thorax is therefore dilated only by the movement of the ribs, it comes to pass that the lungs, inflated by inspiration, cannot, because of the resistance of the diaphragm, attain their usual well-balanced position, and their lobes are necessarily bent divers ways ; and so the bronchia are bent and sometimes greatly twisted so that the air cannot pass freely through them, but, striking against them, will produce, as it does, the sound and whistling. And hence it is that this sort of affection sometimes comes on suddenly after taking flatulent food or drink.

Hiccup has also a claim to be reckoned among convulsive inspirations, for in it the diaphragm is contracted by violent but interrupted and often repeated systole, so that, in consequence of its contraction, the chest is suddenly dilated and the air rushes violently and not without noise into the lungs. For it must not be supposed that hiccup is produced by the movement of the stomach, but by that of the diaphragm. For the stomach when seized by convulsion by no means produces inspiration, as happens in this affection. Further, when the stomach is convulsed, the parts about it contract inwardly, as we can experience in vomiting, but in hiccup the abdomen is forced outwards, and this, as we have shown, arises from the contraction of the diaphragm. Yet since this ailment usually afflicts a too full or otherwise burdened stomach, we must suppose that the stomach, or rather its upper orifice which is attached to the diaphragm, is first affected, and that the diaphragm from its nearness and relationship takes on the convulsive movement.

It seems to be different in the case of that oppression at night, with difficult respiration, which is called nightmare ; for this state seems to be produced not by a convulsion of the parts about the thorax but by an impediment to the proper influx of spirits. This oppression generally attacks those who are falling asleep. For when sleep begins, the spirits which are the instruments of voluntary functions, retire towards the cerebrum, or at least no longer flow copiously from it. Meanwhile the spirits which have for their office involuntary actions and natural movements issue in continual flow either from the cerebrum or from the cerebellum. If, however, in consequence of any confusion, or from morbid matter causing disordered movement of the spirits, those of the latter kind as well as the former, while sleep is coming on, return towards the brain and are detained there, not only voluntary, but also natural actions are necessarily interrupted by the flow of spirits being impeded. Hence the actions of the heart, the thorax, and indeed of the whole body cease, so that the patient is necessarily affected with the very greatest oppression, suffocation, and a kind of immobility. That this motion of the spirits is in the wrong direction may be inferred from this, that the parts of the body, first those more remote from the brain and then those nearer to it, are gradually seized with a certain stiffness and weight in consequence of the lack of vital spirits. Meanwhile the spirits, detained in the brain and moving irregularly there, produce a feeling of giddiness and a disordered imagination. But when the paroxysm ceases, the spirits rushing impetuously from the brain usually excite a convulsive movement of the body accompanied by a sudden shout.

The asthmatic paroxysm which accompanies palpitation of the heart will be discussed in the Seventh Chapter of the Treatise *On Muscular Motion.*

So much then for inspiration which is effected by the aforesaid muscles. When, however, their contraction ceases the ribs sink of their own accord to their natural position, and the diaphragm, now relaxed and flaccid, is raised into the cavity of the thorax by the upward pressure of the viscera. And this we have said takes place without any muscular action in the case of a dead animal. And finally, when the chest is narrowed at almost every part, the lungs must be compressed by it and the air expelled; so that clearly the lungs do not subside of themselves, but follow the movement of the thorax.

But to more violent expirations the abdominal muscles also contribute. For the obliquely ascending and descending muscles (whose tendons are inserted into the lower ribs), in their contraction, draw the ribs downwards and narrow the chest. Further, the whole abdominal muscles simultaneously contracting, press the viscera which lie under them, so that the diaphragm is driven by their pressure and forcibly urged up into the chest. And any one can find out in his own case that in sneezing, coughing, laughing, and in every violent expiration, the muscles of the abdomen are drawn together and contract. Hence in laughter and in violent expirations the hypochondria often suffer pain from the convulsion of the aforesaid muscles.

From this we gather that laughter takes place without any action or contraction of the diaphragm. For in laughter the diaphragm is not, as some have supposed, drawn upwards when contracted by repeated irritations. For in systole and in its contraction, it is drawn downwards, as shown above, and so causes inspiration rather than that expiration which takes place in laughter. From what has been said it is evident that risibility is peculiar to man not because the nerve of the diaphragm communicates in man, but not in beasts, with the cervical plexus, and by means of it with the brain—an opinion which the learned Dr Willis has maintained in his book on the *Anatomy of the Brain*. For laughter does not take place because the diaphragm, on account of an instigation brought by the said nerve from the brain, contracts violently with repeated throbs and compresses the lungs, as this learned man supposed. For it has been shown that laughter does not proceed from the action or systole of the diaphragm, but, on the contrary, from its diastole.

We have spoken thus far of the manner in which respiration takes place, and it now remains for us to inquire into its use. This is indeed a most difficult affair, for there is not more accord as to its necessity than doubt as to its use.

For not only is air inspired useful for tasting and smelling, and expelled, for talking, shouting, coughing, sneezing, and spitting, and again, when retained, for the expulsion of urine and fæces, for parturition, and for moving on the chyle, the lymph, and the blood; the breath we inspire is destined for a still nobler use: from which arises such a necessity of drawing breath that we cannot indeed live a moment without it.

Some suppose that respiration chiefly serves for cooling the heart; but heating rather than such a cooling seems to suit the circulation and fermentation of the blood. Nor indeed is the more frequent respiration in violent exercise for the purpose of cooling the blood which the motion heats. For in violent exertions, be they so momentary that the blood is not much warmed, there is certainly need of more intense respiration than in the greatest state of heat, and in fevers, that is when the blood boils more and is as it were on fire; so that respiration will be seen to serve not so much for cooling as for motion itself, as will be shown afterwards.

But the prevalent opinion is that respiration is necessary to life in order that the blood may be able to pass through the lungs from the right ventricle of the heart into the left. For the fœtus in the uterus, whose blood does not pass through the lungs but through special ducts, does not need to breathe at all. And this they say is the reason why there is not the same necessity for breathing in the uterus as after birth.

But there is no reason why we should say that Nature has constructed the lungs with so much skill and labour only that the blood may pass through them after birth, since it might pass by a shorter and much less obstructed road through the same channels it follows in the unborn fœtus. Nay, it is the case that the blood can pass through the lungs apart from their motion. For if blood or any other liquid is injected by means of a syringe into the pulmonary artery of a dead animal it will pass readily enough into the left ventricle of the heart. And indeed any one can feel for himself that although respiration be temporarily suspended, yet the pulse of the arteries in the wrist is strong enough. But this would not be the case if the blood were not passing at the moment through the lungs to the left ventricle of the heart.

And this will be made still clearer by what is to be said below. Still I will not deny that the movement of the lungs, and the compression of the blood-vessels occasioned by the fall of the thorax in expiration, contribute not a little to send the blood through the lungs; but it is by no means to be supposed that this is the only use of respiration.

Hence some think that respiration serves a further purpose, that of churning, forsooth, and dividing into the smallest particles the thicker venous blood. For otherwise (as they say) the blood would be separated into distinct parts, namely, serum and a purple sediment. But neither is this the chief use of respiration.

For any air, however impure, would suffice for such a movement of the lungs and for the churning of the blood; but air vitiated by contagion, or air which has often been sent out from the lungs, is by no means suitable for respiration and the support of life. With respect, then, to the use of respiration, it may be affirmed that an aërial something essential to life, whatever it may be, passes into the mass of the blood. And thus air driven out of the lungs, these vital particles having been drained from it, is no longer fit for breathing again. But this will be made clearer by the following experiment.

For if, by means of bellows attached to the trachea of an animal, a dog for example, the lungs are inflated, but in such a way that, through openings made here and there at their extremities, some of the air may pass out, the loss of which must be supplied by the bellows that the lungs may not collapse; in this case, I say, the animal will live. And yet that sort of agitation of the blood cannot take place in lungs which are kept inflated to the utmost. Moreover, though the movement of the lungs entirely ceases, yet the blood is transmitted through them to the left ventricle of the heart. But if, on the other hand, the mouth and nose be closed after breath is taken and drawn into the lungs, death will certainly follow, although the lungs remain inflated, because expiration is prevented. And yet the passage of the blood through the lungs is as ready in this case as in the other; for the comminution of the blood cannot be greater in the former case, since in both cases the lungs are equally distended—a clear proof that respiration is not necessary either for the passage of the blood through the lungs or for its agitation. But the reason that an animal lives in the one case and dies in the other is that in the former there is a continual access of fresh air, but none in the latter.

As to expiration, it should be noted that it serves the further use that, along with the air driven out from the lungs, the fumes which are raised by the fermentation of the blood are also blown out.

Let us now inquire what the aërial element is which is so necessary to life that we cannot live for even a moment without it. And indeed it is probable that certain particles of a nitro-saline nature, and these very subtle, agile, and in the highest degree fermentative, are separated from the air by the action of the lungs and conveyed into the mass of the blood. For this aërial salt is so necessary to every form of life that not even plants can grow in soil to which air has not access. But if such soil be exposed to the air and impregnated anew with this fertilising salt, it will again become suitable for the nourishment of plants. So that even plants themselves seem to have a kind of respiration and the necessity of absorbing air.

But it is not so easy to understand the function which this aërial salt exercises in animal life, yet it is probable that nitro-aërial spirit, mixed with the saline-sulphureous particles of the blood, excites in it the necessary fermentation. And yet it is not to be supposed that this effervescence of the blood takes place in the heart alone, but that it goes on first in the pulmonary vessels and afterwards in the arteries no less than in the heart. For I do not recognise that ferment, I know not what, in the left ventricle of the heart. For whence and by what vessels is there so great an influx of it as would suffice for heating so often every day the whole mass of the blood? In the fœtus the blood to a great extent passes directly from the right ventricle of the heart into the aorta, and yet this ought not to be done if so necessary a fermentation took place in the left ventricle. Much less probable is it that the beating of the heart is caused by the rarefaction of the blood in its ventricles as the famous Descartes supposed. For if the pulsation of the heart were caused by the fermentation of the blood in its cavities, then, when the heart beat, its ventricles would be greatly dilated by that blood, just as a bladder is blown into the form of greatest capacity. And indeed the blood would not rush forth so impetuously in the systole as in the diastole of the heart, and not from an impulse derived from the contraction of the heart, but on account of its own rarefaction. But we know in fact, from vivisections, that the ventricles of the heart are contracted when it beats and are not dilated by the rarefaction or explosion of the blood, and also that the blood rushes out when the heart contracts but not when it relaxes. Indeed, if a motion similar to that which takes place in systole is excited in the heart of a dead animal, filled with water or any liquid, the liquid contained in it will immediately rush forth, not indeed because of an explosion (for of course there is no such thing in this case) but because the ventricles actually contract. And further it is obvious that the movement of the heart is not caused by the rarefaction of the blood, because hearts are sometimes observed to beat after being cut out even if the blood has been pressed out of their ventricles. Indeed, if a solution of opium or cold water be injected through the jugular vein, the beating of the heart will immediately become more frequent, as I have often observed; but this cannot be caused by a more frequent heating, for heating is greatly hindered by substances of that kind.

So that obviously the heart seems to be nothing but a muscle, differing but little in its action from other muscles, and we must believe its function to consist in contraction alone and the expulsion of the blood.

But although nitro-aërial particles excite fermentation in the mass of the blood, I do not know whether it is owing to the want of them that the blood, immediately upon respiration being checked, becomes so thick that it is quite incapable of motion, and stagnates in the left ventricle of the heart. For the blood while not yet impregnated with air is sent readily enough from the right ventricle; and indeed the left ventricle differs from the right in no other respect than in the greater power it possesses of driving out the blood even if it be thicker. And yet it is not to be denied that nitro-aërial particles conduce not a little to the fermentation and, consequently, also to the fluidity of the blood, as was said elsewhere. But, you will ask, how is it then that death follows so soon upon breathing being arrested if the blood is not rendered incapable of motion?

There is certainly yet another use of respiration to be looked for, one that makes it so necessary. And what I have thought out on so obscure a matter I shall shortly state.

Life, if I am not mistaken, consists in the distribution of the animal spirits, and their supply is most of all required for the beating of the heart and the flow of blood to the brain. And it appears that respiration chiefly conduces to the motion of the heart in the manner to be stated elsewhere. For it is probable that this aërial salt is altogether necessary for every movement of the muscles; so that without it there could be no pulsation of the heart.

For if it be allowed that the sudden contraction of the muscles results from the intermixture of particles of different kinds, mutually moving each other, then it is scarcely to be supposed that the particles of both kinds, by the effervescence of which the contraction of the muscles is caused, proceed from the mass of the blood; for liquids derived from the same source reunite without any effervescence, so that it appears that something extraneous is required for the production of the motive fermentation.

We may then suppose that nitro-saline particles derived from the inspired air constitute the one kind of motive particles, and that these, when they meet the others, the saline-sulphureous particles supplied by the mass of the blood and residing in the motor parts, produce the effervescence from which muscular contraction results, as will be shown more fully in another place.

And in fact motion is produced in the heart in no different way than in the other muscles; but I do not think, for the reasons assigned above, that the motive effervescence takes place in its ventricles but in its muscular substance, not otherwise than in other muscles.

Wherefore on the suppression of respiration, as that aërial salt required for any motion fails, the beating of the heart and, consequently, the flow of blood to the brain will necessarily be interrupted and death will ensue. But one may live for a certain time without breathing, because the blood contained in the pulmonary vessels and sufficiently impregnated with air is capable of moving the heart at least for a moment.

And this use of respiration can be further confirmed. For in exercises and violent movements there is need of more intense and more frequent respiration, not so much that a greater flow of blood may pass freely through the lungs—for we have shown that this can take place when respiration stops—but because there is a great expenditure of nitro-aërial salt in consequence of the various effervescences made in the contraction of the muscles; so that the venous blood returns to the heart now much impoverished and thick (and we know that this also happens after convulsive movements in epilepsy). Wherefore, that the effete blood may repair the waste, there is of all things need of more intense respiration. Besides, an acceleration of the heart's beat is necessary in violent movements on account of the more copious flow of blood; but this can scarcely be effected without freer supply of nitro-aërial particles—especially since the blood is now effete. So that a main use of respiration clearly appears to be to set up the motion of the muscles and especially of the heart. Hence the frog which is accustomed to live under water for some time without respiration will go on living, even when its heart is cut out; but to animals which require a continuous supply of spirits and consequently an uninterrupted movement of the heart, continuous respiration is altogether necessary, since without it the motion of the heart ceases.

Furthermore, if after the motion of the heart has ceased from the stopping of respiration, air is blown in through a tube fitted to the *vena cava*, we shall see the heart's motion re-established. So that it appears that air is that without which the movements of the heart cannot go on at all. Nor does it matter much how the air is transmitted to the mass of the blood, whether by the lungs or by any other way.

To this I add, lastly, that it is proved by Boyle's experiments that flies, bees, and other insects which can move with half of their body after being cut through the middle, can yet neither move nor live in a place void of air. To these small animals that have neither blood, nor hearts, nor lungs, at all events not in their divided parts, air seems to be needed for no other purpose except motion alone. Finally, according to this hypothesis, it is easy to say whence the beasts of burden that exercise nearly all their muscles the whole day long, derive an adequate supply of explosive material for so great an amount of work; for what the entire mass of the blood is inadequate to supply, the air, that freer fountain, can provide in abundance.

THIRD TREATISE

ON THE RESPIRATION OF THE FŒTUS IN THE UTERUS AND IN THE EGG

SINCE the necessity of breathing is so essential to the sustaining of life that to be deprived of air is the same as to be deprived of the common light and the vital spirit, it will not be out of place to inquire here how it happens that the fœtus can live though imprisoned in the straits of the womb and completely deprived of the access of air. For it is not enough to say that the blood of the child is brought during the period of gestation through the *foramen ovale* and the *ductus arteriosus*, and circulates well enough without the movement of the lungs, while the mass of the blood takes its course, after birth, through the lungs, which it cannot traverse without the help of respiration. For respiration serves another purpose than transmitting the blood through the lungs; otherwise the lungs would be altogether superfluous, since the blood could have been carried round by another passage as is done in the uterus. Nay, the blood can pass through the lungs themselves without the aid of respiration, as has been pointed out elsewhere. Besides, if the fœtus which has breathed air for some minutes only, will die immediately after its breath is stopped, this is not because the movement of the blood is prevented; for the blood could be carried round by the *foramen ovale* and the *ductus arteriosus*, since these passages, to which it is accustomed, are not yet closed.

It is therefore to be absolutely concluded that the necessity for breathing arises from this, that certain nitro-aërial particles requisite for the support of life are transmitted by means of the lungs into the blood, as I consider to be established by what is stated elsewhere. And therefore inasmuch as the fœtus cannot breathe in the womb, as the air is excluded, it is necessary that the lack of respiration should be supplied from another source.

For we cannot agree with those who maintain that the fœtus breathes even in the womb—a view upheld on the strength of the *vagitus uterinus* and the *suctio infantuli*. But indeed it seems to me that air can just as well pass to the blood, without respiration, through the skin and veins, as penetrate the closed uterus and the many membranes which enfold the fœtus. Although I would not deny that vapours arising from the fermentation of juices are perhaps sometimes contained in the amnion; and these may produce the *vagitus uterinus* and the *suctio*, but they cannot serve the purpose of respiration, since they would need to be driven out oftener from the lungs of the embryo. Indeed, for the *suctio infantuli*, there is no need at all that air or vapours should be contained in the amnion, for the external air, by compressing and pushing not only the outer parts of the body, but, by their intervention, all the internal parts also, and, consequently, the liquids of the amnion, is able to cause suction in the uterus.

With regard, then, to the respiration of the fœtus in the womb, we may suppose that the seminal juice which exudes from the membranes of the uterus, or from its caruncles, not only supplies nutriment to the child but also makes up for the want of respiration. And indeed it is probable that the umbilical arteries are formed principally and perhaps exclusively for the sake of respiration. Indeed I know no other purpose for which they should be fashioned by Nature in every fœtus with such wonderful care and skill. I am aware of the diversity of opinion that exists among authors as to the functions of the umbilical arteries, but I do not know whether among the numerous offices hitherto assigned to them their true and peculiar office is to be found.

According to Adrian Spigelius, the umbilical arteries convey the blood from the fœtus to the exterior parts, that is, to the *secundinæ*, for their nourishment. But in fact it is evident if we look at a hatched egg that the membranes (which in the egg correspond to the *secundinæ*) are formed while the umbilical arteries are not yet indicated, from which we may infer that the said arteries are not formed for the sake of the membranes. Besides, the umbilical arteries are so notable in the egg at the very beginning of life, and their offshoots are connected in so wonderful a network, that there can be no doubt that they contribute, in no small degree, to the formation of the fœtus and to the starting of the dance of life. To this I add further that the *secundinæ* are sometimes in excellent condition although the fœtus is quite corrupt, which is indeed a very probable indication that the *secundinæ* draw nutriment rather from the mother than from the fœtus.

The celebrated Harvey has maintained that the use of the umbilical arteries is to supply arterial blood for the concoction and colliquation of the food of the fœtus, and to render it suitable for nutrition ; but it is scarcely probable that the umbilical arteries should be designed for this purpose exclusively, since nutritious juice could be well enough concocted and elaborated by the mother's heat and the warmth of the uterus. And it certainly seems foreign to the method of Nature that blood should be poured on food for its concoction, as if into a dish. For why should not nutritious juice be prepared within the body of the embryo as well in the uterus as after birth? Certainly there is no reason why that should be done through winding paths and the long circuit of the umbilical arteries, which could be managed by a shorter route and with less trouble. And it also makes for this that it is probable that the offshoots of the umbilical vessels are distributed into membranes, but not into juices to colliquate them, as seems to be confirmed by the very examination of a hatched egg. And certainly if the openings of the umbilical arteries terminated in the primogenial juices the said juices would soon be drenched with arterial blood ; but this is not the case. Further, we may believe that capillary vessels, of whatever kind, never end in juices but always in membranes, for otherwise they would be less firm and their openings would be closed by the pressure of the juices on all sides. Indeed, just as the lacteal vessels which originate in the intestinal membranes receive the nutritious juice, passed through these membranes as if through a filter, and convey it into the mass of the blood, so also in an egg and in other objects of conception we must suppose that nutritious juice, properly concocted, enters the openings of the umbilical vessels only by a sort of percolation through the membrane.

In the opinion of others the umbilical arteries are designed with a view to carry off the excess of food which is brought to the child through the umbilical vein. But surely there is no reason for accusing Nature of gluttony, as though it were not enough to cram after birth even to surfeit and vomiting without doing it also in the uterus by the arrangement of Nature. Further, whatever is carried away by the said arteries is brought back again by the umbilical vein, and so the child would be forced, as it were, to return to its vomit. Nor should it be said here that only the cruder parts of the blood are conveyed by the umbilical arteries to the placenta, that, after further decoction there, they may become fit for nutrition. For whence, I ask, that elective attraction in virtue of which it is the cruder parts of the blood rather than the purer which traverse the ducts of the umbilical arteries that stand so widely open? Further, it is scarcely to be believed that nourishment presented to the child is so raw that it has to be thrust out of doors to be further cooked. For how much wiser it would be to prepare it properly at first.

Nor ought we to agree with those who think that the umbilical arteries exist in order that the blood of the embryo may circulate by passing through the said arteries, and then returning by the umbilical vein. For the blood of the infant can be carried round easily enough through the aorta and the *vena cava* just as after birth. Nor is there any ground for saying that these vessels are not yet formed in the embryo; for it is certain that the great artery in which the umbilical arteries originate, is in existence from the first, and indeed it is not in the least to be doubted that the *vena cava* also exists from the very commencement of life. For why should not Nature be as ready to form the *vena cava* as that long circuit of umbilical vessels which are quite useless after birth and have to be destroyed?

Wherefore, since the functions hitherto assigned to the umbilical arteries do not appear to be suitable and real, we may hold with divine old Hippocrates that in the embryo the umbilicus supplies the place of respiration, which is also the opinion of the learned Everard.

But I cannot agree with Everard in the reason he assigns for setting up respiration in the uterus. For this learned man thinks that the blood of the infant is conveyed through the long circuit of the umbilical vessels in order that it may be cooled in its journey. But indeed it is by no means to be believed that such a cooling of the blood takes place in the very warm uterus. And even though there were such cooling, it would serve in no way the purpose of respiration, for, as we have shown elsewhere, this contributes rather to the heating than to the cooling of the blood.

But now that we may prepare the way for our opinion as to respiration in the uterus, we observe, in the first place, that it is probable that the albuminous juice exuding from the impregnated uterus is stored with no small abundance of aërial substance, as may be inferred from its white colour and frothy character. And in further indication of this, the primogenial juices of the egg, which have a great resemblance to the seminal juice of the uterus, appear to abound in air particles. For if the white or the yolk of an egg be put in a glass from which the air is exhausted by means of Boyle's pump, these liquids will immediately become very frothy and swell into an almost infinite number of little bubbles and into a much greater bulk than before, a sufficiently clear proof that certain aërial particles are most intimately mixed with these liquids. To which I add that the humours of an egg when thrown into the fire, give out a succession of explosive cracks, which seem to be caused by the air particles rarefied and violently bursting through the barriers which confined them. And hence it is that the fluids of an egg are possessed of so fermentative a nature. For it is indeed probable that the spermatic portions of the uterus and its carunculæ are naturally adapted for separating aërial particles from arterial blood.

These observations premised, we maintain that the blood of the embryo, conveyed by the umbilical arteries to the placenta or uterine carunculæ, brings not only nutritious juice, but along with this a portion of nitro-aërial particles to the fœtus for its support; so that it seems that the blood of the infant is impregnated with nitro-aërial particles by its circulation in the umbilical vessels, quite in the same way as in the pulmonary vessels. And therefore I think that the placenta should no longer be called a uterine liver but rather a uterine lung.

Should any one object here that such a mode of breathing in the uterus could be carried on without umbilical arteries, since it would suffice if the nutritious juice were to pass, charged with nitro-aërial particles, to the fœtus through the umbilical vein, I answer that to supply the part of respiration there is need of a continuous supply of air, but that the nutritious juice should not be in such abundance as to come to the child in a perpetual stream, and therefore it is necessary that the umbilical arteries should be so formed, that the arterial blood, continuously sent out to the placenta, may be there impregnated with a portion of nutritious juice charged with aërial matter; and that it should return thence, with never interrupted motion to the fœtus, for the purposes at once of nutriment and of respiration.

For indeed it is probable that if arterial blood, which is imbued with nitro-aërial spirit, came to the heart instead of venous, there would be no need at all for respiration. And this seems to be confirmed by the fact that when arterial blood, in what is now a well-known experiment, is transmitted from one dog to another, the dog to which the blood is transferred, although previously panting and breathing violently, yet, after receiving the arterial blood, seems scarcely to breathe at all.

OF THE RESPIRATION OF THE CHICK IN THE EGG

Thus far of respiration in the uterus; it remains for us to discuss briefly the respiration of the chick in the egg. For there can be no doubt that the want of respiration in the egg also is supplied from another source. It is in fact our opinion that the chick in the egg respires through the umbilicus very much in the same way as the child in the uterus. For when I contemplate the really marvellous and complicated network of the umbilical arteries in the incubated egg, and then consider that none of the things essential to animal life are wanting in the egg, except respiration only, assuredly I can arrive at no other conclusion than that the aforesaid vessels are formed to compensate for the lack of respiration.

Wherefore, let us now inquire how the umbilicus in the egg supplies the place of respiration. In regard to this it is probable that the primogenial liquids of the egg (which, as has been already shown, abound with aërial matter), continuously brought by the umbilical vessels to the chick, perform for it the part of not only of nutrition but also of respiration, just as is done in the uterus.

If any one shall here object that there is not so much air contained in the egg as is required to supply the want of respiration during the whole period of incubation, I answer that the air stored in the egg is not common air but is that aërial something which is separated from common air by the action of the lungs. Indeed, of the air which we inhale it is only a very small portion that is transmitted into the mass of the blood; what remains of the air is expelled in expiration as being useless. But what there is of air, pure and vital (such as we are to suppose contained in the egg), may be compared and held to be equal to a great quantity of common air.

Nor is it to be forgotten that the fœtus in the egg and in the uterus makes but the very smallest expenditure of nitro-aërial particles; for these are mostly required for muscular contraction and for carrying on the concoctions in the viscera, as we shall elsewhere show. Hence, according as any one exercises himself more or less, so he has need of a more intense, or of only a more moderate respiration. And undoubtedly in drowsy affections, in which the animal functions are almost suspended, respiration seems to be all but suppressed. Wherefore, as the fœtus in the uterus and in the egg keeps holiday from nearly every movement except that of the heart, a smaller ration of nitro-aërial particles from the arterial blood of the mother, or from the fluids of the egg, abundantly suffices for its requirements.

Let us also ponder briefly whether the gentle warmth produced in the egg by the heat of the incubating fowl does not contribute in some measure to compensate for the want of respiration. For it was elsewhere shown that nitro-aërial particles are detached from the aërial particles by the fermentation of the blood, and that these, in animals, serve the purpose of respiration. Further, it ought to be noted that heat of all kinds is produced by the motion of nitro-aërial particles.

But now as it is altogether needful for the generation of the chick that a gentle warmth be excited in the egg, by the heat of the incubating fowl or otherwise, why should we not suppose that nitro-aërial particles (from the communication of which to the egg its warmth arises) supply to some extent the place of respiration in the egg? Certainly the nature of the white of egg seems to be such as is suitable for the detention and entanglement of nitro-aërial particles, inasmuch as it consists of a viscid fluid, and that, too, impregnated with saline particles liberated from union with sulphureous particles. It corroborates this view that the white of an egg, if whipped rapidly with a rod or spoon, becomes frothy more than all other substances on account of the abundant intermixture with it of aërial matter. It is therefore probable that nitro-aërial particles, when conveyed to the egg by the warmth of the incubating fowl, are detained there by its albugineous humour; and that when at last collected by the almost innumerable ramifications of the umbilical vessels and then brought in great abundance to the fœtus, they compensate in some degree for the want of respiration in the egg. For the chief use of the respiration of animals is to introduce nitro-aërial particles into the mass of the blood, and it does not matter how this is done. And indeed the tepor produced in the egg by the warmth of the incubating fowl produces the same effect in its primogenial juices as the nitro-aërial particles do in the mass of the blood. For just as the nitro-aërial particles passing into the earth along with heat and moisture, effervesce with its saline-sulphureous particles, on which action the life and respiration of plants depend, as has been elsewhere shown, and as nitro-aërial particles densely mixed with the blood through the agency of the lungs excite the fermentation required for animal life, so also the same nitro-aërial particles entering the juices of the egg under the form of a genial heat appear to contribute to some extent to set up in them, vital fermentation and animal movement, and so in a measure to perform the part of respiration. And hence it is that the tepor, whether produced by incubation or in some other way, is so necessary for sustaining the life of the chick in the egg. For if an egg is opened after some days of incubation, in such a way that the salient point comes into view, you will find that according as the egg is exposed to heat or to cold, the little heart of the chick is beating in the one case, and in the other languishing and ceasing to move as if respiration were suppressed.

From what has been said it is not very difficult to understand how it is that the fœtus, if wrapped in its unbroken membranes, can live for several hours after birth without danger of suffocation ; while yet if it has once taken air into the lungs after being stripped of its membranes, it will not be able to survive for a single moment without air, but will immediately die, as has been recorded by the illustrious Harvey. It is not enough to say here that the blood of the infant, after respiration once begins, is taken round by an entirely new path through the lungs, and that it cannot be transmitted through them without their continuous movement. For I think it is clear from what has been said that this answer fails to remove the difficulty. It should rather be said, I think, that the albugineous juice contained in the placenta, or in the membranes in which the fœtus is enclosed, have a supply of nitro-aërial particles large enough to continue for a time the respiration and the life of the infant. Indeed, the fœtus when born and wrapped in its unruptured membranes, seems to be in nearly the same case and to breathe very much in the same way as the chick enclosed in the egg. If, however, the fœtus is stripped of its membranes, and contracts the muscles of the chest and the diaphragm that respiration may begin—certainly no small exertion—there is now a greater expenditure of nitro-aërial particles for muscular effort, and consequently the fœtus is under a greater necessity to breathe, since nothing is any longer received to supply the want of respiration.

It will not be irrelevant to inquire here whether the air which is contained in the cavity in the blunter end of every egg contributes to the respiration of the chick. This cavity lies between two membranes which are stretched over the whole interior of the egg. For of these membranes, the one which is next the shell is in all parts firmly attached to it, but the other, which is next the fluids of the egg, adheres almost everywhere to the first, except that by receding from it a little at the blunt end of the egg, it forms the aforesaid cavity there. Harvey and others have supposed that this cavity lies between the membrane which envelops the fluids and the shell, which is left bare at that place by the other membrane, but I have ascertained that the shell of the egg is everywhere lined by the first membrane and that the cavity lies between the two membranes.

With regard to the purpose served by the air contained in this cavity, I cannot agree with the learned Fabricius who maintains that air is stored in it for the respiration of the chick; for there is so little of it that it would barely suffice for once starting respiration. Besides, the air enclosed in the cavity is completely shut out from the fœtus by the intervening membrane, so that it cannot pass to the fœtus for the purpose of respiration, as will be evident from the following experiment. For let the sharper end of an egg be so broken that its fluids can be poured out in order that the said cavity may be seen, which in eggs that are not yet hatched will be very small. Then let the egg be put into a glass and the air pumped out by Boyle's pump.

When this is done the small portion of air contained in the cavity will at once expand when the pressure of the atmosphere is withdrawn, in virtue of its elastic force, and will push forward the membrane covering it a long way, so that the cavity will enlarge to half the size of the egg, more or less. Nay, the air sometimes by separating the said membrane from the other to which it was previously attached, will push it beyond the cavity of the egg. And from this we may infer that the air cannot pass through the lining membrane and be conveyed to the chick, for if it did, this membrane would not be pushed so far by the enclosed air nor become distended.

But since that air within the egg cannot reach the chick for the function of respiration, let us inquire what purpose it serves. For it is by no means to be believed that the air which Nature has so carefully placed in every egg is altogether useless and superfluous. But in order to understand the function of that air, it must first be noted that the seminal juices of the egg when colliquated by incubation are not rarefied or expanded, but are on the contrary condensed and forced into a narrower space than before. For we remark that the aforesaid cavity is greatly enlarged after a few days' incubation, as will be manifest if the blunt end of the egg is perforated. But this would not at all be the case unless the juices which filled nearly the whole of the egg before incubation were subsequently, by the incubation, condensed and made to occupy less space than before. In fact the humours of the egg are contracted after they have passed into the body of the chick, to about a half less than they were at first, since the cavity enlarges under incubation to about half the size of the whole egg.

But now let us consider briefly how it is that the juices of the egg are condensed to such a degree by incubation ; and it is to be noted that things may be condensed in various ways :

1. If vacant spaces numerously interspersed among the particles of the thing to be condensed are diminished or even removed by the particles approaching each other. But it is not probable that the juices of the egg are condensed in this way only. For it is by no means to be believed that as much empty space should be distributed among the juices of the egg as is required for contracting them to the extent of about one half. For if such were the case the juices of the egg would contract very much on account of the pressure of the atmosphere when the shell is perforated. Yet this does not at all take place.

2. A thing will undergo condensation if its particles which have expanded on being set in motion are afterwards, on the subsidence of the motion, reduced to narrower space. But neither in this way are the juices of the egg condensed, inasmuch as they are colliquated and fermented by incubation, so that the motion of their particles is necessarily greater than before.

3. A thing might be condensed if some more subtle matter interposed among its particles extricated itself, so that the parts of the thing might approach nearer to each other. And indeed it naturally occurs to one to say that the rarer part of the albumen exhales, and that the juice is reduced in consequence to less bulk than before. But, indeed, this cannot happen in the egg, because its very compact shell and also the membranes enveloping the juices of the egg, prevent any part of these juices escaping out of the egg, especially since in the incubated egg a greater space than before is provided for the reception of these juices.

4. Condensation of a thing may take place because some elastic matter distributed among its particles becomes afterwards less elastic; and it is especially in this way that the juices of the egg seem to be condensed. For it is probable that the air distributed among the juices of the egg loses its elastic force on account of the fermentation produced among these juices by incubation, just as takes place in the mass of the blood, as has been shown above.

Since the seminal juices of the egg become more contracted in this way by incubation and are reduced to smaller bulk than before, there would be a vacuum in the incubated egg if prudent nature had not, to avoid this, stored in the egg a small quantity of air which by its elastic force might extend itself into the space left vacant by the condensation of the juices.

And hence it is that the said cavity is so much enlarged by incubation. For we must not suppose that this enlargement of the cavity is produced by the access of new air, but by the pressure of the air inside. For although when the blunter end of the egg is perforated, the said cavity is found to be much larger than in new-laid eggs, yet if the sharper end is broken and the juices of the egg poured out, the membrane spread over the cavity (which, by the contraction of the juices of the egg and by the elastic force of the air, was thrust far into the region of the egg) will immediately fall back, on account of the pressure of the external air introduced into the perforated egg, and be applied anew to the shell; so that the cavity will not now appear larger than in unhatched eggs, unless perhaps the air enclosed in it be still warm and rarefied, in consequence of the warmth of the incubating fowl—a clear enough proof that the enlargement of the cavity is due to the elasticity of the air inside, in virtue of which it expands into and occupies the space left by the contraction of the juices.

Nor yet is it to be supposed that precaution is taken against a vacuum in the egg, as though nature, according to the common belief, abhorred a vacuum, but rather because a vacuum would not be so suitable for the generation of the chick. And indeed it is reasonable to think that the air stored in the egg, in consequence of its elastic force (increased not a little by the fostering warmth of the incubating fowl) gently compresses the colliquated juices of the egg and drives them into the umbilical vessels, and hence contributes not a little to the commencement of animal motion.

It is, moreover, likely that the air inside the egg contributes still more to the growth of the chick and to the building up of its structure. For it should be noted that the seminal juices of the egg, colliquated from the beginning of the incubation, formed a fluid body, and that, therefore, among their particles flowing hither and thither, there must have been interspersed a great number of little spaces. But when the primordial particles go combined together into various parts, they compose no longer a fluid but a solid body, such as is that of the embryo; and, what is to be chiefly noted, the particles, becoming much more compact, are brought into a smaller space, as was remarked above.

But since the air enclosed in the egg is always compressing the primogenial juices by its elasticity, that tends to bring it about that the seminal particles, united most closely together, are reduced to the smallest possible space. And this result is at last attained when the particles adapted for forming this and the other parts mutually embrace each other and pass into the body of the embryo, since the primogenial juices, when converted into the body of the chick, are reduced in bulk by about one half. So that, clearly, that internal air, by compressing and pushing the primordial juices of the egg, appears to perform the same work as the steel plate bent round into numerous coils by which automata are set in motion.

And, lastly, it is also to be noted, that when the juices of the egg are forced into smaller bulk by incubation in the manner aforesaid, the shell of the egg would scarcely be strong enough to resist the pressure of the external air, unless that internal air, its elasticity in no small degree increased by the heat of the incubating fowl, supported it.

FOURTH TREATISE

ON MUSCULAR MOTION AND ANIMAL SPIRITS. INCIDENTALLY, ON THE MOTION OF THE BRAIN, AND ALSO ON THE USE OF THE SPLEEN AND OF THE PANCREAS

CHAPTER I

EXAMINATION OF THE VARIOUS OPINIONS OF AUTHORS AS TO THE WAY IN WHICH MUSCLES CONTRACT

That Nitro-aërial Spirit is, by means of respiration, transmitted into the mass of the blood, and that the fermentation and heating of the blood are produced by it, has been elsewhere shown by us. But I shall now further add concerning the use of that inspired spirit, that it takes the chief part in the origination of animal motions, an opinion which I published now a good while ago, and still firmly hold ; not that I have set myself to stick to it, as fixed to a preconceived hypothesis, but because I consider it most agreeable to reason.

The cause of the production of any kind of motion is so obscure, that the consideration of it may exercise the minds of the anatomists nowadays, no less than of the philosophers long ago. As if, indeed, human ignorance should be nature's laughingstock, for those things that are seen every day in our hand and before our eyes recede furthest away from the grasp and perception of our minds, like the unhappy case of Tantalus. And among these Motion deserves to be reckoned, for we know so little how it takes place that, notwithstanding the evidence of our eyes, its very existence has been sometimes considered doubtful, and one of the famous questions discussed in the Schools was—Is Motion to be taken for granted? And the Sophist so firmly denied this that it would have been all up with its existence, had not Motion itself, stirred up in its own defence, made answer, and set the Peripatetic against the Philosopher. But now, if there are such various difficulties as to motion in general, how much more obscure is that animal motion, in which we see to our astonishment enormous bodies execute quite stupendous movements of their own accord.

No one doubts that the movements of animals are produced by the contraction of the muscles, but how that contraction is brought about is the subject of varied controversy among authors. Still, the most generally received opinion is that the fibres of the muscles are inflated with some elastic matter, so that while they swell as to breadth they contract as to length.

And this inflation of the fibres is thus described by that very distinguished man, Dr Willis, in his Discussion on Muscular Motion. This learned man thinks, namely,—"That the Animal Spirits carried from the brain by the channel of the nerves are stored up in the tendinous fibres, as in suitable repositaries; but that these spirits, on the incitement to motion being given, spring forth from the tendinous into the fleshy fibres, and there, meeting active particles of another sort, supplied by the blood, immediately effervesce with them, so that from the struggle and agitation of them both, the fleshy fibres, being lax and porous, are stuffed out and corrugated, and that the contraction of the muscle is produced by the corrugation at the same time at both ends of all these fibres. But when the contraction is over, the unused spirits that are left again in great part retire into the tendinous fibres, leaving the other particles within the fleshy fibres, and then the blood, as also their nerves repair the waste of these fibres. But as to how the spirits stored in the tendinous fibres are brought thence into the fleshy fibres for the production of motion," our learned author supposes "that an impulse transmitted by the nerves, as it were a token, is required, and that this is done by other spirits sent from the brain, while, namely, these inflowing spirits, by their varying approach to the muscles, regulate the innate spirits in their various movements, whether of expansions or of retreats."

This theory of the learned author is certainly very ingenious, but I am not sure that it is in the same degree in accordance with truth.

For, in the first place, the inflation of the fibres in the way described is beset with various difficulties, and these of no small weight. For if a muscle is contracted by the inflation of its fibres, it would necessarily follow that, being distended towards the outside in its contraction, it would swell into a much greater size; but it has been found by observation that a contracted muscle is drawn more closely together and becomes hard, and that, if it does not become smaller, yet certainly it does not swell up to such an extent as would be required for its contraction, if that were brought about by the inflation of its fibres, as has already been noted some time ago by Lower. But that in some contracted muscles we seem to feel a tumour, does not, I think, come so much from their swelling as from the movement of the belly of the muscle, in its contraction, towards the fixed tendon, so that the ascent of the belly of the muscle caused in this way, raises a hand placed on the muscle and simulates a tumour.

Further, if some elastic matter contained within the passages of the fibres inflated them in the way described, how could it be that the fibres should in a moment subside again, as happens in the glance of the eye and in other instantaneous contractions of the fibres? For neither can I comprehend how that elastic matter should in an instant inflate the fibres and again extricate itself from their passages, for if an easy way out of the fibres lay open to that rarefied matter, it would pass quickly through the fibres and not properly inflate them.

Moreover, it is hardly likely that the animal spirits, in the relaxation of the muscle, return from the fleshy into the tendinous fibres, for it is not easy to conceive what should regulate these spirits in their movement of retreat. Besides as, according to the opinion of the learned author, the animal spirits springing forth into the fibres meet there particles of another kind collected in sufficient abundance, and at once, as a whole, mutually effervesce with them, it would seem that these spirits would either be wholly dissipated, or be changed into something else quite different from what they were before; so that they would become altogether unfit for again exciting effervescence.

Finally, as to the part of the muscle which primarily undergoes contraction, it is probable that not so much the fibres, as the fibrils inserted transversely into them, chiefly undergo contraction, as will be shown afterwards.

As to the swelling of the fleshy fibres observed close to a ligature tied upon them, that seems to be produced by the blood passing through on the one side and on the other, but not from the interrupted motion of the animal spirits. For if such a swelled fibre be wounded, blood immediately escapes in abundance. Besides, that tumour remains constant at the ligature even when the muscle is relaxed and is no longer contracted, and then the animal spirits are not supposed to advance out of the tendinous fibres, but, on the contrary, to retire into them.

But so far I think we may agree with the learned author, for I believe that the contraction of the muscles is produced by particles of different kinds mixed with one another in the structure of the muscle, and mutually effervescing, as will be shown below.

I am quite aware that the learned Dr Steno, in his *Myologiæ Specimen*, published not very long ago, thinks that there is no need that any elastic matter should be added in order to start the contraction of the muscles ; which, in this learned author's opinion, can be effected by a mere change of their form. Thus, "If a muscle should change from an oblique-angled parallelogram into a parallelogram the angles of which are less acute, as is supposed to happen in the contraction of the muscle, then it will be contracted in length, and will also swell up, without the addition of any new matter" ; as is shown in Plate III., Fig. 1, in which, let *a*, *b*, *c*, *d*, be the muscle, *c*, *d*, *a*, *f*, the same contracted, and although it be of the same magnitude as before, and has had no new matter added to it, has yet undergone contraction as to length, and besides, rises at *f* into a tumour. But, indeed, it is hardly to be believed that muscular fibres should be ready to start this sort of motion unless some new matter were added for that end ; for, as the structure of an uncontracted muscle is lax, it would seem that the fibre *b*, *d*, in its contraction should not be carried outwards towards *f*, but rather, on the contrary, should go inwards. Again, if the contracted muscle is of the same size as before, and if no new matter has come to it, how is it that in its contraction it becomes so hard and tense, as any one can find out in himself by placing his hand on a contracting muscle? And finally, what indeed could contract the fibres and cause a change of this sort in the muscle if nothing flowed into it? Nay, it is quite evident that some new matter brought by the channel of the nerves is required for starting the contraction of the muscle, inasmuch as, if the nerve distributed to a muscle be cut, the contraction of that muscle becomes impossible.

I confess, for my part, that if we concede the arrival of new matter for accomplishing the contraction of the muscle, its contraction can be produced by a mere change of its shape ; as will be seen in the figure referred to, in which, when the muscle *a*, *b*, *c*, *d*, is inflated by the motive influx, it necessarily follows that the fibres *a*, *c*, and *b*, *d*, are brought towards a position at right angles to the tendon *c*, *d*, which we assume to be fixed, and that the other, the more mobile tendon, is drawn outwards so that the inflated muscle will be *c*, *d*, *e*, *i*. For that muscle could, by no other change produced in it, be enlarged for the reception of new matter and be thus inflated. But while the muscle is thus changed as to shape, it swells as to breadth, but becomes less as to length; and in this way a muscle can be shortened, although its fibres suffer no contraction.

But whether a change of this kind takes place in a muscle, and its contraction depends on this alone, I shall not say for certain. Still, it does appear to me that a contracted muscle does not swell up so much as would be required if its contraction were caused in this way. Besides, I do not see what part of the muscle should sustain the attack of the motive matter in such contraction, for some kind of membranous vesicles, rather than muscular cords, would be suitable for bearing the force of contraction, and yet the strength of a muscle seems to proceed from its fibrils and cords rather than from any kind of vesicles or membranes. But these things will be discussed more fully below.

CHAPTER II

A SHORT DESCRIPTION OF MUSCLES. ALSO WHAT PART OF A MUSCLE PRIMARILY CONTRACTS

In the anatomical dissection of muscles the first thing that presents itself is a membranous integument spread in all directions over each muscle; under which come into view series of fleshy fibres; these, parallel among themselves, are inserted obliquely into the opposite tendons, which are parallel one to the other, as the eminent Steno first observed.

Next, there come to view the wonderful series of almost infinite membranous fibrils, which, parallel among themselves, cut the fleshy fibres obliquely, for indeed, just as the fleshy fibres are inserted into the tendons, so are the fibrils into the fleshy fibres, but arranged the contrary way; and as the fibres closely joined together seem to form the tendons, so the collection of fibrils seems, in part at all events, to form the fibres themselves; as is seen in Plate III., Fig. 2, which shows the series of fibres and of fibrils, as they are seen in muscles that have been boiled for a sufficiently long time.

Hitherto it has been held by the authors best acquainted with anatomy that the fleshy fibres of the muscle chiefly and primarily undergo contraction; but in our opinion (which I should wish to express with all respect) not the fibres but the fibrils inserted transversely into them, take the chief part in muscular contraction, and this we gather from indications which are at all events probable. For if the contraction took place in the fleshy fibres, then, for a due contraction of the muscle, it would be necessary that the fibres should be much more shortened than the muscle itself; for as the fibres are not arranged according to the length of the muscle, but are inserted obliquely into the tendons, as may be seen in the figure referred to, it follows that the contraction of the muscle is much less than the contraction of the fibres; and that for a proper contraction of the muscle it would be necessary that the fibres should be contracted much more than the muscle itself: but I do not think such a great contraction of the fibres really occurs in motion: for, besides that we cannot in vivisections see this sort of contraction of the fibres, if the fleshy fibres contracted so much, the muscle should swell enormously, but it does not.

Besides, in order that the contraction of the muscle should be effected by the fibrils, there is no need that they should contract and swell up so much, inasmuch as their series are, as is shown in the figure referred to, arranged according to the length of the muscle, so that the contraction of the muscle will be equal to that of the fibrils. But now, as nature is in the habit of taking the shortest road, it is probable that the contraction of the muscle is produced by the contraction of the fibrils rather than by that of the fibres. And it also tells in favour of this, that, as the fibrils are very small and very short, their contraction even to a half would scarcely be anything remarkable; for as the fibrils suffer contraction as a whole, the case is just as if fibres stretched according to the length of the muscle were forced into manifold corrugations, and their contraction, taking place thus, although it be pretty great, can yet occur without notable swelling of the muscle.

To these I further add that the shortened fibrils draw the fleshy fibres together and constrict them: so that it is probable that the contraction of the muscles is accomplished by the fibrils; inasmuch as a muscle when contracted is conspicuously constricted and becomes hard; and it does not seem that this could take place in any other way than by the contraction of the fibrils. But there will be a fuller discussion of the constriction of the contracted muscle later.

Moreover, it is nature's custom very often to carry on her operations by means of very small things, so that the fibres seem too thick and coarse for muscular contraction to take place primarily in them; and it is probable that they serve rather for transmitting blood than for carrying on animal motion, as will be shown below.

Lastly, it tells in favour of this, that the shortness of the fibrils and their almost infinite number contribute to the strength of the muscles and to the more effective performance of their pull. Certainly the fibrils, whether we consider their number, their size, or their position, would seem much more fit for bringing about muscular contraction than the fleshy fibres. And this is further much confirmed by actual inspection; for, so far as I have ever been able to attain in examination by vivisections, the fleshy fibres, in the contraction of a muscle, as if attracted by the transverse fibrils, seemed to come nearer one another, and not to be themselves shortened, but to follow the contraction of the fibrils.

As to the fact that, in consequence of a ligature being tied round each end of the fleshy fibres, muscular contraction ceases and the fibre itself does not, as it otherwise would, swell up, as has been noted by the eminent Dr Willis, I think this due to the interruption, by means of the ligatures, of the motion of the blood and of the animal spirits, an inflow of which is necessary for the contraction of the fibrils.

CHAPTER III

OF THE PARTICLES BY MEANS OF WHICH MUSCULAR CONTRACTION IS EFFECTED; AND IN THE FIRST PLACE OF THE MOTIVE PARTICLES BROUGHT BY THE BLOOD; INCIDENTALLY, OF THE STRUCTURE AND USE OF MUSCULAR FLESH

In the previous chapter I have endeavoured to show that muscular contraction is chiefly caused by the fibrils. Let us now, in the next place, see by what cause the contraction of the fibrils is effected. There can be no doubt that the influx of animal spirits is necessary for the performance of the motive function, inasmuch as if a nerve is cut or obstructed, the muscle to which it is distributed refuses to contract. But it is not to be supposed that the contraction of the muscles depends on the animal spirits alone, since for carrying it out there is absolute need of other particles besides, brought from the mass of the blood. For, seeing that the arterial blood is supplied to the muscles in a continuous flow, and, especially in more violent movements, in a fuller flow than to other parts or than is required for their nutrition, it may be concluded that the arterial blood in its transit deposits something necessary for the contraction of the muscles. For, indeed, such is the structure of muscles that in their contraction they draw themselves together, and thus greatly promote the motion of the blood; and that is why the motion of the blood is so much accelerated in rapid running. And we can easily put this to the test, if, when the median vein has been opened, the muscles attached to the forearm are contracted (and this can be quite well accomplished if the fingers are pressed together in flexion), for when this is done, the blood will be seen to rush out forcibly, being pressed out of the said muscles by their contraction. But this so much accelerated motion of the blood in contracted muscles does not seem to be merely incidental, but to be arranged by the highest wisdom of nature, by which, namely, the motive particles of the blood may be brought in passing to the motor parts; and when they have been deposited from the blood, what remains of it is expelled with a certain push by the constriction of the shortened muscle, so that when the loss is repaired, it may again return with a new store of motive particles. What has been said is besides much confirmed by an experiment made by the most ingenious Steno; for it is established by his observation that a muscle can by no means undergo contraction if the artery distributed to it be tied with a ligature so that the afflux of blood to the muscle is prevented.

Indeed, I think that the chief use of muscular flesh is that it may separate from the mass of the blood certain particles necessary for the contraction of the muscles. Indeed, we may point out that it is the function of all kinds of parenchyma to separate by way of filtration some particles of a definite kind from the mass of the blood, as is manifest in the parenchymata of the liver, the kidneys, and others of the kind. It is therefore probable that the parenchyma of flesh which is associated with every muscle, has been constructed in order that by its means particles of a definite kind, necessary for setting up the contraction of muscles, should be filtered out of the mass of the blood.

But that the structure and use of muscular flesh may be better understood, let us shortly inquire in what manner the blood makes its way through the flesh. For I do not think we should agree with those who assume an extravasation of the blood. The special ground for this opinion is that no nutrition of the parts, so they say, could take place if the blood were always retained in its vessels, just as a river will not fertilise the neighbouring meadows unless its waters flow over its banks to irrigate them. But it seems scarcely admissible that so confused a thing as the extravasation of the blood should occur in the animal economy, where everything is arranged with such admirable art and order. Besides, I really cannot imagine how the extravasated blood could enter the very minute mouths of the veins ; for if the blood were diffused through the mass of the muscle it would seem that the ultimate ramifications of the arteries and veins would be compressed by the blood surrounding them, so that the blood would not be able to enter the mouths of the veins, as they would be closed by that compression. Further, it is plain that the blood is not extravasated in the muscles, because the blood coming to a muscle does not all rush out if the muscle is wounded, but this would happen if the blood were extravasated and diffused through the mass of the muscle.

So that as to the transit of the blood through the muscles it is right that we should hold that the capillary veins and arteries are united by vessels of some different kind, so that there is a sort of continuous passage between them. For I think that the extremities of the arteries terminate in peculiar vessels, which, soon after their origin, divide into an almost infinite number of canals, or rather membranous vesicles, joined here and there by various anastomoses ; but that the various offshoots of these vesicles, at last uniting into one canal, terminate in the gaping mouths of the veins. So that, indeed, while the mass of the blood wanders hither and thither through these tortuous labyrinths, it simulates extravasation. Further, it is probable that those passages, or the collection of the said vesicles, exist separately in each fibre, for in vivisections a cut can be made in the interstices of the fibres without any flow of blood, while blood at once flows out when a fleshy fibre is even slightly wounded.

We may, then, conclude that that collection of sanguiferous vesicles forms the chief part of muscular flesh ; for as to the ruddy solid part of the flesh, that seems to be nothing else than an affusion of blood, which, when coagulated, adheres to these vesicles, for, while the mass of the blood wanders like the Mæander among these channels and glides past them in its placid stream, the thicker particles are deposited on account of the slackness of the motion, and adhere to the sides of the vesicles, and yet, if the blood circulate more quickly, they are carried away with its impetuous rush, and hence it is that in the heat of fever and in the more violent exercises, the muscular parts are despoiled and become lean.

As to the use of the fleshy parenchyma, it is probable that the aforesaid vesicles, along with the sanguineous sediment adjoined to them, act as a filter, by which the motive particles are separated from the mass of the blood, as we have indicated above. And it tells in favour of this, that the parenchyma of the flesh turgid with blood is compressed by the constriction of the contracted muscle and by the natural subsidence of the parts; whence it comes about that the motive particles are driven, as it were, by a powerful squeeze into the motor parts to carry on the function of motion.

As to the nature of the motive particles separated from the mass of the blood, it is our opinion that they are of a saline-sulphureous quality. I think, namely, that sulphureous and saline particles brought to the highest volatility in the mass of the blood by its continuous fermentation in the manner elsewhere described, and most intimately joined together, are separated from the blood by the action of the muscular parenchyma and stored up in the motor parts for setting up their contraction. For we may note that no small loss of fat takes place in the more violent exercises, and that it almost wholly disappears in long-continued hard work; while yet, on the other hand, animals indulging in ease and free from hard work become very obese, and fat is deposited on their muscles in quite sufficient abundance. Whence we may gather that the sulphureous particles of the blood, of which the fat is formed, have some share in the production of muscular contraction. Indeed, animals seem to be emaciated by hard work just because the sulphureous and fat particles of the blood are used up and consumed in muscular contraction, while yet if the motor parts have long ceased from contraction the sulphureous particles are not now spent in producing the contraction of the fibrils, but are carried away through special vessels into suitable receptacles and, brought there in sufficient abundance, constitute fat. But that the fatty particles are carried by special vessels, is proved by the fact that the blood-vessels disseminated through the mass of the muscle do not extend to the fat connected with almost all muscles, so that the sulphureous particles composing the fat cannot come immediately from the blood, but they must be brought by special vessels from the inner part of the muscle; and certainly it has been established by anatomical observation that certain membranous vessels dispersed here and there through the mass of the fat are continued into the inner parts of the muscles. It does not seem likely that the fat thus stored in appropriate receptacles returns to the motor parts, still it is not to be by any means regarded as a useless excrement, but as serving various ends, such as the support of delicate parts and the lubrication of others. But these things will be discussed more at length later. In the meantime let us inquire as to the nature of the animal spirits, the influx of which into the muscles is also necessary for bringing about their contraction.

CHAPTER IV

THE ANIMAL SPIRITS BY WHICH MUSCULAR CONTRACTION IS PRODUCED CONSIST OF NITRO-AËRIAL PARTICLES. INCIDENTALLY, AS TO THE MOTION OF THE BRAIN

As to the nature of the Animal Spirits, we may conclude that they, so far at least as they contribute to animal motion, consist of Nitro-aërial Spirit. But that this may be more clearly understood, let us first suppose that nitro-aërial particles, which we have elsewhere shown to be passed into the blood by respiration, have also some share in setting up the performance of motion, as has long been our opinion. For, indeed, we have remarked a good while ago, in our Treatise on Respiration, that animals in the more violent exercises, as when much urged in running, find of all things a necessity of very much increased respiration; and the cause of this seems to be, that in violent movements the nitro-aërial particles are separated from the mass of the blood to set up the contraction of the muscles and are used up, so that there is need of more frequent respiration by which the blood may repair its losses, and have restored to it anew nitro-aërial particles for carrying on the function of motion. For the purpose of the increased respiration in violent exercises is not that the blood coming in greater abundance to the central organs and pushed on by the more frequent collapse of the lungs, should pass through them more easily; for in febrile heat, when, namely, the blood circulates in a most furious whirl, the respiration is nevertheless no more intense than usual: indeed, it has been elsewhere shown that the blood can pass through the lungs although they do not move. Nor is it, as some one might say, that respiration is increased in violent exercise to cool the heated blood, for it would follow from that that in febrile heat, when the blood is as it were on fire, there would also be need of more intense respiration. And, moreover, we have shown above that inspired air tends rather to the heating than to the cooling of the blood, so that it should rather be said that by the contraction of the muscles, often repeated in violent movements, the blood is deprived of fermentative particles and in some measure coagulated; and that there is need of more intense respiration for this reason, namely, that the loss of the fermentative particles being again repaired, suitable fermentation may be excited in the blood. Hence, indeed, it follows that the nitro-aërial particles, upon which the fermentation of the blood depends, are separated from the mass of the blood in violent movements, are spent in the contraction of the muscles, and are lost.

From what has been said, we conclude that nitro-aërial particles are necessary for the performance of muscular contraction, and this will be still more manifest from what follows.

I think it must be conceded that the motion of the muscles is brought about by particles of different kinds mixed with one another, for indeed I cannot imagine how else animal motion could be produced. For as to elasticity and weight, by which automata are set in motion, neither of them can have a place in the animal structure, inasmuch as motion produced by them would soon come to an end. Therefore it seems that we must conclude that the motive function is effected by particles of different kinds, mixed together, on the determination of the mind, and mutually agitated in most rapid motion. And this is further confirmed by this, that for the contraction of the muscles there is, as we have indicated above, absolute need, not only of animal spirits, brought by the nerves from the brain, but in addition, of other particles supplied by the blood.

Let us now consider, then, what is the nature of those particles by means of which muscular motion is effected. As to this, the most commonly accepted opinion is that muscular motion depends upon salts of different kinds, mixed together and mutually effervescing in the motor parts. For indeed it is necessary for setting up effervescence of this kind, that one of these salts should be of an acid character, but the other purely saline, fixed or volatile; but it is indeed probable that an acid salt never exists in the mass of the blood except in case of disease, inasmuch as, when in a state of health, it is impregnated only with volatile, or it may be acido-saline salt, such as the ammoniacal salt. Besides, it is scarcely to be supposed that an acid salt has a place in the motor parts, because all acids are directly hostile to the tender and delicate structure of the fibrils, and they would be much injured by an acid liquid poured on them. Furthermore, when these opposed salts, of whatever sort they be, come from the mass of the blood, what would prevent them, mixed in the blood, from acting on each other, and, as contrary salts do, destroying each other? And to these considerations we may add that the effervescence of contrary salts would not by any means be suitable for the motor parts, inasmuch as contrary salts mixed together do not combine without coagulation, but yet coagulation can by no means be admitted in the very minute structure of the fibrils. Nor should we omit to notice this, that the effervescence arising from the meeting of contrary salts takes place only slowly, and usually lasts for some time, and that this does not agree with the instantaneous contraction of the muscles, as we have indicated above.

So that for effecting the contraction of the muscles there is required an excitement of the elastic particles, of a kind that can be accomplished instantaneously and without any sort of coagulation. And indeed I do not know if there be in the nature of things any other such fermentation but the singular case of the effervescence of nitro-aërial and saline-sulphureous particles, which mutually, as their nature is, excite themselves to a most rapid motion. We must therefore conclude that it is from that that muscular contraction proceeds. And if the one set of motive particles, as I have tried to show above, are of a saline-sulphureous nature, it is most necessary that the other set of motive particles should be of the nitro-aërial kind, inasmuch as these alone are by their nature fitted for exciting the saline-sulphureous particles.

I think it has been established from what has been elsewhere said, that nitro-aërial and sulphureous particles effervesce when mixed with one another, but to these evidences the following experiment may be added. If, namely, the most highly rectified spirit of wine be mixed with spirit of nitre deprived of its moisture, a conspicuous heat will presently be produced, at all events if the mixture be slightly warmed; and the explanation of this seems to be that the nitro-aërial particles (which we have elsewhere shown to abound in the spirit of nitre) and the very volatile saline-sulphureous particles of which the spirit of wine consists excite one another to motion, as is their nature, for it must not be supposed that the spirit of nitre, as far as its saline and acid part is concerned, is in any way opposed to the sulphureous particles of the wine.

For, no doubt, just as the nitro-aërial spirit meeting with terrestrial sulphur excites that fermentation in which the motion and the life of vegetables consist, so also according as the same spirit, brought by means of respiration into the mass of the blood and there effervescing with the saline-sulphureous particles of the blood, produces the vital heat and motion, as I have elsewhere striven to show, it is probable that animal life and the motive function are brought about by the same particles more exalted and put in a condition of the highest vigour. For indeed I think that the nitro-aërial particles springing forth from the brain into the motor parts effervesce there with the saline-sulphureous particles meeting them, and muscular contraction is caused by their mutual agitation in the way to be stated below. And hence it is that for keeping up animal motion it is essentially necessary that there should never be a deficiency in the mass of the blood of saline-sulphureous pabulum or of nitro-aërial particles; and by how much more intensely the muscular contraction takes place, as in the harder kinds of work, so much greater is the outlay of nitro-aërial and of sulphureous particles, for the repair of which not only is the respiration increased, but besides there must be taken in greater quantity food filled with saline-sulphureous particles. Hence those articles of food which contain abundance of volatile salt and sulphur are specially fitted for restoring the powers worn by long-continued labour.

From what has been said, we may seek the reason why so intense a heat is excited in the motor parts by violent exercise. That heating is commonly attributed to the motion of the body itself, but indeed in animal motion there is no such friction of the parts (from which alone heat arises) as could account for so intense a fervour. We must, therefore, believe that the heat of strongly contracting muscles comes from nitro-aërial particles, at that time much agitated in the muscles; as I have endeavoured elsewhere to show that every kind of heat arises from their motion.

I may here note by the way that the ancients, not altogether without reason, supposed that the source of vital heat was in the heart; not that a sort of *Biolychnium*, whatever that may be, constantly flamed in its sacred cloisters, but that, inasmuch as the heart is unweariedly at work in continuous labour for keeping up the motion of the blood, the nitro-aërial and the sulphureous particles effervesce continuously in its muscular part, and that by their motion a notable heat must be produced.

Further, from the foresaid hypothesis a reason can be deduced why the sweat given out in violent movements is of a saline character, and very penetrating. For the extremely subtle nitro-aërial, as also the saline-sulphureous particles, by which when mixed together the contraction of the muscles is produced, when forced out along with the serous liquid, render it acrid and very penetrating. The reason why the sweat is acido-saline seems to be that the volatile salts of the blood, intimately combined with sulphureous particles, are partly brought to a liquid state by the effervescence which takes place in the muscular contraction, in the way elsewhere described.

From what has hitherto been said, it is to some extent proved that muscular motion depends on nitro-aërial and saline-sulphureous particles mutually moving themselves in the motor parts. But now of these particles by which muscular contraction is produced, some, viz. the animal spirits, are brought into the motor parts from the brain by means of the nerves; while others are supplied from the mass of the blood; as we have shown above. Let us now see whether the nitro-aërial particles are those motive particles supplied from the blood, or, on the other hand, are those coming from the brain—that is, the animal spirits. On this point I was for some time in doubt whether the nitro-aërial particles go immediately from the blood into the motor parts; but on serious consideration of the matter it seems more probable that the motive particles supplied from the blood are of a saline-sulphureous kind, as to some extent appears from what has been said above: whence it follows that the nitro-aërial particles come from the brain, and consequently are themselves the animal spirits. Indeed, it is much more probable that the nitro-aërial spirit should come from the brain than that saline-sulphureous particles should do so. For sulphureous particles do not occur at all in the brain, whereas they are disseminated everywhere throughout the mass of the muscles. Indeed, sulphureous matters seem to be hostile to the animal spirits; for, liquors full of volatile sulphur, such as spirit of wine and the chemical oils of vegetables, when taken too quickly disturb the brain and the animal spirits, and produce not only drunkenness, but not rarely madness and fatal convulsions. While on the contrary the structure of the brain seems to be such as to render it specially fit for separating the nitro-aërial spirit from the blood and for preserving it, as will be more fully stated below.

As to the nature of the animal spirits, the authority of the learned Dr Willis had no small weight with me; he supposes that the animal spirits are of the nature of volatile salt. For this learned man in his treatise on *Diseases of the Brain*, chapter xi., speaks as follows: *And indeed the Animal Spirits when in a healthy and regular condition seem to behave to some extent as a spirituous liquor full of volatile salt which distils from the blood.* But, indeed, with all respect to so eminent a man, since muscular contraction is produced by particles of diverse kinds mixed together and mutually moving themselves (as is the view of the learned author, and as also seems most consonant with reason), if the animal spirits consist of volatile salt, then the other motive particles supplied by the blood must be acid salt, for otherwise the animal spirits meeting them would not effervesce: but it is scarcely to be supposed that acid salt can have a place in a healthy body, much less in the motor parts, as has been shown above. Should we admit that an acid liquid is contained in the fibres, why should not the animal spirits effervescing with it sometimes themselves turn into an acid liquid, as does happen, according to the learned author's opinion, in Melancholia and Mania? Wherefore it seems preferable to suppose that the animal spirits consist of nitro-aërial particles, which proceeding, on the determination of the mind, from the brain into the motor parts, meet there the saline-sulphureous particles, and that by their mutual agitation taking place according to their nature, the contraction of the fibrils is effected in the way to be described below. Indeed, I imagine the animal spirits to be of such a sort that they never undergo change; and as to the diseases which are commonly believed to depend on their vitiated condition, I consider that they arise from the interrupted flow of the animal spirits or from their inordinate motion, as will be more fully stated below.

For, indeed, nitro-aërial particles seem in a high degree to fit the character of animal spirits, inasmuch as they are very subtle, elastic, and agile. For nitro-aërial particles are suited for entering on very rapid and igneous motion, as we have elsewhere shown. The animal spirits are also of this sort : they pass in a moment through the filaments of the nerves, although these have no visible cavity ; and brought at last to the muscles, cause their instantaneous contraction by their own most rapid motion.

Further, the animal spirits, like the nitro-aërial particles, are so slender that they are at once dissipated and leave no vestige of themselves. I further add that nitro-aërial particles, no less than the animal spirits themselves, are necessary for the sustenance of life. In fact it is difficult to conceive why animals should have such a necessity of breathing air, so that not for a moment can they live without it, unless the nitro-aërial spirits had a primary place in animal life and were the animal spirits themselves. Hence, according as there is need of a greater abundance of animal spirits, as in violent movements, or of less, as when the body is at rest, so nitro-aërial particles must be supplied in greater or in less quantity, and this is the reason why the respiration is so much increased in violent movements.

Besides, whence, I would ask, is a supply of animal spirits sufficient for continued work obtained unless, I say, we call to our aid the air, that inexhaustible fountain ? For it is probable that animal spirits are used up in the performance of muscular contraction in much greater quantity than is commonly believed. For I really do not know how a muscle could contract itself with so much force, unless elastic particles were present in sufficient abundance for its contraction; and hence it is that in violent exercises the respiration is so much increased that the great expenditure of animal spirits made in motive effort may be repaired.

And not only for muscular motion but also for sustaining life itself, it seems that there is need of an ample supply of animal spirits; for it is probable that life cannot be sustained without a certain series and continuous flow of animal spirits passing constantly through the brain, or at all events through the cerebellum. And that is the reason why, if the respiration or the motion of the heart or of the blood be stopped even for a moment, the animal at once dies. But more fully of this below.

In confirmation of what has been said, there may be adduced what the learned Malpighi has observed as to the Respiration of Insects: viz.—*Insects which live when their head is cut off, and the separated portions of which live, have pneumonic vessels distributed through the whole duct of their spinal marrow.* For this eminent man observed that the *black points* which are to be found on each side of insects close to the spine, are so many spicula or tracheæ through which the insects draw air; insomuch that if the said spicula be smeared with oil the animal soon dies from want of breath. Further, he found out that some branches of the said tracheæ are inserted here and there into the spinal marrow.

But that we may apply these observations to the present subject, I think we must allow that the animal spirits in the more perfect animals are elaborated only in the brain, and that they are disseminated from that source to the spinal marrow and to the nerves originating in it; whence it comes about that if the head of such animals is removed, the influx of animal spirits into the spinal marrow is altogether shut off, so that the parts of the decapitated body at once collapse and are deprived of animal motion.

But in less perfect animals, such as insects, whose cut-off parts live, the animal spirits are primarily and immediately prepared, not only in the brain but also in the protuberances of the spinal marrow, as it were in so many cerebelli extended through the whole length of the spinal marrow, or rather they are stored as in suitable repositories; and hence it comes to pass that in the cut-off portions of insects, the animal spirits are supplied, for keeping up to some extent life and motion, from the small piece of spinal marrow connected with each portion.

But that the animal spirits should be brought into the spinal marrow of insects, it is most necessary that some spiracula or bronchiæ should be, as is the case, continued into it, so that nitro-aërial particles, of which animal spirits consist, should by their means be carried into the spinal marrow. Hence, if any of the said spiracula be smeared with oil, the neighbouring parts, inasmuch as they are deprived of nitro-aërial particles and of animal spirits, at once become paralysed, the remaining parts being meantime healthy; while yet in perfect animals the nitro-aërial particles are introduced into the blood only through the lungs, and then, by the heart's pulsation and the flow of the blood, are carried to the brain and thence to its spinal appendix. Hence it happens that if the trachea is obstructed, and inspiration suppressed, or if the motion of the heart and of the blood stops, or even if the brain is disordered, the nitro-aërial particles will not be transmitted to the brain for the preparation of animal spirits, and therefore the animal will speedily die.

From these things I conclude that it is to some extent made out that nitro-aërial particles, transmitted by means of respiration to the mass of the blood and thence to the brain, are the animal spirits themselves. And this is in accordance with the fact that animals placed in a glass vessel from which the air is exhausted by means of Boyle's air-pump, after a short time perish miserably in convulsions. For the animal spirits being deprived, by the removal of the air, of their due supplement, enter, as is their wont, upon disorderly movements, and rushing tumultuously into the nervous system, excite convulsive movements, and at last the animal dies for want of air and of spirits.

To these things I further add that those who suffer from English consumption are very much weakened and are almost destitute of animal spirits. And the reason of this seems to be, that as their lungs are wasting and flaccid, the nitro-aërial spirit is no longer brought to the mass of the blood in quantity sufficient for the preparation of animal spirits.

Nor should it be overlooked that in pestilential diseases the brain chiefly suffers, and the economy of the animal spirits is disturbed : for in so far as, in the spread of the plague, the nitro-aërial spirit is tainted by the poison, it must follow that the animal spirits, composed of it, are out of sorts.

At any rate nitro-aërial particles seem to be in the highest degree suitable for the preparation of animal spirits ; for it is the nature of spirits to be sometimes inert and languid, and indeed on the other hand sometimes very active and agile. Similarly the nitro-aërial spirit is wont to assume diverse states, for at one time it is in the most complete rest, and at another with its greatest agility it enters on most excited motion. For nitro-aërial spirit, when mixed with sulphureous particles, is moved with most rapid and sometimes quite igneous motion; but when loosed from the company of sulphureous particles, it attains a state of extreme quiet, as we have elsewhere shown.

Lastly, let us here note that the nature of the brain seems to fit it in a high degree for collecting and storing nitro-aërial particles, as the brain in comparison with other organs lacks the sulphureous particles which agitate and waste the nitro-aërial spirit; and that it has a sort of saline quality which specially fits it for retaining the nitro-aërial spirit.

From this hypothesis of ours the reason may be sought, why, above all other sensibles, light affects the animal spirits, and, by exciting them to motion, produces wakefulness, so that to induce sleep animals must close their eyes and so shut out the external light, while yet to the other sensoria the way lies open for external objects even in sleep. For I have elsewhere tried to show that light depends on the motion of nitro-aërial particles disseminated through the air. Therefore, as animal spirits consist of nitro-aërial particles, it comes to pass that they easily follow the motion of the luminous particles which are of the same kind as themselves. It tells also in the same direction that the eye, when struck with a pretty strong blow, seems to see a flame in front of it. For it is probable that the nitro-aërial particles, thickly occupying the optic nerves, are excited by the violent concussion to the motion requisite for the production of light. But whether some animals, such as the cat, can, at their own will, give a luminous motion to the nitro-aërial spirits in the eye, I cannot certainly decide: it is so far an indication of this that the cat can use its sense of vision even in the dark.

It is in harmony with the hypothesis stated above that animals struck by lightning are not unfrequently killed without any injury, or vestige of a blow. But that the reason of this may be understood, I may repeat here what I have elsewhere endeavoured to show, viz., that lightning is caused by the nitro-aërial particles diffused through the whole atmosphere being thrown by the violent concussion of the air into a luminous, and sometimes really igneous motion. Wherefore, if nitro-aërial particles constitute the animal spirits, it may sometimes happen that they in the brain follow the motion of the nitro-aërial particles forming the lightning in the air; so that the animal spirits would seem not so much struck by lightning as themselves to form lightning. And hence it is that they, being violently moved and as it were set on fire, are dissipated in a moment; and so on account of the flame kindled in its brain the animal, deprived of the common light and breath, is extinguished. But this will be dealt with later.

We may note here in passing that animals have need of more intense respiration for some time after violent exercise. The reason of this seems to be that the blood returned from the brain to the heart is to a great extent deprived of nitro-aërial particles, inasmuch as it had deposited some in the brain and in the cerebellum to supply animal spirits; whence it comes about that no small part of the blood goes without its proper fermentation, because of the lack of nitro-aërial particles, and is to some small extent coagulated. For it has been elsewhere shown that the fermentation and the motion of the blood are caused by nitro-aërial particles. And this has also been noted by the learned Dr Thruston. Wherefore, for the proper fermentation and fluidity of the mass of the blood there is need of a more frequent and a fuller respiration, even for some time after violent movement. And from these things we may seek the reason why the blood drawn during convulsive paroxysms is usually very thick and somewhat grumous. For in more violent muscular contraction there is very great expenditure not only of nitro-aërial but also of saline-sulphureous particles (as it is on their mutual action that the fermentation and fluidity of the blood depend), and therefore the mass of the blood must to some small extent be coagulated. But this is especially the case when the parts which serve for respiration suffer convulsion; for then, on account of the nearly suppressed respiration, the loss of nitro-aërial particles caused by the convulsive movements is not, as in other cases of violent movements, repaired by respiration.

One might at first sight object to what has been said above, that the animal spirits form a chief part of the body, and that it is therefore likely that they should be derived not from the air, as being something external and foreign to the body, but rather from the nobler particles of the blood, when these have been brought to the highest subtlety and vigour: further, that it does not become the admirable artifice of the animal mechanism that it should be set in motion by an external principle. I reply that, of whatever sort the animal spirits may be, they must certainly be supplied from without. For that they should be formed from the blood, as the mass of the blood is daily renewed from food, spirits even arising from that would come from without. Why, then, should the animal spirits not rather be derived from air than from food taken in? Undoubtedly the air is impregnated with most active and subtle particles; and there is such a necessity of inhaling it that not for a moment can we live without it. And indeed it does not seem possible that the immense expenditure of animal spirits can be supplied from any other source but the air.

But as to the artifice of the animal mechanism, it consists in this, that the parts of the body are formed with such perfect adjustment that quite stupendous effects are produced in it by common causes.

I may note here, by the way, that while I hold that nitro-aërial particles are the animal spirits, I do not wish to be so understood as if I thought nitro-aërial spirit to be the sensitive soul itself: for we must suppose that the sensitive soul is something quite different from animal spirits, and that it consists of a special subtle and ethereal matter, but that the nitro-aërial particles, *i.e.*, the animal spirits, are its chief instrument. For, indeed, as to the sensitive soul, I can form no other notion about it than that it is some more divine *aura*, endowed with sense from the first creation and co-extensive with the whole world, and that a little portion of it, contained in a properly disposed subject, exercises functions of the kind which we observe and admire in the bodies of animals; but that that spiritual material, existing out of the bodies of living things, is not to be supposed either to perceive or to do anything but to lie quite dormant and inert, being much as is the case with the sensitive soul when the animal is buried in sleep.

The afflux of arterial blood does not seem sufficient by itself for bringing the nitro-aërial spirits to the brain in ample enough quantity : wherefore I consider it likely that the thicker *meninges* which surrounds the brain undergoes a sort of pulsation, and that by its contraction the blood driven to the brain is compressed ; and that thus the nitro-aërial particles are pressed out of the mass of the blood, and driven into the brain, in a way not very unlike that in which the other motive particles are forced into the motor parts by the constriction of the muscles. Such a pulse of the *dura mater* is confirmed by autopsy itself: for, in fracture of the skull, part of the brain comes into view, it is seen to rise in a tumour, and immediately in turn to subside, which seems to be a motion of the brain after the manner of the heart's pulsation. For, indeed, when I consider the thickness, the strength, and the nervous fibres of the *dura mater*, I can imagine nothing else than that that membrane, like all the others, is intended for the production of motion. For it is probable that the *dura mater* behaves as another diaphragm by the help of which the brain draws in nitro-aërial spirits and in a sense breathes.

It is probable that the above described motion of the *meninges* is natural, but yet that we can at will increase its pulsation, just as we can that of the parts which serve for respiration. And so by its diverse motion various effects are produced in the bodies of animals, as is probable. For according as that membrane contracts itself more strongly or more weakly, the nitro-aërial particles, *i.e.*, the animal spirits, are driven in greater or in less abundance into the brain and thence into the nervous system. Hence if a supply of animal spirits greater than usual is required for carrying on more intensely the motive or the sensitive function, we need to constrict the head and brain, as any one can find out in his own case: and that constriction of the brain seems to proceed to some extent from the harder *meninges* contracting itself more strongly. Certainly, in great anger, when we strive to the utmost of our power to repel or to avenge an injury, the said membrane, as if seized with convulsions, seems to constrict the brain: whence it happens that the nitro-aërial spirits, forced into the brain and thence into the nervous system, produce certain involuntary movements and convulsive tremors.

I further add that it is probable that sneezing is produced by the *dura mater* contracting itself more strongly, and forcibly driving the animal spirits into the nerves devoted to respiration; for it is certainly established that the membranes of the brain are primarily affected in sneezing.

Further, I am not sure whether the pulsation of the heart, or even the respiration, both of which are periodic, do not depend on the pulsation of the harder *meninges* surrounding the cerebellum.

Further, it may be held that epilepsy, as also apoplexy, sometimes arise from the convulsion or from the paralysis of the said membrane: for in these diseases the brain itself is sometimes found free from any sign of disease, and those substances which consist of volatile salt and volatile sulphur, as oil of amber and such like, are specially useful in these diseases: for it is not to be supposed that the saline-sulphureous liquids penetrate the brain itself and strengthen it, inasmuch as sulphureous substances seem to be hostile to the brain, and when taken too quickly, rushing into the cloisters of the brain not unfrequently cause convulsions, as we have elsewhere indicated. But we may believe that the reason why the said oils are useful in these diseases is because they are specially suitable for strengthening the motor parts and the nervous fibres. For it has been found by experience that those things which consist of volatile salt and volatile sulphur are specially suitable for restoring the fibres of the muscles to a proper tone. Whence it comes that medicaments of that sort bring help to the weakened and all but broken membranes of the brain, and consequently afford very great aid in the above-mentioned diseases.

But, to go a little further in our conjectures as to the use of the said membranes, it seems probable that sleep is caused by the membranes surrounding the brain, but not those surrounding the cerebellum, either ceasing from their motion or at all events performing it but remissly, so that the nitro-aërial particles are no longer carried into the brain, and thus the animal functions are necessarily interrupted. For that a need of sleep presses upon animals wearied with labours is not, we must suppose, because of a want of nitro-aërial particles, *i.e.*, of animal spirits, inasmuch as a never-to-be-exhausted stock of them exists in the air, but rather because the saline-sulphureous particles of the blood have been consumed by watching and work ; but the deficiency of saline-sulphureous particles concerns the motor parts but not the brain, except in so far as the *meninges* of the brain, which are to be reckoned among the motor parts, refuse to perform their pulsations if there is a want of saline-sulphureous particles in the blood. Wherefore it would seem that we should hold that sleep takes place because the saline-sulphureous particles are so much consumed by work and watching that, on account of their deficiency, the *meninges* of the brain refuse to perform their movement. It certainly seems to have been arranged by the good providences of nature that when the saline-sulphureous particles are nearly exhausted by vigils, what remains of these particles should be reserved for the natural functions—those really necessary for life. Hence it is necessary that the membrane surrounding the brain should sometimes cease from its motion, so that the small remaining portion of motive particles should be employed for the motion of the membrane surrounding the cerebellum, and the other natural functions. For it must be noticed that although the spontaneous animal functions which are performed by means of the brain are for a time interrupted during sleep, yet the function of the cerebellum and the natural motions go on no less, nay, rather better, when we are asleep than when we are awake.

It goes to confirm what has been said, that when we have recently awakened from sleep we feel our brain turgid with blood and somewhat heavy. The reason of this seems to be that in sleep, the membranes of the brain ceasing from their pulsation, the motion of the blood is not promoted by the contraction of the said membranes as when one is awake, and therefore the blood must be somewhat detained, and stagnate in the membranes of the brain. But meanwhile during sleep the arterial blood, its passage through the brain being to some extent obstructed, is carried in greater abundance to the cerebellum; hence the nitro-aërial particles pass in sleep in greater abundance to the cerebellum and the nerves arising from it, and thus it is that the natural functions presided over by the cerebellum are better performed in sleep than when one is awake. What has been said also gives us a reason why, in diseases characterised by sleepiness, rather remiss respiration suffices for sustaining life. For in these diseases the respiration seems quite suppressed, and the wretched patients have not unfrequently been buried for dead. For as in such diseases the nitro-aërial particles are carried to the cerebellum only, and thence to the nerves dedicated to the natural functions, and even then natural offices are very remissly performed, it follows that the expenditure of nitro-aërial particles is the very smallest, and that for the reparation of this the least trace of respiration suffices.

CHAPTER V

OF THE FERMENTS OF THE STOMACH, THE PANCREAS, AND THE SPLEEN. INCIDENTALLY, OF DISEASES WHICH HAVE REFERENCE TO THE ANIMAL SPIRITS

FROM this hypothesis of ours it is possible conveniently enough to explain not only the effervescence set up in the motor parts, but also how the fermentations and concoctions of whatever sort are carried on in the viscera of animals.

For, in the first place, as to the digestion made in the stomach, the vulgar opinion is that there is in the stomach a certain acid ferment. But whence that acid should derive its origin one cannot very readily say, for anatomical observation establishes that no acid at all resides in a healthy stomach ; and as to acid eructations, they seem to come from ill-digested food and not from any natural acid liquid. We have, then, to inquire how it comes about that iron filings, taking in by the mouth, are corroded in the stomach and turned into a sort of vitriol ; and after taking chalybeate drugs a sulphureous and vitriolic odour is perceived in the throat, such as is usually produced from iron corroded by an acid liquid. Further, milk is coagulated in the stomach as it is when an acid liquid is added to it, and food when eaten not unfrequently becomes sour.

As to these things, I shall in the first place assume that the digestion of food is effected by the animal spirits, brought by means of the nerves, of which there are many inserted in the stomach, and that according as the animal spirits come to the viscera which serve for digestion in greater or in smaller abundance, so the digestion of the food is accomplished more quickly or more slowly. Hence if any one soon after a meal sets himself to study, or thinks much upon a difficult subject, so that the animal spirits are to a great extent detained in the brain on account of strained contemplation and greater agitation of the mind, the function of digestion is not properly performed on account of the deficiency of animal spirits ; but the undigested food remaining in the stomach produces heaviness and discomfort there, as anyone can observe in his own case. Yet, on the other hand, when the mind is free from thought, or even in sleep, the concoction of food goes on at the best ; that is to say, in so far as the animal spirits, not being engaged in carrying on other functions, are abundantly supplied to the viscera devoted to digestion. This is also indicated by the fact that soon after a rather full dinner or supper we usually have an inclination to sleep. For, indeed, when the stomach is replete the animal spirits for the greater part retire to the cerebellum and thence to the lower part of the abdomen to carry out the office of coction, so that scarcely any remain in the brain to perform the voluntary functions and sensation, whence it is that we can hardly keep awake.

But, now, as the animal spirits consist of nitro-aërial particles, there will be no difficulty in understanding how the effects just described are produced by them in the stomach. For although the nitro-aërial spirit is not acid, still iron is corroded by it and vitriols produced, fixed salts are liquefied, and the structures of things are dissolved as by a universal solvent, as I have elsewhere endeavoured to show. It therefore seems established that the much-talked-of ferment of the stomach consists chiefly of nitro-aërial particles, which, deposited in the membranes of the stomach by the nerves, are probably there mixed with a suitable liquid secreted from the blood by means of the glandular membranes of the stomach; and that the fermentative liquid of the stomach is composed of both of them brought into its cavity by special vessels. And hence we may seek the reason why serene and rather thin air so much tends to sharpen the appetite and to promote digestion, as has been found by common experience, while when the air is thicker and loaded with noxious vapours it soon produces discomfort in the stomach. To these I further add, that from the swimming-bladder which in many fishes has been found filled with an aërial substance, there is an open passage into the stomach—certainly a probable indication that something aërial conduces to the digestion of food, as has been remarked by the learned Dr Walter Needham. Indeed, as fishes take only a small quantity of air out of the water, it would seem necessary that the aërial ferment should be stored in some receptacle from which it may be brought in sufficient abundance when required. I am not able to decide certainly whether in terrestrial animals also, besides the nitro-aërial spirits brought by the nerves, an aërial ferment may not in addition come into the stomach directly from the mass of the blood.

I conclude that the digestive liquid of the stomach is not very different in kind from saliva ; for saliva seems to consist of nitro-aërial particles deposited in the maxillary glands by the nerves and there mixed with a serous juice derived from the blood : and that it is to be believed that the saliva mixed in mastication with the food conduces not only to its deglutition but also in no small degree to its digestion.

If the stomach be quite empty of food, its internal membranes are, as is probable, pinched by the nitro-aërial particles, and hunger seems to arise from this.

The food is concocted by the ferment of the stomach into chyme, which, when it has passed into the duodenum immediately meets the bile, by which, as by a new ferment mixed with it, it is further fermented and concocted. For as the bile is composed of saline-sulphureous particles, it necessarily effervesces in a high degree with the chyme, which is full of nitro-aërial particles ; but to what extent bile partakes of a fermentative nature may be gathered from this, that if it is mixed with a farinaceous mass it raises it and ferments it, as has been elsewhere stated.

OF THE FERMENT OF THE PANCREAS

I think that the pancreas must also be counted among the viscera serving for digestion, inasmuch as it is probable that the nerves which are distributed in great number in the pancreas, serve for carrying nitro-aërial particles, which, finding a suitable vehicle secreted by its glands from the mass of the blood, are passed into the duodenum by the duct of Virsungus for the further fermentation of the chyme. For when that pancreatic liquid, full of nitro-aërial particles, meets the chyme, imbued with bilious, that is, saline-sulphureous particles, a very intense effervescence must be excited; and it is probable that by means of this the concoction of the chyme is completed.

After these remarks as to the viscera had been written, a treatise on the pancreatic juice published by the eminent *Regner de Graaf* came into my hands, in which that learned man most clearly proves the existence of the pancreatic juice, and confirms the use which I have assigned to it. But it is scarcely credible that healthy pancreatic juice is of an acid nature: nor is it likely that the effervescence taking place when that juice is mixed with bile in the intestines is caused by the acid salt of the pancreatic juice meeting the fixed or the volatile salt of which the bile consists, as is the opinion of the learned author; for the effervescence of contrary salts does not seem suitable to the animal economy inasmuch as it comes quickly to an end and is always accompanied by coagulation, as has been stated above. Further, any acid salt mixed with a saline-sulphureous liquid changes and destroys its saline-sulphureous particles and coalesces with them into an, as it were, dead and insoluble calx, as in the preparation of *lac sulphuris*, as also happens in the case of bile mixed with an acid liquid. And hence it is that acid salts are most suitable for allaying inordinate fermentation of bile. While yet, on the contrary, in the natural fermentation excited in the intestines, the saline-sulphureous particles are brought to a just volatility, as appears from the fetor of the dejections and the inflammability of the flatus escaping from the belly. Wherefore it is probable that the effervescence produced when the bile and the pancreatic juice are mixed together, arises from the working of the nitro-aërial particles contained in that juice with the saline-sulphureous particles of the bile, which fermentation is fitted to exalt the saline-sulphureous particles of the bile, as we show elsewhere.

OF THE USE OF THE SPLEEN

Let us now, lastly, draw from this hypothesis of ours a conjecture also as to the use of the spleen. If we consider the wonderful structure of the spleen and its notable size, and further regard the numerous branches of nerves devoted to it, and its immense supply of blood, we shall certainly conclude that the spleen (which is to be found in all animals) serves some general purpose. But it is an astonishing thing that animals can live although their spleen is completely extirpated, and indeed bear its loss without any manifest inconvenience. Hence there has been much discussion among anatomists as to the function of the spleen; so that it might seem that this troublesome organ had been made to harass the minds no less of the physician than of those suffering from it. For, indeed, nature has covered the spleen more than other parts with blackness and darkness, as if she wished to hide it and keep it in the dark. Some have assigned to it a use so frivolous and mean, that the indignant spleen could scarcely but burst itself with laughter.

That my views as to the function of the spleen may be understood, I must repeat here what has already been stated, viz., that it is probable that for sustaining animal life it is necessary that the animal spirits, *i.e.*, nitro-aërial particles, should traverse the brain, or at all events the cerebellum, in continuous series, and that that is the reason why on the suppression of the respiration or on the interruption even for an instant of the motion of the heart, the higher animals at once die.

If this is granted, it follows that the nitro-aërial particles must be perpetually carried away again by certain vessels; and, indeed, we may believe that the nerves dedicated to the natural motions, and also those belonging to the viscera, serve to carry away the animal spirits from the cerebellum. But as for natural functions, particularly for the digestion of food, there is need now of more and now of less afflux of nitro-aërial spirits; while the nitro-aërial particles pass through the cerebellum in a nearly uniform flow: it is, hence, necessary that the superabundant nitro-aërial particles, those, namely, which are not required for natural functions, should be conducted elsewhere, and it is probable that the spleen is formed in order that these superabundant nitro-aërial particles should by means of it be properly mixed with the mass of the blood. For these nitro-aërial particles being discharged by the splenetic nerves into the spleen are by its means, being diffused through its parenchyma, most intimately mixed with the mass of the blood: whence it comes about that an effervescence sufficiently intense but yet not too impetuous is excited by the nitro-aërial particles mixed abundantly but uniformly with the saline-sulphureous particles of the blood; so that the spleen really seems to perform these three functions:—

1. That the nitro-aërial particles which pass in continuous series through the brain, but are not required for natural or animal functions, should be carried into the mass of the blood and properly mixed with it.

2. That the nitro-aërial particles may be brought in suitable abundance and with a certain control to the viscera devoted to the digestion of food. For as these viscera are filled with food or empty, so they have need of a greater or of a less afflux of fermentative particles.

3. That the nitro-aërial particles in a condition of motion and vigour should, when most intimately mixed in the substance of the spleen with the saline-sulphureous particles of the blood, excite in the mass of the blood such an effervescence as is fitted to bring its saline-sulphureous particles to a proper volatility.

As to the former uses here assigned to the spleen, the plexus and various communications among the splenetic nerves and among those distributed to the other viscera seem to confirm them. For by the communications it is brought about that the nitro-aërial particles are brought to the viscera serving for the digestion of food, and for other natural functions now in greater and now in less abundance as there is need, and what of the nitro-aërial particles is over is turned aside into the spleen.

And in this we may seek the reason why, when the spleen is affected by scirrhus, or obstructed in any other way, the patient suffers from eructation and hypochondriac flatus: for inasmuch as the nitro-aërial particles cannot now, as at other times, pass through the spleen on account of its obstruction, they are carried into the stomach, the pancreas, and the other viscera of digestion without any moderation or control. But the digestion of the food and chyme is carried on too quickly by these particles mixed in too great abundance with the chyme in the stomach and other parts of the abdomen, and consequently very impetuous and flatulent fermentations must arise.

Further, as the nitro-aërial spirits cannot be discharged into the obstructed spleen, it is probable that they to some extent regurgitate towards the brain and the cerebellum, whence it comes to pass that the motion of the animal spirits will be perverted, and consequently the brain and the animal economy perturbed, as often happens in hypochondriac affections; although this may also be produced by another cause, as will be set forth below.

And besides, this also points in the same way that, namely, the spleen in infants is florid and scarlet, although later it acquires a black-blue colour. For that difference of colour seems to come from this, that in infants the nitro-aërial particles come only sparingly to the spleen, so that as yet scarcely any fermentation is excited in it, as will be more fully stated presently. But the reason why the spleen does not perform its function in infants seems to be this. The fœtus in the uterus, as also for some time after birth, has hardly any, at all events no violent exercise, and the heart and the organs of respiration do not as yet need to work at all hard: for these natural functions are specially increased in violent motion of the body, as has been shown above. Hence the nitro-aërial particles are transmitted through the cerebellum and the nerves arising from it only gently and with a constantly even flow ; particularly in the uterus, where, namely, the infant does not yet exercise the respiratory organs. Whence it comes that the spleen cannot be of any use, in so far, that is to say, as the nitro-aërial particles traversing the cerebellum in a slender stream are almost all required for the natural functions : but when the animal has grown up and exercised itself in violent labours, it is necessary that these nitro-aërial particles should be transmitted through the cerebellum in greater abundance, so that the nitro-aërial spirits may now make for themselves sufficiently open channels in the cerebellum, and that the structure of the brain may become more compact and firm, and so the nitro-aërial particles in the future may more easily and in fuller rush pass through the brain and the nerves arising from it. But when these are not all required for carrying on in the usual way the natural functions, it is most necessary that the superabundance of nitro-aërial particles should be discharged into the mass of the blood and most intimately mixed with it by means of some organ, such as the spleen.

I add, besides, that while the fœtus is in the uterus, the viscera devoted to the digestion of food have either nothing to do, or at all events act very remissly. Whence it happens that the nitro-aërial spirits do not go at all to these viscera, or at all events only in very small amount ; and there is thus no need that any part of them should be diverted to the spleen. Further, as the infant immediately after birth takes food continuously, or at all events at frequently repeated times, so that its stomach is to some extent filled with food, and consequently the nitro-aërial spirits ought to come to the viscera devoted to digestion in an always even flow and without any regulation, yet when the animal has come to the limit of its growth it takes food at fixed periods, and its stomach is sometimes filled with food and sometimes nearly empty: and hence it is necessary that the nitro-aërial particles should be carried now in larger now in smaller abundance to the viscera of digestion. But that the afflux of nitro-aërial spirits to the viscera should take place under control, it is necessary that there should be some organ in the parenchyma of which the excess of nitro-aërial spirits may be deposited, as has been shown above.

As the primary function of the spleen has no place in infants, so neither is it necessary that its other office, that is, the more intense effervescence of the blood (which we have said takes place in the spleen), should be exercised in infants. For it is probable that the said fermentation excited in the spleen has for its chief effect the bringing of the saline-sulphureous particles of the blood to a proper volatility for motive and procreative functions, and these functions do not well suit a tender age.

We gather from what follows that an effervescence of that sort is excited in the parenchyma of the spleen by the nitro-aërial particles brought along the nerves and mixed with the saline-sulphureous particles of the blood.

For it has been made out by observation that if the spleen is obstructed by scirrhus, the mass of the blood lacks its proper fermentation and becomes crude and vapid, so that dropsy and chlorosis often result. But I confess I do not know whence that fermentation excited in the spleen should arise, unless from nitro-aërial particles along with the saline-sulphureous particles of the blood; for I cannot agree with those who hold that the thicker and more fixed part of the blood is detained in the substance of the spleen, and that it, by being long kept, acquires a fermentative character; for if particles of blood anywhere cease from movement and stagnate, these particles will prevent those immediately following from continuing their motion, and thus, the circulation of the blood being in that part interrupted, swelling and inflammation will necessarily result. Moreover, the numerous branches of nerves disseminated in the spleen seem to serve no other purpose but that of conveying fermentative particles.

Besides, as, when the fermentation in the spleen is too remissly performed the blood becomes too crude, so on the other hand if it is increased beyond a right extent the mass of the blood acquires a somewhat dried and atrabilious character. Indeed, as in fire the nitro-aërial particles effervescing most intensely with the sulphureous particles burn them up in an instant and turn them into smoke, as we elsewhere show, so also if in the parenchyma of the spleen the nitro-aërial and the saline-sulphureous particles work together too much, it comes to pass that these particles become to some small extent dried up and sooty, and from them, fixed in the substance of the spleen, its dark purple colour seems to come. But that atrabilious diathesis of the blood may arise from a twofold cause.

1. If the saline-sulphureous particles of the blood are detained in the spleen by reason of its parenchyma being obstructed; for thus they, by effervescing too long with the nitro-aërial particles, become torrefied and dried. Indeed the mass of the spleen seems to be composed of two kinds of vessels; of which one, consisting of nervous fibres, is destined for the

distribution of nitro-aërial spirits, and the other for the diffusion of the blood, and besides for the reception of nitro-aërial particles. If the obstruction takes place in the former, the mass of the blood will lack its due ferment, the influx of fermentative spirits being shut off from the spleen; but if the latter are obstructed, the saline-sulphureous particles of the blood detained too long in the parenchyma of the spleen and too much fermented, will acquire an acid and dried up, or what is the same thing, an atrabilous character.

2. If the substance of the spleen is corroded by an ulcer, or in any other way broken, the nitro-aërial particles will not now as at other times be mixed in its parenchyma intimately and uniformly with the mass of the blood, but in a too crowded and confused way, whence a too intense effervescence is set up, on account of which the saline-sulphureous particles of the blood become much dried up and sooty.

From what has been said, it is not difficult to understand how it is that an animal can live when deprived of so notable an organ as the spleen; for although the offices here assigned to the spleen conduce in no small degree to the correct carrying on of the animal economy and to perfect health, still they are not so necessary but that life can be kept up in a so-so way without them.

We may here note, by the way, that the parts of all sorts of animals contain an ample quantity of volatile salt, but scarcely any fixed salt, as is shown by their distillation and combustion; and yet many animals live on vegetables only, and in these there is little or no volatile salt to be found, but much fixed or rather nitrous salt: so that it would plainly seem that the fixed or nitrous salts of the food are transformed inside the bodies of animals—in that chemical workshop of nature—into volatile salts. And the same thing is indicated by the fact that if plants which contain no volatile salt are allowed so far to putrefy until they have been converted into worms, then at last volatile salt can be obtained in abundance from them by distillation.

As to the mode in which the fixed salts of the food are volatilised, it is probable that the various digestions and fermentations taking place in the bodies of animals contribute in no small degree to the volatilisation of the salts: indeed we may hold that the spleen is the chief workshop in which the nitrous salts of the food are worked up into volatile salts. For, indeed, it is to be observed that when vegetables are burned, their nitrous salts are volatilised; and they flying off along with sulphureous particles, somewhat dried up, constitute the soot. And soot abounds in volatile salt, although the vegetables which, when burned, yield it, before their deflagration, contained only fixed or nitrous salt. And, as when nitre and sulphur are mixed together and burned, as in gunpowder, it is probable that the fixed salt, of which nitre in part consists, is volatilised during the deflagration and flies off in the vapours; for otherwise the fixed salt of the nitre would remain after its combustion, for in the deflagration of nitre, the nitro-aërial particles which it contains, set in very swift motion by means of the ignited sulphureous particles, most finely break up the saline particles with which they were previously firmly combined, and, as is probable, makes them volatile.

But if indeed the exceedingly impetuous and igneous effervescence of the nitro-aërial and sulphureous particles suffices to volatilise in an instant the fixed salts of vegetables, why should we not suppose that that very intense working of nitro-aërial and sulphureous particles excited in the spleen should suffice to volatilise the fixed, or rather the nitrous, salts of the chyle? It tends in this direction also that the saline-sulphureous particles in the spleen become somewhat burnt and sooty, as we have indicated above. We might here also show that fermentations of all kinds excited in the bodies of animals come from the working of nitro-aërial and saline-sulphureous particles, if this were not foreign to our present subject.

OF THE DISEASES WHICH HAVE TO DO WITH ANIMAL SPIRITS

From the hypothesis as to the nature of animal spirits stated above, we can at once say in what the animal economy consists, and also can give an explanation of many diseases which have to do with the motive function.

For properly keeping up the animal life, it is first of all necessary that the lungs should be in a healthy condition, so that the nitro-aërial particles may be transmitted in sufficient abundance into the mass of the blood and intimately mixed with its sulphureous particles. Hence, if the lungs are corrupted or even wasting and flaccid, the mass of the blood will not be properly fermented, on account of the deficiency of nitro-aërial particles: and so, as the motive particles of both kinds would be deficient, there would follow extreme lowering of the powers, and weakness, and consumption of the body.

Further, in order that the animal function should be properly carried on, it is necessary that the nitro-aërial particles brought with the mass of the blood should be transmitted into the brain and the nerves arising in it. If the nitro-aërial particles are either not carried in sufficient quantity to the brain, or are unable to pass along the nerves on account of their obstruction, the animal economy and the motive function cannot but be disturbed. And from this cause apoplexy, paralysis, and diseases of that kind seem not rarely to originate, as has been stated above.

As the nitro-aërial particles have to be sent to the brain, so on the other hand the saline-sulphureous particles have to be altogether excluded from it; for if they, either on account of their extreme tenuity, or because the passages of the brain are too open, or on account of too intense fermentation of the blood, make their way into the brain, they excite the nitro-aërial particles, *i.e.*, the animal spirits, to inordinate motions. Hence the animal economy is much perturbed, as probably happens in intoxication, epilepsy, and other diseases of the kind: for liquors full of volatile sulphur, such as spirit of wine and the chemical oils of vegetables, when taken too quickly, not infrequently produce the diseases mentioned. An observation which I have made more than once points in the same direction. For I have known persons subject to paroxysms of a maniacal sort and also convulsions, whose sputum, while they were suffering from such paroxysms, burst into flame like oil, or rather in the manner of gunpowder, when it was put into the fire: it is indeed probable that in this case the saline-sulphureous particles of the blood had been carried so far that they burst into the recesses of the brain and there excited the animal spirits to inordinate motions.

As to melancholia and mania, it is probable that the saline-sulphureous particles of the blood detained in the parenchyma of the spleen and long effervescing there with the nitro-aërial spirits acquire an atrabilious and malignant character; and that, besides, they become so subtle that they penetrate deep into the brain and perturb the animal spirits. For it is probable that diseases of this kind which concern the brain arise not so much from a diseased condition of the animal spirits as from their perturbed motion or even from their deficiency; for as the animal spirits consist of an ethereal matter, they canno be subject to any change, as we have indicated above.

It is, besides, required for the establishment of animal life and the motive function that the mass of the blood should be impregnated with saline-sulphureous particles duly perfected, so that, namely, the nitro-aërial particles mixed with them may excite a proper effervescence in the mass of the blood. For it is necessary for the sustenance of animal life that the saline-sulphureous particles of the blood should, by continuous fermentation of the blood, be brought to a proper volatility; so that they being separated from the blood by means of the muscular flesh, may be transmitted into the motor parts, as I have endeavoured to show above. But if, indeed, the saline-sulphureous particles are not properly exalted by reason of a too slight fermentation of the blood, or are not promptly enough separated from the mass of the blood on account of a diseased condition of the muscular flesh, the motive function can scarcely be set up. And so it is probable that the spontaneous lassitude and incapacity for motion which accompany scorbutus and the icteric disease proceed from this cause; for these symptoms seem in the said diseases to come not so much from a deficiency of animal spirits as from a disorder of the blood and of the muscular flesh.

We may note by the way, that the slighter convulsions, like those twitchings of the tendons which trouble many in continued fevers, may to some extent arise from a diseased condition of the muscular flesh, in so far as not only the saline-sulphureous but also the nitro-aërial particles passing through the now nearly wasted away parenchyma of the flesh, are transmitted into the motor parts, and by their mutual effervescence the often slight contractions of the fibrils are, as is likely, produced.

From what has been said, the reason can be given why bodily exercises are so useful in the icteric disease and scorbutus, and also for warding off the paroxyms of intermittent fevers. For in exercise the nitro-aërial particles are sent in greater abundance into the mass of the blood by the more intense respiration, and the motion of the blood is much promoted by the contraction of the muscles; whence it comes about that the fermentation of the blood is increased and it is comminuted and worked up. Further, the saline-sulphureous particles brought to vigour are secreted from the blood and used up in muscular contraction, and it is by their excess in the mass of the blood that febrile heat is excited, as has been elsewhere said.

CHAPTER VI

OF THE MODE IN WHICH THE FIBRILS CONTRACT. ALSO, OF THE MOTION OF CONTRACTING MUSCLES

So far as to the motive particles by which muscular contraction is effected : it remains to enquire how the muscles are contracted by them. I cannot, for reasons given above, admit that muscles are contracted by the inflation of the fibres, a view approved by some. I further add that the fibrils, which in our opinion primarily undergo contraction, seem to be solid bodies ; so that they are quite incapable of being inflated.

As to the contraction of the fibrils, as far as I can make out from anatomical observation and from mental conjecture, the fibrils in their contraction seem to undergo contortion. Certainly this mode of contraction of the fibrils, namely, by contortion, is most in harmony with the constriction and hardness of contracted muscles. Further, a contortion of the fibrils is specially suitable for the very strong pull of muscles which is sometimes exerted with quite wonderful force. And to these things we further add, that the motion of the nitro-aërial particles, by which in our opinion muscular contraction is effected, is of a sort fitted for twisting the fibrils, as, I think, will be established by the following experiment.

Let a very fine music string be grasped by the fingers of both hands at a short distance from each other, and be held a such a height above a lighted candle that the string may be sufficiently heated but not burnt. When this is done we shall feel the string contract with a distinct enough force as it is affected by the heat of the lamp; but that contraction of the string is not produced in the common way, but the string seems to undergo a spontaneous movement, and to run together into itself in the manner of fibrils—a very pleasant sight. But if the contracted string be removed from the lamp it can be drawn out to its former length with very little effort. As to the cause from which the contraction of the string proceeds, we may believe that the nitro-aërial particles, bursting out of the flame of the lamp, produce that contraction of the string; for we have elsewhere shown that igneous particles of any kind (and it is by them that the said string is contracted) are nothing else than nitro-aërial particles in a state of motion. But the contraction of the string seems to proceed from its being twisted by the nitro-aërial particles: for if one end of the cord is left free, or if it is held less firmly between the fingers and brought towards the lamp, we shall see the cord rotated pretty quickly. For as the nitro-aërial particles bursting out of the lamp with the circumgyratory motion proper to them act on the said cord, they drive it round with their own motion and twist it.

And it is certainly probable that the contraction of the fibrils is accomplished in a not very dissimilar way: for the fibrils, in which the contraction primarily takes place seem, in as far as the microscope can help us, to be very like an extremely slender music string. Besides, we suppose that the contraction of the fibrils is caused by nitro-aërial particles set in motion, and even pretty intensely warmed in the motor parts. Wherefore, if a coarse and thick cord undergoes contraction when touched by nitro-aërial particles, how much more should the extremely slender fibrils, fashioned with the highest skill, follow the motion of the nitro-aërial particles? And it tells in the same direction that a muscle, like the aforesaid cord, cannot be brought again to its original length without the exercise of some force. And this is the reason why a cramp is wont to be produced in a muscle when its antagonist is paralysed. For when a muscle is once contracted, it will remain in that state until it has been restored to its former length by the pull of its antagonist.

From this hypothesis of ours it is not difficult to understand how it is that when the motor parts have been struck by lightning they are not unfrequently convulsed, and remain tense, although the skin surrounding the muscles is often uninjured. For I have tried to show above that the nitro-aërial particles, not only those in the brain but also those in the motor parts, are sometimes as it were set on fire when struck by lightning: nay, the nitro-aërial particles thrown into an almost igneous motion, twist and wrench the fibrils so violently that they cannot again be drawn out, but will remain always tense; just as it would happen to the above-mentioned string if it were brought too near the flame of the lamp.

OF THE MOTION OF CONTRACTING MUSCLES

Now that we have in this way considered how muscular contraction is caused, it remains for us to enquire shortly as to the motion of the muscles as they contract, and also that of the parts attached to them: but that this matter may be more clearly understood, I shall premise the following.

In the first place, we lay it down that if any string with both ends free contracts uniformly, both of its ends will be drawn towards the centre.

In the second place, if one end of the string to be contracted were free but the other end fixed, that the free end and also all parts of the string would in contraction be drawn towards the fixed end; as is shown in Plate III., Fig. 4, which represents the string so arranged and the same when contracted. For in the said figure not only the free end of the string, *c*, but also its middle point, *a*, and so also all parts of it, are drawn in the contraction of the string towards the end, *b*, which we suppose fixed. And similarly we infer that if a greater weight be attached to the one end and a smaller weight to the other, the smaller weight will in the contraction of the string, be drawn towards the greater as to a fixed end.

From what has been said, it will not be difficult to understand the motion of the shortened muscles and fibres. For if a fibre, fibril, or muscle, both of the ends of which are free, undergoes contraction, it is not to be doubted that both its ends will approach the centre; but if one of the ends is attached to an immovable part, as is the case with the muscles which serve for local motion, the contraction will be altogether to the more fixed part; and the movable end, which in a muscle is called the termination, is drawn in contraction towards the immovable end, or what is the same thing, the head of the muscle. And this also occurs in the contractions of the fibres or fibrils. I am quite aware that the learned Dr Willis has expressed a contrary opinion in his treatise *On Muscular Motion*, for that distinguished man altogether denies that the fibres contract from the termination towards the origin; and indeed affirms that when they are shortened, both of their ends are drawn towards the middle. But with all respect to such a man, when one end of a fibre is fixed to an immovable tendon, it seems to me necessary that the more movable end of the contracted fibre, as also its middle, should be moved towards the immovable end, as is evident from the premises.

Therefore, as to the contraction of the fibres, of the fibrils, and consequently of the muscles, I think we must hold that they by no means, of their proper nature, contract towards the origin rather than towards the termination; but do so only because what we call the origin is inserted into an immovable part. So that if the bone into which the termination of the muscle is inserted should be more fixed than that other one to which the origin is attached, then in its contraction the muscle will be drawn towards its termination. For example, let any immovable thing be grasped by one hand, and then the flexor muscles of the forearm be strongly contracted (which will occur if we make an effort to draw the thing grasped towards us), in this case, I say, as the thing grasped, being immovable, cannot be moved to us, the said muscles in their contraction will drag our arm and our whole body which is attached to it towards the thing grasped; so that it will happen that these muscles in their contraction will be drawn towards their termination and not towards their origin; inasmuch as the bone of the forearm in which the terminations of the said muscles are inserted becomes, on account of the immobility of the thing grasped, the more fixed end: as is shown in Plate III., Fig. 3, in which while the flexor muscles of the forearm, *a*, *c*, contract, they do not as in other cases bend the forearm but the arm; and in their contraction carry *b*, *c*, towards the terminal tendon, *c*.

As to the motion of the sphincters, as also of membraneous bodies, the motion of which ends in themselves, they are carried in their contraction towards the centre of that space which they surround.

CHAPTER VII

OF THE CONTRACTION OF THE DIAPHRAGM. ALSO, OF THE PULSATION OF THE HEART. INCIDENTALLY, OF THE PALPITATION OF THE HEART. ALSO, OF THE MOTION OF ANIMALS WHEN THEY RAISE THEMSELVES ALOFT

In the last chapter we have spoken of the motion of contracting muscles in general, and specially of the flexor muscles. Let us now further consider more closely how the diaphragm moves when it contracts, also in what manner the pulsation of the heart is produced ; and, finally, how the extensor muscles, by means of which jumping is effected, contract.

First, then, as to the contraction of the diaphragm, although I have discoursed of it in my recently published treatise *On Respiration*, still there are some things which may be added here.

In the first place, then, I affirm as indubitable that if a curved surface, the circumference of which is on all sides fixed to an immovable part, undergoes contraction, it will all round be carried towards the immovable part, and the curved surface will approximate to a plane but will never go beyond the plane ; as is shown in Plate III., Fig. 5, in which let *b*, *a*, *c* be the curved surface and *b* and *c* its fixed ends. As it contracts it will be brought towards the plane *b*, *e*, *c*. So that the contracted surface *b*, *d*, *c*, that is to say its middle point *d*, is nearer to the ends, *b* and *c*, than the middle point, *a*, of the surface not yet contracted, but that surface, however much contracted, can never go beyond the plane, *b*, *c*, as the line, *b*, *e*, *c*, is the shortest between the ends, *b*, *c*.

These things being granted, we assert that the diaphragm in its contraction is moved towards a plane; in other words, when it contracts, it goes towards the cavity of the abdomen, that is, outwards and downwards. For it is known from anatomical observation that the form of the diaphragm is curved, and that its concave surface or sinus looks towards the belly; but now as any curved surface whatsoever moves, in contracting, towards a plane, that is, towards the region to which its concave surface looks, and as the parts of the abdomen to which the concave surface of the diaphragm looks are relatively to it, inferior and exterior, it necessarily follows that when the diaphragm, in its contraction, moves towards them, in its contracted state it goes downwards and outwards. And besides, this can be seen in vivisections by simple inspection. In this connection I am much astonished that the eminent Dr Willis, in his *Answer to Dr Highmore*, has expressed the opinion, "That the diaphragm in violent inspiration, such as sometimes occurs in the case of dying persons, descends beyond the plane and becomes very convex towards the intestines." For this does not seem very likely: for it is not "possible that such a descent of the diaphragm should be caused by the lungs taking a long inspiration," as the distinguished author supposed, inasmuch as the inspiration and the swelling of the lungs are caused by the descent of the diaphragm and the dilatation of the chest. But the lungs never cause the descent of the diaphragm, but in all cases follow its motion, as we have elsewhere shown ; and the diaphragm, in contraction, is brought towards a plane but never recedes from it, as appears from what has been premised.

I admit that in wounds of the chest or of the diaphragm itself, the diaphragm is sometimes convex downwards, but this does not occur in inspiration nor does it arise from the lungs taking a long breath ; it happens only in the contraction of the chest and in expiration. For, indeed, in the dilation of the chest gaping from a wound it has suffered, the air rushes partly through the trachea into the lungs, but partly also, by reason of its own elastic force, makes its way through the opening of the wound into the cavity of the chest ; so that when in expiration the space of the chest is suddenly diminished, the air contained in the cavity between the lungs and the internal wall of the chest, as it cannot at once escape through the wound, compressed by the contraction of the chest, pushes the diaphragm, which is in diastole and relaxed, and pressing it downwards makes it convex, as I have assured myself by inspection in vivisections.

It is to be further noted as to the diaphragm that its fleshy fibres proceeding from its membranous centre, go out into the surrounding parts, into which it is inserted : whence it comes about that as some fibres stretched out in one direction, some in the opposite, contract with equal effort, the membrane between them is held in equilibrium : yet meanwhile the diaphragm as a whole is carried downwards and outwards, just as if it had fibres stretched over the whole of its length.

OF THE PULSATION OF THE HEART

As to the contraction of the heart, the distinguished Dr Lower has given such an accurate description of its motion, as also of its structure, that it is scarcely possible to add anything to it. That the heart is really a muscle, and that the contraction of its fibres diminishes the space of its ventricles and drives out the blood, is quite indubitable. But here we come across a by no means small difficulty: for seeing that the heart is contracted and becomes smaller in its systole, how is it that at that very time it strikes the left side of the chest? For it would seem that in its contraction the heart should rather move inwards and further from the wall of the chest.

Hence some have supposed that it is not the heart but the great artery, distended with blood, which strikes the chest. But in fact the great artery, soon after its origin, turns backwards towards the spine, so that its pulsation could not be felt outside. Nor can we say that the auricles of the heart in their contraction strike the thorax, because they, just like the heart, become smaller in their systole, and besides, their deeper position is less suitable for striking the chest.

And lastly, we cannot believe that the apex of the heart impinges on and strikes the thorax, because of its vibration and jump; for I have found from vivisections that the pulsation of the heart is produced by its ventricles swelling up into a tumour: in fact, if the thorax of a dog be quickly opened, and a finger, or the hand, be placed on the left ventricle of the heart, you will feel the ventricle swell up with a jump and strongly strike the hand or the finger placed on it. Indeed, if the finger be held at some distance from the left ventricle, when the heart contracts the side of that ventricle will be briskly dashed against the finger. And that this is really the case has already been remarked by the eminent men, our own Harvey and Lower.

Harvey held that this kind of pulsation of the heart arises thus: that while the walls of the ventricles contract as to length, they must increase in thickness, just as when other muscles contract they swell up, becoming broader. I admit that the walls of the ventricles of the heart do in contraction become somewhat thicker, but it is scarcely credible that they swell up sufficiently to account for the pulsation against the chest; for it has been made out that muscles do not in their contraction swell up so much as is commonly believed.

I think, then, that we should hold as to the pulsation of the heart, that the fibres of the heart, in their contraction, draw its cone towards its base as to the more fixed end; whence it comes about that the walls of the ventricles are carried outwards, the pressure of the contained blood contributing to this. To this it may at once be objected, with Harvey, that a curved fibre in contracting becomes straighter and is not distended in a circle, and so as the walls of the ventricles of the heart are in a circular position they will be carried inwards towards a straight line, and not outwards. I reply, if both ends of the curved fibre were fixed, it would, when shortened, be brought towards a straight position; but if one of the ends of the fibre be movable, and even is in fact moved, it can, in its contraction, be distended circularly: for, in Plate III., Fig. 9, let *a*, *i*, *b*, be a curved fibre, or the internal wall of the left ventricle of the heart, and *a*, *e*, the septum of the heart. Now, I say, inasmuch as in the contraction of the wall, *a*, *b*, its one end, *b*, that is the cone of the heart, is carried towards *c*, because of the simultaneous contraction of *a*, *e*, the septum of the heart, that wall is, in its contraction, carried outwards to *d*, and forms the line *a*, *d*, *c*, which, although shorter than the line, *a*, *i*, *b*, still rises as a tumour at *d*. The distention of the wall is to no small extent due to the impulse of the blood which, strongly compressed by the contraction of the heart, as it cannot all immediately be driven into the great artery, gives a blow to the wall of the ventricle.

Should any one ask here how the blood contained within the contracted ventricle, *a*, *d*, *c*, is expelled from it (for it is not to be supposed that in the systole of the heart the extreme part of the heart moves quite up to the base of the heart, but that some space remains between them), I answer that it has not yet been established that all the blood leaves the ventricles each time that they contract. And at any rate, if the ventricles of the heart were nearly emptied of blood it would seem that what remains of the blood could not be compressed by them with force sufficient for the projection of the blood to the furthest members of the body. If, however, all the blood is expelled from the ventricles, we must suppose that the walls of the ventricles continue their contraction for some little time after that first rush of the heart by which the pulsation is started. For otherwise it seems impossible that all the blood should be driven by the beat of the heart so instantaneously into the great artery. And it tells in favour of this, that in the section of animals, when the dying heart is opened, the movement of contraction is seen to be completed not at one stroke, but to begin in the septum and be gradually propagated in the walls of the ventricles. No doubt, for first setting the blood in motion there is need of a powerful contraction and jump of the whole heart; while yet a more gentle constriction of the ventricles may suffice for keeping it going.

For the further expulsion of the blood from the heart, its septum and ventricles contract not only as to their length but also as to their breadth; for the fissures or little excavations with which the cavities of the heart are hollowed out, have a position which suits the heart when constricted from all sides, as the eminent Lower has noticed. And it tells in favour of this that the fibres of the heart are attached all round to the orifice of the great artery; whence it comes that in their contraction they pull the sides of that orifice in all directions and open a wide door for the blood bursting out.

Nor should we omit to state that the long diameter of the septum of the heart is not straight, but is gibbous and convex on the right; but in respect of the left ventricle it is concave, or at least plane; as is shown in Plate III., Fig. 9. Now this form of the septum conduces in no small degree to the propulsion of the blood from the left ventricle to the remotest parts of the body; for when the wall of the right ventricle contracts, the blood thus compressed pushes against the convex side of the septum, and the septum meanwhile contracting by its own force, becomes straighter, and still further narrows the space of the left ventricle, as may be seen in Plate III., Fig. 9. So that, in fact, the pressure of the contracting right ventricle also contributes no little to the contraction of the left.

It is further to be noted that as the contracted septum is thus made more nearly plane, the heart as a whole is carried to the left, as may be seen in the figure. For when a motion has been once impressed on the heart it continues for some time, in accordance with the nature of projectiles; and thus it comes about that the heart brought to the left, strikes the left side of the chest forcibly, though the greater thickness of the wall of the left ventricle, its more powerful pulse, not to mention its very shape, so different from that of the right ventricle, contribute in no small degree to this effect. And to these things I may add that the spiral fibres in the walls of the left ventricle are so arranged that, when they contract, the heart is twisted by them, so that the right ventricle is carried backwards towards the spine, and the left somewhat forwards towards the chest, so as to strike it.

From what has so far been said it follows, that if the blood cannot escape with sufficient freedom from the ventricles of the heart, on account of an obstruction of any sort whatever, either of the pulmonary vessels or of the great artery, it cannot but be that, as the heart violently contracts to expel as far as may be the load of blood, the walls of the ventricles are widely distended into a ball, and in consequence the heart is dashed violently against the chest; so that the ribs are sometimes pushed out by its blows. And it may be supposed that palpitation of the heart, especially when long continued, proceeds from this cause; for although in this affection the pulsation of the heart is very violent, still the pulse of the arteries at the wrist is usually quite languid; as has been observed by others as well as by ourselves. Besides, it points in this direction that in the bodies of persons who have died of palpitation of the heart, the blood-vessels of the lungs, and also to some extent the great artery, are sometimes found obstructed and almost closed by the accretion of cartilage in their interior, or by clots of blood, and the ventricles of the heart, or at least one of them, inordinately distended with grumous blood.

In confirmation of what has just been said, I may here submit an observation made not long since by the learned and ingenious Dr Thomas Millington, who has kindly given me an account of it, as follows.

A young man of good position, about twenty years of age, of a delicate and slender habit of body, suffered for some years from palpitation of the heart, which was so violent that the ribs were forced outwards by the heart's impact, and a quite notable tumour was formed on the left side of the chest; and if the hand were placed on it, the heart could be felt violently dashed against the thorax, and one could, so to speak, hold it in the hand. But yet the pulse of the arteries in the wrist was very languid. I can myself testify that this was the case with the said patient.

Moreover this patient became breathless and suffered from violent palpitation and faintness after any brisker movement. And at last, after a long drive in a carriage, he had a more violent attack, with frequent swoons and coldness of the extremities, and died. On opening the body the heart was found tumid, especially its right ventricle, which was larger than usual and turgid with coagulated blood; indeed its muscular wall was very thick and strong. Further, the pulmonary artery and vein were distended with grumous blood, and that vein, where it opens into the left ventricle of the heart, was nearly closed by cartilage adhering to its interior, so that blood could scarcely enter the ventricle. Indeed, there can be no doubt that the obstruction of the pulmonary vein was the cause not only of the palpitation from which the patient suffered, but also of the above-mentioned phenomena. For as the blood could not, on account of the obstruction, pass into the left ventricle of the heart, the pulmonary blood-vessels and also the right ventricle were necessarily distended with blood. So, too, the heart, and especially that ventricle, was forced to contract violently so as to propel the mass of the blood as much as possible through the lungs into the left ventricle of the heart. And this also accounts for the great thickness and strength of the right ventricle, since muscles accustomed to more violent exercise increase more than others. But that the heart should have been dashed against the left side of the chest with such violence that its ribs were bent outwards, although it was the right ventricle of the heart only that was swollen like a tumour, is to be explained by what has been said above ; for, indeed, as the blood could not be all expelled from the right ventricle because of the aforesaid obstruction, it, compressed by the violent contraction of the muscular wall of that ventricle, drove the septum of the heart and so also the heart itself to the left side, as we have shown above.

It is also to be noted that palpitation of the heart, such, namely, as occurs suddenly and unexpectedly, may come from this, that the blood, strongly fermenting and set in violent motion, rushes too quickly into the ventricles of the heart ; so that the heart must needs contract with more than its usual force to expel the greater quantity of blood. For it must not be forgotten that the heart, like other muscles, is liable to convulsive movements, and its palpitation seems sometimes to be referable to these.

As to the asthmatic paroxysm, to which the aforesaid patient was subject after any more violent motion, that seems to have arisen thus, that the blood which, on account of the contraction of the muscles, reached the right ventricle of the heart in a fuller stream, could not freely pass through the lungs and make its way to the left ventricle of the heart. But I would not have this so understood as if I supposed that the blood detained in the lungs so stuffed them up as to prevent the entrance of air into them; for, however the blood-vessels of the lungs are distended with blood or any other liquid, the lungs themselves do not swell up; but all the same they do enlarge when air is blown into them, and quite promptly subside when the air is expelled, as may be tested on the body of any animal by passing blood or any other liquid into the pulmonary vein after ligature of the pulmonary artery, and then inflating the lungs by means of bellows attached to the trachea. And in fact if the blood stagnate in the pulmonary vessels, the patient is affected just as if the respiration were suppressed; for there are two things specially effected by the respiration:—

First, that the mass of the blood should be led through the lungs into the left ventricle of the heart (yet that takes place not so much for the motion of the lungs, as that the blood may ferment with the nitro-aërial particles mixed with it by respiration, and be protected from coagulation, as has been shown above). Wherefore, if the blood for any reason should stagnate in the lungs, as happened in the case mentioned, the patient at once becomes breathless, and suffocation is nearly produced.

The other effect of respiration is that the nitro-aërial particles should, by means of the blood, be carried to the brain in quantity sufficient for the renewal and completion of the animal spirits; and unless that takes place, a failure of the spirits and swooning will soon follow on the suppression of the other respiration—that of the brain.

HOW JUMPING IS PRODUCED

We shall add here as a corollary something as to that motion by which animals lift themselves altogether from the ground and jump. This motion is produced, according to the view of the distinguished Willis, not by the contraction of muscles, but rather by some elastic force. For, indeed, that learned man says in his *Answer to Dr Highmore*—"If there is no attraction except to an immovable part, how can an animal move its whole body and completely lift itself from the ground: surely the motion of the whole follows the motion of the individual motor parts, wherefore if these can be drawn only one towards another and not lift themselves wholly by some elastic power, I confess that I do not understand how and by what further artifices an animal is able to lift itself as a whole and jump hither and thither." But I really cannot conceive what the learned man understands by an elastic force of the organs: there is, however, no reason why we should seek refuge in it, for the motion of animals raising themselves from the ground can be produced quite conveniently by the contraction of the extended muscles, as will be made plain by what follows. For in Plate III., Fig. 6, let *a*, *b*, be a stick, which we shall suppose to be rotated about *a*, as a centre; I say that while it is moving in a circle, all the parts of it, say *c*, *b*, strive as far as in them is to recede from the centres of the circles which they describe, and tend to move in the straight lines, *c*, *e*, and *b*, *e*: for a similar argument holds here as in the case of a stone rotated in a sling, which always strives to get out of the sling and to begin a movement in a straight line, as has long ago been noted by the acute Descartes. But now if we suppose that while the stick is in rotation it be set free from the centre, *a*, when it has come to a horizontal position, it will no longer move in a circle, but ascend straight up. These things being premised, let (in Plate III., Fig. 7), *a*, *e*, be the tibia, *c*, *n*, the thigh-bone, *e*, *b*, *e*, the muscles which extend the tibia; when the animal is in the attitude for jumping, the thigh-bone, *c*, *n*, by the contraction of these muscles will be rotated about the protuberance of the tibia at *e*, just as the aforesaid stick was rotated about the centre. Whence it comes about that the thigh-bone thus circumgyrated will strive to move up; and indeed if it has been driven round with sufficiently brisk motion by these muscles, that effort will attain its end, and the thigh-bone will carry the tibia aloft with it. And the case is quite similar with the muscles extending the foot, the tibia, the thigh, and the back: when they contract strongly, the parts into which they are inserted are driven in a circle and carried upwards, as is seen in Plate III., Fig. 8, which shows the muscles extending the foot, the tibia, the thigh, and the back, with the bones into which they are inserted. The flexor muscles of the arm, the forearm, and the hand have been drawn in the figure because these muscles contribute not a little to the lifting of the body, in so far of course as they, when violently contracted, forcibly raise up the parts annexed to them, as any one can feel for himself when he jumps. But this is to be noted here, that these bones along with the parts attached to them are not carried straight up by the contraction of the said muscles, but that the thighs are carried up and forwards, while on the other hand the back is carried up and backward, as is made clear in the figure; yet so that in the motion of animals moving straight up, the force which carries some parts forwards and that which carries other parts backwards, being in equilibrium, mutually destroy one another, while the other motion by which all the parts are carried upwards is unimpaired; but as the animals jump forwards or jump backwards so must the force by which some parts are directed forwards, or that by which others are directed backwards, prevail.

But that this may be still more clearly made apparent, I say the said extensor muscles raise the animal aloft in the same way as anything is thrown from the hand.

For as to the force impressed upon things projected, it is not to be thought of as anything else than motion alone: when a thing is once set in motion it will continue in it until it is retarded by bodies in the way, or by the gravity inherent in itself; for, since (as has been noted by the acute Descartes), if a thing is at rest we do not believe that it will ever begin to move unless it is impelled to do so by some cause, so there is no reason why we should suppose that if a thing is moving it should intermit its motion of its own accord, and not as hindered by something else. For as inanimate things obviously cannot dispose of themselves, but are altogether subject to the disposition of others, they must continue in the state in which they are until their state is changed by some external cause. As to the impetus with which a stone from a sling or a ball from a gun is projected, we can suppose it to be nothing else than the very rapid motion in which the projectiles were when they emerged from the sling or the gun. But to give an instance of what has been said, let us suppose a pendulum let go at any point of the circle, while it descends to the lowest point of the circle it does not remain there at rest but mounts up to almost the same height on the opposite side of the circle. But what is it that causes this ascent of the pendulum? For as to gravity, it is not its character to cause the ascent but on the contrary the descent of things; and yet in this case the gravity of the pendulum is the cause of its ascent; in so far as the pendulum when it has come to the lowest point of the circle has acquired, on account of the continued impulse of its own gravity, a so much accelerated motion that it can carry itself up: for so much higher as is the point from which the pendulum is let go, so much higher does it ascend on the other side; and that happens only because the pendulum, let fall from a higher point, acquires, at the lowest point of the circle, a more rapid motion and thence also a greater impressed force. Nor is the case otherwise when a ball is discharged from a longer gun; for so it gains a much accelerated motion, and in consequence also a very great impressed force. Obviously the accelerated motion or the impressed force of a stone descending from on high, and of a ball shot out of a long gun, are increased for quite the same reason, because, as the stone descends or the ball passes along the gun, the impetus of gravity, or the impelling force, is renewed every moment.

But to bring what has been said to bear upon the present matter, when an animal is bent, so as to be in a suitable position for jumping (and we note that to begin jumping it is quite necessary that the animal should first bend itself), and then the extensor muscles contract all at once with a sufficiently powerful impulse, it comes to pass that all the parts of the animal are carried upwards, as follows from what has been said above : but in as far as all the parts of the animal, and thus the animal itself, are, by the action of the said muscles, set in motion upwards, these muscles carry the animal on high for the same reason as projectiles are moved in any direction ; inasmuch as the impressed force of projectiles is nothing else than motion determined in a particular direction in which the thing has been made to move by some impelling cause. And so far as to muscular motion.

FIFTH TREATISE

ON RICKETS. TO WHICH IS APPENDED A METHOD OF TREATMENT. SECOND EDITION.

THERE has been only one, as far as I know, who has written anything on the subject of rickets, namely, the distinguished Dr Glisson ; and that may seem strange, because as a rule disease scarcely rages so much as the incurable passion of writing about it. And the very accurate treatise on this disease published a good while ago by Dr Glisson, and the authority of such a man, would have deterred me from writing, but that I knew that he would readily forgive one who reverences the truth although he may to some extent differ from him. Yet, as I hasten with him to the same goal, it cannot but be that I shall for a great part of the way tread in his footsteps : wherefore I shall sometimes, with apologies to so eminent a man, repeat some things he has said before, as this cannot be avoided.

This disease made its appearance some forty years ago in the western parts of England ; and since then (as it is the way of diseases and other evils to spread themselves) has infested infants' cradles through nearly the whole of England, though more rarely in the northern parts.

It is, then, infants that specially suffer from this disease; it is pretty frequent among those from six months to a year and a half old, most frequent between the ages of a year and a half and two years and a half, so that the chief time of attack comprises the two years immediately following the age of six months.

The diagnosis of this disease, as of others, depends on a knowledge of the symptoms, which are the following.

The proportion of parts is irregular, namely: the head larger than normal, the face in better condition, the intelligence very acute for the child's age, the external parts (especially the muscles), slender and thin, the skin lax and flaccid, the bones for the most part bent, and round the joints prominent and nodose, the spine also variously curved, the chest narrow, the sternum indeed acuminated, the ends of the ribs knobbed, the abdomen somewhat tumid and tense: so far for what is to be seen externally.

Internally, the liver is seen to be above the normal size, as are nearly all the parenchymata; the stomach and intestines present a greater mass than in health; the mesenteric glands are larger than normal, even if they are not strumous: so far as to the abdomen.

In the thorax the lungs are observed to be infarcted and tumid, sometimes purulent and strumous, and very often adherent to the pleura. The jugular veins and the carotid arteries are sometimes found larger than normal, but the brain is normal except in proportion and size.

To these has to be added a weakening of nearly all parts of the body, also a certain torpor, and indisposition to work and exercise; for the children can only play sitting, and can scarcely stand on their feet; and at last, as the disease advances, the weak neck can hardly sustain the weight of the head.

Such and so many are the symptoms of this disease; we have next to consider what is the evil which is the fruitful parent of this numerous brood.

And in the first place we assume that the cause of this disease does not consist in the access of vitiated blood from the heart; for in this case the whole mass of the blood would be corrupted, and as this is indiscriminately distributed to all parts, the impure blood would affect all parts equally; but this never happens in this disease. For the head, as also the viscera, except that they are larger than usual, are healthy; indeed the very parenchymata, which chiefly consist of affused blood, inasmuch as they closely resemble those of healthy persons, testify that the blood is sound: for it is absurd to assume some sort of elective attraction in virtue of which the head and the other healthy organs attract what is good in the blood, but send away the bad blood to other parts: for this attraction, if it existed, would be present equally in all parts, as there is the same congruence with good blood and need of it in all parts; and this is what is supposed to produce motion of this sort.

Secondly, we assert that the origin of this affection does not consist in a depraved constitution of the parts themselves, as if parts suffering from intemperate cold and moisture were unfit to receive the blood coming from the heart: for whence arises such excessive moisture and coldness of some parts, when all are similarly and equally irrigated by the spirits of the warming blood? Nor do I think coldness innate in the parts, but in them over and above their nature; nor is it to be believed that the parts themselves resist their own nutrition. For they take no active part in nutrition, but only passively receive the nutriment brought to them: so that I do not believe in any unfitness in the parts, which would lead to their being less nourished, but only that there are obstructions which prevent their reception of the aliment: wherefore the cause of this disease cannot be in the constitution of the parts themselves; nor are we to look on the moisture as the cause of this disease, but rather as its consequence.

When, then, parts do indeed grow cold and are not nourished, although saturated with quite suitable blood, we must certainly conclude that something besides blood alone is required for warmth and nutrition. And whatever this may be, it must necessarily be conveyed by some sort of vessels. The arteries bring the blood, the veins carry it back; and the nerves alone remain as possible carriers of the nutritive juice, or, at all events, spirits. And lest any one should doubt that the nerves carry something required for nutrition, I shall mention an experiment known to every one, namely, that if the nerve serving any part whatever be divided, not only does sensation in that part cease, but also nutrition so that the said part withers up.

But indeed this nervous juice does not alone perform the whole function of nutrition. For besides it, the blood diffused by the arteries has no small share in nutrition. For it is the case that the nervous juice when mixed with the blood produces a certain effervescence and heat, and thus suitable material is precipitated for the nourishment of the parts: and so the blood in this disease, although in itself laudable enough, yet lacking the necessary ferment on account of the defect of this nervous influx, is not able either to excite the required heat in the parts or to perform the function of nutrition.

We need not, then, hesitate to assert that the disease of rickets arises from an unequal distribution of the nervous juice, from the defect of which, or from its superabundance, some parts, defrauded of nutriment, shrink, others, saturated more than enough, grow to an excessive size.

The error causing this inequality does not lie with the flow from the brain, for if this fountain were vitiated no suitable nutriment could come to any part: for indeed the head and the other parts which are supplied by the cerebral nerves enjoy sufficiently good nutriment, though more than enough of it. And yet those parts which have nerves originating in the spinal marrow, being defrauded of nutritive juice, are emaciated. This is a clear proof that a sufficient supply of vital spirits is elaborated in the brain as in the public workshop of the whole body; but that the spinal marrow, as the highway leading from that emporium, is overlaid and obstructed by thick and glutinous humours, so that the access of nervous nutriment is cut off: whence it comes that the nerves arising from the spinal marrow lacking that nutritious juice, bring no supply of it to the languishing parts to which they are distributed. Thus these parts suffer from atrophy and extreme leanness. And it is thus reasonable that we should assign this as the cause of the disease; specially as all the symptoms proper to this disease can be clearly and easily shown to be derivable from this source, as shall be made plain in what follows.

1. It happens in this disease that the head increases in size beyond the just proportion; and this is what

we should expect from our supposition, for the nutritious juice of the brain, which in other cases is in great part sent into the spinal marrow, here, as that road is closed, is all dispensed by the cerebral nerves. Hence, as the head is supplied by these nerves turgid with nutritious juice, it obtains a more liberal supply of nutriment and necessarily grows in a more than proportional degree. Hence, also, the face is better conditioned and the mind more acute than accords with the age, for as the exhaustion of the spirits makes us torpid and languid, so their abundance, congested in the brain, makes us wise and ingenious.

2. The abdominal viscera are wont for the most part to exceed the normal proportion ; and here the same argument holds as in the case of the head, for it has been certainly made out that all those plexuses of nerves devoted to the service of the lower part of the abdomen are for the most part derivatives of the vagus and of the intercostal nerves, which have their origin from the brain ; so that we need not wonder if these viscera grow largely, enjoying as they do a fuller nourishment brought by the said nerves. For although the nerves of an intercostal pair receive branches from the spinal marrow, which can bring no nutritious juice, still that defect is abundantly compensated by the fuller supply from the brain. As to the liver and the other parenchymata, which seem to consist mainly of affused blood, perhaps the nervous juice is not so necessary for their nutrition. But as the muscles of the abdomen which cover these organs derive their nerves from the spinal marrow, and as they cannot bring any nutriment at all from that dried-up fountain, it follows that the viscera within swelling up, press with their mass against these muscles and make them tense, as the

muscles do not grow in the same proportion as the viscera.

3. Further, the mesentery is affected with enlarged glands and strumous tumours. That the cause of this symptom may be better understood, I shall shortly explain the origin of glands. The nervous juice when mixed with blood in due fermentation, passes into nutritive and fleshy substance; but if the nerve, replete and turgid, pours out its juice, which is very like white of egg, into the interstices of the flesh, the matter so poured out does not now produce flesh, for want of the blood required for this, but is coagulated into a glandular body very like itself, as is shown by observation. For when I have been about to make some experiment on a dog, I have wounded a nerve: on this occurring, the dog is painfully racked with convulsions: on dissection after about three months, I found a pretty large glandular concretion at the place where the wound had been inflicted, and this seemed to have its origin from the nervous juice escaping from the puncture of the nerve. This being assumed, we see that when the nervous juice is transferred from the replete brain to the abdomen, by the before-mentioned vagus and intercostal nerves, in such quantity that it cannot be transformed into the substance of the viscera, that juice is deposited in the interstices of the membranes, of which there are many, and there gives rise to numerous strumous swellings. And not in the abdomen only, but wherever there are nerves arising from the brain and turgid with that juice, there strumous tumours may be seen: but these disappear soon after the disease is cured; for the nervous juice which was poured out in such abundance from the brain, by the nerves thence arising, and produced the

strumous swellings, is now for the most part diverted into the spinal marrow, so that the strumæ deprived of nutriment, soon dry up.

4. The bones are always more or less bent in this disease, particularly the bones of the leg and of the fore-arm. The joints also are often inclined outwards; the extremities of the ribs, where they join with the cartilages of the sternum, are nodose; the bones of the joints are protuberant; also the whole spine is bent variously, here outward, here inward. I do not think that this curvature of the bones is owing to their flexibility, because the children afflicted with this disease have rather larger and firmer bones than others, as will later be shown at some length; yet as this curvature of the bones is very notable, I may be allowed to discuss it somewhat more fully, and I shall first give the learned Dr Glisson's opinion, and then shortly state my own.

Dr Glisson says: "Let us compare the bones which are apt to be curved to a pillar; and indeed we may do so quite properly, for if they are erect, they correspond to a sort of pillar, and thence we deduce a demonstration explanatory of this circumstance: let, then, *a*, *b*, *c* (Plate IV., Fig. 1) be a pillar composed of three stones placed one on the top of the other; we shall suppose it such that every side is perpendicular and of the same height; if, then, we insert a wedge on the right side between the stones *a* and *b*, along the line *f*, *d*, the head of the pillar, that is, the highest stone, will be inclined towards *d*, and make an angle at *d*, and the height of the pillar will be greater on the right side than on the left, as is shown in Plate IV., Fig. 2. Similarly, if you push in another wedge along the line *g*, *e*, between the stones *b* and *c*, the pillar will be still further inclined, and there will be an angle at *e*. The pillar will therefore be inclined to the left, as is seen in the same figure. But if the pillar is made of many stones, and a wedge is inserted, as described, between every pair of them, the stones will not have the form of a pillar but of a portion of an arc, as may be seen in Fig. 3.

"To accommodate this to the present question, if the said bones are more fully nourished on one side and therefore grow more on that side than on the opposite, it must necessarily follow that they will on that very account curve, for the fuller nutrition of that side brings about the curvature of the bone in the same way as the wedge driven in brings about the curvature of the pillar, except that while a wedge is driven in at certain points only of the side of the pillar, the fuller nutrition affects the side of the bone nearly equally along its whole length, and it is on account of this uniform nutrition that the inclination of the bone produces an exact segment of a circle without any angles."

According to this hypothesis, viz., that one side is more fully nourished, the learned author by his ingenious comment demonstrates the curvature of the bones. But with all respect for such a man we may ask, whence comes it that one side should have that fuller nutrition when the blood by which the bones are nourished is in this disease, no less than in healthy persons, equally distributed? And if there were any such inequality, a fuller supply of aliment might be supposed in the posterior side of the tibia, as that is less exposed to cold, and so softer; and thus the posterior would be the elongated and convex side of the curved tibia, and the anterior the concave; but the opposite is the case, for in this disease the tibia is prominent in front.

And further, from the very form of the curved bones, we may gather that they grow equally on both sides: for the bones have the form shown in Fig. 4, which represents the tibia, where the concave side, *a*, is as long as the convex, *b*; for if it were otherwise, and the tibia had the form shown in Fig. 5, the thigh-bone, *b*, could not be supported on it without manifest obliquity of the body, as may be seen from the figure.

We must, therefore, look for some other cause of this curvature; and to see our way more clearly, the following points should be noted:—

1*st*. We assert that in this disease the bones are not to be reckoned among the parts affected in respect to nutrition, for these are nourished and grow no less than in healthy persons, as observation shows; for the blood alone suffices for their nutrition, and there does not seem to be any need of the nervous juice as in the nutrition of other parts; for as bones are not supposed to have any sensation of themselves, they must be held to have little or nothing to do with nerves.

2*nd*. We take it for granted that in this disease, the nervous and muscular parts do not grow at all because of the lack of the nervous juice necessary for their nutrition.

These things being premised, let *a* in Plate IV., Fig. 6, be the tibia, *b* the muscles attached to that bone behind and forming the calf. As, then, the tibia, *a*, grows and lengthens, while at the same time it is held, as by a string, by the muscles, which do not grow in the same proportion, it follows necessarily that that bone, strained by the shorter fibres of the muscles, should be bent like a bow. I may illustrate this hypothesis of mine by an example. If a string be tied above and below to a young and growing tree, yet so that the tree is not strained by the string, as is shown in Plate IV., Fig. 7, every one will admit that the said tree will be bent as it grows, as in Plate IV., Fig. 8. And the mathematical proof of this is obvious, for if any line is elongated while its extremities remain fixed, the line will cease to be a straight line, and this is what happens to the bones in this disease.

And this may be further confirmed by the fact that the bent bones always have their concave side turned towards the attached muscle, just as a bow and its string, as may be seen in the case of the tibia, which is prominent and convex in front, but concave on the posterior side which looks towards the muscles; and the same is the case with the other bones—no invalid argument that the bones are bent by the muscles just as the bow by its string.

And this gives us the reason why quacks regularly and successfully apply friction to the concave and not to the convex side of the bones; for by the more plentiful supply of nutritious juice which such friction calls forth, the muscle situated on the concave side of the bone is nourished and grows, so that it is not surprising that when the string is elongated the bone strained and bent by it is also relaxed and becomes straighter. And this is the reason why persons who have recovered from this disease grow very much in height; for the bones not only grow as in other persons, but in their return from curvature to straightness are more elongated.

The spine is also variously bent, partly inwards, partly outwards; and this arises from the various position of the muscles in different parts of the spine, for the spine in its upper part is curved inwards by muscles attached to it externally, but in the lower part it is curved outwards by the powerful *Psoas* muscles attached internally, as is shown in Plate IV., Fig. 9, in which *a, a* is the spine, *b* the muscles attached externally and bending the spine inwards in its upper part, *c* the internal *Psoas* muscles bending it outwards.

And I think that this cause of curvature is not confined to this disease, but acts also in other cases; for if in tender age a muscle should be emaciated because of any defect of nutrition, it must follow that it will bend the bone to which it is attached.

The thigh-bone and the humerus, where the muscles pull equally, being attached on all sides, are rarely bent to any side, being held in equilibrium: while yet, as they cannot extend in length they must do so in thickness, and even sometimes develop nodes.

5. It occurs, besides, in this disease that the chest is narrow and sharp: and this symptom can also easily be illustrated on our hypothesis; for the ribs cannot expand their arches unless the intercostal muscles are also increased, as can be seen in Plate IV., Fig. 10, in which the portions of the ribs, *a*, *a*, *a*, *a*, cannot be elongated unless the intercostal muscles attached to them are similarly extended. But we have assumed that the said muscles, inasmuch as they are served by nerves derived from the spinal marrow, cannot from defect of aliment be elongated, so that the ribs cannot expand further and therefore neither can the chest, but the ribs are nourished, and yet, impeded by the said muscles, cannot grow in length; it follows, therefore, that they must develop nodes, as they do. But this kind of growth does not correspond to the abundance of nourishment, wherefore the anterior extremities of the ribs are elongated into

points, because this is the only mode of growth left free to them, as is shown in Fig. 11, in which *a*, *a* are ribs, the extremities of which, *b*, *b*, grow outwards in points, for they cannot be bent in, as this would be still more opposed to their natural position.

To this kind of narrowness of the chest the muscles of the abdomen also contribute in no small degree, because, being, as we have said, extenuated and tense, they thus draw down the lower ribs to which they are attached, and so narrow the chest.

By quite a similar line of reasoning we could illustrate the disorders of the other bones; for instance, the bones of the articulation in the wrist and in the ankle, which, on account of their shortness, cannot be bent, form protuberant nodes. But what has been said may suffice as to the curvature of the bones.

6. The above-mentioned narrowness of the chest would lead us to expect that the lungs, not having space for expansion, should be stuffed up with grumous blood and swelled, as indeed they are, hence they sometimes become purulent and the pleuræ are often adherent; hence also the patients suffer from asthma and difficulty of breathing.

7. As to the very great bodily weakness and incapacity for any movement in this disease, while the emaciation of the muscles contributes something to this symptom, yet this alone does not seem sufficient, for the weakness is out of proportion to the emaciation of the muscles, for the patients cannot stand on their feet, nor, as the disease progresses, sustain the weight of their head. So that we must look for some other cause of such weakness, and this can be nothing else than deficiency of animal spirits, necessarily consequent on the above-mentioned obstructions of the nerves. For animal spirits are

absolutely required not only for nutrition but also for motion.

And so, in fine, we have deduced the symptoms of this disease from obstruction of the spinal marrow, as its source.

And here it may be asked how it comes about that older persons are never attacked by this disease, seeing that they, as well as infants, suffer from obstruction of nerves, as in paralysis and other diseases of that kind. I answer that although perhaps children are chiefly liable to this disease, yet adults sometimes suffer from the same disorder under another name. Yet that the above-described symptoms for the most part never appear in older patients, arises from a difference not of disease but of age. For as the great size of the head, the curvature of the bones, and some other of the symptoms are produced by the abnormal increase in size of parts, it is altogether impossible that adults, already at the limit of growth, *i.e.*, unable to grow any more at all, should grow abnormally: so that in adults suffering from this disease the head does not, as in children, increase beyond its proper size, because the head has already attained the limit of growth which the very laws of nature forbid it to exceed. But while the parts cannot increase abnormally in adults, still the disease does the one thing that in their case it can, by emaciating them.

As to the prognosis, this disease is usually not lethal in itself; yet sometimes by the aggravation of the symptoms it degenerates into Phthisis, Tabes, Hectic Fever, Dropsy of the Lungs or Ascites, and so at last is fatal to the patient. But the prognosis can be more easily established by the following rules:—

1. This disorder is most dangerous and often fatal

if it comes on before birth or immediately after birth.

2. The sooner after birth this disease comes on, so much the more dangerous is it.

3. The more the symptoms increase in severity, viz., if the disproportion of parts and the emaciation are extreme, so much the more difficult is the cure.

4. If this disorder has the above-named diseases conjoined with it, it hardly ever terminates in recovery.

5. If the patients are not cured before the fifth year, they will be invalids during their whole life.

6. Scabies or itching occurring in the course of this disease contributes much to its cure.

7. We need have no doubt of the recovery of those in whom the symptoms of the disease do not increase but rather diminish.

Method of Treatment

Now that we have discussed the cause of this disease and its prognosis, it remains for us to consider its prevention and cure.

In so far, then, as the cause of this disorder consists in obstruction of the spinal marrow, and weakness of the nerves thence arising, the chief indications both of prevention and cure are that the nerves be strengthened and the obstructions prevented or removed. For this end, cathartic drugs, blood-letting, also digestives, diuretics, diaphoretics, and specifics will be called into use; formulæ for these and their mode of use will be given below.

As to the cure of the disease, I think it should be begun by catharsis; which is more suitable in this disorder, because phlegmatic humours are often collected in great quantity in the lower part of the belly, and the abdominal viscera are frequently affected with strumous tumours. Catharsis may be set up by means of enemata, emetics, or mild purgatives.

The Use of Enemata, and some Examples

If the bowels are constipated, or the intestines infested with wind and colic spasms, enemata may often be employed. These are composed, not only of solvent, but sometimes also of alterative and strengthening drugs. I subjoin some formulæ of such :

℞ *Fol. Malv. m. j. Flor. Melilot. Chamæm. Sambuci ana P. j. Sem. Anis. Fœnic. contus. ana ʒss. Coq. in lactis vaccin. rec. s. q. Colaturæ ℥iiij. v. vel vj. adde sacch. rubr. syr. Viol. vel Rosat. ana ℥j. M. F. Enema Injic. tepidè longe à pastu.*

℞ *Rad. Alth. Contus. ℥ss. Fol. Malv. Parietar. ana m. ss. flor. Chamæm. Sambuci ana P. j. sem. Carminat. ʒij. Coq. in s. q. seri lactis cerevisiati, Colaturæ ℥v. vel vj. adde Elect. Lenitivi, vel Diacassiæ ℥ss. Butyr. rec. ʒvj. M. F. Enema. Injic. tepidè.*

Strengthening enemata may be compounded as follows :—

℞ *Fimi equi non castrati recent. ℥j.ss. flor. Rorismar. Salv. ana P. j. baccar. Junip. ʒij. sem. Anis. Fœnic. ana ʒss. digerantur calide, & clause cum seri lactis cerevis. s. q. in Colaturæ ℥iiij. v. vel vj. solve sacch. rubr. ℥j. Butyr. rec. ʒvj. M. F. Enema, addi insuper potest, si visum est, Mannæ Calabr. ʒvj. Vel*

℞ *Millep. lotor. n.* 20 *vel* 30, *quibus contusis affunde seri lactis ex vino albo parati ℥iiij. vel v. in expressione solve sacch. rubri ℥j. Terebinth. Venet. in vitello ovi unius solut. ʒj. vel ij. M. F. Enema tepide injiciendum.*

The Use of Emetics, and some Formulæ

If the stomach is loaded with vicious humours, and these tend upwards, emetics should be exhibited; yet in preparing them, account must be taken of the tender age. The vomits should consist rather of vitriolic salt and wine of squills than of antimonial drugs, because it is not altogether safe to administer these to infants, on account of the risk of convulsions; although sometimes even antimonial emetics may be of use.

℞ *Vini, vel Oxymel. Scillit. ab ℥ss. ad ℥j. quo sumpto, post horæ ss. serum lactis Cerevis. in magnâ copiâ superbibatur, dein digito, vel penna gutturi immissis, vomitus provocetur, & aliquoties repetatur.* Vel

℞ *Oxymel. Scillit. ab. ℥ss. ad ℥j. si vomitus non succedat, post horæ ss. sal vitrioli ℈ss. vel gr. xv. in haustu seri lactis cerevis. exhibeatur.*

If the patient is strong enough, more powerful emetics may be used. Such as:

℞ *Infus. Croci Metallor. per subsidentiam optime depuratæ à ʒj. ad ʒij. pro ratione ætatis, viriumque: Oxymel Scillit. ʒiij. vel ℥ss. Aq. Jugland. simpl. vel Centaur. Minor. ʒvj. m. f. vomitor.*

Some Examples of Cathartics

Some days after the vomit, or even if there has been no vomiting, a mild purgation is to be set up and repeated at intervals. As:

℞ *Syrup. Augustan. vel e Cichor cum Rhabarb. à ʒvj. ad ℥j. Cremor. Tartar. gr. x. vel. xv. misce, capiat primo mane, aut per se, aut in haustu seri lactis cerevisiati.*

℞ *Mann. Calabr. ab ℥ss. ad ℥j. Tartari Vitriol. à gr. v. ad gr. x. misce, sumatur mane in jusculo, aut sero lactis cerevisiati.*

℞ *Rad. Polypod. querc. Lapathi acut. ana ʒvj. Cort. rad. Sambuci, Ebuli ana ℥ss. rad. Osmundæ regalis, Filicis mar. Cichor. ana ℥ss. herb. Agrimon. Hepat. Veronic. Ling. Cervin. Asplenii ana m. ss. Coquant. in aq. fontan. lib. iii. ad tertiæ partis absumptionem. Liquor Coletur in matracium,* *cui imponantur fol. Sennæ ℥ij. Rhabarb. ℥j. Epithymi, Santali Citrin. ana ʒij. sem. Anis. Fœnic. ana ʒj. Sal. Absynth. ʒi.ss. f. infusio calida, & clausa per horas* 12. *Colaturæ per subsidentiam depuratæ, adde sacchari æqualem quantitatem, & solâ sacchari dissolutione, aut leni ebullitione f. s. a. syrupus. Dosis Cochlear. j. ad iij. vel per se, aut in liquore appropriato.*

Vel Infusioni superiori purganti adde Cassiæ, & Tamarindor. cum parte infusionis ejusdem extract. Mannæ Colatæ, sacchar opt. ana ℥j. ss. Evaporent leni calore ad consistentiam electuarii. Dosis qu. nucis Jugland. plus minus pro ratione operationis.

℞ *Specier. Hier. picr. simpl. ʒj. Rhabarb. opt. pulv. ʒss. Tartar. Vitriol. ℈j. Gum. Ammon. in aceto solut. gr. xv. cum s. q. Elixir proprietat. Paracel. f. Mass. Pil. cujus ℈ss. ad ℈j. in exiguas pilulas formetur, & exhibeatur horâ somni.*

Bochetum ex Rhabarb. & sant. citr. in aquis destillatis idoneis fact. ex usu esse potest.

If the patient is affected with worms or strumous swellings, or if there is a suspicion of Lues Venerea, the following bolus may be given at intervals :—

℞ *Mercur. dulc. à gr.* 6. *ad x. Resin. Jalap. vel Scammon. gr. ij. ad iiij. ol. Junip. Chym. gutt. j. F. Pulv. qui cum pulpæ pomi coct. aut conserv. Viol. ʒj. in bolum redigatur. Capiat primo mane, vel*

℞ *Mercur. dulc. gr. vj. ad x. Conserv. flor. Cichor. ʒss. m. f. Bolus. Detur primo mane, superbibendo mox syrupi, vel infusionis purgantis dosin idoneam.*

Surgical Remedies

After purgation has been gently set up, if the patient is of a sanguineous temperament, blood-letting has its place. Our empirics are used to abstract blood in small quantities by a scarification made in the concha of the ear ; they perform this operation rather with a blunt knife than with a sharp scalpel, and repeat it two or three times at intervals of about seven days. Although practical men make much of this kind of scarification, I do not know whether leeches would not do as well, or even better. For I do not think that the leeches by their sucking would cause any increased flow of blood to the head. For whatever blood comes by their suction to the part to which they are applied is removed by the suction itself; and as to the increased flow of blood caused by the depletion of the blood-vessels, that occurs in venesection as well.

Further, in this disorder issues are of special use, particularly an issue between the first and second cervical vertebræ, for in that position, close to the origin of the disease, it has the greater efficacy. And the usefulness of issues consists in this, that they largely contribute to the evacuation of the superfluous serosity of the brain, and so to the diminution of its abnormal size, and also to the drying up of the excessive humidity of the spinal marrow, and consequently to the strengthening of the nerves thence arising. A seton can supply the place of an issue.

As to blisters, there can be no doubt that, placed over the vertebræ of the neck or behind the ears, they will bring relief; but their action is suddenly put forth, and their frequent use seems to be too troublesome and painful to the children.

Further, cupping without scarification, applied along the spine, seems to be of no small importance for correcting the humid and frigid condition of the spinal marrow and for removing the torpor of the nerves. Indeed, I think cupping, even with slight scarification, should sometimes be used close to the upper cervical vertebræ.

Alterative Specifics

Besides cathartics and surgical remedies, alterative specifics may be employed, and to these diaphoretics and diuretics should at times be added. I shall give some examples of these.

The specific remedies which have been found specially fit for combating this disorder are either simple or composite. Among the simple the following have proved themselves most useful :—*Lignum Guaiacum, & ejus cortex. Sassafras. lign. Lentiscinum. Rosmarin. Partes nodosæ lign. Abietini. rad. Chinæ. Sarsaparil. Tria Santala; rad. Osmundæ regalis, seu potius spicæ rad. ejusdem; rad. Filicis maris, vel potius gemmæ vix dum è terrâ erumpentes. Rad. Graminis; Asparagi. Eryng. Cichor. Bardan. Cort. rad. Capparum; Herbæ Capillares, imprimis Trichomanes; Ceterach. Ruta muraria; Ling. Cervin. Hepatica. Veronica mas. Agrimonia; Beccabung. Nasturt. aquat; Fol. & Flor. Salviæ, Rorismar. Betonicæ, Lamiæ, Tamarisci. Item Chalybis præpar. ut ejus sal, aut Vitriolum. Tartarum; Castoreum; Flores sulphuris; Vermes Terrestres; Millepedes præparati, & similia.*

These may be compounded as follows :—

℞ *Spicar. rad. Osmund. regal. vel rad. Filic. mar. aut Gemmarum rad. ejusdem vix dum è terra erumpentium m. j. Coquantur in lactis, vel aq. font. lib. i. ad tertiæ partis consumpt. Colatura saccharo edulcoretur, & sumatur bis, aut ter in die.*

℞ *Fol. Teæ ʒj. flor. salv. Beton. ana ʒss. quibus vasi idoneo impositis, superfundatur aqua, quæ aliquandiu ferbuerit, lib. j. infundantur clause, & tepide per horam circiter unam, colatura saccharo edulcoretur, & bibatur ut supra.*

Bochetum, è Rad. Chin. Sarsapar, & Sassafras aq. fontan. incoct. ex usu esse potest.

℞ *Spicar. rad. Osmund. regal. rad. Bardan. Gramin. Cichor. ana ℥j. Herb. Veronic. mar. Agrimon. ling. Cervin. Hepat. Capil. vener. ana m. ss. ras. C. C. Ebor. ana ℥ss.*

Passul. exacin. ℥*j. coquantur in aq. fontan. lib. iiij. ad tertiæ partis consump. adde vini alb. vel Rhenani lib. ss. & statim coletur in vas idoneum, cui imponantur, Fol. Beccabung. Nasturt. summit. Abietis ana m. ss. Baccar. junip.* ℥*ss. F. infusio tepida, & clausa per horas circiter duas. Colatura servetur vitris occlusis, & edulcoretur pro libitu. Dos.* ℥*ij. vel* ℥*iij. horis medicis.*

℞ *Lign. Lentiscin. Rosmarin. rad. Sarsaparil. Osmund. regal. vel Filic. mar. ana* ℥*iij. Herb. Agrimon. Capil. Ven. Veronic. Ling. Cervin. Salv. Beton. ana. m. ij. summitat. Abietis, Tamarisc. ana. m. j. coquantur in cerevisiæ non lupulatæ cong. iiij. ad unius consumptionem, colatura fermentetur, & in doliolum reponatur, in quo suspendatur sacculus ex telâ rarâ confectus, & sequentibus repletus, viz. Milleped. in vino alb. lotor. & leviter contus. n. 200. Baccar. Junip.* ℥*ij. Nuc. Myrist. incis. n. ij. unâ cum frustulo chalybis ad sacculum submergendum. Si scorbuti suspicio sit, vasi insuper imponi possunt, Fol. Beccabung. Nasturt. aquat. ana m. ij. maneant per septimanas circiter duas, dein liquor sumatur pro potu ordinario.*

℞ *Conserv. rad. Cichor, Flor Beton. ana* ℥*j. Conserv. Anthos, Flor. Tamarisc. Flaved. Limon. ana* ℥*ss. Myrobal. condit. n. ij. Pulv. è Chelis Cancror. compos.* ʒ*j. Cremor. Tartari, Flor. Sal. Armoniaci ana* ʒ*ss. santali Citrin.* ℈*j. cum syrup. de Coral. s. q. f. Elect. capiat qu. nuc. Moschat. mane, & horâ quintâ pom. superbibendo liquoris appropriati haustulum.*

℞ *Pulv. Milleped. præpar.* ʒ*ij. Nuc. Myrist.* ʒ*ss. flor. sal. Armoniaci* ℈*ij. f. Pulvis. Dos. gr. viij. ad xv. in Apozemate, Jusculo, aut liquore quovis convenienti.*

Pulvis iste cum Balsami Capiv. q. s. in mas. Pil. redigi potest, quæ in pilulas exiguas formetur. Dos. circiter ℈*ss.*

℞ *Rad. Osmund. regal. vel Filic. mar.* ʒ*iss. rad. Pæon. Mar.* ʒ*j. lign. sassafras. santali citrin. sem. Nasturt. aquat. ana* ℈*j. flavedinis Aurant. Condit,* ʒ*ij. f. Pulvis. Dos.* ℈*ss. ad* ℈*j. ut supra. E pulvere præscripto cum sacchari albi in aq. Cerasor. nigr. solut. & ad Tabulat. cocti septuplo, Tabulæ formari possunt, quæ singulæ pendant* ʒ*j.*

Capiat ℨss. ad ℨj. bis in die superbibendo liquorem appropriatum.

℞ *Rad. Ari, Pæon. Mar. Osmund. regal. vel Filic. mar. ana ℥iiij. fol. salv. Beton. Rorismar. Nasturt. aquat. Beccabung. Veron. Mar. Hepat. summit. Abietis ana m. iij. jugland. virid. lib. ss. Milleped. lotor. ℥iij. Lumbric. mundat. lib. j. Castor. opt. ℨj. Incis. & contus. affunde seri lactis ex vino alb. parat. lib. vj. destillentur Communi destillatorio, liquor totus misceatur. Dos. ℥j. ad ℥ij. bis in die post dosin medicamenti solidi.*

Among the remedies which have been tried in this disease, the most celebrated is that devised by Boyle, the name of it is *Ens Veneris.* It is composed of sal armoniac and edulcorated colcothar, two or three times sublimated together; the dose is from three to six grains taken in a suitable liquid at bed time. I suppose that the efficiency of this drug chiefly depends on the sal armoniac, as that is specially suitable, on account of the extreme tenuity of its parts, for removing the obstructions that produce this disease. And it is likely that the flowers of sal armoniac will in this disorder be as effectual, or even more; for while the sal armoniac is sublimed along with colcothar, a narcotic sulphur from the colcothar, of an earthy or cupreous nature, sublimes with the sal armoniac. Now this sort of sulphur does not seem very suitable for this disease.

An artificial salt, of an ammoniacal nature, and of great virtue, can be prepared as follows:—

℞ *Sal. volatile C. C. Sanguinis, aut Urinæ s. q. quibus phialæ oblongæ immissis, sp. sal. rectificat, aut etiam sulph. per Campan. rectific. guttatim affundatur, quousque ebullitio non amplius excitabitur: sal hic resolutus, per filtrum trajiciatur, & leni calore ad siccitatem salis abstrahatur. Dos. gr. iij. ad vj. primo mane, aut horâ somni in liquore appropriato.*

Those medicines that consist of purely saline sal volatile are of marked efficacy in this disease : among such are to be reckoned the spirits of blood, of hartshorn, of sal armoniac, and the like, especially these spirits impregnated with amber or castoreum.

Elixir propriet. with tincture of salt of tartar, even when prepared in the common way, is sometimes used, not only because it is best for the digestion, but also because it is suitable for killing worms and for preventing corruption of the humours, and for gently moving the bowels. The dose is six to ten grains in two spoonfuls of a suitable liquid.

If the lungs are obstructed (as often is the case) with viscid humours, and the mesentery affected with strumous glands, balsam of sulphur may be exhibited. Of this, three or four drops may be taken in a suitable liquid, or in syrup.

Sometimes also the use of steel is appropriate, as it is signally endowed with aperient virtue, and not only assists the digestion but also confirms and strengthens the tone of the viscera ; but it must be used with caution, for steel is altogether to be avoided in coughs, pleurisy, obstruction of the lungs, hectic fever, and other diseases of that kind.

Diaphoretics are often added to the above-mentioned medicines, such as decoction of guaiacum and others of that sort, which are to be taken in bed and perspiration promoted as far as the strength will allow.

We may here also refer to the use of the bath, whether natural, such as the warm springs of Bath, which have proved themselves of much use in this disorder in provoking perspiration and in strengthening the nerves ; and indeed I have found by frequent observation that the use of these warm springs contributes much to the reduction of the abdominal swelling which is so marked a feature in this disease. Artificial baths prepared with aqueous decoctions of cephalic herbs and tartar or nitre may also be used.

Subsequent fomentation has been found valuable. Let the patient be placed in a sufficiently large tub, and be surrounded with tepid malt which has been for a short time infused in boiling water (as in the process of making beer), and let him remain there almost covered till perspiration is brought on.

Of the Symptoms

The symptoms supervening in this disease have to be considered. Of these, the most common is diarrhœa, for the cure of which mild cathartics, such as infusion of rhubarb, tamarinds, and sandal-wood, or a bolus compound of these, are specially useful. Sometimes recourse must be had to astringents and bland opiates, of which formulæ are to be found here and there in books, but these are to be preceded by purgation and sometimes also by a vomit.

In addition, immoderate sweating is apt to occur in this disorder. If this comes on in a febrile paroxysm it may be critical, but it must not be rashly suppressed. But if it be inordinate and exceeding what might be expected from the cause, it is an indication that the body is loaded with cacochymical humours, and accordingly sweating of that sort is to be corrected by mild purgation repeated at intervals; the purgation is to be brought about chiefly by rhubarb; sometimes a vomit may be joined with it, nor should aperients and digestives be omitted.

Difficult dentition often occurs in this disease, and not unfrequently brings on fever. When this is the case, a gentle evacuation, best by clysters, should be produced: although sometimes purgation and also vomiting (which when gently brought on has proved very serviceable) may be used. If the tooth is about to pierce the gum, nurses are used to rub it with a stick of polished coral: instead of that, the root of *Althea* or of *Lapathus acutus* may be used. It is even sometimes right to make a way for the coming tooth by means of an incision. Also epispastic plasters behind the ears bring relief. Yet if pain and sleeplessness call for hypnotics, syrup of poppies may be given in a dose of one to two drachms.

Besides internal medicines and surgical care, external appliances will also be employed. Among these are to be reckoned exercises of all kinds. If the strength permits it, walking is much to be used. But often the children play sitting, and are exercised by being carried on the nurses' arms and by being rocked in the cradle. For exercise promotes the flow of blood and of animal spirits also to the muscular parts, and thus heat is produced in these enfeebled parts, as the mass of the blood is forced into swifter motion by the contraction of the muscles, and is impregnated with fermentative particles in the lungs by the respiration made more intense by the exercise; while on the other hand the blood becomes thick by constant repose and is made fitter for producing obstructions.

Further, rubbings are of no small moment in curing this disease: they may be made with warm woollen cloths. The parts to be rubbed are the spine, which, as has been shown above, is primarily at fault; also the muscular parts, yet with this caution, that there is to be no rubbing where the bones are prominent, but the concave parts of the bones are to be more liberally rubbed: the reason for this has been given above.

Of similar service is handling of the hypochondria, pressing the viscera now upwards, now downwards, and sometimes introducing the points of the fingers below the false ribs; for in this way care is taken that the liver and the viscera do not suffer from any unnatural adhesion to the peritoneum or elsewhere, as not unfrequently happens in this disorder, on account of the tenseness of the hypochondria.

Bandages are, besides, of service, placed on the legs above the knee, and on the arms above the forearm, but they should be loose and soft, so as not to interfere with the growth of the part to which they are applied; indeed the usefulness of bandages consists in this, that they tend to divert the flow of blood from the head and to conduct it to the emaciated parts.

Reference should also be made of the use of splints applied to some parts, as also of leggings, which are of no small use, not only for strengthening the parts, but also for diminishing the curvature of the bones and the flexure of the joints. In their use, care should be taken that they press a little on the protuberant part of the bone but scarcely touch the hollow.

For keeping the trunk of the body erect, stays may be made, sewed in double cloth with numerous pieces of whalebone placed between; they are to be so adapted to the child's body that the spine is held erect and the prominent bones repressed.

Nor should we forget the artificial suspension of the body by means of a pendulous instrument, so formed of belts that the chest is embraced by it below the armpits, that the head is surrounded by another belt coming below the chin, and the hands held by a pair of handles, so that the weight of the body is sustained partly by the hands, partly by the head, partly by the armpits of the child.

In conclusion, I add some remarks as to external applications.

Fomentation with any kind of wine, or even with common aqua vitæ, does much to strengthen the nervous parts, and may be applied to the weak parts and especially to the spine : when that has been done, these parts should be rubbed with oil or a suitable ointment—as to these later. The following decoction may be used instead of wine :—

℞ *Rad. Osmund. regal. aut Filic. Mar. ana ℥iij. Fol. Beton. Salv. Rorismar. majoran. Nasturt. aquat. ana m. j. flor. Chamæmel. Melilot. Sambuci ana P. j. Baccar. Lauri, Junip. ana ℥ss. Coq. in aq. font. s. q. ad lib. ij. adde vini albi, vel aquæ vitæ vulg. lib. j. Colatura usui servetur.*

℞ *Fol. Sambuci, Lauri, Majoran. salv. Rorismarin. Beton. summitat. Lavendulæ ana m. ij. Baccar. junip. lauri ana ℥j. incisa & contusa vasi idoneo indantur cum butyri Maial. vel butyr. recent. non saliti, lib. iij. & aquæ vitæ lib. ss. Coqu. lentè ad aq. vitæ consumpt. Expressioni adhuc calidæ adde ol. nuc. Myrist. per expressionem ℥ss. Balsam. Peruv. ʒj. misce F. unguentum, loco butyri maialis substitui potest medullæ Bovinæ, vel sevi Cervini, & Ol. Lumbricor. vel Vulpin. ana lib. i. ss.*

Unguenta tepide coram igne luculento applicentur, & calidâ manu ad siccitatem usque affricentur: quô autem magis penetrent, liquoris appropriati aliquantulum iisdem tempore usus admisceatur.

If the abdomen be tense and tumid, the following ointment may be applied :—

℞ *Ol. de Cappar. Absinth. Sambuc. ana ℥j. ung. de succis aperit. vel unguent. supra præscript. ℥j. ss. Gum. Ammoniaci in aceto solut. ℥ss. F. linimentum : cui tempore usus portiuncula liquoris sequentis admisceri potest.*

℞ *Rad. Bryon. alb. ℥j. fol. Absinth. Centaur. Salviæ ana m. j. Flor. Sambuci. Melilot, ana P. j. Baccar. Lauri,* *Juniper. ana ℥ij. Coqu. in aq. font. lib. iij. ad dimidias. adde vini Rhenani lib. ss. Colatura usui servetur.*

While the ointments are being applied to the hypochondria, the viscera should be manipulated by the nurse's hand, as directed above.

Plasters may also be used here. As:

℞ *Emplastr. de Melilot. compos. s. q. extendatur super alutam & Hypochondris applicetur. Si tumor juxta regionem Hepatis fuerit, adde Emplastri prædicti ℥j. Santal. Citr. pulv. ʒj. ol. de Absinth. & ceræ s. q. pro emplastro conficiendo. Vel*

℞ *Succ. Beccabung. Nasturt. aquat. Sambuci, Absinth. ana ℥j. Succi depurati lento calore ad consistentiam extracti redigantur, quibus adde Gum. Ammoniaci in vino solut, & ad spissitudinem cocti ℥ij. Terebinth. Venet. ℥j. sant. Citrin. pulv. ʒij. ol. de Cappar. & Ceræ s. q. f. Empl. applicand. ut suprâ.*

If the lungs are affected, the chest should be rubbed with pectoral ointment or with ointment of dialthæa, or with both mixed, to which at the time of application there should be mixed a little expressed oil of nutmeg. Or

℞ *Glycyrrhizæ rec. ℥iij. Butyr. rec. non salit. lib. j. Contundantur simul in mortario lapideo, & macerentur calore Balnei per horas* 4, *dein exprimantur; idemq; labor ter iteretur cum pari quantitate novæ Glycyrrhizæ. Unguentum s. a. depuratum, usui servetur. Cui tempore usus unguent. Pector. æqu. quant. cum ol. nuc. Myrist. per express. parum admisceatur.*

As this disease arises from obstruction and weakness of the spinal marrow, fomentations and strengthening ointments such as have just been prescribed should be applied to it, and to these a small quantity of balsam of tolu may be mixed. Plasters are also of use, such as nervine plaster or plaster of betony, or also the following:—

℞ *Unguent. primò præscript.* ℥*ij*. *Gum. Ammon. in vino solut. Pic. Burgund. ana* ℥*ss. Mastich. Oliban. Carann. ana* ʒ*ij. Castor.* ʒ*ss. Lumbricor. præpar.* ʒ*iss. Sal. Armoniac.* ʒ*ij. Ceræ. s. q. pro emplastro conficiendo.* A sufficient portion of this is to be spread on leather, the shape of which may be varied; for as the upper or the lower parts are weak the plaster is to be applied to the upper or to the lower parts of the spine, sometimes, indeed, to its whole length. So far of the method of treatment.

Tab 1

Fig 1

Fig 2

Fig 3

Fig 4

Fig 5

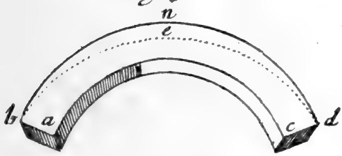


Fig 6

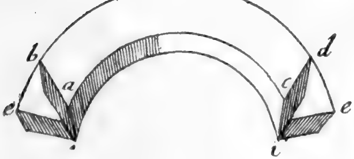
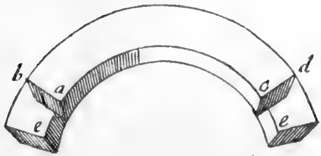


Fig 7



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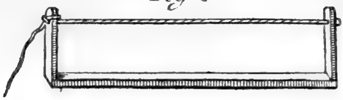

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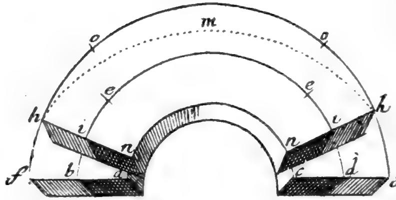

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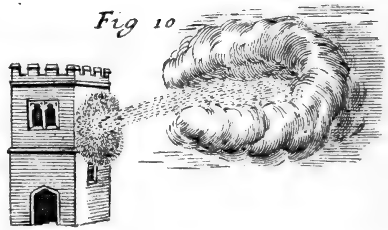

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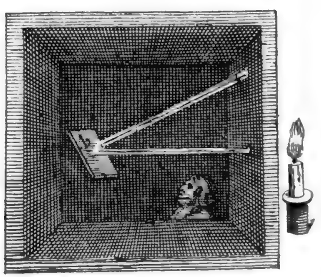

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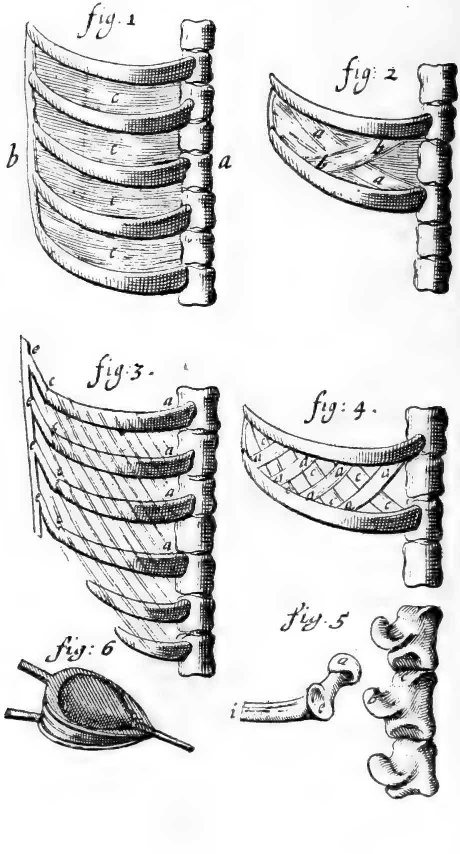


Tab. 3

Fig. 1

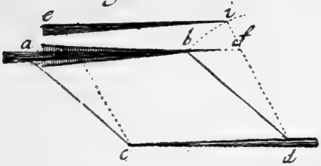


Fig 2

Fig 3

Fig 4

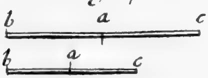
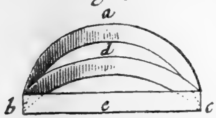


Fig 5



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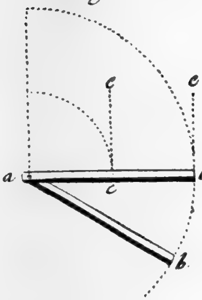

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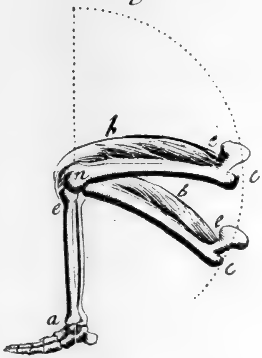

Fig 8

Fig 9

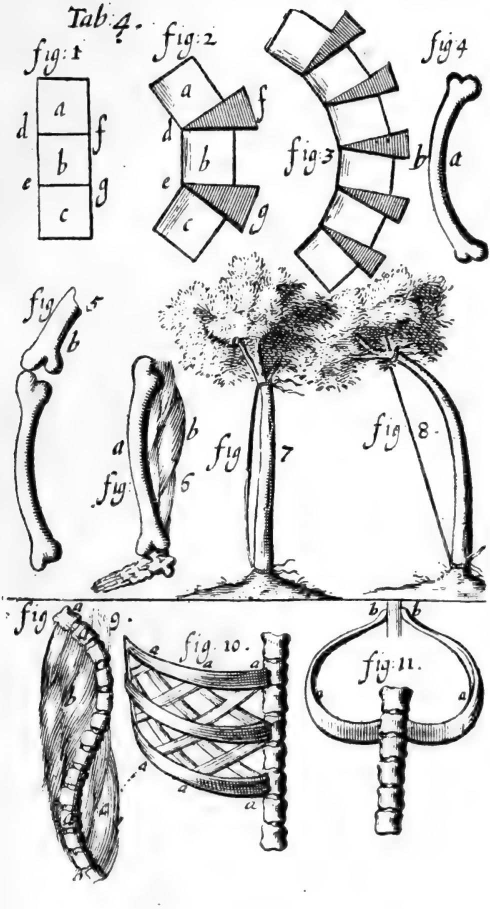
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fig 5
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fig 8.
fig 9.
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fig. 10.
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fig. 11.
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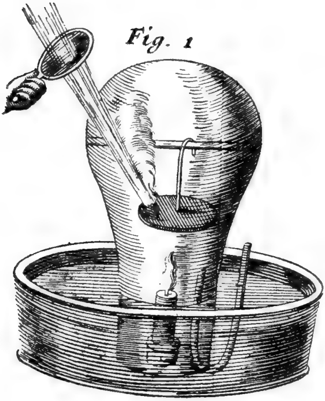
Tab. 5

Fig. 1



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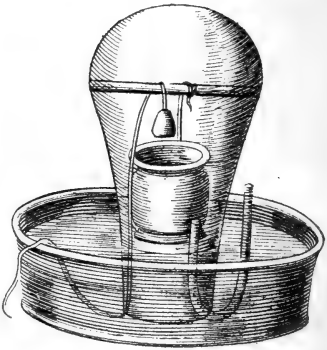

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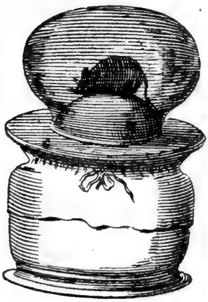


Fig. 5

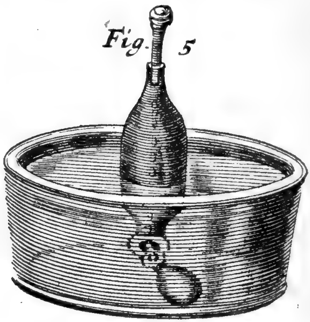


Fig. 3

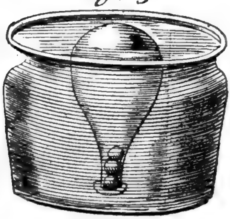
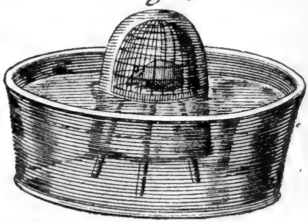


Fig. 6



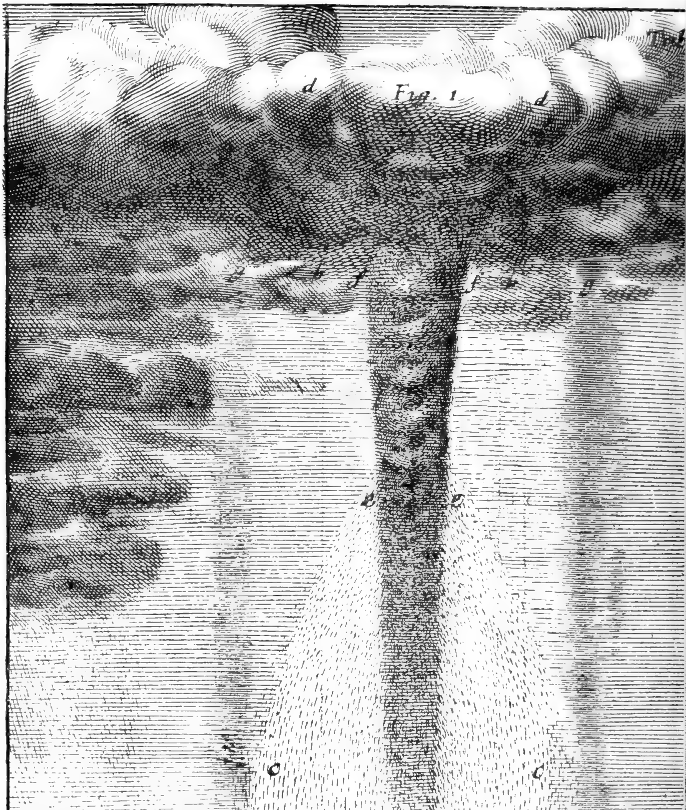


Fig. 1
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g
g
e
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a
a
b

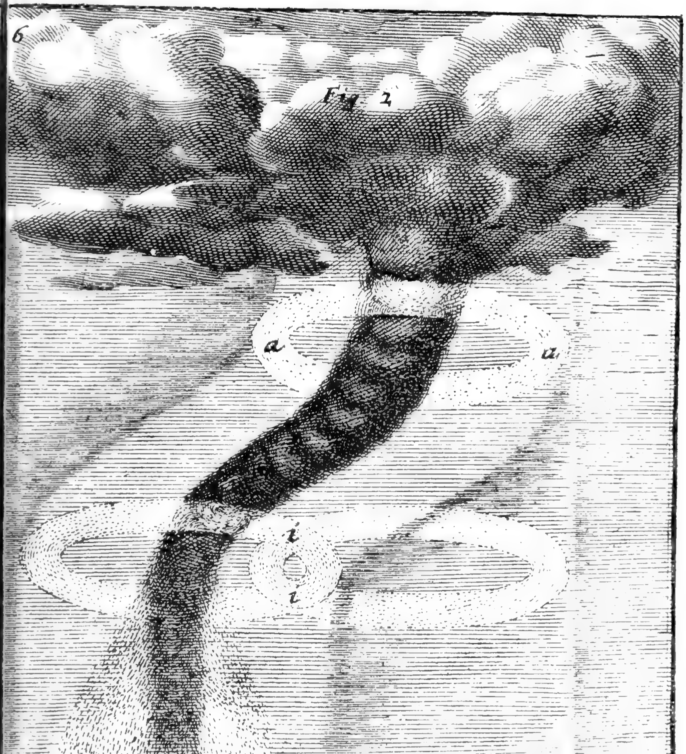
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Fig. 2
a
a
i
i
d
b