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LECTURES

ON THE

PHYSICAL PHENOMENA

OF

LIVING BEINGS.

BY CARLO MATTEUCCI,

PROFESSOR IN THE UNIVERSITY OF PISA.

TRANSLATED UNDER THE SUPERINTENDENCE OF

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SOCIETY.

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TO

MICHAEL FARADAY, D.C.L. F.R.S.

FULLERIAN PROFESSOR OF CHEMISTRY IN THE
ROYAL INSTITUTION.

Dear Dr. Faraday,

Professor Matteucci has requested me to dedicate to you, in his name, this English edition of his Lectures on the Physical Phenomena of Living Beings. With that request I most cordially comply. To no one could the following pages be more appropriately inscribed than to yourself, to whom the physical sciences are indebted for some of the most brilliant and splendid discoveries of this prolific age. To no one could I with more pleasure address this work than to you, for whom I have ever entertained the warmest feelings of admiration, respect, and esteem.

Believe me, my dear Dr. Faraday,
Ever faithfully yours,

JONATHAN PEREIRA.

Finsbury Square, Sept. 1847.

PREFACE

TO

THE ENGLISH EDITION.

In 1844, Professor Matteucci was appointed by the Government of Tuscany to deliver, in the University of Pisa, a course of Lectures on the Physical Phenomena of Living Beings. These Lectures were subsequently published; and their popularity is attested by the fact, that they have already passed through two editions in Italy, and one in France.

The present translation has been made from a copy furnished to the Editor by Professor Matteucci, and containing a very large number of additions and corrections. Although the French edition is, as far as matter is concerned, more complete than the Italian ones, it contains, on the other hand, numerous errors ascribable to the translator. These have been corrected by M. Matteucci, who has also embodied the results of his most recent investigations in the present edition, which must not, therefore, be regarded as a mere translation of any of the editions hitherto published.

The Editor has introduced some additional woodcut and has appended a few notes, which it is hoped wi increase the utility of the work.

The Editor thinks it right to state that the presen translation was advertised for publication several month before any translation of M. Matteucci's work appeare in the medical journals.

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LIST OF THE WOODCUTS.

*Those marked with an Asterisk * were not contained in previous editions of this Work.*

Figures.

ENGLISH VALUE

OF THE

FRENCH WEIGHTS AND MEASURES

ALLUDED TO IN THIS WORK.

	Troy Grains.
Grain (French)	0·820421
Once	472·562500
Livre	7561·000000
Gramme	15·434023
Kilogramme	15434·02344

	English Inches.
Millimetre	0·03937
Centimetre	0·39371
Metre	39·37100

	Eng. Cubic Inches.		Imp. Pints.
Litre	61·028	=	1·7608
Hectolitre	6102·800	=	176·08

PHYSICAL PHENOMENA
OF
LIVING BODIES.

LECTURE I.

INTRODUCTION. — GENERALITIES.

Argument. — Living beings are endowed with the general properties of all natural bodies. *Imbibition. Elasticity. Gravity. Caloric. Electricity. Light. Affinity.* All the phenomena of living beings are not explicable by reference to physical and chemical forces merely. The action of physical agents is modified by the organisation and molecular structure of living bodies. *Catalytic actions.* The *Cell* is the elementary organ of living bodies; the mechanism of its life explicable by the phenomenon of endosmose. *Vital phenomena;* a complete explanation of them is, at present, not possible. *General conclusions.* Objects and limits of the study of the physico-chemical phenomena of living beings. Precision of language and accuracy of method as necessary for physiology and medicine as for the physical sciences.

Gentlemen,

I never felt more diffident of my own abilities than now, when about to discharge the duty imposed on me, of delivering a course of lectures on the *Physical Phenomena of Living Bodies.* But, while I am fully sensible of the difficulties of such an undertaking, m supported by the hope that my efforts will be rew ed by the great benefit you will derive therefrom. is perhaps the first time that a course of lectures, er this name, has been introduced into medico-physical ucation. We have no work which treats exclusiv of this subject: the germs, indeed, are scattered her nd there, but hitherto they have never been viewed in he light most favourable for their development.

If at the commencement of a course of lectures he teacher usually finds it requisite to give an exac efinition of the science he is about to treat of, to w its limits and its objects,—in a word, to sketch a n and programme of his course,—assuredly the nece y for such preliminaries was never more obvious th n the present instance.

General Properties of Living Beings. — Living b gs are endowed with the general properties of all na l bodies. The most ultra-vitalist never dreamt of enying that living organised matter is extended, npenetrable, divisible, and porous. How can we be e that caloric, electricity, light, and chemical affinity t on these beings in a manner entirely different from t which they are known to do on the other bodie f nature?

Yet you will find in some much-esteemed work n Physiology, tables of the differences between, and n of the presumed opposite characters of, organic an organic bodies. I should enter into a long and us s discussion, were I to attempt to demonstrate to u that many of these pretended differences have little or no value. Animals and vegetables grow by intussusception, minerals by juxtaposition ; — in other words, in the former, growth takes place by internal juxtaposition, in the latter by external juxtaposition; for organised bodies conceal in their interior the dissolved elements of new formations, whilst, on the contrary, these elements are situated externally in the case of inorganic bodies.

During life there is a continual struggle between the physical and vital forces: death is the triumph of the former over the latter. But shall this be deemed a sufficient proof that vital and physical forces are essentially distinct, and opposite in their modes of action? Would it be correct to say, that the different parts which together form an arch are endowed with a force opposed to gravity merely because they do not fall?

Imbibition. — Organised living beings, like all other bodies in nature, are extended, impenetrable, divisible, and porous. Plunge them into water, or any other liquid, and you will find that, like sand, pounded glass, porous substances, and bodies formed of capillary tubes, they imbibe. This property is of the greatest importance to them. In a great number of animals, life may be suspended for a considerable time with impunity; but, on contact with water, which they have the power of imbibing, they return to life, and recommence their movements. Who is ignorant of the beautiful experiments made by our illustrious countryman, Spallanzani, on the *rotifera?* Observe this tendon and this membrane; they are hard and shrivelled. One might suppose that they never could have formed any part of an organised body; yet, if we plunge them into water, it will be seen that, in proportion as they imbibe moisture they become soft, supple, and elastic, and assume that condition which, in the living body, fits them for fulfilling those functions for which they were ordained.

Elasticity. — Elasticity belongs to living beings as well as to other bodies of nature. Here are pieces of intestine and of artery; I can stretch or compress them more or less as I please. If I open this stop-cock, which is fixed to the trachea, you perceive that the lung collapses; whilst it swells up and expands again when I force air into it. Do not imagine that these different organs could fulfil their respective functions without the elasticity of the parenchyma of the lung, of the intestine, or of the artery. Destroy this, and these functions are stopped, or at the least they are altered.

Gravity. — Gravity acts upon the solid, liquid, and gaseous parts of living beings, as on all other natural bodies. We could never explain the functions of respiration and absorption if we did not take into consideration the physical properties of the solids, liquids, and gases of the economy, and their conditions of equilibrium.

Caloric. — Apply a sufficient degree of heat to an organic body, and you will observe the evolution of gas, the disengagement of aqueous vapour, and the combustion of carbon and hydrogen in the air, producing carbonic acid and water. If at first the heat frequently ardens and shrivels organic matters, instead of dilating nd liquefying them, as it usually does with inorganic ubstances, you cannot surely attribute this difference o vital action, since life has long been extinct when hese phenomena appear.

All these effects are owing to a peculiar structure and to he physico-chemical properties of the elements of which he tissues are composed. In fact, organised beings, hen subjected to the action of heat, first lose the water ith which they are impregnated,—an effect which com- nences in the part to which the heat is most directly pplied; the substance then curls up like horn, just as piece of paper does which has been moistened more n one side than on the other, the largest surface forming he convexity of the new shape produced by the con- raction.

These organic bodies often contain albumen, which oagulates under the action of a strong heat; their ele- ients separate in the gaseous form, producing more imple and consequently more stable combinations.

Electricity.—The electrical discharge traverses or- anised bodies, and diffuses itself in their interior ith more or less facility, according to their different egrees of humidity. When the spark passes through hem, it volatilises and burns them, reducing them ashes. When the electric current traverses the uids of living beings, it effects the decomposition of he salts contained within them; acids being evolved at ne pole, bases at the other. Albumen coagulates at the positive pole, where oxygen and a frothy acid liqui are set free; hydrogen appears at the negative pol along with an alkaline liquid.

Light.—With regard to the luminous rays, no one ca be ignorant of the fact, that in traversing the humou of the eye they deviate from a right line, sometim diverging sometimes converging, according to the different density of the humours and the conformation o the parts which contain them, as in a dioptric instrument.

Affinity.—Let me add, that the elements of whic human beings are composed are always obedient to th general laws of affinity; the chemist can recognise an separate them by the ordinary process of analysis Subject them to the influence of chlorine, bromine, o iodine, and hydrogen will be the first element whic will be separated to combine with these metalloids and form hydracids.

All oxidising agencies, when tolerably energetic, convert organic matters into acids.

Phenomena of Living Beings.—From these considerations are we to conclude that all the phenomena of living beings are explicable, by the general properties which belong to them in common with all the bodies of nature, and by the sole action of the great physical forces, caloric, light, electricity, and attraction? Such an inference would be as far from the truth as the conclusion of those who have denied and still deny these general properties to living beings, and who regard them as entirely beyond the influence of physical agents.

Examine those phenomena of living bodies, which, if I may be permitted so to call them, are *the most physical, the most chemical,* and you will find considerable differences in the mode of action of physical and chemical agents in the organism,—differences which are inexplicable in the present state of our knowledge of the laws governing these forces. Does not the phenomenon of vision itself, which may be termed a purely physical phenomenon, present peculiarities which remain up to the present moment unexplained? If the latest discoveries in science enable us to account for the distinctness of vision at all distances, and the absence of colour on the edges of the image, how, by the aid of physical laws, can we explain the perception of a single object in its natural position, by a double and inverted image? What could we not say of hearing and the voice, which are simply effects of some particular vibrations of the air, propagated by solids, according to the general laws of acoustics? To questions such as these, science can give no completely satisfactory answer.

Organisation.—The chemical action of light, which decomposes carbonic acid, carries the carbon under the form of new combinations into the interior of vegetables, disengages the oxygen, and thus produces what the most powerful chemical affinities cannot accomplish, is certainly different from that which decomposes some oxides and metallic chlorides, an effect for the production of which the feeblest chemical actions are sufficient. Apply an electric current to the nerves of a living animal, and the peculiarity of the resulting phenomena will pro to you the immense difference which exists betwee the effects of the great forces of nature, according a the body in which they occur is living and organise or inorganic and dead.

What, then, is the cause of these extraordinary diffe ences in the modes of action of physical agents on livin beings and on other bodies of nature? Here is a primar question of the highest importance, and one to which th existing state of our knowledge furnishes no satisfactor reply. But let us not, on that account, abandon the analo gics which the physical sciences offer us; a ray of ligh which penetrates a piece of glass or a body of water in an oblique direction, deviates from the straight line whilst, on the contrary, if it fall on a crystal of carbonate of lime (calcareous spar) it is split into two other rays, each of which deviates from the direction o the primitive ray, but in unequal degrees. The cause of the difference of these phenomena resides in the difference of physical structure existing between glass and crystallised calcareous carbonate, and perhaps also in the different chemical nature of their molecules. These modifications of the luminous ray, however, arise more from diversity of structure, or the peculiar arrangement of the molecules, than from differences of chemical composition. Indeed, we know that glass acts differently upon rays of light, according as it is more or less compressed in different directions, without its chemical composition undergoing any change.

Who could confound an organised being with an norganic body? In these groups of closed vesicles, of different dimensions, united and disposed in an irregular manner, there is assuredly *something* essentially different from a mass of polyhedral particles, composing a crystal. To say, with some micrographers, that organisation is crystallisation effected in a liquid which the first formed crystals imbibe, is equivalent to admitting that the tructure of a stalactite is identical with that of the lungs nd the liver. Molecules, composed of at least three lements, into each of which a great number of elementry atoms enter, must necessarily form chemical systems, whose affinities differ from those which are possessed by molecules chiefly composed of two elements, and in which the number of elementary atoms is smaller. And f the general chemical actions, by showing us that combinations become weaker in proportion as the number of he elementary atoms increases, are sufficient to explain he tendency of organic bodies to resolve themselves nto more simple combinations; if chemistry furnishes us with many instances of this same tendency in some norganic compounds, whose composition has many nalogies with that of organic bodies, it is not, thereore, to be inferred that the laws of inorganic chemistry re sufficient to give us a complete explanation of all he chemical phenomena of life. We must then conlude that organisation and the molecular structure of iving beings effect important modifications in the action f physical and chemical agents.

Actions of Contact or Catalysis.—We must not, hower, omit to add that each successive day increases the number of a particular class of chemical phenome whose explanation is impossible by the ordinary la of affinity only; I refer now to actions of *contact* *catalysis.* In the greater number of these we find th a substance, usually in very small quantity, excites other compounds, without itself undergoing any mod fication, considerable changes either of chemical com position or of physical properties. To this category phenomena belong the various kinds of fermentatio We shall find that the number of catalytic actions living beings is immense. Admitted. We can al produce them in our laboratories; they are of the san nature as those which platinum black effects on a mi ture of hydrogen and oxygen, and which finely divide silver produces on peroxide of hydrogen.

Cell-life.—I ought here also to mention a fact of in portance which I shall have occasion hereafter to spea of more fully. The *cell* is certainly the elementar organ, the molecule of organic bodies. We can now b the aid of the phenomenon of endosmose alone, effecte entirely under the dominion of physical forces, explai the mechanism of cell-life; we can tell how the ma terials necessary for nutrition are able to penetrate th cell, whilst others are eliminated. We shall go ove together an extensive series of physiological facts, o which endosmose has furnished the explanation.

Vital Phenomena not completely explicable.—We ma also add, and we intend to demonstrate the fact to you that light, heat, and electricity are produced in living beings, by the same physico-chemical actions as those which take place in inorganic bodies, and that they offer the same results. But, with the aid of this knowledge and of these analogies, dare we hope to obtain a complete explanation of all the phenomena of living beings? For the present, at least, this would be a vain hope.

Open an animal, examine its kidneys and its liver, and then ask yourselves by what physical force you can explain how the blood, which is carried to an organ, forms bile and urine. Can we, by having recourse to chemical affinities, however modified, and aided by the peculiar structure of the organs, and even also by the actions of contact,—can we, I will not say comprehend, but even obtain a glimpse of the way in which the various organs effect the separation and transformation of the constituent parts of the blood, in which all the organic elements are mixed, partly suspended, partly dissolved, and of which they have need in order to repair their continual losses? What can we say of the functions of the nerves and generation?

Conclusions. — We conclude, therefore:—

1st. That living beings have the general properties of all the bodies of nature; that these properties are influential in the production of the phenomena proper to them; and that, consequently, we must not neglect or disregard them when we attempt to explain these phenomena.

2dly. That the great physical agents, caloric, light, electricity, and molecular attraction, act on living beings as well as on all the bodies of nature, and that their action must necessarily be influential in the production of the functions peculiar to these beings.

3dly. That these forces, when acting on organised matter, sometimes have their general mode of action modified, and that this difference is owing to a diversity in the structure and chemical composition of organised bodies.

4thly. That there are also in living beings phenomena which we call vital; that these are numerous and of the highest importance, and that, in the present state of science, we are unable to explain how their production can be influenced by physical agents, though the action of these be modified by the organism. This is the reason that we have a study,—a science whose object is the physico-chemical phenomena of living bodies; as there is one for experimental physiology. The intimate and necessary connection is found in the third class of facts which we distinguished. Organisation modifies the action of physical agents, and the study of these modifications requires the co-operation of physics and experimental physiology. Do not forget that we have formed a fourth class of phenomena of living beings, which we have called vital. I term them vital phenomena not vital forces, and indeed the difference is truly vital.

If Newton had called the force which rules the wondrous system of the celestial machine merely attraction, or attractive force, his name would long since have fallen into oblivion; but by demonstrating that attraction is exercised in the direct ratio of the masses, and in he inverse ratio of the squares of the distance, and by hus unfolding the eternal laws of this force, Newton as rendered his name immortal.

To speak of the vital forces, to give them a definition, o interpret phenomena by their aid, and yet to be ig-orant of the laws which govern them, is doing nothing, r rather it is doing what is worse than nothing. It is to tempt an impossibility, it is to content the mind to no urpose, to stop the search after truth. To state that he liver separates the elements of the bile from the lood by means of the vital force, is merely to assert hat the bile is formed in the liver. By thus varying he expression a dangerous illusion is established.

Physico-chemical Phenomena. — I believe that I have early shown the object we ought to aim at in studying he phenomena of living beings, which returns in its timate analysis to the examination of the physico-hemical phenomena of these bodies, of the modifitions which organisation effects in the general action physical agents, and, lastly, to the investigation the laws, at present empirical, of the purely vital henomena.

I hope I have succeeded in fully determining what e the limits within which we ought to confine ourlves, in the vast domain of physiology, and what part the subject we ought to study under the title of the *ysico-chemical phenomena of living beings.* The genelities which I have now stated must be sufficient to ove the importance of understanding the functions of ing beings.

Precision of Language.—In these lectures I propose to myself another, and not less important, object it is to introduce, in the exposition of physiological facts, and in the investigation of their laws, that precision of language, that exactitude of expression, that rigorous method, which are too often discarded in the study of physiology and of medicine, and which have hitherto been almost exclusively characteristic of the physical sciences.

Every advance in this direction, however slight it may at first appear, will in fact be of great importance to physiology; it will be a certain conquest gained, since it will be founded upon knowledge, independent of the science of the organism, and of which the bases will be established and supported on physical theories, each proposition of which has been rigorously demonstrated by experiment.

LECTURE II.

MOLECULAR ATTRACTION; CAPILLARY FORCES; IMBIBITION.

GUMENT.—Necessity of food for living beings. Capillary attraction, mbibition, and endosmose require to be studied on account of their gency in the phenomena of absorption and exhalation. *Capillary ttraction*, phenomena of; theory. *Imbibition*, phenomena of; differs or different liquids and at different temperatures. Its agency important in animals and plants. Hales's experiments on the imbibition f plants; his results due to atmospheric pressure. The effects of hemical affinity produced by capillary forces and molecular atraction; fresh water obtained by the filtration of salt water through and.

ERY one knows that a living body requires, for its itinued existence, the constant introduction of new stances into its system. These substances, the ater number of which are solids, are transformed l reduced to the liquid state by means of certain ctions of the organism. In this state they pass o particular cavities, from whence, after having ungone other transformations, they escape. We saw, the first lecture, how the porosity of the tissues of ng beings allowed them to imbibe and to become regnated with those liquids with which they came contact. We cannot, therefore, give you a satistory account of the phenomena of absorption and exhalation without considering the part which is playe by capillary attraction, imbibition, and endosmose,—ph nomena which, as we already know, can be exercised b inorganic bodies. The importance of studying these tw functions is so great, that I purpose devoting the whol of this lecture to the examination of the purely physic phenomena of capillarity and imbibition; in order tha by means of the information thus communicated, yo may be enabled to judge what part they play in th functions of absorption and exhalation.

Capillarity. — As I purpose to confine myself to simple detail of facts, I shall here state, in the form c propositions, the principal results, drawn from observations, of the phenomena of capillarity.

1st. When a body is plunged into a liquid, the latte is either elevated or depressed around the solid, an presents, at its point of contact with it, a concav or a convex surface, according as it is either elevate or depressed. In the first case, the immersed body said to be wetted or moistened, as when glass is introduced into water; in the second case, of which th immersion of glass in mercury is an example, the soli does not become moistened.

2dly. When we plunge two bodies sufficiently nea to each other into a liquid, the latter is either elevate or depressed between them, according as they are c are not moistened by the liquid. It is requisite tha the two bodies should be so near to each other that th two curved surfaces formed by the liquid may touc The elevation or depression of the liquid above c low its level, is in the inverse ratio of the distance of e two bodies from each other.

3dly. If we plunge, into a liquid, a glass tube open both extremities, the liquid rises or falls in the tube, d the effect is greater in proportion to the smallness the bore of the tube. If we compare the elevation depression which takes place in a cylindrical tube, ith that which is observed between two glass plates parated from each other by an interval equal to the ternal diameter of the tube, it will be found that the evation or depression in the tube is twice as great as at between the glass plates. The liquid rises and heres to the glass or moistens it; on the contrary, it lls in the tube, if the liquid be not able to moisten it. In a tube of 1 millimetre [about $\frac{1}{25}$ of an English ch] in diameter, the water rises 30 millimetres [about $\frac{1}{5}$ English inches]; and mercury falls 13 millimetres bout $\frac{1}{2}$ an English inch.] It will be readily admitted at capillary actions must exercise great influence er the functions of the tissues of animals and vegebles, when we reflect that the interstices and the capilry tubes of the tissues have a diameter of from $\frac{1}{100}$ to $\frac{1}{50}$ of a millimetre [about the $\frac{1}{2540}$ to about $\frac{1}{5080}$ of English inch.]

4thly. The concave surface of the elevated liquid, d the convexity of the depressed liquid, belong to hemisphere whose diameter is equal to that of the be.

5thly. If a drop of water be introduced into a conical ass tube [held in a horizontal position] it will run to the narrower end; but if a drop of mercury be int duced, it will, on the contrary, run to the wider end.

6thly. The phenomena in question are entirely ind pendent of the volume of the solid body plunged in the liquid, and consequently the thickness of the sid of the capillary tube, in which they are observed, without influence on them.

7thly. These phenomena occur equally in air at t ordinary pressure, in condensed or rarified air, in vacuum, and in any gaseous medium.

8thly. All bodies, of whatever nature, yield, if su ceptible of being moistened, the same results, provid that before immersing them in the liquid we make layer of it adhere to them.

9thly. For the same liquid, and with the same tub the elevation or depression of the interior liquid colum is in proportion to the temperature of the liquid, and a greater ratio than that of the diminished density pr duced by the heat.

10thly. The elevations and depressions of which w have just now spoken, are independent of the density the liquids. Thus, if we represent by 100 the elevatio of water in a tube, that of alcohol will be 40, that the volatile oil of lavender 37, and that of a saturat solution of common salt 88.

11thly. Two bodies within a certain distance of ea other, and floating upon a liquid, mutually attract ea other and adhere, provided that both or neither of the be susceptible of being moistened. If one only be su ceptible of being moistened, they repel each other. O

s principle we explain the tendency possessed by
all light bodies floating on water, to approach the
es of the vessels containing them.

12thly. Whatever be the height to which a liquid
es, it never flows over the upper opening of the ca-
lary tube. This indeed is a necessary consequence
the facts already stated. For it must be remembered
t the surface of the column of liquid elevated in the
be is always concave outwardly. Hence if we pour
ter into one leg of a bent capillary tube until the
umn terminates by a surface at first horizontal, then
vex outwardly, it will be found that the other
umn of liquid remained concave, and is constantly
re elevated than the other. Thus then, in the
enomena of capillarity, a force of depression is de-
loped when the surface becomes convex. Do not
pose that the water which drips from a wick of
ton moistened with this liquid, and of which one
is bent downwards, does so by reason of capillarity;
we have only to hold the wick horizontally, and the
charge immediately ceases.

Theory.—I cannot dwell on these phenomena so far
to give you the theory, which belongs entirely to the
ain of the highest mathematical analysis. The ex-
imental results which I have adduced are sufficient
prove that the phenomena depend on that force which
call molecular attraction—a force which is exerted
ween the molecules of the solid and those of the liquid,
between those of the liquid itself, and which ceases to

act immediately the smallest [appreciable] intervals separate the molecules.

To avoid any false application of the phenomena of capillary attraction to the animal economy, it must be constantly borne in mind that a space completely filled with liquid is incapable of exercising any capillary influence; that the action of a capillary tube on liquids is due, less to the substance of the tube, than to the nature of the liquid with which its inner surface is moistened, and, finally, that liquids never overflow the upper aperture of the tubes in which they are elevated, by the mere agency of capillarity.

Imbibition.—The phenomena of *Imbibition*, of *Hygroscopicity*, &c. are generally of the same nature as the preceding, and depend on the same force. A piece of sugar, a wick of cotton, and a cylinder of sand, of ashes, or of sawdust placed in contact with water, or any other liquid which moistens them, immediately draw up the liquid into their whole mass; that is to say, they imbibe it. It is the same with certain tissues, as cartilages and tendons, which, being dried and then plunged into water, resume in a few hours all the properties they possessed during life. This effect is the result of the action of the absorbed water. So also in the celebrated experiments with the *rotifera*, which are restored to life and motion when moistened by a drop of water.

The phenomena of imbibition have an influence on the filtration of liquids. For when these hold in suspension solid particles, the latter are separated and left on the filter, the substance of which imbibes the liquid.

Then a drop of chocolate or ink falls upon cloth or ering paper, it produces a dark central spot, sur- unded by a zone of a paler coloured liquid. The same ect takes place when blood is extravasated in the sub- taneous cellular tissue; the serum extends to the rgins, and separates from the colouring matter.

In the phenomena of imbibition, we have to consider st, the force of adhesion between the liquid, and the rfaces of particular solids placed in contact with em, and afterwards capillary action, properly so lled; for in sugar, in a mass of sand or of ashes, and organised tissues, there certainly exist very minute vities, which ramify internally in a more or less tuous manner.

Imbibition of different Liquids.—The phenomena of bibition deserve to be more attentively studied than ey have yet been. I shall lay before you the results some experiments which, in conjunction with Pro- sor Cima, I made on this subject. I wish that it d been in my power to have extended them.

We filled some tubes of glass of two centimetres out $\frac{3}{4}$ of an English inch] with very white sand, ich had been sifted through a very fine sieve. e extremity to be immersed in water was closed a piece of cloth. We had previously taken the ecaution of drying the sand in a salt-water bath. was then introduced by the upper aperture of the be, care being taken not to shake it when the tube s full, lest the sand within should become unequally mpressed. Six tubes thus prepared were plunged at the same instant into six different liquids, at a temperature of + 12° centig. [=53·6° Fahr.]. The action o imbibition, by which the liquids were elevated in th tubes, continued for ten hours, at first being rapid, bu gradually becoming slower until it ultimately ceased Each tube was plunged into its liquid to a depth o about ½ centimetre [about ⅙ of an English inch] and in order that this depth should remain constant we added, from time to time, more liquid.

The following are the greatest heights to which th different liquids rose. All the saline solutions were o the same density, viz. 10° of Baumé's areometer [sp. gr 1·075].

	Millim.
Solution of carbonate of potash	85
Solution of sulphate of copper	75
Serum of blood	70
Solution of carbonate of ammonia	62
Distilled water	60
Solution of common salt	58
White of egg, diluted with its own volume of water	35
Milk	55

This table shows how much imbibition differs in th case of different liquids: in solutions thickened with gun with boiled starch or with oil, scarcely any imbibitio takes place; and it is also very feeble in concentrate saline solutions and in all liquids holding very finely divided particles of solid matter in suspension. In the latte use it effects a kind of filtration. This phenomenon of mbibition may, in the case of solutions holding in suspension very finely divided molecules of solid matter, be ery valuable for ascertaining the different properties of he blood according to its density. In fact, in certain maladies, its density and its viscidity are much diminished; nd in these cases serous infiltrations take place, as they o also, for the same reasons, after profuse sanguineous ischarges. We shall hereafter find that alcohol, ether, ater, &c., as well as aqueous solutions, when introuced into the stomach of living animals, disappear, but t unequal intervals of time, oil remaining there for a ery long period.

Believing that it would be of importance to compare lcohol at 36° Baumé [sp. gr. 0·844] with distilled ater, I provided myself with tubes filled with sand, ounded glass and sawdust, and here are the elevations hat I obtained.

Tube with Sand.	Tube with pounded Glass.	Tube with Sawdust.
Alcohol, 85 millim.	175 millim.	125 millim.
Water, 175 —	182 —	60 —

Thus we see clearly that with either sand or pounded lass alcohol rises less than water; a fact which is in ccordance with that which happens with capillary tubes.

In another experiment I plunged into the same quid, namely water, two tubes, both holding pounded lass, but the one containing twice as much as the ther; consequently the powder was finer in the first ube. The following were the results obtained:—

In the first tube the liquid rose to 170 millimetres; in the second to 107 millimetres only in the same period.

It is not easy to give an explanation of the relation existing between the elevations in these two tubes. It is, however, natural to suppose that the liquid should rise more in the tube which contains double the quantity of matter, if we reflect on the augmentation of the solid surface which attracts the liquid, and on the smaller diameter of the capillary cavities.

This phenomenon of imbibition is continually witnessed in a great number of instances in the tissues of animals and vegetables. The latter, from being abundantly furnished with small spaces and capillary tubes, imbibe with the greatest facility, and absorb solutions with which they are placed in contact. This also is the case with the cellular tissue and the parenchyma of the lungs; but the opposite effect takes place with the epidermis.

Imbibition modified by Temperature.—I have likewise sought for some difference in the phenomena of imbibition at different temperatures. Two tubes prepared with sand were plunged into water, the one at a temperature of + 55° centig. [= 131° F.], the other at + 15° centig. [= 59° F.], and the results obtained were as follow:—

	Elevation after 70 seconds.	Elevation after 11 minutes.
Tube at + 55° centig. at + 15 —	10 millim. 6 —	175 millim. 12 —

The influence of temperature on imbibition is, there- re, very considerable. Now we know that in animals sorption either by the skin, or in the interior of the onomy, is more active in proportion as the liquid is armer.

Imbibition in different hydrometric Conditions of the ir. — I satisfied myself that imbibition was equal in r saturated with moisture and in dry air.

Imbibition at different Degrees of atmospheric Pressure. - Another result, not less singular, is observed when e study imbibition by sand, ashes, and sawdust, in e vacuum of the air-pump, and in air at ordinary essure. No difference is perceptible in the height of e column of water at the end of ten minutes; but in e experiment with sand, this peculiarity was observed, at for the first few minutes the rise of the liquid was ore rapid in the tube placed in a vacuum than in the e which was contained in the air.

Imbibition limited. — It may be asked whether, by e action of imbibition, a liquid can rise to an un- nited height. It would at first sight appear, that a lumn of sand, of ashes, or other pulverised bodies, one d of which is immersed in a liquid constantly main- ined at the same level, should, by the force of im- bition, elevate the liquid to any height till the whole lumn has imbibed. Indeed, if we consider separately e action of each of the layers forming the column, e may suppose that after the imbibition of the first yer in contact with the liquid, the particles of the cond layer immediately above will deprive the first of a portion of its water, and the latter will re-obtai from the liquid mass the amount it had lost. By repeating this reasoning for all the successive layers o the column, we arrive at the conclusion that each take up the same amount of liquid as if it had acted separately, and thus, if the level of the liquid be kept constantly at the same height, the entire column, howeve long it may be, will imbibe. But experience does no confirm this reasoning: the liquid rises at first rapidly then the ascending movement slackens, and at a certai height the liquid remains stationary. This fact canno be ascribed to the evaporation in the upper layers o the column; for water rises to the same height in column of sand, whether it be surrounded by the vapou of water, or by dry air. I can only account for it b admitting the existence of small canals in the whol length of the column of powder, and then, consequently capillary action, as well as the adhesion of the liqui to the surface of the grains of sand, will intervene.

Agency of Imbibition in living Beings. — It is impossible not to perceive that imbibition plays an importan part in the action of the juices of plants, as well as i the phenomena of the capillary circulation of the bloo in animals. In another lecture we shall show that al the parts of living plants and animals soon become impregnated with a saline solution, into which some par of them is immersed, and that its presence is easily recognised by tests. It will be sufficient for me t refer to the experiments of Hales and the more recen ones of Boucherie. The latter saw a poplar 28 metre [nearly 92 feet] in height absorb by its trunk, in 6 days he enormous quantity of 3 hectolitres [about 66 imerial gallons] of a solution of pyrolignite of iron.

Hales's Experiments. — I shall here notice the expeiments of Hales, made with the view of measuring hat he calls the force of aspiration* of powders and f the stems of trees.

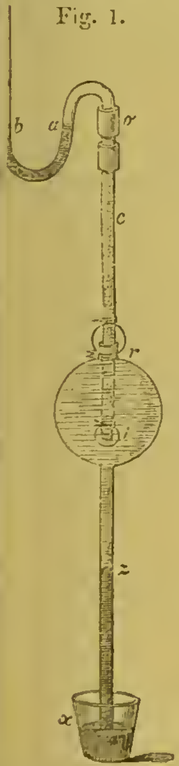

Fig. 1.

Hales's Apparatus for ascertaining the force with which powders imbibe moisture.

[This experimenter filled a glass tube, *c r i*, 3 feet long, and $\frac{7}{8}$ of an inch diameter (*fig.* 1.), with well dried and sifted wood ashes, pressing them close with a rammer. "I tied," he observes, "a piece of linen over the end of the tube at *i*, to keep the ashes from falling out. I then cemented the tube *c* fast at *r*, to the aqueo-mercurial gauge, *r z*; and when I had filled the gauge full of water, I immersed it in the cistern of mercury, *x*; then to the upper end of the tube *c*, at *o*, I screwed on the mercurial gauge, *a*, *b*.

"The ashes, as they imbibed the water, drew the mercury up 3 or 4 inches, in a few hours, towards *z*; but the three following days it rose but 1 inch, $\frac{1}{2}$ inch, and $\frac{1}{4}$, and so less and less, so that in 5 or 6 days it ceased rising. The highest it rose was 7 inches,

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* I cannot find the term "*force of aspiration*" in Hales's *Vegetable Statics*. The phrases which Hales employs are, "*the force with which rees imbibe moisture*," and "*the imbibing power*."—J. P.

which was equal to raising water 8 feet."

In another experiment Hales substituted a tube 8 feet long, and ½ inch diameter, filled with red lead, for the tube containing wood ashes, and obtained therewith a rise of 8 inches of mercury.*]

If we substitute the branch of a tree for the tube filled with wood ashes, [as in *fig.* 2], the mercury rises, as in the preceding experiments made with powders. Hales regarded this phenomenon as dependent on a force which he terms "the force of aspiration."†

Here are some experiments which explain these facts in a simple and satisfactory manner. It is easy to show that the ascension of mercury takes place equally in two tubes prepared in the

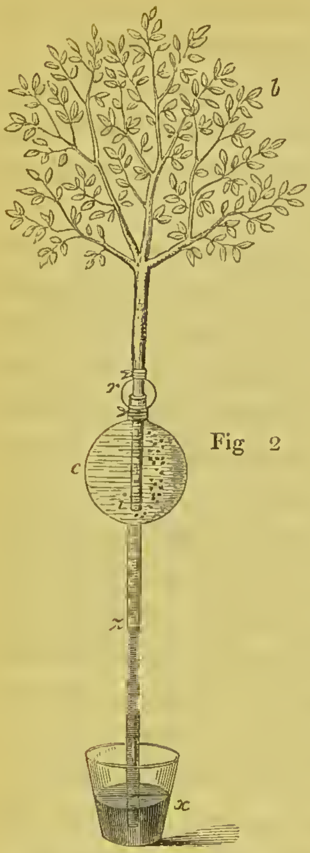

Fig 2

Hales's Apparatus for ascertaining the force with which plants imbibe moisture.
b, the branch.
r, *c*, *z*, the aqueo-mercurial guage.
i, end of branch from which air escapes.
x, the cistern of mercury.

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* That portion of the text inclosed between brackets has been introduced as a substitute for the less precise statement of Matteucci. I have employed the language of Hales, and have added his figures.—J. P.

† See foot-note, p. 27.

anner of Hales, but differing from each other in this, at in one of them the tube containing the ashes is pen at the top, while in the other it is closed.

It must, however, be observed, that the experiment ould not be attended with this result if the column of shes were short, or the latter less up-heaped. With an pparatus similar to that of Hales I made the following bservations. I luted a leaden tube to the top of a lass one containing the pulverised substance. By the id of this I could easily remove the air from above the shes. At the moment when the quicksilver began to se I produced a vacuum, and the mercurial column ot only did not descend, but it continued to ascend. t is, then, indubitable that the ashes form, above the olumn of water, a wall or partition which performs the ffice of a closed tube; in fact Hales's apparatus is a arometer. In another experiment, at the moment hen the mercury began to rise, I covered the whole ith a bell-glass, and produced a vacuum; the mercury stantly fell. I have obtained the same results by bstituting a stem with leaves for the tube filled with shes. If I introduce the upper part of this stem into balloon, as I remove the air the mercury continues to se, but on the contrary, if I form a vacuum over the ercurial reservoir, it immediately falls. Hence, then, e conclude that what Hales calls the force of aspiration mbibition) is a simple barometrical phenomenon. hether the column of ashes or the leaves and trunk a tree form the upper closed part of the barometer, e water penetrates the powders or vegetable tissues by imbibition, and the atmospheric pressure gradually effects the rise of the liquid.

Exhalation by Leaves.—We must, however, remark a very curious fact, which takes place when we employ a branch with leaves as in all the other experiments of Hales; it is, that the column of water continues to ascend; so that we are forced to conclude that the vapour of water is exhaled by the leaves, without these ceasing to act as the accurately closed wall of a barometer. It would appear that Magnus has obtained a like result by closing, with a piece of membrane, the upper part of the tube. From what we have said, the ascension of the column of water should go on, but it is probable that the phenomenon becomes at first less manifest, and then completely ceases on account of the disorganisation which occurs both in the membrane and the leaves.

Chemical Effects produced by Capillary Forces.—I shall not leave this subject without mentioning to you some experiments tried with the view of producing by the mere operation of capillary forces and molecular attraction the effects of chemical affinity. If we reflect that any kind of liquid constantly ascends to the same height in a capillary tube; that during imbibition more or less heat is produced, as the experiments of Pouillet have demonstrated; that, moreover, according to Becquerel, there is a disengagement of electricity; and, lastly, that capillary attraction is exerted at very minute distances only, and between the molecules of bodies; we cannot deny that this force combines the principal characters of hemical affinity. We know the experiment of Doc- eireiner, that if a mixture of water and alcohol con- ined in a bladder be exposed to the air, water con- antly escapes from the mixture. In this case the ater is imbibed by the membrane more readily than e alcohol, and is dissipated by evaporation.

Salt Water made fresh by Filtration.—Another and ore conclusive fact is mentioned by Berzelius: a line solution filtering through a long tube filled with nd runs out more or less, completely deprived of lt. I have confirmed this experiment by using a tube about 8 mètres [about 26 feet] long, filled with nd, and I have found that the density of the liquid, troduced by the upper aperture of the tube, was to at of the liquid escaping from the other end, as : 0·91. But it is necessary to state that this dif- rence of density was not always maintained; after certain time, the saline solution is as dense at its exit om the tube as at its entrance. This proves that the composition of the saline solution takes place in the st action of contact between it and the particles of nd.*

I have obtained an inverse result by employing a lution of carbonate of soda, which I caused to pass

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* The facts here mentioned respecting the alteration in the density of saline solution by filtration are very interesting. They are susceptible numerous applications in geology. For example, the marine origin of sh-water springs has hitherto been deemed impossible, "because," as a cent writer observes, "sea water cannot be freed from its salt by filtra- n." Matteucci's experiments demonstrate the possibility of this gin. — J. P.

through a tube 3 metres [nearly 10 feet] long, filled with sand. The density of the liquid at its exit was to that at its entrance as 1·005 : 1.

The phenomena we have just referred to are very important on account of the applications that can be made of them to some of the functions of living beings which are not completely explicable by the mere action of capillarity and molecular attraction.

LECTURE III.

ENDOSMOSE.

RGUMENT. — *Endosmose* and *Exosmose;* explanation of the terms Dutrochet's endosmometer; membranes and other solids through which endosmose takes place; liquids which effect it; velocity and intensity of the current; its direction affected by the density and temperature of the liquid; its force. Theory of endosmose. Endosmose of organised cells. Matteucci and Cima's experiments; double-action endosmometer; arrangement of the membranes used in three classes.

ass 1. Skins of the torpedo, frog, and eel; influence on the current — of the direction of the surface of the skins — of their fresh or dried state — of the skin of different regions — and of the nature of the liquid.

lass 2. Mucous membrane of the stomachs of the lamb, dog, and cat, and of the gizzard of the fowl; influence on the current — of the direction of the surface of the membrane — and of the nature of the liquid.

lass 3. Mucous membrane of the bladders of the ox and pig; influence on the current — of the direction of the surface of the membrane — of its fresh, dried, and putrid state — and of the nature of the liquid.

eneral conclusions.

utrochet's explanation of exosmose stated and objected to.

hysiological applications; the current is promoted, in skins, in the direction towards the secreting surface; relation of this phenomenon to secretion; absorption from mucous membranes; nutrition of ovules of *Mammalia*, and opening of the sperm-sacs of the *Cephalopoda*, effected by endosmose; endosmose of organised cells; endosmotic action of purgatives; endosmose of liquids in motion: remarkable influence of hydrochlorate of morphia on endosmose.

AVING considered the phenomena of capillary attracon and imbibition, it becomes necessary, in order that ou may be enabled to apply your knowledge of these bjects to the functions of exhalation and absorption, that I should make you acquainted with another phenomenon, which, although exclusively within the domain of physics, yet, nevertheless, by the physiological applications of which it is susceptible, is really connected with the study of organised beings. I refer now to the phenomenon discovered by Dutrochet *, and termed by him *endosmose.* It is the mutual action of two liquids on each other when separated by a membrane. Although its theory is not yet completely known, the subject is nevertheless of the highest importance.

I shall commence by explaining to you the fundamental fact in its simplest form. Here is a glass tube whose lower extremity, closed by a piece of bladder, is expanded into the form of a funnel. This instrument is called an *endosmometer.*

If we pour into it an aqueous solution of either gum or sugar, and then immerse the closed extremity in pure

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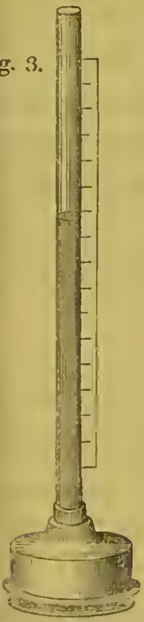
* Dutrochet's first memoir on endosmose and exosmose was read to the *Académie Royale des Sciences*, on the 23rd of July, 1827 (see the *Ann. de Chim. et de Phys.* tom. xxxvii. p. 393. 1827).

Ten years previously, my friend Mr. Porrett, the present treasurer of the Chemical Society of London, had communicated to the editor of the *Annals of Philosophy* (vol. viii. p. 74. for July, 1816) a paper on two "*curious galvanic experiments*," one of which was the production of endosmose between two liquids separated by a membrane and subjected to the action of voltaic electricity. He called the phenomenon, *electro-filtration ;* and asks, whether jointly with electro-chemical action, it is not "in constant operation in the minute vessels and pores of the animal system."

M. Parrot, of St. Petersburgh, has recently presented to the *Académie Royale des Sciences*, an inaugural dissertation, published in 1803, giving an account of the phenomena presented by two liquids of unequal density, separated by a permeable organic diaphragm, and pointing out their applications to physiology and pathology. (*Comptes rendus*, tom. xix. pp. 607. 619. for Sept. 23rd. 1844.)—J. P.

ater, we shall find that, notwithstanding the excess of pressure exercised by the solution, the water continually passes into the tube by filtration through the membrane. The liquid within becomes thus elevated to a certain extent, and may even flow over by the upper aperture. At the same time, a certain quantity of the mucilaginous or saccharine liquid escapes from the tube through the bladder, and mixes with the water; but the quantity is necessarily less than that of the water which passed in the opposite direction through the membrane. Dutrochet has called the first of these phenomena *endosmose,* and the second *exosmose.*

ig. 3.



Endosmometer.

Endosmose through Membranes, &c.—Membranes prouce endosmose until they begin to putrefy, when the henomenon ceases, and the liquid which had risen into ie tube, descends and filters through the membrane.

It is not membranes only which are endowed with is property: very thin plates of slate, or better still baked clay, produce the same effect, though in a iore feeble degree. Calcareous and siliceous laminæ, n the contrary, have no effect of the kind: with them idosmose does not take place.

Intensity of the Current.—The nature of the liquid nployed greatly influences the phenomenon. Endososе is more obvious in proportion as the density of the quid in the tube exceeds that of the exterior liquid.

It might seem that the intensity of the current is proportional to the difference of the densities of the two liquids; but alcohol, which is lighter than water, causes, when introduced into the tube, endosmose of the water which is placed exterior to it. Saline solutions produce, through membranes, very energetic, but less durable effects.

Increase of temperature augments the rapidity of the current of endosmose.

Dutrochet found that the slightest trace of sulphuretted hydrogen destroyed endosmose. It is probable, however, that this effect arose from the alteration in the condition of the membrane when it commences to evolve this gas; for fresh membrane placed in contact with sulphuretted hydrogen is very active.

Velocity of the Current.—Dutrochet endeavoured to measure the velocity with which a liquid passes, by virtue of endosmose, from the exterior to the interior of the tube. Here are the results of his experiments:—With a membrane of 40 millimetres in diameter, and a tube of 2 millimetres, a solution of sugar, whose density was 1·145 rose 34 divisions in the space of an hour and a half; each division being 2 millimetres. In another experiment, he employed a solution of sugar, the density of which was 1·228, and the ascent in the same space of time was 53 divisions. Lastly, in a third experiment, with a solution of sugar of the density of 1·083, the column mounted 19½ divisions in the same interval of time. It is evident, therefore, that the velocity of endosmose is proportional to the excess of density of the

terior liquid over that of the water employed as the
xterior liquid.

Dutrochet took different solutions having the same
ensity, and compared them with water, from which
ey were separated by bladder.

The following ratios express the variable intensity of
ndosmose obtained in the different cases:—

Intensity of Endosmose of various Liquids with Water

(Dutrochet).

Solution of	gelatine	3
„	gum	5·17
„	sugar	11
„	albumen	12.

From this table we see, that of all organic substances
luble in water, albumen produces endosmose with the
reatest force.

Direction of the Current.—Among the most curious
cts discovered by Dutrochet, whilst studying endos-
ose, must be mentioned that of the variation in the
irection of the current between certain acid solutions
d water, according to their density and temperature.
his is especially manifested by a solution of hydro-
loric acid. Thus, with hydrochloric acid at the den-
ty of 1·02, endosmose takes place from the water to
e acid, whilst at the density of 1·015, the current is
the opposite direction; that is to say, it is from the
id to the water.

Force of the Current.—Dutrochet endeavoured to de-
rmine the amount of pressure exercised by the column

of liquid elevated by endosmose in the different cases. For this purpose he employed the apparatus which Hales first, and afterwards Mirbel and Chevreul, used for measuring the pressure of the juices in plants. In this apparatus the pressure is estimated by the height of a column of mercury raised by the liquid.

In studying endosmose under this point of view, Dutrochet proved that, all other things being equal, the force, which produces the current of endosmose, is proportional to the excess of the density of the interior liquid over that of the water. We have already seen that the velocity of endosmose likewise varies. It, therefore, follows, that assuming this law to be true in all cases, syrup, whose density is 1·3, produces a current capable of raising a column of mercury of 127 inches (3 metres 42 centimetres), or what amounts to the same thing, equal to the enormous pressure of 4½ atmospheres.

Theory of Endosmose. — Dutrochet has endeavoured to give an explanation of the phenomena of endosmose; and Poisson and Becquerel have proposed others. Thus, some ascribe endosmose to the action of an electric current developed by the contact of the two different liquids;—a current which will produce the passage of the water through the membrane, from the positive to the negative pole, as in the well-known experiment of Porret. But to render this explanation probable, it would be necessary to prove that the contact of water with alcohol, solution of sugar, &c., developes electricity; which is not the case. Poisson supposed that the least ense liquid entered the capillary tubes of the membrane, nd that this capillary thread, drawn down by the pure ater, and up by the denser liquid, must be elevated in irtue of molecular attraction. But this explanation ecomes inadmissible when we consider that alcohol, hich is lighter than water, produces endosmose; and hat certain calcareous and siliceous stones, placed under he same conditions as membranes and plates of clay, do ot give rise to the same effect.

Up to the present time we have not any satisfactory heory of endosmose; but we know that the general onditions of the phenomenon are as follow :—

1st. That the two liquids should have an affinity for he interposed membrane.

2dly. That the two liquids should have an affinity for ach other, and be miscible.

If one of these conditions be wanting, endosmose does ot take place. Experiment proves that the current of ndosmose is not produced by the least dense liquid, nor y the most viscid one, nor by that which is endowed ith the greatest force of ascent in capillary tubes. The urrent is in general determined by the liquid which has he greatest affinity for the interposed substance, and by hich it is imbibed with the greatest rapidity. In fact t is evident that the membrane imbibes the two liquids nequally; and that the one which is imbibed with he greatest facility, ought to mix with, and augment he volume of, the other.

Endosmose in Living Beings.—What we have here tated must be sufficient to convince you that this phenomenon is perhaps one of the most important physical facts applicable to the functions of living beings. Microscopic observation has now put beyond doubt that, in all tissues, whether vegetable or animal, and in those liquids which are produced by the alteration of organised and living beings, there are constantly found, at a certain epoch, microscopic corpuscles, which have a peculiar and characteristic form, and are called elementary or primitive cells. These bodies consist of an exceedingly delicate membrane, which has a spherical form, encloses a liquid, and has on its inner side a small organised body, called the *nucleus* or *cyto-blast.* The cells float at first in a liquid, which Schwann has named *cyto-blastema,* and they ultimately become included in, and almost confounded with it, when this liquid acquires a greater or less density. In different tissues, the elementary cells are more or less closely approximated to each other; the *cyto-blastema,* or intercellular substance, being invariably the bond of union between them. The life of the elementary cells certainly plays the most essential part in the development and preservation of the tissues of living bodies; and, since these cells are found under conditions favourable to endosmose, we have no reason for refusing to admit its existence. A vesicle filled with a liquid, and placed in the midst of another liquid, may act on the outer one, receive the surrounding liquor, and reject the one it had previously contained, by operating in a manner analogous to endosmose.

Matteucci and Cima's Experiments. — We must, however, confess that hitherto very few investigations have een undertaken with the view of making such applicaons of endosmose to physiology as the subject appears be susceptible of. To do this it was necessary to ary the liquids between which endosmose takes place, nd to select the membranes, so that we might always eep as close as possible to the conditions under which he analogies, between this phenomenon and those which xist in the interior of living bodies, have been observed. n conjunction with Professor Cima I undertook this quiry, and I shall now state the results of our reearches.

Classes of Membranes.—The membranes which we ubmitted to experiment may be arranged in three lasses; the first including the skin of the frog, the toredo, and the eel; the second, the stomach of the lamb, he cat, and the dog, and the gizzard of the fowl; and he third, the bladder of the ox and of the pig.

Apparatus. — We shall not stop to describe our aparatus, as it differed in no way from the endosmometer f Dutrochet. But I may observe that in all our exeriments we used, at the same time, two endosmomeers, the bore of whose tubes was exactly three millietres [about $\frac{1}{8}$th of an English inch] in diameter; nd their scale was divided into millimètres. In a lass vessel, sufficiently large to hold the two intruments, we placed a support upon which was firmly ixed a metallic plate perforated with a great number of mall apertures. Upon this plate we put the two enlosmometers; and in order that they might not be liable o a change of position, we loaded them with a large leaden plate pierced with two holes, whose diameter was equal to that of the neck of the instruments.

In the course of the experiments, the interposed membrane in one of the endosmometers was placed in the reverse position to that of the other: for example, if made of skin, it was so placed that, in one case, the external surface was directed towards the interior of the instrument, whilst, in the other, the internal surface was turned in this direction.

All the experiments were made at a temperature of from + 12° to + 15° centigr. [= 53·6° Fahr. to 59°.] In the greater number of cases they lasted for two hours, and were repeated several times. We took care to provide the two endosmometers employed in the comparative experiments, with two portions of membrane of equal size taken from the same animal, and from two symmetrical regions of the body or organ employed. The liquids used were, besides spring water, the following, of which we give, once for all, the density according to the degrees of Baumé's areometer: —

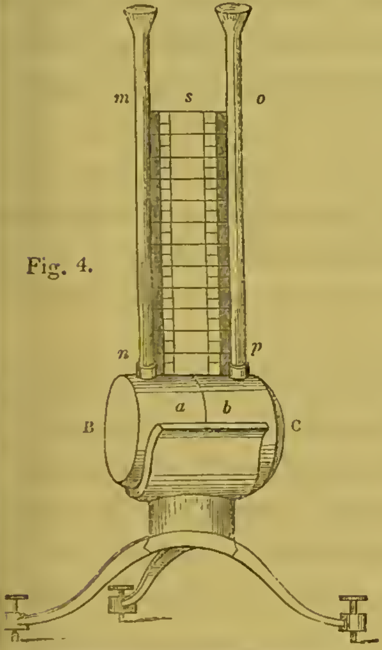
Density of the Liquids in the Endosmometrical Experiments.

	Baumé's Areometer.	Specific Gravity.
Solution of sugar -	19°	1·152
white of egg -	4°	1·029
gum arabic -	5°	1·036
Alcohol - -	34°	0·855

These liquids were usually contained in the interior of the instruments, the water being placed externally.

In some particular instances we altered the arrange-ients of the liquids and instruments, by employing a eparate vessel for each endosmometer. We also used nother instrument, of which a figure and description re subjoined: B and C are two cylindrical brass receivers, ground into each other; B has at *a* a brass plate perforated by holes, on which the membrane submitted to experiment is placed: C is also furnished with a plate perforated with holes, which, when the two cylinders B C are united, as in the subjoined figure, fits accurately on the membrane, which is thus pressed between the two plates. In this condition it can only yield to the greater pressure exercised upon it by a liquid, denser than that contained in the other portion f the cylinder. *m n*, *o p* are two tubes of equal alibre: the first communicates with the receiver B; the ther, with the receiver C. When we wish to use the nstrument we introduce the denser liquid into B, and ill the tube *m n*, with it to a certain height. C is then illed with water by plunging it in a vessel of this liquid.

Fig. 4.



Double-action Endosmometer.

The two cylinders are then fitted together under water, and the two receivers pressed together by a vice, in order that the liquid contained in c may not escape through the fissure of the joining. We place the instrument on a level, and then put the two liquids at 0° of the scale *s*. With this instrument we obtain at the same time the value both of the elevation and of the depression of the two liquids, which gives a great precision and much experimental facility, by thus doubling the results.

Class 1. *Experiments with the Skins of the Frog, Torpedo, and Eel.*—I shall first state the results obtained by employing, as the membranes, the skins of frogs, torpedoes, and eels, with the before-mentioned solutions for the liquids.

Influence of the Position of the Membrane.—In our first trial we noticed, in a very clear manner, the marked influence exercised upon the phenomenon of endosmose by the position of the interposed membrane. It was in fact this first discovery which led us to examine, in this point of view, the effects produced by the bladder and stomach of divers animals. With some trouble we obtained entire skins, and deprived them of all adherent sub-cutaneous cellular tissue. After having thus prepared them, we cut off those parts which, in the torpedo and eel, are perforated by the cutaneous appendages, and so obtained membranes well fitted for our experiments.

Skin of the Torpedo.—By employing the skin of the torpedo, placed in one endosmometer with its external urface towards the interior of the instrument, and in he other reversely, and by filling the two endosmoneters with a solution of gum Arabic, we observed that, vhilst the liquid in the first instrument rose 30 millinetres, it rose in the second sometimes 18, and someimes only 6 millimetres. In certain cases we saw the iquid elevated 20 millimetres or more, in the first tube, vhilst it scarcely rose at all in the second.

These differences are equally observed, with a soution of sugar. Thus, this liquid which rises 30 nd even 80 millimetres when the external surface f the skin is turned towards the instrument where the iquid is contained, rises at the utmost only 2 millinetres when the membrane is placed in the contrary lirection. In one case where the first-named arrangenent was adopted, the liquid rose 80 millimetres; but eached only 20 millimetres when the second arrangenent was resorted to.

With albuminous solutions the elevation was 26 nillimetres when the external surface of the skin was in ontact with it, and 13 millimetres when placed in the ontrary direction.

Skin of the Frog.—The results obtained with the kin of the frog agree in general with those furnished y the skin of the torpedo. We remarked that the lirection of the endosmotic current was constant, and vas from the water to a solution of sugar, or of um, or of albumen. We observed that the memrane possessed the property of rendering endosmose nore or less intense, according to its position relatively to the two liquids. In arranging the skin of the frog in the two endosmometers in the usual manner, we obtained an elevation of 36 millimetres when the external surface was in contact with the solution of sugar, and of 24 millimetres in the reverse arrangement. In several cases the former was exactly double the latter. There was likewise a very marked difference, and always of the same kind, when we used solutions of albumen and of gum Arabic. With the first there was an elevation of 24 millimetres, with the second 32 millimetres, when the external surface of the skin was in contact with them; whilst there was only 12 millimetres for a solution of albumen, and 16 millimetres for a solution of gum, when the internal surface was turned towards them.

Eel Skins.—The differences which we have already remarked from using a solution of sugar and skins of frogs and the torpedo, exist equally in the case of the eel skin. But what is singular with the latter is, that the difference is not manifested at the commencement of the experiment. At first the elevation of the liquid is alike in both instruments; but after a lapse of two hours we perceive that, in the endosmometer in which the external surface of the skin is turned towards the solution of sugar, the elevation is 30 millimetres, while in the other instrument it is only 20 millimetres. With an albuminous solution and gum water, the differences observed from the commencement of the experiment are the same as those which usually take place; and, although we ultimately find the albuminous solution ises 8 millimetres, and the solution of gum rises 20 millimetres, when the external surface of the skin is urned towards the liquid; we observe, on the conrary, that when the position of the skin is reversed, he albuminous solution rises only 4 millimetres, and he solution of gum 17 millimetres.

The fresh condition seems more necessary for the kin of the eel, than for that of the frog and torpedo, hen we wish to mark the difference in the elevation f the liquids contained in the endosmometers. If the kin of the eel has been removed from the animal for ne or two days, no difference is observed between the wo positions of the membrane; and the solutions of ugar, albumen, and gum respectively rise, in the same pace of time, to an equal extent in both instruments.

Endosmose between Alcohol and Water.—By employing water and alcohol, Dutrochet obtained the current n the direction from the former to the latter. This onsequently formed an exception to all the other cases, n which he found that the direction of the current was om the least to the most dense liquid.

The influence of the position of the skin, employed s the membrane interposed between these two liquids, as been rendered evident by our experiments; but the osition favourable to the current, which is constantly om the water towards the alcohol, is not the same for e three kinds of skins alluded to. Thus, when we se the skin of the frog, the current is promoted from e external to the internal surface, by being directed ways from the water to the alcohol. In various and frequently repeated experiments, we have observed an elevation of 20, 24, and 40 millimetres when the internal surface of the skin was placed towards the alcohol, whilst in the reverse position the corresponding elevations were only 4, 12, and 20 millimetres. Under analogous circumstances, the position of the membrane being favourable, the elevation was 28 millimetres; in the other position, on the contrary, there was no elevation. With the skin of the eel the reverse takes place. With this the current is favoured from the internal to the external surface; and whilst the alcohol contained in the instrument rises to the height of 20 millimetres, when in contact with the external surface of the skin, it only rises to 10 millimetres when the position is reversed.

This difference of elevation always takes place in the same direction, and is confirmed for the eel-skin, as for the skin of the torpedo. The elevation has been at 50 millimetres in one instrument, and 20 in the other.

Some anomalies which we observed in our earliest trials, led us to study, with greater precision, the circumstances under which endosmose takes place through the skin of the torpedo, when interposed between water and alcohol. We constantly found these differences when the skin of the torpedo was fresh, and had not been used for previous experiments, but it continued only during the first hour of the experiment or a little after. The elevations subsequently follow a different law, and the height in the endosmometer, when the external surface of the skin is in contact with the water, goes on iminishing, afterwards ceases, and ultimately the diection of the current changes.

Out of the numerous experiments which we have ried, we select the following, in which we have marked he elevations hour by hour. We shall call the endosnometer, in which the interior surface of the memrane was in contact with the water, A; and the one n which this surface is towards the interior of the nstrument, B.

Endosmose through the Skin of the Torpedo.

Endosmometer A. Internal surface of the membrane towards the water.	Milli-metres.	*Endosmometer* B. Internal surface of the membrane towards the alcohol.	Milli-metres.
Elevation during the 1st hour	23	Elevation during the 1st hour	17
" " " 2nd "	25	" " " 2nd "	3
" " " 3rd "	25	" " " 3rd "	0
" " " 4th "	25	Depression in the 4th "	3

Conclusions. — We conclude, therefore,

1st. That so long as the skin of the torpedo is fresh, ndosmose takes place in the usual manner, from the ater to the alcohol; but invariably with this difference, hat, whilst the internal surface of the skin is in contact ith the water, the elevation is as 3, and, in the reverse osition, as 2.

2dly. That while in the first position of the memrane A, the force of endosmose remains constant for everal hours; in the second position B, the same force minishes, and after some time is extinguished.

3rdly. That, after a certain time, the direction of the current changes, and is then from the alcohol to the water, when the internal surface of the skin is turned towards the alcohol; whilst it remains constant in the contrary position of the skin.

We think that we ought to ascribe these peculiarities with alcohol, to the chemical action which this liquid exercises upon the substance of the membrane, and to the consecutive alteration of structure.

The diminution of the intensity of endosmose observed with the skin of the torpedo, but only in the less favourable position of the membrane, is confirmed, whatever may be its position, by employing the skin of the frog; but this decrease does not proceed regularly, as we may observe in the following table, in which A and B represent the same endosmometers as in the preceding table: —

Endosmose through the Skin of the Frog.

Endosmometer A. Internal surface of the membrane towards the water.		*Endosmometer* B. Internal surface of the membrane towards the alcohol.	
	Millimetres.		Millimetres.
Elevation during the 1st hour	23	Elevation during the 1st hour	30
“ “ “ 2nd “	40	“ “ “ 2nd “	55
“ “ “ 3rd “	12	“ “ “ 3rd “	15
“ “ “ 4th “	22	“ “ “ 4th “	35
“ “ “ 5th } “	56	“ “ “ 5th } “	58
“ “ “ 6th }		“ “ “ 6th }	

During the night, the liquid overflowed both the endosmometers, but there was no inversion of the current, as had taken place with the skin of the torpedo. Neither was this phenomenon observed with the skin of the eel, even when the experiments were continued for more than fifteen hours; but the increments were irregular, as with the skin of the frog.

Skin of different Regions. — It was important to determine whether the force of endosmose varied when the skin was taken from different regions of the body. The experiments made to ascertain this were not very numerous; and we shall content ourselves by saying that no difference was observed in the current of endosmose, whether we used the skin which, in the torpedo, covers the electric organs, or that which covers the back; and that we detected no difference by employing the skin of the belly or that of the back of the frog.

Endosmose of different Liquids. — We undertook a long series of experiments to determine the respective forces of endosmose with different liquids, through the three skins above mentioned. For this purpose three endosmometers were simultaneously prepared; one with the skin of the torpedo, a second with the skin of the frog, and a third with eel skin; in all the cases the skins were placed with their external surface towards the interior of the instrument, which contained sometimes solution of sugar or of albumen, sometimes gum water or alcohol. The endosmometers were plunged into a glass vessel filled with spring water: this arrangement presented the advantage of enabling us to observe immediately the difference in the height of the liquids when traversing the three kinds of skins. The following table shows the intensity of endosmose of each of the liquids when traversing the different skins: —

Relative Intensity of Endosmose through different Skins.

	Skin of Torpedo.	Skin of Frog.	Skin of Eel.
Solution of sugar -	100 millim.	25 millim.	15 millim.
" " albumen -	30 "	15 "	8 "
" " gum -	120 "	22 "	6 "
Alcohol - -	35 "	80 "	55 "

This table proves,

1st. That with the skin of the torpedo the current of endosmose is strongest when we use a solution of sugar, of gum, or of albumen, for the internal liquid.

2dly. That with these same liquids it is less with the skin of the eel than with that of the frog.

3rdly. That we have a current of endosmose from water to alcohol, stronger with the skin of the frog than with the eel skin, and with the latter more energetic than with the skin of the torpedo.

4thly. That this current through the skin of the frog still continues strongest from the water to alcohol, although the skin be not placed, with respect to the liquids, in the manner most favourable for the production of the phenomenon.

5thly. That the intensity of endosmose for the same skin varies for each liquid; and for this reason, these liquids ought to be arranged for the different skins in the following order, commencing with that which gives the strongest current, and proceeding to that which yields the weakest:—

Order of Intensity of different Liquids for different Skins.

Skin of the Torpedo.	Skin of the Frog.	Skin of the Eel.
Solution of gum	Alcohol	Alcohol
“ sugar	Solution of sugar	Solution of sugar
Alcohol	“ gum	“ albumen
Solution of albumen	“ albumen	“ gum

These later results prove, that the order in which Dutrochet arranged these liquids, relatively to the intensity of endosmose which takes place between them and water, ought not to be considered as constant for every case.

Class 2. *Experiments with the Gastric Membrane of the Lamb, Dog, and Cat, and Gizzard of the Fowl.*—We shall reserve our general conclusions from what we have now stated, and pass on to the observations we have made when using membranes which we have placed in the second class; namely, the stomach of the lamb, the dog, and the cat, and the gizzard of the fowl.

In all our experiments we carefully removed every portion of muscular fibre from these organs before applying them to the endosmometers; so that we used the mucous membrane only. The greatest number of our experiments were made with stomachs taken from these animals immediately after death: where it was otherwise, we shall mention it.

Stomach of the Lamb.—In using the stomach of the lamb, prepared, as we have already stated, with a solution of sugar in the interior of the two endosmometers, and placing the membrane with its internal surface (namely, that which is naturally turned towards the interior of the cavity of the stomach) directed towards the interior of the instrument, the elevation of the liquid was 56 millimetres in one case, and 54 in another; but in the reverse position of the membrane it was 72 millimetres in the first, and 66 in the second. These two experiments lasted but one hour and a quarter; endosmose was then favoured by employing a solution of sugar, and was directed from the interior to the exterior of the stomach.

The contrary takes place in making use of the solution of white of egg. When this liquid was in contact with the internal surface of the stomach, it rose in the instrument 23, 28, and 35 millimetres in the space of two hours as usual.

But when we introduced into the endosmometers a solution of gum Arabic, the elevation in the two opposite positions of the membrane was sometimes nothing, sometimes equal, and only 8 millimetres in the two instruments; in some cases there was in one, 12 millimetres, when the internal surface of the membrane was in contact with the gum solution; and 8 millimetres, when the position was reversed. The intensity of the endosmose between the water and the gum solution is excessively weak when it is exerted through the mucous membrane of the stomach of the lamb; and it is necessary, therefore, to prolong the experiment beyond the ordinary time, in order to obtain a sufficiently obvious elevation. Those we have now mentioned, were obtained after more than four hours experimenting. It must also be observed, that the current of endosmose through the membrane soon ceases when we employ the two liquids now referred to. Indeed, it often happens that the solution of gum, after having attained a slight elevation, does not exceed this at the end of two hours, nor even by prolonging the experiment for many hours.

The favourable position for endosmose between water and the solution of sugar, which we have observed when we use the stomach of the lamb, is not the same when we employ the stomach of the cat and the dog; with the stomach of the cat, the elevation to which the solution of sugar reached in the tube of the instrument was either 30 millimetres or 15 millimetres, according as the internal surface of the membrane was placed towards the interior of the instrument, or had the reverse position. With the stomach of the dog, the elevation was, in the first case, 68 millimetres, and, in the second, 3 millimetres.

Stomach of the Cat. — With the stomach of the cat, endosmose from water to the solution of gum is also directed from the external to the internal surface of this organ. Thus, when the mucous surface of the membrane is in contact with gum water, the elevation reaches 38 millimetres, and when in the other position, only 14. This difference is observed only when the stomach is very fresh; should the animal from which it has been taken, have been dead some time, we observe that, at the commencement of the experiment, there is a slight elevation sometimes equal in both instruments;

sometimes greater, sometimes less, in the one than in the other; but the liquid soon descends. By changing the position of the liquids, that is to say, by putting the solution of gum outside, and the pure water within the instrument, the water descends.

Stomach of the Dog. — The same phenomena were produced when we employed the stomach of the dog. We have not made any experiments with the stomach of the latter animal immediately after its death, using for the internal liquid an albuminous solution. The results which we have now explained were made several hours after the death of the animal. The albuminous solution rose to an equal height in the two instruments in four different experiments. In one of them, which we select from amongst the others, this elevation was 20 millimetres in one hour; and it did not vary for three hours more, in the endosmometer where the internal surface of the stomach was towards the interior of the instrument, whilst in the same interval of time it fell 25 millimetres in the other endosmometer. In general, it rarely occurred that the column remained stationary in either of them. In most cases (we always refer to the membrane of the stomach of the dog when it is not fresh) the liquid descended in both instruments after having manifested a greater or less elevation; but the diminution of height was double, and often treble, in the endosmometer in which the external surface of the membrane was turned towards the albuminous solution. By reversing the position of the liquids, and placing the solution of white of egg outside, and the water inside ıe endosmometers, we found that the internal liquid escended equally in both. These depressions are caused y the cessation of endosmose, in consequence of the terations supervening in the structure of the membrane me time after death; but the influence of the position the two surfaces continues, to a certain point, even the altered membrane. We have, indeed, remarked at the fall of the albuminous solution is double, and en treble, in the endosmometer where the external urface of the membrane is towards the inner side of e instrument.

Gizzard of the Fowl. — With the mucous membrane the gizzard of the fowl, using a solution of sugar and re water, endosmose is stronger from the external to e internal surface of the membrane, though, in eneral, the difference of the elevation between the quids of the two endosmometers is not very great. hus, when the internal part of the membrane was wards the interior of the instrument, the elevation as 48 millimetres, whilst it was 43 in the contrary sition. It is not unusual to see in the first position a rtain elevation, as of 17 millimetres, of 20 millimetres, c., whilst in the second the liquid remains unmoved. e ought, also, to mention the promptitude with which e current of endosmose from water to a solution of gar ceases, through the gizzard of the fowl. Gene-lly the liquid column becomes stationary in both bes at the end of two hours at most.

Endosmose between water and the albuminous solution through this membrane (the gizzard of the fowl) seems to take place indifferently, whatever be the position of the surfaces with reference to the two liquids. We have proved this fact several times. In a solitary instance we saw the liquid rise 15 millimetres in the endosmometer, where the internal surface of the membrane was towards the interior of the instrument, whilst the liquid in the other instrument only rose 5 millimetres. We obtained the same results with a solution of gum as with the albuminous solution. In both positions of the mucous membrane of the gizzard of the fowl, the elevation of the liquid was also the same, when we prolonged the experiment several hours. When, in some rare cases, we remarked a slight difference, at most of 1 or 2 millimetres, it was always in that endosmometer in which the inner surface of the membrane was in contact with the solution of gum.

To complete our account of the results obtained by the employment of the membranes in this second class, it only remains for us to add the phenomena observed when we employed alcohol for the inner liquid, putting it successively in contact with each of the surfaces of these membranes. With the stomachs of the lamb, the cat, and the dog, endosmose was invariably directed from the water to alcohol, and was promoted from the internal to the external surface of the membrane. Indeed, we have seen in the endosmometer where the external surface of the mucous membrane of the stomach of the lamb was turned towards the interior of the instrument which contained alcohol, the elevation attained 88 millimetres, and only 10 millimetres in the ntrary position; afterwards the liquid in the first endos- ometer rose 40 other millimetres, and remained station- y, and sometimes even fell in the second instrument.

With the stomach of the cat, alcohol rose 22 milli- etres in the tube in two hours, when the external sur- ace of the membrane was turned towards the interior of e endosmometer, but with a contrary arrangement it id not rise more than 2 millimetres. Sometimes even hen, in the first position of the membrane, the ele- tion was from 20 to 24 millimetres, in the second, ere was no elevation.

With the stomach of the dog, the elevation of alcohol the tube, was 24 millimetres when the mucous surface as in contact with water, but was only 16 millimetres hen placed in the reverse position. Six hours after, e liquid rose again 40 other millimetres in the first se, and 25 millimetres only in the second one. In other experiment, after the time mentioned, the ele- tions were 130 millimetres and 6 millimetres.

With the stomachs which we have hitherto employed, dosmose, which is promoted from the internal to the ternal surface of the membrane, always takes place om water to alcohol, as in Dutrochet's experiments. is remarkable that, with the internal membrane of e gizzard of the fowl, endosmose takes place in the ntrary way, namely, from alcohol to water; and this lds good, whatever be the position of the mem- ane with respect to the two liquids. This exception, hich we at first attributed to some defect in the embrane employed, we have repeatedly verified, sometimes by introducing, as usual, the alcohol into the interior of the instrument, in which case we have seen the alcohol fall below the level, and sometimes by placing it externally, when the water constantly mounted in the tube. The influence of the position of the membrane is equally evident in this case. We shall commence by giving the diminutions of height marked in the case where the alcohol was in the interior of the instrument. When the internal surface of the mucous membrane was towards the interior of the endosmometer, the diminution of the height of the alcohol was from 24 to 28, and even more than this, in the space of six hours, whilst it was only 11 and 12 millimetres in the other position. In another experiment, which we have selected out of a very large number, the pure water being placed in the interior of the instrument, the elevation was 32 millimetres when the external surface of the membrane was towards the interior of the endosmometer, and 16 millimetres in the other position, in about the space of three hours. Consequently endosmose between alcohol is promoted from the internal surface to the external surface of the gizzard of the fowl.

Class 3. *Bladder of the Ox.* — In the last place, we pass to the exposition of what we have observed when employing for the interposed membrane, the mucous lining of the bladder of the ox, in the fresh state, and deprived of the muscular layers, as in the case of the stomachs. When this membrane was employed, and a solution of sugar introduced into the interior of the two endosmometers, the height which the liquids attained in the

ibes was, when the internal surface of the membrane
as in contact with the saccharine liquid, 80, and even
13, millimetres in the space of two hours; but it was
ly 63, or 72, millimetres when the position of the
embrane was reversed. The current of endosmose,
erefore, is promoted in this instance from the ex-
rnal to the internal surface of the membrane. The
ntrary effect is obtained with a solution of gum Ara-
c. The elevation is 18, and sometimes only 7, milli-
etres, when the internal surface is turned towards the
terior of the instrument containing the gum solution;
hereas, when the membrane is arranged the reverse
y, the elevation is 52 millimetres, or, in some cases,
millimetres.

With the solution of gum Arabic, we sometimes saw
e liquid first fall in both tubes, and after a cer-
in time, rise to heights which are pretty nearly the
me as those observed with a solution of sugar. In
e case the liquid fell in both instruments 7 milli-
etres during the first hour; after that time it began
rise again; and three hours later the elevation was
millimetres in the endosmometer where the internal
rface of the membrane was in contact with the solu-
tion of gum, and 8 millimetres in the endosmometer
ere this surface was in contact with the water. With
albuminous solution and pure water, endosmose does
t take place through the mucous membrane of the
dder of the ox in a fresh state: the liquid falls in
th the tubes, whether the interior of the instrument
ntains the albuminous solution or pure water. Yet

it should be mentioned, that when the inner surface of the membrane is in contact with the albuminous solution placed outside the instrument, the diminution of height is less than when the position is reversed; and that the contrary takes place when this solution is in contact with the external surface of the membrane.

Lastly, with alcohol and pure water there is endosmose from the water to the alcohol, as in most cases; but the elevation is sometimes 24 millimetres, sometimes 59 millimetres, when the external surface of the membrane is in contact with the alcohol, and sometimes 26, or 37, millimetres in the reverse arrangement.

Condition of the Membranes. — Some differences, as obvious as those observed when using fresh membranes, disappear entirely, or nearly so, when we employ membranes dried or altered by a more or less advanced state of putrefaction. We have not much varied the experiments proper for determining the influence of the condition of desiccation and putrid alteration of membranes, and we intend hereafter to resume our examination of this subject. It is, however, certain that when employing the ordinary liquids, and interposing between them and pure water the dried bladders of a pig and an ox, which have been moistened before the experiment, so as to enable them to be applied to the endosmometer, that there is either no difference in the elevation of the liquids in the two tubes, even after several hours, whatever be the position of the surfaces of the membrane; or a very slight difference in the instrument, in which the internal surface of the bladder is towards the interior of the endosmoıeter, sometimes in the other one. When employing ladders which have been left for some hours in water, e occasionally observe a certain regularity of effects, with fresh bladders; but if we employ them very ct, after they have soaked a whole night in water, e observe no elevation in the liquids of the endosıometers, or the elevation, which is always slight, is qual in the two tubes. We can, in some cases, exain the anomalies presented by the bladders in this ate. Thus any one can perceive on a wet bladder, ıat the muscular fibres are swollen, in proportion the time the bladder has been in water. These uscular fasciculi acquire thus a certain thickness, ıey approximate to one another, and acquire, in some gree, a condition analogous to that of freshness. ut we have several times seen that endosmose does ɔt take place with bladders, gizzards, and fresh stoachs, from which the muscular laminæ had not been moved. If the bladder be slightly moistened, the uscular bundles are, it is true, a little more expanded, ıt nevertheless there exist between them interstices rough which endosmose certainly takes place: the equality of the interstices, however, even in two mmetrical portions of the same bladder, must produce gue and uncertain results.

We have only employed the gizzard of the fowl in a ɔre or less altered state, in order to determine what fluence putrefaction has over the phenomenon of ensmose; a great uncertainty exists with respect to the sults furnished by the gizzard in this state. Sometimes, indeed, the liquid did not pass at all, sometimes it had an equal elevation in both instruments. Whatever were the liquids employed, or the position of the membrane, endosmose was energetic, but sometimes in one direction, and sometimes in the other; and occasionally, indeed, it was depressed to the level of the liquids in both instruments. In speaking elsewhere of that which we observed in making use of the skin and gastric mucous membrane of certain animals, we have remarked that the phenomena of endosmose vary according as we employ these membranes immediately after death, or some hours subsequently. All these facts demonstrate clearly the intimate relation which exists between the phenomenon of endosmose and the physiological condition of the membranes.

The phenomenon of endosmose, like every thing going on in organised tissues, is devoid of that constancy and regularity observed in physical phenomena elsewhere. To this variable and accidental organic condition of the fresh membranes must be ascribed the singular fact, that while in certain cases we obtain an elevation of perhaps 80 millimetres, yet sometimes with the same liquid, the same membrane, and in the same relative position, the rise does not exceed 10 millimetres. We must also ascribe to a constant anatomico-physiological condition, connected with the function of the same membrane, that constant difference of elevation in the two different positions of the membrane, whatever this difference may in other respects be. It is important to study the phenomenon with the view of recognising he accidental circumstances which cause the variation f endosmose through the fresh membranes; as, for xample, the privation of nourishment in relation to he stomach, the administration of certain substances to he animal before killing it, &c. With this object we nade one comparative experiment only, which induces s to believe that endosmose through the skin of the eel s most energetic when the skin has been removed after he animal has been for some time out of the water.

Conclusions.—The novelty and importance of the esults we have obtained, must be my excuse for relating hem in this extended form. The general conclusions vhich we have deduced from them are as follows:—

1st. The membrane interposed between the two quids is very actively concerned, according to its naure, in the intensity and direction of the endosmotic urrent.

2ndly. There is, in general, for each membrane a ertain position in which endosmose is most intense; nd the cases are very rare in which, with fresh memrane, endosmose takes place equally, whatever be the lative position of the membrane to the two liquids.

3rdly. The direction which is most favourable to enosmose through skins, is usually from the internal to e external surface, with the exception of the skin of e frog, in which endosmose, in the single case of water d alcohol, is promoted from the external to the internal rface.

4thly. The direction favourable to endosmose through stomachs and urinary bladders varies, with different liquids, much more than that through skins.

5thly. The phenomenon of endosmose is intimately connected with the physiological condition of the membranes.

6thly. With membranes, dried or altered by putrefaction, either we do not observe the usual difference arising from the position of their surfaces, or endosmose no longer takes place.

Exosmose.—To give an accurate account of the subject of our experiments, and of the conclusions we have drawn from them, it is necessary to consider *exosmose* in a point of view different from that in which it has hitherto been regarded. The augmentation of volume presented by the internal liquid, which is usually the denser one, is considered by Dutrochet as the result of a difference between the *in-going strong current and the out-going weak current.* According to this view, that liquid which receives from the other one more than it gives, should increase in proportion to the excess which it receives, or to the difference between the strong current and the weak one. But all the facts which we have observed, lead us to the conclusion that different membranes allow the passage of water to the liquid in the endosmometers more easily in one direction than in another, and more readily with some liquids than with others. But a great number of difficulties are met with in considering the phenomena in this manner. We refrain from enumerating them, as they must present themselves to any one who has followed us in the exposition of the facts we have observed. We shall merely observe that, in ascribing every thing o endosmose, the presence of the solution of gum, or f sugar, in the interior of the endosmometer, gives us o explanation of the phenomena which are observed with the internal membrane of the stomach of the lamb, nd with the mucous membrane of the bladder of the ox; nd that these phenomena are susceptible of a more asy and natural explanation, by assuming that, by xosmose, the various membranes give to the different quids a more or less easy passage towards the water, ccording to the surface with which these liquids are n contact; and by supposing that the passage of the water towards the denser liquid is always constant, in ccordance with the almost general law of endosmose. But it was necessary to have recourse to experiment o determine whether our mode of considering the henomenon was correct; and it was requisite not only o prove the existence of exosmose, as M. Dutrochet ad done, but also to measure it in the same manner s endosmose.

In these researches we preferred using the skins of ogs and eels, and employing salt water as the denser quid, and also, in some cases, a solution of sugar.

We began by preparing two endosmometers in the sual way; in one, putting the skin of the frog or l with its internal surface towards the interior of the strument; and, in the other, placing the membrane in e contrary position. We introduced into the two ensmometers equal volumes of salt water of known density, and plunged these instruments into two separate glass vessels, each containing a volume of distilled water equal to that of the salt water. After some hours, we carefully measured the volumes of liquid contained in the endosmometers, and also those of the distilled water remaining in the two vessels, and thus we found which of the two liquids had risen most in the tubes. We observed that endosmose from water to a saline solution through these skins, was most promoted from the internal to the external surface. By determining the density of the liquids contained in the two vessels, we ound that in the endosmometer, *in which the volume of salt water was most increased, the density of the liquid was preserved better than in the other; and,* vice versâ, *in the vessel in which the diminution of distilled water was greatest, the quantity of the saline solution introduced by exosmose was less than in the other vessel from which a smaller volume of distilled water had disappeared.*

In the following table are given the numbers furnished by two of the numerous experiments which we made, and which led us to these results. The first column indicates, in tenths of cubic centimetres, the volumes of the liquids in the endosmometers after the experiment; the second column, the weight of a given quantity of these liquids; the third, the volumes of distilled water found in the two external vessels after the experiment; and the fourth, the weight acquired during the experiment by a given quantity of water in these same vessels. The weight of the same quantity of the saline solution, before the experiment, was

17·350 grammes; and that of an equal quantity of distilled water 16·025 grammes.

Experiments with Water and a Solution of Common Salt.

I. Volumes of the Liquids in the Endosmometers after the Experiment, in cubic centimetres.	II. Weights of a given Volume of the Liquids.	III. Volumes of distilled Water in the outer Vessels after the Experiment, in cubic centimetres.	IV. Increase of Weight, during the Experiment, of a given Volume of Water in the outer Vessels.
Skin of the Frog.			
150 c. c.	17·835 gr.	112·5 c. c.	16·105 gr.
149	17·680	113·5	16·405
Skin of the Eel.			
222·5 c. c.	17·145 gr.	200 c. c.	16·170 gr.
217·5	47·130	205	16·220

In some cases, we precipitated the chlorine of the salt contained in the two external vessels, by means of nitrate of silver. The last column of this second table gives the quantity of chloride of silver thus obtained.

Experiments with Water and a Solution of Common Salt.

I. Volumes of the Liquids in the Endometers after the Experiment.	II. Weights of a given Volume of the Liquids.	III. Volumes of distilled Water in Outer Vessels after the Experiment.	IV. Chloride of Silver obtained from the Salt found in the outer Vessels.
Skin of the Frog.			
172 c. c.	17·190 gr.	160 c. c.	190 gr.
171	17·175	161	280

We have observed a similar result with a solution of sugar and the skin of the eel; the weight of a given volume of the saccharine solution was, before the experiment, 18·180 gr.

Experiments with Water and a Solution of Sugar.

I. Volumes of the Liquids in the Endosmometers after the Experiment.	II. Weights of a given Volume of the Liquids.	III. Volumes of Distilled Water in the Outer Vessels after the Experiment.	IV. Increase of Weight, during the Experiment, of a given Volume of Water in the outer Vessels.
Skin of the Eel.			
193 c. c.	18·035 gr.	181 c. c.	16·045 gr.
191	18·010	183	16·050

These facts cannot be explained by assuming that the elevation and increase of volume of the liquid of the two endosmometers arise merely from the difference between the current of endosmose and that of exosmose. If it were so, the endosmometer in which the largest quantity of saline solution had accumulated, ought to contain a liquid less dense than that in the other which presented a less augmentation of volume. These facts may be, on the contrary, completely explained, by assuming that the current of endosmose has been equal, or nearly so, in the two positions of the membrane, and that the difference depends altogether in the current of exosmose, which is weakest in that endosmometer in which the elevation is most considerable, and stronger in the one in which the elevation is the slightest. These results give a great importance to the influence of the membrane interposed between the two liquids in the phenomenon of endosmose; for, by its particular nature alone and its physiological function, we can explain the more or less easy passage of different denser liquids towards other less dense ones through the membrane itself.

Endosmose applied to Physiology.—We certainly feel the necessity of having recourse to other experiments in order to exhaust the subject of endosmose, which must play so important a part in all the acts of organised beings. It is certain that the results which we have obtained in this series of experiments, and the view we have taken of endosmose, lead to a more correct application of the phenomenon of endosmose to the functions of organised beings.

Endosmose promotes the Mucous Secretion of the Skin.—The exosmose of a solution of sugar, of albumen, and of gum, towards water, is promoted from the internal to the external surface, in all the skins examined. It is precisely in this same direction through the skin of the torpedo, the eel, the frog, and other animals, that a copious secretion of mucus takes place. The endosmose of water to a solution of sugar, of gum, and of albumen, is less intense from the external to the internal surface of the skin than when it takes place by the reverse arrangement. Consequently, if we do not admit that this secretion of mucus, and this weak absorption of the water wherein these animals live (functions which, for their normal performance, ought always to bear a certain relation to each other), are entirely due to the phenomena we have discovered; at all events, we cannot deny that they must be promoted by it. Undoubtedly this function of the skin would not go on, or would do so imperfectly, if in these animals which live constantly in water this membrane acted endosmotically in an opposite direction to that which we have found it to do.

Endosmose in relation to the Function of the Stomach.—This constancy observed in the direction most favourable for endosmose and exosmose through skins, does not hold good for the mucous membrane of the stomach of different animals. But every one knows how much more complicated the function of the stomach is, and that all the substances introduced into this organ are either not absorbed, or are absorbed unequally. Moreover, we repeat, that this subject requires elucidation by fresh experiments. When we remark that the direction most favourable to endosmose between water and a saccharine solution, for example, is not the same for the stomach of a ruminant as for the stomach of a carnivorous animal; it clearly follows that the phenomenon of endosmose must be intimately connected with the great differences which exist in the digestive functions of these two orders of animals. I am anxious to explain to you all the details of the experiments which I made with Professor Cima on the subject of endosmose, being well convinced of the great importance of this phenomenon in the vital functions.

Ovules nourished and Spermatophora opened by Endosmose.—It is by endosmose that physiologists now explain the manner in which the nutrition of the ovules in the oviducts of mammalia is effected; and how the sacs, which contain the sperm of the cephalopodous molluscs (or e spermatophora) open immediately they are brought contact with water.

Endosmose of Cells.—A cell is the elementary organ f all animal and vegetable tissues, and cell-life involves n act of endosmose: this shows how much the phenoenon of endosmose requires to be more completely tudied, in order that we may be enabled to make of it ll the applications of which it is susceptible.

Endosmotic Action of Purgatives.—I cannot conclude is lecture without referring to the recent experiments f Poiseuille, made with the view of explaining by dosmose the purgative action of certain substances. e found that there was endosmose through animal ssues from the serum of the blood to Seidlitz water, d to solutions of sulphate of soda and common salt. ow this is precisely what happens when we use these edicines internally: the rejected excrements contain abundant quantity of albumen. In this case we must mit that endosmose takes place through the capillary ssels of the intestine, from the serum of the blood to e saline solution introduced into the alimentary canal.

Endosmose of Liquids in Motion.—But to remove all ubt of the propriety of Poiseuille's applications of this et, it was necessary to demonstrate that endosmose kes place when one of the liquids is in motion, and being continually renewed. This has been recently ne by Dr. Bacchetti, who has shown that the rapidity endosmose is considerably augmented when one of e liquids was continually renewed. This result, oreover, is in accordance with the principles of the theory of endosmose: the interchange of liquids, constantly going on through the membrane, leads to the suspension of the action of endosmose; or, in other words, the conditions for the production of the phenomenon are so much the better preserved, as the liquids remain longer without mixing. Poiseuille has also shown that endosmose ceases to take place in a membrane after a certain time of action, but that the membrane re-acquires this property by submitting it to the action of other liquids.

Remarkable Influence of Morphia.—The most remarkable fact discovered by Poiseuille is, that of the influence exercised by hydrochlorate of morphia. This substance, when added to saline solutions, very considerably weakens the endosmose from the serum to the solution; and ultimately changes the direction of the current. This fact has been confirmed by Dr. Bacchetti. How can we make an entire abstraction of this fact in the explanation of the action of morphia, and of the preparations of opium in diarrhœa, as well as of the constipation which they produce?

LECTURE IV.

ABSORPTION AND EXHALATION IN ANIMALS AND PLANTS.

GUMENT. — *Absorption in animals* consists of two acts, imbibition and
transmission. All vessels absorb; proofs that blood-vessels absorb;
proofs that the lymphatics and lacteals absorb. Physical conditions of
absorption. Laws of absorption.
halation; its mechanism similar to that of absorption. Transform-
ations effected during absorption and exhalation.
sorption in plants; summary of facts concerning it. The movement
of the juices of plants inexplicable by capillarity and imbibition merely.
Sponglets. Evaporation or transpiration by the leaves. The ascent
of liquid in plants depends on both the root and leaves.

IE preceding lectures on the phenomena of capillarity,
ibibition, and endosmose, have been delivered prin-
pally for the purpose of preparing you for the study
absorption and exhalation. It is not for me to speak
the researches that have been made expressly for the
rpose of determining on which of the various organs
ese functions especially devolve. In treatises on
ysiology, you will find them exclusively ascribed
netimes to the veins, sometimes to the lymphatic
ssels.

We find it difficult to account for so many discussions
ien we reflect on the structure of all these different
sues, and on the necessary existence of the pheno-

mena of absorption and exhalation in a large series of the lower animals apparently devoid of lymphatic vessels.

Absorption in Animals consists of Imbibition and Transmission.—Absorption, considered as a function of living animals, consists not merely of the imbibition of a liquid by a tissue, but also of the passage into the blood-vessels of the liquid with which such tissue is in contact. It is at the blood that the absorbed matter ought to arrive; this is the final object of the phenomenon. Let us distinguish, then, two things in absorption: the introduction of the substance to be absorbed through the interstices of an organised body, and its subsequent passage into the circulation.

It is easy to demonstrate the existence of the first part of this function. Here is a frog, whose inferior extremities only, have been immersed for several hours in a solution of ferrocyanide of potassium. If we remove the animal from the liquid, carefully wash it with distilled water, and then cut it in pieces, we can easily prove that the solution has penetrated into every part. Wherever we touch the viscera or tissues with a glass rod moistened with a solution of the chloride of iron, a more or less deep blue stain is produced.

I shall the more insist on this manner of demonstrating the reality of absorption, because it explains to us very clearly the two parts of which we have stated this function to consist. If a living frog be immersed by its inferior extremities only, in a solution of ferrocyanide of potassium, and the animal soon after killed, we can scarcely detect any traces of the salt in the nuscles of the legs and thighs; whereas the heart and ungs give very distinct evidence of it when they are ouched with chloride of iron.

One experiment more, and the conclusion will be evilent. I immerse another frog, which has been dead for ome minutes, in the same solution, and leave it there or the same time that I did the other. When tested, he lungs and heart offer no greater evidences of the resence of the ferrocyanide than does any other part f the body.

Here is the explanation of these experiments:—he solution was introduced into the body of the frog imply by imbibition; and this phenomenon, being ffected in the living as well as in the dead frog, cerainly cannot be regarded as different from the imbibiion which we have already studied, which belongs to oth organic and inorganic bodies, and which is the conequence of their cellular and vascular structure, &c.

But there is something more than this. In the heart nd lungs of a living frog we find a much larger quanity of the absorbed solution than in the other parts of he body, although these latter were much nearer the art immersed. These viscera are the centre of the cirulatory system: in them commence or terminate the runks of the blood-vessels. The solution of the ferroyanide, therefore, has penetrated the blood-vessels by mbibition, mingled with the blood, and thus arrived at he heart and lungs.

We have another very simple experiment proving he same facts:—I take two frogs, and from one remove the heart; the animals are equally lively. Both are placed in a large glass containing a solution of the extract of nux vomica. The animal with the heart is soon poisoned, and long before the other becomes affected.

All Vessels absorb.—It has for some time been a subject of discussion whether the lymphatic vessels or the veins were exclusively endowed with the power of absorption: that is, whether a substance could be directly introduced into the blood-vessels by passing through the tissue of their sides; or whether it must necessarily first pass through the lymphatics. As every part of an organised being more or less easily imbibes water, saline solutions and serum, it is clear that the first part of absorption may take place through the sides of the lymphatics, as well also as through those of the blood-vessels. Microscopic anatomy, by unveiling the manner in which the blood-vessels and lymphatics terminate, has confirmed the preceding conclusion. I feel bound to cite here the principal results of the observations of our countryman Panizza.

There is no fact which demonstrates the existence of free extremities in the ramifications of blood-vessels, which everywhere present a very close and continuous reticulated texture. The arterial network is uninterruptedly continuous with the venous network which, in general, predominates over the former. The lymphatic system, likewise, never terminates by independent extremities, but everywhere presents the aspect of a very fine and close trellis work. Anatomy, which agrees with physiology, leads us to the conclusion that the first art of absorption can be effected only by the aid of e porosities proper to the structure of organised eings. In this way the absorbed matters arrive at, d mix with, the blood, the chyle, and the lymph, and e carried away by these liquids, and distributed over e body.

Absorption by Blood-Vessels.—I consider it now most superfluous to quote the experiments of Ma- endie, Segalas, and the later ones of Panizza, all of hich prove that absorption takes place principally by e intervention of the blood vessels.

The following is the manner in which the last-men- oned physiologist proceeded:—Having made an in- sion ten inches long in the belly of a horse extended the ground, he drew out a fold of small intestine, in hich arose several small veins, which, after a short urse, terminated in a single very large mesenteric nk, before any small veins from the glands had ptied themselves into it. This fold, nine inches in gth, was tied by a double ligature in such a manner at it could receive blood by a single artery only, and uld return none to the heart except through the nous trunk above described. An aperture was then de into the fold for the purpose of admitting a brass be, which was so fastened by means of thread that substance to be introduced should not touch the eding edge of the opening. This being done, a ature was passed under the vein receiving the blood m the fold. The ligature was tightened; and, in ler that the circulation should not be stopped, the vein was opened to allow the escape of the returning blood. Then, by means of a glass funnel and the brass tube, some concentrated hydrocyanic acid was introduced into the fold, and the tube then closed. The venous blood returning from the intestine was immediately collected, and found to contain hydrocyanic acid; but the animal presented no symptoms of poisoning, notwithstanding that the nerves and lymphatics remained untouched.

In another experiment Panizza, instead of tying and opening the venous trunk where the small veins discharged themselves, merely compressed it at the moment the hydrocyanic acid was introduced. There was no symptom of poisoning; but, shortly after the removal of the pressure, symptoms of poisoning appeared; and, the vein being opened, the contained blood was found to be impregnated with the acid.

Lastly, in a third experiment, Panizza quickly, but carefully, removed, all the lymphatic vessels and nerves of the intestinal fold; and, hydrocyanic acid being poured in, speedily destroyed the animal, provided that the vein was untouched. Venous absorption is thus proved by the most accurate experiments.

In some works on physiology it is stated that the fact of substances being detected in the urine, a few minutes after their introduction into the stomach, is opposed to the opinion that absorption takes place by means of the blood-vessels. But this objection soon falls to the ground, when we consider the rapidity of the circulation of the blood.

Absorption by Lymphatics and Lacteals. — On the other hand, that absorption can be also effected by the lymphatic vessels, is a fact too well known and too evident, to render a demonstration of it necessary. Kill an animal two or three hours after it has taken food, expose the intestines, and carefully examine the mesentery, and you will find that the chyliferous vessels are filled with a milky liquid analogous to that which flows abundantly from the thoracic duct, which is the principal reservoir into which these vessels discharge their contents. This liquid is the *chyle* which, by the act of digestion, is formed in the intestine, from which it is absorbed by the chyliferous vessels. How many examples has pathological anatomy furnished us with, in which these vessels have been found full of pus, by reason of their proximity to suppurating parts! The chyliferous and lymphatic vessels are, therefore, endowed with the power of absorption.

Physical Conditions of Absorption. — In a word, absorption is always effected under the following conditions:—

1st. A vessel with organic sides or walls.

2dly. An exterior liquid capable of being imbibed by the tissue composing the walls.

3rdly. An internal liquid, also capable of being imbibed by the walls; of intermixing with the exterior liquid; and of circulating in the vessel with more or less rapidity.

Nothing, consequently, can be more physical than a phenomenon thus constituted.

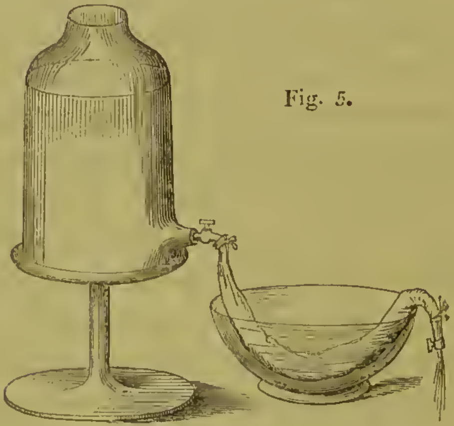


Fig. 5.

Apparatus to illustrate Physical Absorption.

I will demonstrate, by an experiment, the truth of this assertion. Here is a long piece of vein taken from a large animal; it is attached at one end to a tube connected with an opening in the lower part of the side of a glass bottle; the other extremity is tied to a small bent glass tube, furnished with a stop cock. I fill the bottle, and, consequently, the vein also, with water. I immerse a portion of the vein in water, acidulated with sulphuric or hydrochloric acid. At first the liquid in the bottle gives no indications of the presence of the acid; but after a certain time it does. If, instead of waiting some time, and leaving the liquid in repose, I open the stopcock, I can immediately detect the presence of the acid in the liquid which flows out; but in the bottle it is not yet discoverable. That which happens with a portion of vein, will take place in the same way with the arterial trunk, and with tubes of clay, pasteboard, and wood. If the acidulated solution be contained in the interior of the vein; and if into the liquid of the basin, in which the vein is immersed, we pour some tincture of litmus, the same phenomenon is observed; that is, the acid will pass out through the coats of the vein, with a facility proportionate to the velocity of the current. The conditions of the phenomenon are always the same: two liquids miscible with each other, and separated by a membrane capable of imbibing them, and the movement of the internal liquid which carries, in a given direction, the liquid which has traversed the membrane.*

Suppose for a moment that the direction of the circuation of the blood was inverse to that which it really s, but without any alteration in the structure and disosition of the blood-vessels: we should then no longer ay that the veins absorbed, but, on the contrary, that aborption took place by the arteries. Such is the very imple physical phenomenon of this function.

Laws of Absorption. — I am anxious also to demontrate to you the laws of absorption, which were disovered by experimental physiology; and you will then eadily perceive, that they are the necessary consequences f the principles which we have laid down.

1st. "The more the matters are soluble, divided, and

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* This apparatus I have long been in the habit of using in the lecture oom, (see my *Elements of Materia Medica*, vol. i. p. 112. 2nd edit. 1842). is more convenient in practice to have the vein connected with the ttle by a stopcock, as represented in the above figure, which differs this respect from the figure given in Matteucci's lectures. — J. P.

fitted for entering into combination with the organic juices, and for becoming constituent parts of the blood, the more easily are they absorbed."

The language used in stating this law is not very scientific, but I wish to give it to you as it is found in the most modern accredited works on physiology. This law is an evident consequence of the manner in which we have explained the phenomenon of absorption. It is desirable that physiologists should study with attention the various degrees of power with which different liquids are imbibed by organic tissues, for such study will certainly prove highly important to therapeutics.

Let me show you some facts which will assist us in understanding these researches. We have here two rabbits; about two hours ago some water was introduced into the stomach of the one, and some oil into that of the other. In the stomach of the first there remains not a trace of the water, but in that of the second we find all the oil; and it would still have been found there had we delayed opening the animal for several hours longer. If, instead of pure water, we had used a mixture of water and alcohol, the absorption would have been more rapid. An acid or saline solution would also have been absorbed, but not so rapidly as pure water.

"2d. The intensity of the absorbing power of different organs is chiefly dependent on the number of their vessels, the flaccidity of their tissue, and the conducting power of the parts which cover them."

I am repeating word for word what I find in works on physiology. It is evident that the terms *flaccidity* of tissue, and *the conducting power of the parts which cover them,* mean nothing more than that the texture of organic solids is more or less favourable to imbibition. *The greatest number of vessels* merely signifies the largest number of points of contact between the body to be absorbed, and the liquid with which it is to be mixed and carried away. This is the reason why the lungs, as we have seen, are the best fitted for absorption, and why they are the first to manifest the presence of the absorbed body. In fact, anatomy teaches us that the lungs, more than any other part of the animal economy, possess a structure fitted for imbibition, and a highly developed vascular system. The cellular tissue, likewise, is very permeable to liquids, but not being so well supplied with blood-vessels as the lungs are, absorption is there effected more slowly. The skin, on the contrary, being covered by epidermis, which is of a very compact texture, and devoid of blood-vessels, performs with difficulty this function; but, by removing he epidermis, we render the skin more capable of absorption.

"3dly. Absorption varies according to the quantity of liquid which exists in the organism, and is in the nverse ratio of the plethoric state of the animal."

If you remember the phenomenon of imbibition, you will easily comprehend this law of the function. A mass f sand, if saturated with liquid, cannot further imbibe; nd, on the other hand, imbibes so much the more apidly, as it is further removed from the point of aturation.

Dutrochet exposed a plant to the air until it had lost by evaporation 0.15 of its weight; and afterwards, by plunging it into water, he found that in each of the first four hours of immersion, it absorbed $1^{gr}.05$ (20 grains), and lost $0^{gr}.40$ (8 grains); subsequently it absorbed only $0^{gr}.45$ (9 grains), and lost as much by exhalation. Edwards found that frogs absorbed water the more rapidly in proportion as they had lost more of their weight by transpiration.

Magendie mentions that a dog, from which a large quantity of blood had been drawn, died rapidly from poisoning by strychnia; whilst another animal, into whose veins a large quantity of water had been injected, did not present symptoms of poisoning.

"4thly. Within certain limits, absorption is in proportion to the temperature of the absorbing body, and of the body absorbed."

Every one knows that warm drinks operate more quickly than cold ones. So also with imbibition, which, as we have seen, varies greatly according to the temperature. I have said, that this variation can only be effected within certain limits, for beyond these the structure of the organised body will be altered.

"5thly. According to Fodéra, the electric current favours absorption."

If we admit the experiments of this physiologist, it is not easy to explain the fact now stated, because, when we employ the electric current in imbibition, we do not observe this influence. The fact mentioned by Porret alone*, and which consists in the transit of water from the positive to the negative pole, may in some way explain the results obtained by Fodéra.

"6thly. Lastly, absorption varies according to the rapidity with which the liquid moves in the absorbing vessel."

It is unnecessary to state how this rapidity serves to carry more or less quickly, and to a given distance, the absorbed body. It is equally easy to understand, that as the molecules of the liquid contained in the vessel are the more frequently renewed, so the actions of affinity, which tend to promote the absorption of the body into the interior of the vessel, become more energetic. This, probably, is the reason why absorption is slower by the chyliferous and lymphatic vessels than by the veins. Hence, many coloured substances, alcoholic liquids, and saline solutions, introduced into the stomach, are found n the blood, without our being able to discover them n the chyliferous vessels and the thoracic duct. Friction on the skin, and the peristaltic motion of the bowels, n this way aid absorption by promoting the movement of the liquids in the vessels.

Exhalation. — The function of exhalation is, in general, effected by the same mechanism, and under the ame laws as those of absorption. Through the coats

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* The fact is alluded to by Dr. Faraday (*Experimental Researches* ı *Electricity*, 13th series, § 1646.), and has been made the subject of a aper by Mr. James Napier (*On Electrical Endosmose;* in the *Memoirs* nd *Proceedings of the Chemical Society*, vol. iii. p. 28.). — J. P.

of a vessel, capable of imbibing the contained liquid, a portion of it is constantly passing out, or *exhaling*. The portion which escapes varies according to the nature of the liquid; that is to say, according to the greater or less facility with which it is imbibed by the tissue of the vessel. In proportion as the sides of this vessel are externally more or less moist, so will the internal liquid flow out with more or less difficulty. The exhalation will be augmented if, in consequence of the very large quantity of contained liquid, the walls of the vessel are subjected to increased pressure. All these particularities of exhalation, which result from what we regarded simply as a physical phenomenon, and which depend on the same principles as absorption, are demonstrated by experimental physiology.

Edwards has proved, that cutaneous exhalation is, in some cases, ten times greater in dry than in moist air, and that it is doubled in passing from 0° to + 20° centig. [= 32° Fahr. to 68°]. He also found that transpiration was augmented by agitating the atmospheric air around the body of the animal. These results are evidently the very natural consequences of physical principles, too well known to be repeated here.

Transformations effected by Absorption and Exhalation. — Some of the phenomena of absorption and exhalation in living beings, are accomplished by the transformation of the absorbed or exhaled body. The liquid which is imbibed by a membrane, and exhaled from its opposite surface, is not identical with that which was placed in contact with the absorbing membrane. This happens in most of the cases of exhalation, and principally in the secretions.

We are very far from expecting to find, in the present condition of physico-chemical knowledge, an explanation of the phenomena of the secretions. We must admit that they form as yet one of the most obscure subjects of the animal economy. With respect to exhalation it may be observed, that in some cases it is effected by a kind of filtration. A liquid which contains in suspension insoluble particles, is separated by filtration into two portions: the liquid part, which is imbibed by the substance of the filter, and is strained; and the solid part, which remains on the filter. Anatomists know that by injecting into the veins or arteries a solution of gelatine coloured by very finely powdered vermilion, the solution becomes colourless when it percolates through the coats of the vessels. Every contusion of the skin produces a spot, the centre of which is of a dark blue, and the circumference of a green colour surrounded by yellow. In this case, the extravasated clot of blood is separated from the serum, which is imbibed by the neighbouring tissues.

Do not forget the fact which has been demonstrated to you respecting imbibition. Salt water becomes fresh by traversing a bed of sand; while a solution of carbonate of soda, filtered under the same conditions, becomes denser. Imbibition, capillarity, and the simple play of molecular attraction, can overcome affinities.

The old notion, therefore, according to which the secreting organs were considered as mere filtering apparatus, is not entirely without foundation.

In another lecture we shall find, that membranes, and in general all organised tissues, are capable of being traversed by gaseous bodies. Fodéra was the first to prove, that sulphuretted hydrogen, inclosed in a portion of the intestinal tube, diffuses itself throughout the body of the animal and occasions death.

Absorption in Vegetables. — A few words, also, on absorption in plants. Here, in these small glasses, are a great number of plants, more or less immersed in a very dilute solution of acetate of iron. Some of them are haricots [kidney-beans, *Phaseolus vulgaris*]; others beans [*Vicia Faba*]. Some have been deprived of their leaves; others have been cut in two, and immersed by their stalks only: these have been deprived of the extremity of the roots; those have been put into the liquid after the roots had withered. Lastly, others were completely dried before they were placed in the fluid.

By employing ferrocyanide of potassium as a test, we can demonstrate the ascent of the ferruginous solution in the interior of each of the plants, above the level of the liquid in which they are immersed.

We perceive, that in the living plant, furnished with leaves and roots, the liquid has been considerably elevated; in those that were withered, and which have recovered their freshness in the aqueous solution, absorption has been yet greater; while, lastly, it is most abundant in those which had been previously deprived of their roots.

Whatever may be the liquid employed, it is always absorbed by the plant, except in the case of some acid alkaline, or very concentrated saline solution, which, by altering and destroying the structure of the plant, and by coagulating its juices, cannot be absorbed by it.

Our best information on this subject is derived from Saussure's celebrated work, entitled, *Recherches Chimiques sur la Végétation.* Here is a summary of it:—

1st. The roots absorb aqueous solutions of salts, but in a much smaller proportion than water.

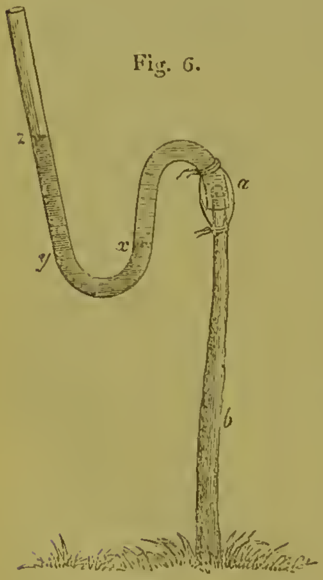
2ndly. Removal or alteration of the roots, and, in general, every thing which lessens the force of vegetation, favours the introduction of salts into the plant.

3rdly. A plant does not equally absorb all the salts ontained in the same solution. This is confirmed by he fact, that certain salts are invariably met with in some plants. Professor Piria has always found manganese in the seeds of *Lupinus albus.*

Let us now examine whether the absorption of the utritive juices, which takes place by means of the roots, and the movement of these juices in the plants hemselves, should be considered as simple phenomena f capillarity, or imbibition.

At the commencement of spring, the sap rises from he roots to the leaves, through the central part of the runk; and at this time a liquid, called the *succus proprius*, *proper juice,* whose composition is different from that of the sap (or *succus communis*), passes in a contrary direction; that is, from the leaves through the cortical tissues to the roots. If we bore a hole to the centre of the trunk of a growing plant, we can collect a considerable quantity of sap, which is denser in proportion as it is taken higher up towards the leaves. If, on the contrary, we pass a ligature around the trunk, or if we remove a circular layer of the bark, the swelling, which we shall soon see formed above the knot or ring on the side of the leaves, will prove the existence of the descending current of the *proper juice*. Hales has shown that the quantity of liquid absorbed by a plant in vegetation, increases in proportion with the superficies of its leaves; a fact which he explains by ascribing it to evaporation.

Fig. 6.



Hales's experiment to ascertain the force of the sap of the vine.

This double movement of juices in the interior of vegetables, is a thing inexplicable by the mere forces of capillarity and imbibition. But there is also something more. You have all, doubtless, seen, that in cutting across a vine stem in the spring, there runs out an enormous quantity of liquid. Hales, by fixing to the top of the stem (*Fig.* 6. *a*, *b*) a mercurial gauge (*a*, *y*) consisting of a curved glass tube (*a*, *x*, *y*, *z*), containing mercury, and open at the other extremity, observed that the mercury rose 38 inches (equal to 43 feet $3\frac{1}{5}$ inches of water) higher in the longer open leg (*y*, *z*) of the tube than in the other leg (*x*); and this he ascribed to the force of the sap which came out of the stem and forced up the mercury.

This *force of impulsion*, and the discharge of the liquid of the plant from a cut, are facts incompatible with the effects of capillarity and imbibition. A liquid rising in a capillary tube, cannot overflow the tube by the effect of the same force which raises it.

Dutrochet has demonstrated by a very simple experiment, that the force of impulsion which produces the ascension of the juice of a plant, has its seat in the ultimate extremities of the roots. This distinguished physiologist made successive sections of the trunk of a vine, approaching each time nearer the roots; and finally, he cut even the roots themselves contained in the ground, and he observed that the flow of sap continued. One of the ultimate filaments of the rootlet, when plunged in water, also allowed the juice to escape from it. It is then in the *spongelets* that the force of impulsion resides. Dutrochet adds that he has discovered in the cortical cellules of the spongelet a liquid denser than water, and coagulable by nitric acid. He, therefore, fancied that he had discovered in the spongelet, or rather in its cells, filled with a liquid denser than the water which surrounds them, a group of endosmometers; hence the phenomenon of the ascent of the liquid in the plant becomes a case of endosmose. It is desirable, however, that the identity of these phenomena should be better demonstrated than it has been by Dutrochet's observations. Be it as it may, the explanation given by this author is one which, in the present state of science, is the least improbable.

How does the juice ascend in a plant whose roots have been removed, and whose lower extremity has been plunged in water? The great height to which the liquid can ascend in the trunk of a tree, appears to be opposed to the explanation of the phenomenon by considering it as an effect of imbibition or of capillarity; phenomena which, we know, act within much narrower limits than those observed in the trunks of trees. Hales, finding that the quantity of sap which rises in a plant, was proportional to the superficies of its leaves, concluded that in consequence of the evaporation of the liquid contained in the superficial cells of the leaves, these absorbed, by capillary attraction, more liquid from the cells beneath, and that in this way a kind of suction gradually became propagated to the cut extremity.

By drying in different degrees some plants of the herb mercury (*Mercurialis annua*), Dutrochet ascertained that they did not absorb proportionally to their degree of dryness; for one of the plants which had lost a third of its weight by evaporation, absorbed much less than another which had only lost a tenth. Notwithstanding that the degree of dryness was greater, the absorption was less; and yet the plant had not been sufficiently dried for its texture to have become altered.

Evaporation or transpiration by the leaves is not then the cause of the ascent of a liquid in the stem of a plant plunged in water; or, what amounts to the same thing, it is not the cavities of the superficial cells which occasion the ascent of the juice. The latter is not effected without the presence in the vegetable tissue of a certain quantity of water, which perhaps acts by adhesion to the new water which should rise, just as occurs with a sponge which is more rapidly impregnated with water when it has been previously moistened than when it has been dried. Dutrochet also tried the effect of drying a plant, of allowing it to recover the water it had lost, and of plunging it again into that liquid; and he found that, although the water was restored, the ascent took place only when the plant had regained its original degree of turgescence. This turgescence of the leaf-cells takes place, according to Dutrochet, by an action of endosmose, in consequence of which the liquid is transpired by the leaves in an active manner, very different to that of a liquid which evaporates in the air. Lastly, I must remind you, that Dutrochet has demonstrated that the influence of light on the ascent of the sap in plants is exercised in respiration and in the fixation of oxygen in the vegetable tissue.

The phenomenon of the ascent of liquids in plants does not, therefore, depend solely on capillarity and imbibition. The cause resides principally in the roots, and in the next place in the leaves. It is probable that an action of endosmose occurs in the extremity of the roots; and it is not unnatural to suppose that a similar cause acts in the movements of the chyle and lymph in the lymphatic and chyliferous vessels — a movement which continues some time after death.

LECTURE V.

DIGESTION.

ARGUMENT. — *Digestion:* Its final object; changes effected by it.

Aliments are divisible into three classes.

Class 1. *Azotised substances;* albumine, fibrine, and caseine. — Mulder's proteine theory; digestion of azotised substances; gastric juice; movements of the stomach.

Class 2. *Amylaceous substances;* starch, sugar, and gum. — Digestion and conversion of starch into dextrine and sugar, and ultimately into lactic acid. Diabetes.

Class 3. *Fatty substances;* fat and oil. — Origin of fat in herbivora; division and liquefaction of fats in the stomach; use of the alkali of the bile and pancreatic juice; formation of an emulsion which is absorbed; agency of fat in the formation of cells.

Gases of the stomach and intestines.

Inorganic substances found in the organism.

Aliments. — THE existence and preservation of an animal are dependent on the continual introduction into his body of certain substances, called *aliments.* These, which are usually solids, undergo in the digestive apparatus a series of modifications, by means of which they are resolved into fæcal matters which are rejected, and other matters which mix with, and eventually become converted into, blood.

Digestion. — The ultimate object of digestion is the preservation of the integrity of the organism, by restoring to the blood the immediate principles which it is constantly losing during the act of nutrition. Reasoning leads us to suppose, that all parts of the organism undergo transformation and renewal with greater or less rapidity. We are led to this conclusion by a number of experiments presented to us by experimental physiology, but which require to be varied and extended.

The division of alimentary substances, the rendering of them soluble, and the consequent facilitation of their absorption, are, in short, the changes which take place during digestion. Nothing can be more physical than a function which is exercised merely to modify the physical condition of matter. It is desirable, however, to see this character of digestion verified in its details.

Before we begin to speak of the physico-chemical phenomena of this process, I must explain to you briefly some generalities.

Varieties of Aliments. — All alimentary substances may, with respect to their composition, be reduced to three well-characterised classes: the first comprises substances which are azotised and neutral, namely, albumen, fibrine, and caseine; the second includes fatty bodies; and the third comprehends gum, starch, and sugar, whose composition may perhaps be represented by water and carbon. Experiment has demonstrated, that the alimentary substances of the two latter classes are by themselves insufficient for the nourishment of an animal; and that they must always be conjoined with those of the first.

We shall hereafter find that the alimentary substances of these classes serve distinct purposes in the animal economy

Azotised Substances.—With respect to the substances included in the first class: I cannot pass over in silence the important discoveries recently made by Mulder and Leibig.

Albumen, fibrine, and caseine, are identical in their composition; the proportion of the carbon to azote, in all these three, being 8 equivalents of the former to 1 equivalent of the latter. They appear to differ from each other, merely in the small quantity of phosphorus and sulphur which they contain. If these two bodies be abstracted, there remains a principle common to the three, and which has been termed by Mulder, *proteine;* the formula of which, as adopted by Liebig, is

$$C^{48}\,H^{36}\,N^{6}\,O^{14}.$$

We must, then, regard these substances, although endowed with very different physical properties, as isomeric, and as modifications of proteine.*

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* Since the delivery of Matteucci's lectures Liebig's views respecting Mulder's theory of proteine have undergone an entire change, and he now declares this theory to be untenable and fallacious. In his *Researches on the Chemistry of Food* (1847) he observes, that "it now appears, as the result of the more accurate investigations of Laskowski, Rüling, Verdeil, Walther, and Fleitmann, that the amount of sulphur present in the blood constituents is three times, in many cases, four times, as great as the apparently well-established analysis of the author of the proteine theory had indicated. It further appears, that a body, destitute of sulphur and having the composition of proteine, is not obtained by the methods given by Mulder; that fibrine differs in composition from albumen; that the albumen of eggs contains not less, but more sulphur than the albumen of the blood." (p. 27.) He also asserts that, "the existence of phosphorus, as an essential element of [some of] these substances [fibrine and albumen], has not been in any way established." (p. 23.)—J. P.

The other important fact discovered by Dumas and Liebig is, that *vegetable albumen* is identical with *animal albumen;* that, in the farina of the cereal grains there exists a substance analogous to *caseine;* and, that in *gluten* there is a substance like animal fibrine.

There is not, then, any essential difference between the aliments of the herbivorous, and those of the carnivorous animals, except that the first is drawn from plants, the second from animals.

Since the composition of the blood, as well as the greater number of the animal tissues and fluids, is analogous to the organic neutral substances now referred to; and since, moreover, in becoming part of the animal organism they undergo no change of chemical composition, but merely acquire during nutrition a new form, it is fair to assume that, in the act of digestion, the azotised neutral alimentary substances merely dissolve in order to pass, without any other alteration, into the blood.

The isomerism of these substances is equally demonstrated by the beautiful discovery of Denis, that fibrine is converted into albumen by dissolving it in a saturated solution of nitre. This fact is the more curious, because it appears to hold good for the fibrine of venous blood only; the fibrine of arterial blood, neither dissolving in a solution of this salt, nor becoming converted into albumen. Scherer has tried the effect of exposing the fibrine of venous blood to an atmosphere of oxygen, and found that the oxygen was converted into carbonic acid, and the fibrine thereby lost its property of being convertible into albumen by a solution of nitre.

Some physiological experiments have long since proved, that the digestion of similar alimentary substances is a purely physical act, which takes place independently of the living organism. No one is ignorant of the celebrated experiments of our countryman Spallanzani: meat, gluten, and coagulated albumen, introduced into the stomach in perforated metallic tubes, were dissolved and digested, as though they had been free in the stomach. This solution is effected, as we shall presently find, by means of one of those actions of which we spoke in our first lecture, and which have been called *catalytic* or *actions of contact.*

The recent experiments of Melsens, and especially those of Bernard and Barreswil, have shown, that the gastric juice contains a free acid, which should be lactic acid[*]; and that it holds in solution a peculiar substance called *pepsine,* which has been obtained in a sufficiently pure state. It is this same substance which has been lately examined by Payen, who termed it *gastcrase.* The acidity of the gastric juice is greater or less, according to the quality of the aliments; in an empty stomach, it is weaker: it augments by contact with food, and it has its maximum when the elements are composed of fibrine, albumen, &c.

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[*] Liebig (*Researches on the Chemistry of Food,* p. 138.) infers from Lehmann's experiments, that the gastric juice contains lactic acid, and is similar to the juice of muscles. — J. P.

I here show you, in glasses, an infusion of pepsine to which a few drops of hydrochloric acid have been added. Into one of these small glasses has been put some coagulated albumen; into another some fibrine. The vessels thus prepared have been placed for ten or twelve hours in an atmosphere heated to 30° centig. [=86° Fahr.], and the albumen and fibrine have already in a great measure disappeared; there remain only some small fragments, which are already transparent, on the edges, and which will shortly entirely disappear. If I neutralise the acid, and then evaporate the solution, I can easily reproduce the albumen and fibrine, which have not been changed in their nature, but have merely dissolved by contact with the acid infusion of pepsine. This substance acts, therefore, in the solution of fibrine and albumen, as a body endowed with catalytic properties, and their solution is effected by an action of contact.

It is only in the stomach, or by certain glands situated in the mucous membrane of this viscus, that the acid solution of pepsine, or the gastric juice is separated. I have tried the effect of placing pieces of the small or large intestine in a very weak solution of hydrochloric acid: the solution never acquired a solvent property; it became gastric juice only by contact with the membrane of the stomach.

The property with which pepsine is endowed, requires the constant presence of a free mineral or organic acid. If, on the other hand, pepsine be dissolved in an alkaline liquid, its catalytic action becomes modified, as we shall hereafter find.

Lastly, I may remark, that pepsine loses its properties and becomes insoluble, when heated beyond 50° centig. [=122° Fahr.].

Azotised neutral substances, dissolved in the stomach by the acid liquid, or by the catalytic action of pepsine, pass into the blood merely by the imbibition of the coats of the capillary blood-vessels of the stomach. Water, and coloured alcoholic drinks, introduced into the stomach, are also absorbed; they do not pass beyond this viscus, nor are they to be found in the chyle; yet they reach the blood. Bouchardat and Sandras fed animals with fibrine, coloured with either saffron or cochineal, and yet could never detect a trace of the colouring matter in the chyle. Moreover, animals fed on fibrine, and others which were kept fasting, yielded, when killed, chyle always of the same kind: the contents of the intestines in no way differed, except, that in the animals fed on fibrine, a portion of the latter was found in the stomach incompletely dissolved. We know, also, from the celebrated experiments of Tiedman and Gmelin, that the quantity of fibrine contained in the lymph and chyle, after a long fast, is not less than that which is found there after digestion. The results are the same when coagulated albumen, gluten, or caseous matter, is employed instead of fibrine. The digestion of these azotised neutral substances is, therefore, a mere solution, effected by an action of contact, and an absorption of this solution taking place chiefly in the stomach. Thus, then, nothing is more physical than this part of digestion.

The mastication of aliments impregnated with a slightly alkaline and warm liquid, is an entirely physical operation similar to that which we practise in our laboratories in order to effect the division of a body, and thereby to promote its solution.

The gastric juice which the stomach secretes, especially at the moment of digestion, is an infusion of pepsine in acidulated water; and if we cause it to act on coagulated albumen, on fibrine or caseine, the solution of these substances can be effected as well in a properly warmed receiver as in the stomach.

The movement of the walls of the stomach promotes the action of the infusion of pepsine upon the substances to be dissolved, just as all agitation aids the reaction of two dissolved bodies, or the solution of a solid in a liquid.

This movement of the walls of the stomach, is also of assistance in another way: by incessantly renewing the points of contact between them and the matter which they contain, the absorption of the liquid portion of this substance is effected more readily. The influence which the division of the eight pair of nerves has in disturbing digestion, is ascribable, in part, to the cessation of these movements, which are dependent on the action of the nerves. Moreover, their section produces a great disturbance in other functions indispensable to the integrity of the animal economy.

Amylaceous Substances.—I shall now speak of the digestion of amylaceous matters, a subject on which we have been much enlightened by the beautiful experiment of Sandras and Bouchardat. This experiment is very easily performed. A few drops of pancreatic juice, added to some boiled starch or starch jelly, at the temperature of + 35° to 40° centig. [= 95° to 104° Fahr.] shortly dissolved it; the liquid became transparent, and, subsequently, every trace of starch disappeared.

The same effect is produced if, instead of the pancreatic fluid, we employ a piece of the pancreas of the pigeon, or of some other animal. I shall use the pancreas of a pigeon. Having pounded it, I add some of the pounded substance to the fecula, and heat the mixture to 40° centig. [= 104° Fahr.]. The fecula dissolves, and is converted into dextrine or sugar.

This is the state to which starchy substances are reduced before they are absorbed. There must exist, then, in the juice of the pancreas, and perhaps, also, as Magendie asserts, in the saliva, a substance which acts upon starch like diastase.

It is singular that this action requires the presence of a free alkali; for, if the pancreatic juice be acidulated, it ceases to act on starch, but, according to Bernard and Barreswil, acquires the property of acting upon the neutral azotised substances. We must, therefore, conclude, that a single organic substance has the property of dissolving fecula and the azotised neutral matters, provided that we add, when we act on the first, a free alkali, and when on the second, a free acid.

We have now to ascertain whether the starch thus converted into dextrine and sugar, by the saliva and pancreatic juice, passes in this state into the blood, or whether it is converted into lactic acid.

It is in the blood of some diabetic patients only, that sugar has been found. The hypothesis, that the conversion of starch into dextrine and sugar terminates in the formation of lactic acid, which is absorbed and passes into the circulation, seems to be more in accordance with facts. We must not forget the important discovery made by Fremy, of the property which certain animal membranes acquire when kept for some time in contact with water, of converting large quantities of sugar into lactic acid.

These same azotised substances, which, under certain conditions, excite the lactic fermentation, when taken in another state, which I shall call a more advanced stage of transformation, and the nature of which up to the present we are ignorant of, cease to produce lactic acid by their action upon sugar; and, on the contrary, they aid alcoholic fermentation, by transforming the sugar into carbonic acid and alcohol. Moreover, we know that a solution of sugar injected into the veins of an animal, soon makes its appearance in the urine.

From our knowledge of organic chemistry, and the well known results obtained by actions of contact, we may conclude that starch is convertible, in the bowels, into lactic acid, by first passing into the intermediate states of dextrine and sugar.

It is neither unreasonable, nor in opposition to the present state of knowledge, to suppose that a portion of sugar, into which starch has been converted, not only suffers in the intestines the lactic fermentation, but also undergoes some other transformation there, analogous to that, in the midst of which we now know the infusory animalcules are produced.

The recent experiments of Gruby and Delafond prove beyond doubt, that very large numbers of these animalcules are especially found in the stomachs of herbivora.

Diabetes.—I cannot close this subject without offering a few remarks on the researches which have been made to discover the cause and the curative treatment of diabetes.

Bouchardat first broached the opinion, which has since been generally adopted, that in this malady starch was converted in the intestines into sugar, which passed into the blood and urine.

Hence, a diet entirely excluding starch, and composed principally of neutral azotised substances, has been prescribed as a remedy for diabetes; and cases are mentioned in which by this means a cure has been effected.

Nevertheless, all that we have now said is contradicted by the numerous experiments of Dr. Capezzuoli, which tend to prove, that the quantity of sugar found in the urine of diabetic patients is not at all proportionate to that of fecula taken as aliment; and, that even under a diet composed exclusively of neutral azotised substances, the same amount of sugar is found as when the aliments contained much fecula.*

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* That by the exclusive use of animal food the quantity of sugar in the urine of diabetic patients may be greatly reduced, is a fact which I myself, in common with many others, have repeatedly noticed. But, as

Dr. Capezzuoli found sugar in the contents of the intestines, and in the matters vomited by diabetic patients, after a meal of starchy substances exclusively. But the quantity of sugar was the same in a healthy man as in a diabetic patient. This fact must always possess great importance for the theory of digestion; the transformation of fecula into sugar being thus demonstrated by experiment.

Lastly, Dr. Capezzuoli found traces of sugar in the blood, and in the contents of an abscess in a diabetic patient. The abundant production of grape-sugar in these diseases, which appear to be always accompanied with great wasting, remains yet to be accounted for.

Fatty Substances.—We must, lastly, consider the digestion of fatty matters, which are taken in very large quantities into the stomachs of the carnivora, and which, undergoing scarcely any modifications in their composition, are conveyed into the adipose tissues.

For this purpose, I must say a few words on the important question lately mooted by chemists, respecting the origin of fat in herbivorous animals.

Liebig maintains, that it is produced by means of a transformation of fecula. This loses a portion of its oxygen, which is expelled from the organism in combination with carbon.

Dumas, Boussingault, and Payen, on the contrary, believe that the quantity of fatty matter in hay, beet-

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I have elsewhere observed (*A Treatise on Food and Diet*, p. 500.), "I have never seen this secretion entirely lose its saccharine condition by even the most rigorous adoption of animal diet."—J. P.

root, and straw, is sufficient to account for the amount of fat found in animals fed with these substances. Boussingault has demonstrated the truth of this opinion by observations made on a cow. He found, that whilst the quantity of fatty matter existing in the aliments on which she was nourished weighed 1614 grammes, the amount found in her milk was only 1413 grammes; showing an excess of 201 grammes of the fat contained in the aliments, over that obtained from the products of the animal.

The same chemist also found, by means of experiments made on pigs and geese, that in these animals a much larger quantity of fat is produced than is contained in their food: and Persoz has arrived at the same conclusion.

It cannot, therefore, be denied that the animal economy possesses the faculty of transforming a portion of substances used for nourishment into fat. Chemical knowledge is of no assistance to us, in this instance, in explaining this transformation.*

On the other hand, it has been proved, by a great number of physiological observations, that in animals fed on fat, the chyle is more abundant and milky than usual, and that these same substances may be extracted

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* Chemists have succeeded in converting sugar into fatty matter. "When a solution of sugar is left to ferment, at a high temperature, in contact with putrefying caseine, there is separated from the elements of the sugar, if the oxygen of the air be excluded, a certain quantity of carbonic acid and of hydrogen gases, and we obtain, as is now well known, a fatty or oily acid, the *butyric acid*." (Liebig's *Animal Chemistry*, p. 113. 3d ed. 1846.) —J. P.

from it. Moreover, when submitted to the microscope, small globules of fat are perceived in it.

The experiments of Sandras and Bouchardat have proved this fact, beyond doubt. These chemists fed animals with oil of sweet almonds, and found this substance in the chyle; and they obtained a like result with suet. When wax was employed, they found only a small quantity of this substance in the chyle; but which, however, was augmented when the wax was introduced in a state of solution in oil.

These gentlemen have also examined the contents of the stomach and intestines of animals fed exclusively upon fat; and they found in the stomach a large portion of fat, solidifiable by cold, and contained in a very acid liquor. There was, also, both in the large and small intestines, a kind of thick pap (*bouillie*), from which ether extracted a large quantity of fat.

It results from these facts, of the truth of which I have satisfied myself, that fatty matters suffer no change in the stomach, and that they pass into the intestines without having undergone any modifications except being divided and liquefied by the heat of this organ. Moreover, by causing the gastric juice to act on fat out of the stomach, no change appears to be effected in it.

The alkali of the bile and of the pancreatic juice saturate, in the intestines, the acid of the gastric juice. Here, then, is a fresh proof that, in the intestines, the solvent action on the neutral azotised substances ceases.

Absorption of Fat.—It is difficult to determine precisely by analogies derived from chemistry, what becomes of the fatty substances after they have passed out of the stomach. It is certain that they are there absorbed, and that the chyliferous vessels may be considered as almost exclusively charged with this function.

Here are some experiments by means of which I have endeavoured to diminish the obscurity which hangs over this part of the digestive process. I put into a matrass a solution of 300 grammes [=9 oz. 132 grs. troy] of distilled water, $1\frac{3}{10}$ grammes [about 20 grains troy] of caustic potash. This solution has not any perceptible alkaline taste, and acts very feebly on litmus paper; it is a liquid whose alkalinity is about equal to that of the lymph and chyle. By means of a salt water bath, I expose the matrass to a temperature of from 35° to 40° centig. [=95° to 104° Fahr.]; I then add some drops of olive oil and shake the mixture; it instantly becomes so milky that it might be mistaken for milk itself. The liquid thus obtained, when left to itself, preserves its analogy to milk, and separates into two layers, the one more opaque at the top, in which are evidently small globules of fatty matter; the other, below, and less opaque, although still having a milky aspect. I have filled a piece of intestine with this species of emulsion, and plunged it into the alkaline solution already described, whose temperature was maintained at from +35° to 40° centigs. [=95° to 104° Fahr.]. After some time, the latter becomes turbid and acquires the characters of the interior emulsion. We, therefore, presume, that a portion of the emulsion has passed through the membrane and become diffused externally.

I may mention to you another experiment which appears to me still more conclusive. I filled an endosmometer with a very weak alkaline solution, and plunged it into the emulsion. The membrane employed was, as usual, ox bladder, and the two liquids were at the temperature of + 30° centig. at the commencement of the experiment. Endosmose took place; the emulsion passed into the alkaline solution; and the liquid rose in the tube to the height of 30 millimeters in a very short time.

These physical phenomena, which, although they do not explain all the peculiarities of the digestion of fatty substances, nevertheless contribute to render them less obscure. The chyliferous vessels, which terminate in closed or blind extremities (*en cul-de-sac*), and are enveloped by intestinal mucus, are, especially in a fasting animal, filled with an alkaline liquid, very analogous to lymph. After digestion, particularly when the animal has been fed on fatty substances, the liquid of the chyliferous vessels differs from what it was previously, merely by the addition of fatty corpuscles, which give it the milky appearance. It is natural to suppose, that this chemical affinity, which produces the milky liquid in the mixture of the alkaline solution and oil, is also exerted through the membrane of the chyliferous vessels, which certainly imbibes as much of the alkaline solution as of the milky liquid, formed by the action of the alkali on the fatty bodies.

The phenomena of endosmose, of which I have spoken, may also be admitted, with great probability, as one of the causes which produce absorption by the chyliferous vessels. It is certain, that absorption could not take place, if the inner sides of the intestines were not bathed with some liquid, for which the fatty bodies had some affinity.

It is easy to demonstrate, by experiments, that the alkaline condition of the intestinal coats favours this absorption. Fill two funnels with sand, equally shaken down in each. Pour into one, pure water, into the other, an alkaline solution; when the liquids have filtered through, pour an equal quantity of oil on each filter. For several hours, the oil will remain upon the surface of the sand, which has been moistened with pure water; whilst in the other funnel, in which the sand has been moistened with the alkaline solution, the oil will rapidly disappear by imbibition.

The neutral azotised substances which pass into the blood, after having been dissolved by the gastric juice, would rapidly destroy the neutral or slightly alkaline condition necessary to the preservation of the qualities of the blood; but the alkali of the chyle, of the lymph, of the bile, and of the pancreatic fluid, preserve the neutrality of it.

Origin of Cells. — The chyle and the lymph hold in suspension a great number of small grains, which are from 1 to 2 thousandths of a line in diameter, and which appear to be formed of a fatty substance, enveloped in a membrane, which is supposed to consist of a substance analogous to proteine. These same granulations exist in yolk of egg, in the milk, chyle, lymph, and in all the liquids exuded in pathological cases, or destined for new formations. These elementary granulations have been seen to unite and form a globule, a cell analogous to blood-cells; hence, they have been regarded as the morphological elements of all animal tissues.

Recently, Donné observed, by injecting milk into the blood-vessels, that the globules of milk disappeared, after some time, by becoming covered with an albuminous layer, like a bladder; that they then become reduced to the condition of white globules of the blood, which, finally, also disappeared, being probably transformed into red globules. Afterwards, all the blood re-acquired the appearance which it had before the milk had been injected into it.

The organic element seems, then, to be reduced to a vesicle consisting of a layer of albumen, collected together and organised around a *nucleus*, formed principally of a fatty substance.

I can bear testimony to an important experiment made by Ascherson: it consists in putting a fatty liquid in contact with albumen. This latter instantly coagulates, as you here see. If you mix them together, and put a drop of the mixture under the microscope, you will perceive a group of vesicles, each formed of a granule of oil, enveloped by an albuminous membrane, in some degree coagulated, and, it appears, like what the real adipose cells would do, on the stage of the microscope. We can see this still better, by putting, on a plate of glass, a drop of oil, and one of albumen, and slowly bringing them in contact: it is curious to observe, by the microscope, the almost instantaneous formation of a very delicate and elastic membrane, which soon acquires numerous folds. Ascherson has proved, that this formation, produced by albumen and oil, is really of a cellular nature. By adding a little water to a drop of this formation, he saw the cells swell up, and at the same time some small drops of oil escaped. By using diluted acetic acid instead of water, the cellules appeared to him to become so voluminous, that they burst. In oil, on the contrary, they became compressed, and diminished in size.[*]

Evidently these facts, which nevertheless require to be varied and extended, belong to the phenomenon of endosmose, and cannot be comprehended without admitting the cellular formation. Here, then, is a physico-chemical operation, which may lead to the discovery of the formation of elementary granulations. Fatty substances, and combinations of proteine, are constantly introduced into the organism: they are met with in all

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[*] Ascherson's hypothesis of the formation of cells is quite inadmissible. 'Caseine, albumen, and fibrine, in solution, may have the property of eparating fat into globules; they may form a layer or membrane around drop of oil, and so prevent the oil from running together. This proerty is possessed by gum arabic also, and, even though it were possessed by o other substance whatever, a *passive* layer of a solid substance around fluid is entirely different from an *active* membrane, from which manifold ctions proceed in organic nature. Such theories tell us nothing more han does the formation of froth in soap water."—(Mulder.)—J. P.

animal tissues; the globules of fat, which pass into the chyliferous tubes, and are there found in an albuminous liquid, must soon become enveloped by analogous membranes; and ought, for this reason, to form vesicles resembling those which microscopic observation discovers in the chyle, the lymph, and the blood.

Gases in the Stomach and Intestines.—In concluding this lecture, I have only to add a few words on the gas contained in the stomach and intestines, as well as on the inorganic substances, which form, more or less directly, an integral part of the animal organism.

Observation has proved, that oxygen is scarcely ever met with in the gases of the stomach, and more especially in those of the intestines; but that these are principally composed of azote, carbonic acid, a certain quantity of carburetted hydrogen, and sometimes traces of sulphuretted hydrogen. Evidently a large quantity of atmospheric air is introduced into the stomach: that is to say, it is swallowed with the food. The oxygen of the air disappears in the stomach, perhaps by filtration through the membranes, and reaches the blood; or, what is still more probable, by taking part in those modifications which we know occur in order to transform the azotised albuminous substances into ferment. Carbonic acid appears very abundantly developed in this case; and the enormous volumes of this gas, which is disengaged in some animals fed on fresh and moist herbs, may be here referred to.

It is curious to observe, that the production and disappearance of this abundant quantity of gas in the stomach and intestines, take place and succeed each other sometimes with so much rapidity, that one cannot have recourse to chemical reactions to explain them. The presence of hydrogen cannot, at present, be referred to any of the physico-chemical changes which we have seen to take place during digestion.

I have shown, by experiment, that oxygen is not necessary to the solvent action which the gastric juice exercises on fibrine and coagulated albumen, as Liebig seems to suppose. A piece of the stomach of a pig was put, along with some fibrine and coagulated albumen, into slightly acidulated water; the water had boiled for several hours, and the prepared liquid was covered with a thick film of oil. The fibrine and albumen were dissolved in this bath quite as well as in another similar one which was left in contact with the air.

Inorganic Constituents of the Body. — The inorganic substances found in the organism, have been evidently introduced from without, and formed part of the aliments: they cannot reach the blood unless they have been dissolved in water, and in the gastric juice of the stomach. Whatever is incapable of fulfilling these conditions, is necessarily rejected with the excrements. Physicians should never forget this truth, in selecting and preparing substances for administration to their patients. Experience has now proved, that it is by no means surprising if large doses of certain inorganic salts introduced into the stomach do not produce any effect; for they are rejected in the excrementitious matters.

LECTURE VI.

RESPIRATION. — GASEOUS ENDOSMOSE.

Argument. — Phenomena of respiration. Respiratory organs in different class of animals. Mechanism of respiration; inspiration; expiration.

Changes produced in the air by respiration; changes effected by fishes in the air dissolved in the water in which they live. Respiration is both pulmonary and cutaneous.

Changes produced in the organism by respiration; arterialisation of the blood. Respiration of gases.

Physico-chemical nature of the process of respiration; changes produced by atmospheric oxygen in venous blood drawn from an animal; nature of the changes; experiments of Magnus; atmospheric oxygen acts on blood through membrane; the process is, perhaps, *gaseous endosmose;* diffusion of gases; Valentin and Brunner's researches.

Conclusions.

Phenomena of Respiration.— The action of the oxygen of the atmospheric air on venous blood;—the changes produced in the air by its introduction into the pulmonary cells;—and the modifications effected in the blood which traverses the capillary network, on the delicate walls of the bronchial vesicles;—are the principle phenomena which constitute the function of respiration, and which will form the subject of the present lecture.

Organs of Respiration. — No animal exists, — even amongst those possessing the lowest degree of organisation, — whose life is not essentially connected with those modifications which atmospheric oxygen produces in its substance. The organs, by means of which this action takes place, are more or less developed, and have diversities of form and structure, adapting them to the medium in which the animal usually lives.

In fishes, for example, the organ of respiration is a mucous membrane, provided with many folds, and divided into filaments, or lamellæ, and abounding in blood vessels. It is always in contact with the water, which is introduced through the mouth, and expelled by the branchial fissures. In these animals every thing is arranged so as to give the greatest possible extent of surface for the contact of the water, in which the atmospheric air is dissolved, with the vascular walls. In the common ray, the branchiæ or gills have a surface of 2250 square inches.

In reptiles, birds, and mammals, the respiratory organ consists of an expansion of the bronchial tubes, which ramify like a tree, and whose very delicate extremities terminate by a large number of spheroidal vesicles placed side by side, and surrounded by small blood vessels. The respiration of some reptiles, at least during the first period of their existence, is both that of fishes and mammals: hence they have at the same time both branchiæ and lungs.

Mechanism of Respiration. — The movements necessary to this function are partly voluntary, partly involuntary. They may be reduced to two acts, one by which air is introduced, another by which it is expelled. All the air passages dilate during *inspiration*, all contract during *expiration*. The combined action of the muscular force, and of the elasticity of the osseous and cartilaginous parts of the thorax, as well as of that which is peculiar to the walls of the air vesicles, and, lastly, the physical properties of the air itself, are the causes of the respiratory movements.

The whole thoracic cavity dilates during inspiration, and the air rushes into the bronchia; whereas during expiration, this cavity contracts, and the cells of the lungs, being elastic, resume their primitive volume, whereby the air, being thus compressed, and possessing an elasticity more or less great, in proportion to the degree of heat communicated to it by the lungs, is expelled. The simple action of a pair of bellows shows you the whole mechanism of the respiratory movements.

In fishes, this motion takes place without the co-operation of the ribs. The branchial arches open, the lamellæ separate, and the contact between them and the water thereby takes place. They then close, and the water escapes by the branchial fissure, which remains open until the operculum falls.

In the lower animals respiration is less energetic, and the motions of inspiration are almost involuntary. In the annelides and mollusca, the current of water, in which the air is dissolved, seems aided by the vibratile movements of the cilia placed on the branchiæ of these animals.

Air respired. — A man, at one respiration, introduces into his lungs about 20 cubic inches, or rather more than half an imperial pint, of atmospheric air; the air expired contains usually from 3 to 5 per cent. of carbonic acid; but, after a very deep expiration, as much as 6 or 8 per cent. At the same time the inspired air has lost from 4 to 6 per cent. of its oxygen.

The numbers I have quoted have been selected from many others as being those which appear most worthy of confidence. From them it is easy to calculate the quantity of oxygen which a man absorbs by respiration in a day; assuming that he makes from 15 to 20 inspirations in a minute. According to Lavoisier and Seguin, the oxygen consumed in the respiration by an adult man, weighs 1015 grammes [about 15,675 troy grains]. The quantity which disappears during respiration in man and birds is, by volume, nearly equal to that of the carbonic acid evolved. Some very accurate observers have found that the volume of oxygen absorbed is greater than that of the carbonic acid produced. This difference is especially manifest in the carnivora, in which Dulong found that the oxygen which disappears is sometimes double the volume of the carbonic acid formed.

By making an animal respire in a definite volume of air, Dulong and Despretz have proved beyond question, that a remarkable quantity of azote is always produced. This fact shows that the excess of azote thus exhaled comes from the aliments, and perhaps also from that azote which, as we stated, is found in the stomach and intestines as the residue of the air introduced with the food. And if the quantity of azote contained in atmospheric air be invariable, Boussingault has demonstrated that this arises from the fact that some plants absorb this gas.

The same changes which respiration produces in the composition of atmospheric air, also take place in the air dissolved in water. We know, that in both common and sea water there exists, dissolved, a certain quantity of atmospheric air, which may be disengaged by ebullition, by bringing it in contact with some other gases, or by putting the water *in vacuo*. These phenomena, which are altogether physical, take place, according to the well known laws of the absorption of gases by liquids, discovered by Dalton.

The experiments of Morren, likewise, prove that a certain quantity of carbonic acid is dissolved in these waters, and which seems to vary in the inverse ratio of the oxygen which also exists there. The proportion of oxygen contained in a definite volume of air dissolved in water, exceeds that met with in atmospheric air. Humboldt and Gay Lussac found in air obtained from fresh water 32 per cent. of oxygen. According to Morren, the quantity of oxygen in sea water appears to vary at different hours of the day, and is at a maximum about noon; the reverse holds good for the carbonic acid.

Fishes absorb a portion of this dissolved oxygen, and yield up carbonic acid, which is absorbed by the water; and it is only by the continued solution of fresh portions of atmospheric air that the respiration of these animals can go on. This is the reason why they quickly die in water deprived of air by ebullition, or in water covered with oil.

I may here mention an experiment which I made some time ago on the respiration of the torpedo. The air dissolved in the water of the Adriatic, taken near the shore, consists, in 100 parts, of 11 carbonic acid, of 60·5 azote, and 29·5 oxygen. A large torpedo was kept for 45 minutes in about a gallon of this water. The animal was frequently excited, gave many shocks, and soon died. The air dissolved in the water, did not yield a trace of oxygen, but contained 36 per cent. of carbonic acid, the remaining parts in the hundred being azote.

Experience has proved, that these changes in atmospheric air, effected by contact with a living animal, take place not only in the lungs, but also, in different degrees, at the entire surface of the body of the animal. Frogs, confined in a definite quantity of air, and either deprived of their lungs or in some way prevented from carrying on pulmonary respiration, continue to live. After some time, it is found that a portion of the oxygen has disappeared, and has been replaced by carbonic acid.

Humboldt and Provençal observed, that tench live without much suffering, though their heads and branchiæ were out of water, and their bodies alone immersed. Spallanzani and Edwards have farther proved, that cutaneous respiration is indispensable in the batrachians; so that frogs can live several days without lungs, but on the other hand, they perish in a few hours if they are flayed or have their skin varnished. A snake whose whole body is varnished soon dies. Sorg immersed one of his arms in oxygen for four hours; at the expiration of this time he found that about two-thirds of the gas had disappeared. Davy analysed the air which had been injected into the pleura of a dog, and found that after a short time it yielded only slight traces of oxygen.

The mechanism of respiration, and the chemical changes which accompany this function, take place, therefore, in all animals in the same manner. Oxygen disappears in the respiratory organs, and, at the same time, carbonic acid is exhaled from them: there is more azote in the expired air than in the air which was inspired; the volume of the carbonic acid evolved is never greater than that of the oxygen absorbed; and in certain animals it is one half less than the latter, and the returned air is saturated with aqueous vapour.

Effects of Respiration.—Whilst the respiratory act is effecting the changes in the atmospheric air, which I have now described, what happens in the organism? None of you can be ignorant of the fact that, during respiration, the venous blood, being carried to the lungs, loses its blackness and acquires a bright vermilion colour, becomes arterial, is returned to the heart, and from this organ is sent to all parts of the body. The interruption of this transformation occasions speedy death.

I could adduce a great number of experiments to prove, that the change of the venous into arterial blood takes place in the lungs during the act of respiration. Bichat divided both the trachea and arteries of a dog, and immediately applied a stop cock to the opening of each of these vessels. By closing the stop cock of the trachea, shortly after an inspiration, the arterial blood became blackish, and before it had flowed a minute was completely venous. On repeating the experiment, and closing the stop cock as soon as possible after an expiration, the arterial blood flowed for a few seconds, and was of a black colour. When we removed the air from the lungs, by means of an air-pump, the blood immediately issued from the artery, black; when, on the contrary, we threw a little air into the lungs, the blood preserved for a longer time its vermilion colour. By carefully opening, from time to time, the stop cock of the trachea, an alternate shower of red and black blood was obtained.

Here is a rabbit, to the trachea of which a stop cock is fixed; observe the peritonæum which has been exposed. You perceive that the red colour of these vessels changes to a deep red when the stop cock is kept closed for a few moments, but returns to its natural tint when the stop cock is re-opened. In asphyxiated animals the tissues of all parts of the body, the kidneys, the muscles, the tongue, and the lips, assume a blackish colour. If the two pneumo-gastric nerves of an animal be divided, the respiratory movements soon become disturbed; and at the same time all the blood preserves its black colour, and the lips, the nostrils, and the pharynx of the animal lose their red tint.

If, instead of introducing atmospheric air into the lungs of an animal, we make it breathe azote, carburetted hydrogen, pure hydrogen, carbonic oxide, carbonic acid, binoxide of azote, or sulphuretted hydrogen, death takes place more or less rapidly, and the blood of every part of the body is found black. Besides atmospheric air, oxygen and protoxide of azote are capable of maintaining respiration for a few minutes. Perhaps, in oxygen gas this function would be maintained for a considerable time, were it not that by breathing this gas the respiratory movements become more frequent, the arterial pulsations quicker, and the blood every where acquires a very bright red colour. In protoxide of azote respiration may be continued for a few minutes without any serious inconvenience; but, as in the case of oxygen, the respiratory movement is accelerated, the cerebral functions become disturbed, and a kind of drunkenness ensues.

We are now acquainted with the phenomena which take place during respiration, both in the air itself and in the organism; oxygen is absorbed, carbonic acid is exhaled, black venous blood is changed into red arterial blood, and these two modifications occur in the same organ, where, from its peculiar structure, the atmospheric air, which yields up its oxygen, and the venous blood which becomes red, are nearly in contact, or separated only by an extremely thin membrane.

Respiration is a physico-chemical process.—Are these modifications of the air and the blood, phenomena which occur in living beings only? Do we find, that any changes analogous to those which happen during respiration, ever take place between atmospheric oxygen and venous blood drawn from a living being? The most simple experiment will soon answer these queries, and leave no doubt in your minds of the absolutely *physico-chemical* nature of this function.

Here I have a mass of blood which has been coagulated for several hours: you perceive that its surface is red, while that of the piece which I cut off is blackish; but before many minutes have elapsed it becomes red. I direct some carbonic acid upon the red surface of this clot, and it becomes almost immediately black. I pass a current of this gas through a liquid formed of blood dissolved in water, and you find that it will soon become black. This blackish liquid, being placed in a flask filled with oxygen, and agitated for a few moments, loses its deep colour and acquires a vermilion tint. Sulphuretted hydrogen is the only body, which having acted on the blood, even in very small quantities, renders this fluid incapable of being arterialised by oxygen.

Since the time of Priestley it has been known, that if blood, which has become blackish from the action of carbonic acid, be put into a moist bladder which is placed in contact with oxygen, the blood again becomes red, the interposed membrane not preventing the change of colour.

It is then proved, by experiment, that the change from black to red in the colour of the blood, which constantly accompanies the introduction of oxygen into the air vessels of living animals, under circumstances identical with those which I have pointed out, is a phenomenon entirely physico-chemical, and consists in the action of oxygen upon a liquid which has its origin in the living organism.

Nature of Arterialisation.—What, then, is the nature of this change? What are the laws which govern it? Here are the details which must still engage us; and in such investigations we shall rely on the splendid researches of Magnus.

If we receive the venous blood, which escapes from an aperture in the vein of a living animal, in a vessel containing pure hydrogen gas, and shake the two together, we shall find in the vessel some carbonic acid. This certainly cannot be the result of the chemical combination of hydrogen with the elements of the blood; nor can it be supposed that the acid was expelled from the blood by the affinity of hydrogen, for the body with which we may fancy the carbonic acid was combined. The carbonic acid, therefore, must have been dissolved in the blood, and have been disengaged by hydrogen, by the action which one gas has on another of a different nature, dissolved in a liquid. If we had substituted arterial for venous blood, we should have obtained a smaller quantity of carbonic acid. If we substitute azote for hydrogen, there is also disengaged, by the contact of that gas with the blood, carbonic acid, the quantity of which from venous blood should be more than double that obtained from arterial blood. By this method we not only obtain carbonic acid, but also some oxygen and azote, which are disengaged along with it. The results obtained by Magnus are so deserving of confidence and so important, that I think it my duty to make you acquainted with his numerical results. He extracted and analysed the gases dissolved in the blood, by means of a peculiar apparatus, by the aid of which he made a vacuum over the blood itself, and so collected the gases which were set free. If I were to introduce a certain quantity of blood, at the moment when it is drawn from the animal, into the vacuum of the barometer, you would observe that the column of mercury would fall considerably; and by this means also we might collect the gases of the blood. Here is a table containing the numbers obtained by Magnus.

Table of Gases evolved from the Blood.

	Cubic Centimetres.	
125 parts arterial blood of a horse yielded - -	9·8 of gas, composed of	5·4 carbonic acid. 1·9 oxygen. 2·5 azote.
205 parts venous blood of a horse yielded - -	12·2 of gas, composed of	8·8 carbonic acid. 2·3 oxygen. 1·1 azote.
130 parts arterial blood of a horse yielded - -	16·3 of gas, composed of	10·7 carbonic acid. 4·1 oxygen. 1·5 azote.
170 parts venous blood of a horse yielded - -	18·9 of gas, composed of	12·4 carbonic acid. 2·5 oxygen. 4·0 azote.
123 parts arterial blood of a calf yielded - -	14·5 of gas, composed of	9·4 carbonic acid. 3·5 oxygen. 1·6 azote.
108 parts arterial blood of a calf yielded - -	12·6 of gas, composed of	7·0 carbonic acid. 3·0 oxygen. 2·6 azote.
253 parts venous blood of a calf yielded - -	13·3 of gas, composed of	10·2 carbonic acid. 1·8 oxygen. 1·5 azote.
140 parts venous blood of a calf yielded - -	7·7 of gas, composed of	6·1 carbonic acid. 1·0 oxygen. 0·6 azote.

By taking the mean of these numbers for 100 parts of blood, we find that—

	Cubic Centimetres.	
For 100 parts of arterial blood yielded - -	10·4276 of gas, composed of	6·4967 carbonic acid. 2·4178 oxygen. 1·5131 azote.
For 100 parts of venous blood yielded - -	7·6825 of gas, composed of	5·5041 carbonic acid. 1·1703 oxygen. 1·0081 azote.

It is desirable that the experiments of Magnus should be repeated and extended, principally in order to ascertain the absolute quantities of the different gases of the blood.

The following results are also of the highest interest for the theory of respiration:—

1st. There exists in arterial blood a larger quantity of gas than in venous blood.

2dly. The quantity of oxygen found in arterial blood is double that which exists in venous blood.

3dly. The ratio between the oxygen and carbonic acid is from $\frac{1}{3}$ to almost $\frac{1}{2}$ in arterial blood, whilst it is only from $\frac{1}{4}$ to even $\frac{1}{5}$ in venous blood.

Lastly, when we consider the means by which we extract the gases from the blood, such as the presence of hydrogen, or a vacuum, it becomes evident that these gases are dissolved there. Hence we must admit, that the gases thus disengaged from the blood, are set free by the presence of other gases, obeying, in this respect, the physical laws relating to the interchange which takes place between gases dissolved in liquids, and those which are free.

We have seen that the change of colour, which venous blood undergoes in becoming arterial, — a change effected by oxygen, — also takes place when oxygen is separated from the blood by a membrane. It is essential, therefore, to prove that these phenomena, — that is, the reciprocal action of the gases, and the modification which the colour of the blood undergoes, — are effected out of the living body, through layers of membranes, and in virtue of laws which are altogether physical.

Any gas placed in a well-closed bladder, soon traverses it, and filters rapidly through its pores; while, at the same time, atmospheric air is introduced in its place. If the volume of the exterior gas be not infinite, compared to that of the gas contained within the bladder, the interchange soon ceases, and both outside and inside we shall find an uniform mixture of the two gases. Place a bladder, filled with water slightly acidulated with carbonic acid, under a bell-glass receiver, filled with hydrogen, oxygen, or azote, and a portion of carbonic acid will leave the water, and be found free in the receiver; while, at the same time, a portion of the exterior gas will supply its place, by becoming dissolved in the water. In general, two gases, one of which is free, or dissolved in a liquid, and the other separated from it by a membrane, mix in definite proportions.

Gaseous Endosmose. — It is desirable that an extended series of experiments should be carried on, in order to determine the laws of this phenomenon, having in view the reciprocal properties of the gases, their density, and the nature of the interposed membranes. Perhaps, a phenomenon analogous to that of endosmose may happen between the gases. Here is an experiment which shows in what way the gases act through membranes, and proves that there is a something resembling endosmose in the interchange which takes place. I partially filled the lungs of a recently killed lamb with oxygen gas, having previously carefully extracted, by suction, all the air that it was possible to withdraw. The trachea being closely tied, I introduced the lungs under a bell-glass, filled with carbonic acid, and inverted under [over ?] water. In a few moments the lungs began to swell up, and became as much distended as the size of the receiver would admit them to do. I analysed the gas after the experiment, and found that the carbonic acid had penetrated into the pulmonary cells, and that the oxygen had been disengaged therefrom. The interchange, nevertheless, had not been in equal volumes; the quantity of carbonic acid which had been introduced into the lungs, being much greater than that of the oxygen which had escaped. In a lung prepared in the way I have just mentioned, I found that in four hours, the gas contained in it was composed of $\frac{2}{3}$ oxygen, and $\frac{1}{3}$ carbonic acid, while that in the receiver was $\frac{1}{4}$ oxygen, and $\frac{3}{4}$ carbonic acid.

Soap bubbles, filled with atmospheric air, or hydrogen, falling into carbonic acid, demonstrated to Marianin a phenomenon resembling that which has been observed with the lungs. The bubbles augment in size; and it is curious that, when thus dilated, they fall to th bottom of the vessel containing the acid. The excess of carbonic acid, which has penetrated the bubble, is the cause of the augmented volume. The newly introduced gas produces the increase of weight of the whole, and counterbalances the diminution arising from the increased volume. But, at the same time, the film of water of the bubble certainly dissolves some carbonic acid, and in this way becomes heavier. I have tried the effect of placing a carefully closed bladder, having very thin sides, and filled with oxygen, in contact with carbonic acid. Taking the precaution that the bladder was not moist, the distention did not take place; but after a certain time, we found that the interchange between the two gases had occurred, though the quantity of carbonic acid which was introduced, did not exceed that of the oxygen which had escaped. Lastly, I filled the lungs entirely with carbonic acid, and in this state introduced them into oxygen; they collapsed, and the two gases mixed, but the volume of oxygen ntroduced was less than that of the carbonic acid which passed out. In all these cases, we must also consider, besides the reciprocal action of the two gases hrough the membrane, the presence of the water, which bathes the membranes,—water in which the carbonic acid is soluble. The liquid acid thus formed is, on one ide, in the presence of a gas different to that which it holds in solution, and in regard to which the free gas cts as in a vacuum. We must, then, take into account he greater quantity of carbonic acid introduced into he soap bubble, or into the lungs, by attributing it to a particular action of the two gases, such as would constitute gaseous endosmose, and to an effect of the gas at first dissolved, then exhaled. In order to clear up this question, it is necessary to have recourse to gases which have no affinity for water.

Lastly, we must bear in mind the law discovered by Graham, of the diffusion of gases: the diffusiveness, or diffusion-volume, of gases separated from each other by a membrane, is inversely proportional to the squar roots of their density.* According to the recent researches of Valentin and Brunner, this law is verified in the phenomenon of respiration.†

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* In the French edition of Matteucci's lectures, Graham's law i erroneously stated. I have, therefore, corrected the text in the Englis translation.

Graham's law may be thus expressed mathematically: —

$$\text{Diffusiveness} = \frac{1}{\sqrt{\text{sp. gr.}}}$$

The following Table shows the specific gravities, the square roots o the specific gravities, and the diffusiveness of several gaseous substances.

Gaseous Substances.	Specific Gravities.	Square Roots of Specific Gravities.	Diffusiveness.
Air - - -	1·000	1·000	1·000
Hydrogen - -	0·069	0·2627	3·806
Oxygen - -	1·1026	1·05	0·952
Nitrogen - -	0·976	0·987	1·013
Carbonic acid -	1·5241	1·234	0·810
Steam - -	0·6202	0·788	1·269

J. P.

† According to Graham's law of the diffusion of gases, when they a separated by an animal membrane and are under equal pressure, th become mixed inversely as the square roots of their densities; cons quently 1·17585 volume of oxygen is absorbed for one volume of expir carbonic acid. Comparison of the figures shows us that the mixture the two gases in respiration takes place entirely according to the law

Some facts obtained from experimental physiology, and which I have yet to notice, furnish evidence of the strongest kind in favour of our conclusions. Spallanzani, Nysten, Martigny, and Edwards, removed the air from the lungs of some frogs, by making pressure on the breast and abdomen. In this condition, some of the animals were put into hydrogen, and some into azote. Dogs, rabbits, and a great number of other animals, were likewise submitted to these experiments. It was invariably found that the hydrogen or azote was absorbed, and in its place carbonic acid and azote were exhaled. In pure azote, it was carbonic acid only. By introducing, by a syringe, a mixture containing more oxygen than exists in atmospheric air, after having exhausted the lungs by a syringe, it was observed, that the exhaled carbonic acid was in greater proportion than that which is disengaged by respiring air. Frogs pro-

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diffusion of gases; for a method of experimenting, as accurate as possible, gave results in which the figures obtained for the carbonic acid and absorbed oxygen almost exactly agreed with those reckoned according to the law of the diffusion of gases:—

Volume per cent. of Carbonic Acid in the expired Air.	Oxygen absorbed.	Carbonic Acid calculated.	Difference between the calculated and real quantity of Carbonic Acid expired.
3·850	4·690	3·994	+ 0·144
3·593	4·931	4·199	+ 0·606
3·949	4·887	4·162	+ 0·213
3·777	4·914	4·192	+ 0·415
3·759	4·922	4·192	+ 0·433
4·483	5·698	4·853	+ 0·370
4·752	6·362	5·418	+ 0·660
4·588	6·253	5·325	+ 0·737

Chemical Gazette, vol. ii. 1844, p. 159.—J. P.

duce carbonic acid in hydrogen and in azote, even when they have been deprived of their lungs.

Conclusion. — From all that has been stated, we cannot hesitate to conclude, that the respiratory function is a purely physico-chemical phenomenon; that the gases dissolved in venous blood are set free by the absorption of other gases; that a portion of the carbonic acid of venous blood is exhaled by the absorption of atmospheric oxygen by this blood; that it is not in the lungs, at least for the most part, that the expired carbonic acid is formed; that this gas exists dissolved in venous blood, and is set free, during the act of respiration, by the presence of oxygen, which is introduced in the same manner as is done by azote or hydrogen in the artificial respiration of these gases; and that, from the experiments of Magnus, it is proved, that the quantity of carbonic acid dissolved in the five pounds [about 6½ lbs. troy] of blood, which traverse the lungs in one minute, is nearly double that which is exhaled in the same time.

LECTURE VII.

SANGUIFICATION. — NUTRITION. — ANIMAL HEAT.

Argument. — *Hæmatosis* or *Sanguification.* Composition of the blood; blood corpuscles. Arterialisation of the blood; influence on this process of atmospheric oxygen, — of the removal of carbonic acid, — and of the serum; agency of the iron of the blood. Conversion of arterial into venous blood.

Nutrition; effected during the passage of the blood through the capillaries; renovation of the tissues; catalytic action of the blood corpuscles. Chemical changes which the blood undergoes in the capillaries. Transformations of the alkaline salts of the vegetable acids, of benzoic acid, and of salicine. Conversion of food into living tissues. Formation of urea out of the tissues. Uses of fat. Physiological nature of bile.

Animal heat. Heat produced in the body by the combustion (oxidation) of carbon and hydrogen. Influence of the division of the pneumogastric nerves. Experiments of Dulong, of Andral, and Gavarret. Conclusions.

Heat evolved by *plants* during germination and fecundation.

In the preceding lecture I have shown that, during respiration, a portion of the oxygen of the inspired air disappears, and that, in its place, is found a volume, either equal or less, of carbonic acid; that the expired air is saturated with vapour, and that at the very moment when these changes are taking place in the lungs, the venous blood is converted into arterial blood. We have also seen that all these phenomena occur out of the living body and under the same conditions as when they take place within it. It now remains for us to examine in detail this modification of the blood. Which of the organic elements of the blood undergoes this change? In what does it chemically consist? If I must reply with precision to these questions, I admit that hitherto experiments made for the purpose of solving them, have thrown very little light on the subject, and I can only select, from amongst an immense number of experiments, those which appear generally to be the least imperfect and the least discordant.

Composition of the Blood. — Micrographers now define the blood to be a liquid chiefly composed of water, in which are dissolved various salts, albumen, fibrine, and fatty bodies; and in which is suspended a great number of red globules, having a definite form, and whose diameter is greater or less in different animals. These globules are analogous to a vesicle where the coloured involucre is soluble in acetic acid.* I will show you a beautiful experiment of Müller, which will give you a correct idea of the composition of the blood.

I puncture the hearts of a number of living frogs, and receive the effluent blood on a paper filter. There flows through the paper a yellowish liquid, while the red globular matter remains on the filter. In a few moments

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* The globules, or more correctly the corpuscles, of the blood are believed to consist of three parts: —

1. A *capsule*, shell, envelope, or involucre, composed of an albuminous substance sometimes called *globulin*.
2. A *nucleus*.
3. An intermediate *red colouring matter*, apparently in a fluid state, and called *hæmatin* or *hæmatosine*. — J. P.

the filtered liquid coagulates and yields a clot composed of fibrine. Thus we see, on one side, the colouring matter, and on the other, the serum wherein the fibrine was dissolved. If the blood had not been filtered, the fibrine would have equally coagulated; but in doing so it would have enclosed in its mass the suspended globular matter, as happens with blood out of the living body. According to circumstances which are altogether physical, such as the temperature of the blood drawn, the density of the serum, and the different proportions of globules and fibrine, coagulation takes place more or less quickly, is more or less abundant, and the coagulum formed offers a greater or less resistance.

Arterialisation of the Blood. — If we take the clot formed in blood left to itself, and treat it with oxygen, we observe that it acquires a vermilion colour. This clot, left in the air, and then cut, is blackish internally, but red externally. The fresh surfaces formed by the incision, soon become red by exposure to the air. It is undoubtedly the globules of the blood which undergo this change of colour by contact with air.

Baudrimont and Martin Saint-Ange have recently shown that at the period of incubation, an absorption of oxygen and exhalation of carbonic acid take place through the calcareous envelope of the egg; and they have proved, that if these phenomena be prevented, the small red globules do not appear in the embryo, which does not become developed.

M. Dumas has shown that the globules of the blood remain unaffected, and without dissolving, if we carefully expose them to the contact of atmospheric air continually renewed.

It yet remains to be ascertained whether these globules become red solely by the oxygen which they absorb, or by the loss of carbonic acid which they suffer during respiration; or if, on the contrary, the blood becomes venous in consequence of the larger quantity of carbonic acid with which it is charged, or on account of the smaller quantity of oxygen which remains with it, or if it be the effect of these two circumstances combined. To explain this matter clearly accurate experiments are wanting.

Magnus has proved that venous blood, in losing the greatest possible quantity of carbonic acid, becomes less deeply coloured, but without ever acquiring a vermilion tint. This fact leads us to assume that these two causes have a simultaneous influence on the change of colour which the blood undergoes during respiration.

I ought to add, that if we carefully remove all the serum which surrounds the coagulum, and afterwards wash the latter with distilled water to deprive it of every trace of serum, it in this state no longer acquires, by contact with oxygen, that beautiful vermilion colour which it assumes when it is immersed in the serum. Here is a saturated solution of common salt, which I pour, drop by drop, on the blood clot; and you perceive that those parts on which the drops fall, acquire a vermilion colour, whilst the other parts of the surface undergo no change.

It appears, then, that the salts of the serum are not passive in the modification which the colour of the blood undergoes in the presence of oxygen. We now know that the serum absorbs a much greater quantity of carbonic acid than can be dissolved by water. We may, therefore, say that the serum influences the change of colour of the blood, in consequence of containing a portion of carbonic acid, of which it is afterwards deprived by the oxygen.

Agency of Iron.—But how can we account chemically for the change in the colour of the blood globules? On this point science has not hitherto enlightened us. The large quantity of iron (5 or 6 for 100) which invariably exists in these globules, and is met with, in this proportion, in no other animal substance, has given rise to the idea that this metal, found in the blood, sometimes in the condition of peroxide, and sometimes as a carbonate [of the protoxide], must have some influence in effecting the change of colour. In fact, oxygen expels carbonic acid from carbonate of iron, while carbonic acid replaces the oxygen of the peroxide, according to the relative proportions of oxygen and carbonic acid present.

Mülder and Liebig appear to have adopted these opinions. All the best established clinical results apparently prove that the use of iron in certain maladies restores in some way the colour of the blood. Nevertheless, Scherer has recently satisfied himself that he obtained the colouring matter of the blood entirely free from iron. If Scherer's observation be ultimately confirmed, and if, moreover, it be proved that this colouring matter, deprived of iron, undergoes, by contact with oxygen and carbonic acid, the changes which we have seen take place in the blood globules, we shall be compelled to give up the idea that iron intervenes in the change of the colour of the blood.

Conversion of arterial into venous Blood.—The arterial blood, propelled by incessantly renewed contractions of the heart, as well as by the successive dilations and contractions of the coats of the arterial vessels, owing to their peculiar elasticity, arrives with this red colour in the last capillary ramifications. Still contained in these vessels, it traverses all the tissues, loses its red colour, and returns by the veins to the heart in order to pass again into the lungs.

Renovation of the Tissues.—Physiologists tells us that nutrition takes place during the passage of the arterial blood through the capillaries. They assume that every portion of the animal tissues is incessantly renewed and transformed; and that these phenomena vary in intensity, and are proportionate to the different degrees of activity of the capillary systems of the different tissues. In truth, experimental evidence of this renovation is still wanting; and that which consists in the colouration of the osseous parts of animals fed with colouring substances, and then in their decolorisation when such nutrition is stopped, has always appeared to me insufficient. It must, however, be admitted that physiological facts, generally support the idea of this renovation.

If I were here to mention to you the various kinds of experimental information still wanting, and which are necessary to elucidate the function of nutrition, I should occupy much more of your time than if I undertook to describe all that we really know on this subject.

Catalytic Action of the Blood Globules.—The blood globules, making no part of the tissue, though being essential to nutrition, may, with a certain amount of probability, be regarded as the *catalytic* body which promotes the transformation and continual renovation of the tissues. An analogy of this characteristic of the globules is to be found in the necessity which they experience of being charged with oxygen in order to acquire this property.

Observe also that as in vegetables, diastase converts starch into dextrine, which is afterwards formed into cellulose and wood, that is to say, into bodies which are isomeric with each other, so [in animals] the blood globules change albumen into fibrine,—a transformation which certainly occurs in the embryo.

I wish I were able to say that the reality of these changes had been demonstrated as in the case of starch. I have made a great many experiments, having this object in view; but the results which I have hitherto obtained still leave me in doubt. I kept for a month, at the constant temperature of + 40° centig. [= 104° Fahr.], the albumen of the egg mixed with a small quantity of the blood globules of a fowl in the presence of oxygen. A receiver used for collecting water offered me the convenience of a medium constantly at the same degree of heat. I saw that oxygen in part disappeared, that it was replaced by carbonic acid, and that at the bottom of the receiver was deposited a great number of reddish flocculi; yet the primitive liquid was limpid, and scarcely coloured. These flocculi on examination did not appear to me to be identical with fibrine. Nevertheless, I do not wish to conclude from these negative results that the principle, on which my experiments were founded, was false. It is a subject which claims very long and very varied researches.

Changes in the Blood of the Capillaries.—Let us return to our first subject. In the act of nutrition, a part of the oxygen of the arterial blood disappears, and is replaced by an excess of carbonic acid in the venous blood. In the capillary vessels, oxygen combines with carbon; it is certainly here that this combination takes place; and since we find that the volume of carbonic acid expired is smaller than than that of the oxygen which has disappeared in respiration, we must admit that not only the carbon, but even the hydrogen, which made part of the organic elements of the blood and the tissues, combines with the oxygen to form water. Here, then, is another instance of combustion besides that of carbon.

The acetates, the tartrates, and the oxalates, which enter in a state of solution into the blood, escape by the urinary passages in the form of carbonates. Benzoic acid introduced into the circulation escapes in the condition of hippuric acid also through these passages. I may also mention that, in conjunction with Professor Piria, I introduced into the circulating blood of a living animal a solution of salicine. After some time we discovered in the urine a substance derived from salicine, and which had the property of forming a violet precipitate on the addition of a salt of iron.

An important observation recently made by Dessains deserves to be noticed here. By boiling hippuric acid with a solution of hydrochloric acid, benzoic acid was precipitated, and hydrochloric acid obtained in solution, combined with a saccharine azotised matter, which was found to be Braconnot's gelatine-sugar. We know that this substance (gelatine-sugar) is obtained by treating neutral azotised matters (proteine and gelatine) with acids. We further know that hippuric acid replaces, in the herbivora, the secretion of urea in the carnivora. We may infer from this, that gelatine-sugar is one of the first products of the transformation of neutral azotised matters, which are the materials of the tissues. We may thus comprehend how, by adding benzoic acid, which combines with them, hippuric acid may be obtained.

Combustion during Circulation. — All these facts prove beyond doubt that the principal chemical action observed in the circulation of the blood, and in nutrition, is a combustion, that is, a combination of oxygen with the carbon and hydrogen of the organic tissues. But, I repeat, up to the present time, great obscurity prevails in our knowledge of the series of these phenomena. What difference exists between the chemical composition of all the elements of the arterial blood, and that of all the elements of the venous blood?*

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* According to Dr. G. O. Rees (*Proceedings of the Royal Society*, for

What is the nature of this difference in the blood before and after its passage through the kidneys, the liver, and the various tissues? These are among the numerous questions which should be resolved by very accurate experiments, and by researches agreeing in their results, before prosecuting our investigations on the phenomena of nutrition and secretion.

Source of Evolved Products. — As we have seen, the aliments pass into the blood, after having undergone various modifications by the act of digestion. Among these, many are identical with the organic elements of the animal tissues: this is the case with the neutral azotised substances. So, also, with the fatty matters of the aliments, which are found in the adipose tissues scarcely, if at all, altered. It would be both unreasonable and absurd to assume that the urea, carbonic acid, and water, which are the definite products of transformations going on during nutrition, are derived from those organic elements of the blood, which were introduced by the aliments. We must suppose that these products are the results of transformations which the materials of our tissues have undergone, and which are replaced by new organic elements supplied

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June 3d, 1847), venous blood contains a phosphoric fat, and arterial blood, a tribasic phosphate of soda. "The venous corpuscles are known to contain fat in combination with phosphorus. This compound ingredient of the corpuscles, on coming into contact with atmospheric oxygen during the respiratory act, is consumed, and combining with that oxygen, forms the carbonic acid and water which are expired; and also phosphoric acid, which, uniting with the alkali of the liquor sanguinis, forms a tribasic phosphate of soda. This salt, like many others, acts upon hæmatosine in such a manner as to produce the well-known bright arterial tint." — J. P.

by the food. And, in fact, the production of urea takes place in animals nourished for a long time with sugar, starch, and gum, just as it had done before this kind of food was used. The same thing is observed in animals that have died from inanition.

For the sake of accuracy I shall quote, on the subject of these transformations, some illustrations taken from Liebig's "*Animal Chemistry, or Organic Chemistry in its Applications to Physiology and Pathology.*"

A serpent, kept for some time without food, and then fed on a goat, a rabbit, or a bird, expelled from the body, apparently unchanged, the hair, hoofs, horns, feathers, or bones of the devoured animal; exhaled carbonic acid and water; and evacuated by the urinary passages urate of ammonia.

When the serpent had regained its original weight no trace of its prey was discoverable. Let us analyse this simple case of nutrition. The urate of ammonia contains 1 equivalent of azote for 2 equivalents of carbon; the muscles and the blood of the animal eaten contain 8 equivalents of carbon for 1 of azote; and if we add to this the carbon contained in the fat and nervous substance, it is obvious that the serpent took more than 8 equivalents of carbon for every equivalent of azote. Now in the excrements only 2 equivalents of carbon were found; the 6 equivalents missing, must have been given out in the form of carbonic acid. I shall not stop to repeat to you our belief that the urate of ammonia and carbonic acid are derived from the transformed animal tissues, the place of which has been supplied by equivalents taken from the organic elements of the digested animal. We always find as much carbon and azote in the products of the transformation which the tissues suffer, in the presence of arterial blood, as the tissues themselves derive from the blood or the aliments.

What I have now told you of the serpent, holds good with the lion and all carnivora: in their urine there is urea only, which contains an equal number of equivalents of azote and carbon. As these animals are nourished on meat which contains azote and carbon in the ratio of 8 equivalents to 1, it follows that all the introduced carbon, beyond the amount which we find in the urine, must have disappeared in the process of respiration, have been burnt and converted into carbonic acid. The respiration of the lion is certainly much more active than that of the serpent.

The fifteen or twenty grammes [about 231½ grs. to about 309 grs. troy] of azote which a man daily passes in his urine, as well as the excess of azote which he expires, comes from the neutral azotised substances with which he is nourished, and more directly from the transformed tissues which the alimentary substances are about to replace.

Boussingault has proved by experiment that, in the urine of the horse, is found all the azote which formed part of its food; and he has thereby demonstrated, that the excess of azote expired equally proceeds from the aliments.

It is impossible, in the present state of science, to say precisely through what series of modifications and intermediate products, the muscles, cartilages, &c., pass, in order to be converted into urea under the action of the oxygen of the blood globules. By adding to the formula of proteine, which at the same time is also the formula of albumen, caseine, and fibrine, as much oxygen as is necessary to convert it into urea, and to convert the excess of hydrogen and carbon into water and carbonic acid, we obtain quantities of carbonic acid and water which are much smaller than those produced by respiration. Here is a numerical example deduced from the experiments of Boussingault, which I give you the better to establish the fact, that the carbon of azotised foods converted into urea, is much less than that which animals produce in the form of carbonic acid. The following are the numbers: a horse was kept in a perfect state of health, by eating daily 7½ kilogrammes of hay and 2¼ kilogrammes of oats. Analytical researches show that the azote of the hay is 1·5, and that of the oats 2·2 per cent. If we assume that all the azote of the aliments is reduced in the blood to the condition of fibrine and albumen, there would then be 140 grammes of azote introduced into the blood, and destined to replace the azote which goes out in the products of the transformed tissues. The quantity of carbon taken simultaneously with the azote [in the fibrine and albumen] amounts to 480 grammes; and of those only a portion can be converted into carbonic acid during respiration, since the horse converts part of the carbon into urea, and a portion into hippuric acid. But a horse, according to the experiments of this chemist, evolves by respiration in the space of one day, 2465 grammes of carbon in the state of carbonic acid. It is very clear, then, that the carbon of the azotised principles of the aliments is only a small portion of that which is found in the carbonic acid expired.

Necessity for the Elements of Respiration. — Hence arises the necessity of other kinds of foods, to supply the deficiency of carbon of the azotised aliments. Starch, gum, or sugar, and the fatty bodies, come under this category. In all cases where the animal economy is destined to grow, as in the young animal, nature has augmented, in its foods, the proportion of those ingredients which furnish the carbon and hydrogen lost by respiration; and in this way the azotised aliments, destined for the growth of the tissues, are economised.

By successively ascertaining the weight of the fatty matters, and the azotised neutral matters in the egg of the chick, during the period of incubation, and in the chick itself after its escape from the egg, Dr. Cappenzuoli has recently discovered, that about the seventeenth day, that is to say, a short time before the chick is hatched, there is a sensible diminution of the fatty and azotised neutral matters, and that this diminution gradually augments.

With respect to the fatty bodies, it seems that these are exclusively employed in respiration, in those cases only where the starch, sugar, and gum are insufficient. Hence, the fat disappears in hybernating animals, and in those which remain a long time without food. These bodies appear to be, physiologically, destined for the formation of the cerebral and nervous substance, and to fill the interstices of the cellular tissue, where are kept in store the materials for respiration.*

Use of the Bile. — I must also notice Liebig's hypothetical views respecting the liver. Physiologists no longer regard the bile as an excretion only. This becomes obvious, when we remember that Berzelius found only 9 parts of a matter resembling bile in 1000 parts of human excrements; that is to say, a man who secretes in one day from 500 to 700 grammes of bile, will reject with his excrements only $\frac{1}{50}$ or $\frac{1}{75}$ part only. Moreover, it cannot be supposed, that a matter so slightly azotised as bile is, can be useful in nutrition. Lastly, we have seen that bile performs little or no part in digestion. Liebig assumes, that the bile poured into the duodenum forms a soluble combination with potash, which is absorbed and converted into carbonate of potash by yielding up a portion of its carbon to the oxygen. In confirmation of these views, experiments are still wanting; and the more so, as it is only in some pathological cases, and under certain atmospherical conditions, that traces of biliary matter have been discovered in the blood.

Whether these hypothetical notions respecting nutrition, be or be not well founded, one thing is certain,

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* Fatty matter appears also to be intimately concerned in growth and nutrition, healthy and diseased. See some interesting remarks on this subject by Mr. Gulliver, in *The Works of Hewson*, p. 88., published by the Sydenham Society. — J. P.

namely, that an adult man absorbs in one day about 1015 grammes of oxygen. The observations of Dumas, Andrat, Gavarret, and the still more recent ones of Scharling, show that, on an average, a man exhales in a day 224 grammes of carbon, in the form of carbonic acid; that men exhale more than women, and children more than men; and that more is exhaled in the waking state than in sleep. A horse gives out, in the form of carbonic acid, 2465 grammes of carbon, consuming, for this, 6504 grammes of oxygen. A milch cow exhales 2212 grammes of carbon, in the form of carbonic acid, for which 5833 grammes of oxygen have been consumed. The quantities of food, then, are necessarily proportionate to the quantity of oxygen respired, and of carbonic acid exhaled. The activity of the respiratory movements, the density of the respired air, and the quantity of carbon introduced in the aliments, ought to be in proportion to each other, in order to preserve the materials of the animal economy. Letellier has lately proved that, with birds and guinea-pigs, the quantity of oxygen consumed in respiration is smaller, in proportion as the temperature of the air is higher. The carbonic acid exhaled at 0° centig. [=32° Fahr.] he found to be double that produced at the temperature of +15° to 20° centig. [=59° Fahr. to 68°.]

In those animals in which the activity of the respiratory movements is greater, the capillary circulation more rapid, and the quantity of the blood-globules more considerable, the portion of the fat in their tissues is very small. This is the case with birds, the hyena, and the tiger. If these animals be allowed but little or no exercise, fat soon accumulates in their tissues. From the experiments of Treviramus, we learn that, if their weights be equal, a cold-blooded animal consumes ten times less oxygen than a mammal, and nineteen times less than a bird.

Finally, I think it important to notice the results of a great number of experiments made by Boussingault, to determine, by a comparison of the composition of the aliments with that of the excrements, whether any azote be exhaled during respiration by graminivorous animals. By taking the mean of his results, we find that a turtle-dove consumes, in 24 hours, 5·10 grammes of carbon, and in the same space of time gives out 18·70 grammes of carbonic acid (*i. e.* 565·165 cubic inches), and 0·16 grammes of azote (*i. e.* 7·69 cubic inches). The azote, therefore, is $\frac{1}{100}$ part of the volume of carbonic acid, a proportion below that found by Dulong and Despretz. The hydrogen consumed in one day is 0·07 gramme; so that from these results we learn that a turtle-dove weighing 187 grammes, and respiring freely at the temperature of +8° to 10° centig. [=46·5 Fahr. to 50°] can, by consuming in 24 hours 5·1 grammes of carbon, and 0·07 grammes of hydrogen, develop all the heat required to maintain the body at the temperature of +41° to 42° centig. [=105°·8 Fahr. to 107°.6], and at the same time exhale about 3 grammes of aqueous vapour from the lungs and skin.

Animal Heat.—It is, then, indisputable that an animal is an actual apparatus of combustion, in which carbon is constantly burnt, and from which carbonic acid is always escaping. Such a calorific apparatus has been so constituted as to have, in comparison with the temperature of the surrounding air, a constant, or but a slightly variable, excess of caloric. This excess varies, according to the rapidity of the combustion in this animal calorific apparatus, and according to the constant temperature of the surrounding medium in which it lives. 1 gramme of iron, which oxydises in the air, and 1 gramme of iron, which oxydises in oxygen gas, develop each the same amount of heat; but the latter oxydises in a second, perhaps, whilst the other takes several hours to perform the same process. Hence, the temperature possessed by the one is vastly greater than that of the other. A mass of grape husks, laid in a heap, undergoes fermentations, and becomes very hot; but the same quantity, arranged in a thin layer, evolves an equal amount of heat, but which is not perceptible, in consequence of being too much dispersed. In this way, we can understand the difference of temperature between warm and cold-blooded animals. We cannot have any doubt as to the source of animal heat. It is found in the chemical re-actions of respiration effected in the capillaries, in the transformation of tissues, and especially in the combination of oxygen with carbon.*

I have no wish, nor, indeed, am I able, to describe any other hypothesis relating to the sources of animal heat

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* If Dr. G. O. Rees's theory of respiration be correct (see foot-not at p. 145.), the oxidation of phosphorus, contained in the venous corpuscles, must be one source of animal heat. — J. P.

When, in consequence of dividing the pneumo-gastric nerves, or the spinal marrow, we find, by a thermometer placed in the tissues of the animal, that the temperature falls, and from this conclude that *innervation* was the direct cause of animal heat, we do not consider that respiration and the circulation of the blood have been diminished in consequence of the division of the nerves and spinal cord. Instead of discussing hypotheses like these, it is preferable to examine more deeply, and in detail, the chemical actions which we consider to be the sole source of animal heat.

Natural philosophers are anxious to prove the truth of these hypotheses. An animal exhales, in a given time, a certain quantity of carbonic acid and water, and simultaneously develops a quantity of heat, which may be measured by the quantity of water which it is capable of heating in the same space of time. If the carbonic acid and the water, which the animal exhales, be the products of the combustion of carbon and of hydrogen, the heat given out by the animal ought, these philosophers say, to be equal to that which the same quantities of carbon and hydrogen produce when burnt in the air.

By setting out with the data furnished by a calorimeter, in which the animal was placed, noting the temperature acquired by the water, and measuring, at the same time, the oxygen which the animal or its products, carbonic acid and water, absorbed, Dulong, and afterwards Despretz, found that for every 100 parts of heat produced by the animal, and received by the calorimeter, 80 or 90 only were produced by the combustion of the carbon and hydrogen,—calculating from the carbonic acid and water evolved by the animal.

If we reflect that the temperature of an animal placed in a calorimeter, is always higher than that of the surrounding water, and that the animal is in consequence cooled during the experiment, we find, in the fact of this refrigeration, a plausible explanation of the excess of caloric met with. And, indeed, the numerous experiments of Despretz have clearly proved that the excess of heat received by a calorimeter over that which is due to respiratory combustion, is greater in proportion as the animal is younger and its temperature higher. We know, moreover, from the beautiful experiments of Edwards, that young animals cool much more rapidly than adults.

These considerations are sufficient to show that the excess met with in the calorimeter can be explained, without having recourse to any special power, to a vital property which engenders heat.

I must also add, that after the death of the celebrated Dulong, there was found in his unpublished papers an account of several other experiments relating to the heat developed by the combustion of hydrogen. This heat should be much more considerable than that which was first found by Dulong and Despretz. The numbers fixed by the later experiments of Dulong have since been confirmed by those of Fabre and Silberman. Now, in adopting this new number, we no longer find an excess of heat yielded to the calorimeter over that developed by the combustion of hydrogen and carbon, but, on the contrary, a deficiency.

We have, therefore, no occasion to seek for other sources of animal heat than the chemical processes of respiration and nutrition; but I think it is an error to attempt to make a rigorous comparison of the results of experiments on ordinary combustion produced in a calorimeter, with those which happen in an animal; and to admit, as the source of animal heat, one only of the numerous chemical actions which take place within the the same animal.

In fact, the carbonic acid with which the venous blood is charged, and which is produced by the union of atmospheric oxygen with the carbon of the organic elements of the different tissues which become modified, cannot arise from the carbon existing in a free state in these tissues, but in combinations with which we are far from being perfectly acquainted.

The experiments of Dulong prove that one body combined with another does not produce, in burning, or in combining with oxygen, the same amount of heat which it would do if it were employed in its free state. The heat which bicarburetted hydrogen, marsh gas, and oil of turpentine, produce, by burning in oxygen, and forming water and carbonic acid, is not equal to, but is generally less than, the amount of heat which would have been furnished, had the volumes of gas composing them been burnt separately. The experiments of Hess and Andrews, which tend to prove that, in a given combination, an absolute quantity of heat is developed, whatever may be the condition of the two combining bodies, have related solely to the successive combinations of the same body, as in the case of sulphuric acid, which combines with different numbers of atoms of water.

If we must limit the explanation of the production of animal heat, exclusively to the chemical combination of carbon and of hydrogen with oxygen, it will be difficult to interpret the results which have been arrived at by Andral and Gavarret in their study of the exhalation of carbonic acid during the act of respiration in man.

From the very extensive and apparently accurate experiments of these distinguished physiologists, it appears that carbonic acid which is exhaled during respiration varies much according to the sex, the age, and some particular physiological conditions. The difference between the numbers 5 and 14·4, expressing with the latter the quantities, taken in grammes, of carbon, which contribute to form the carbonic acid expired during an hour. The first of these numbers was obtained in a child of eight years old, and the other in a young man of twenty-six years of age. Observe, however, that in children the temperature being considerably higher than in adults, and the mass which is heated in these latter being larger, the loss of heat which they suffer ought to be proportionately greater.

Andral and Gavarret have also found, that in females the establishment of puberty does not augment the quantity of carbonic acid exhaled, but that this exhalation becomes more active when age or other causes put a stop to the phenomenon of menstruation.

Notwithstanding this, we remark no perceptible difference of temperature in the body of a female before, after, or during the period of menstruation, or in the state of pregnancy. And without having recourse to these experimental data, it will be sufficient to consider that in certain maladies there is a rapid diminution of temperature, and in others, on the contrary, a very great increase throughout the body, without our being able to admit of a corresponding variation in the respiratory function.

Conclusions.—Let us conclude, then, that in the existing state of physico-chemical knowledge, it must be assumed that the chemical actions which take place in animals during the transformation of their tissues, under the influence of atmospheric oxygen, are the source of heat in them; that among these, combustion of carbon and of hydrogen ought to be considered as one of the principal, but not the only one; and that experimental data are yet wanting to discover the exact ratio between the heat produced by an animal, and the heat evolved by chemical actions going on within it, and by those which we are able to produce with our apparatus.

Heat of Vegetables.—I shall not leave this subject without telling you, that in vegetables, also, the heat developed by germination is a phenomenon of chemical action, due to the combination of oxygen with the carbon of the germinating seed. We know that in the process of germination, there is an absorption of oxygen, and the evolution of carbonic acid, and that diastase converts starch into dextrine and sugar, which afterwards disappears by producing carbonic acid. It is curious, that in plants, as in animals, there are starch and sugar, which, by burning, disengage the heat necessary to their existence. In a like manner must be explained the heat which accompanies the fecundation of plants. Hence, we find, that in the sugar-cane, the beet-root, and the carrot, the sugar disappears after the flowering and fructification.

LECTURE VIII.

PHOSPHORESCENCE OF ORGANISED BEINGS.

Argument. — General remarks.

Phosphorescence of the *Glow-worm;* effects of heat and cold on it; influence of carbonic acid, of hydrogen, of atmospheric air, of chlorine, of oxygen, of mixed gases, of sulphuretted hydrogen, and of rarefied air. General conclusions. Cause of the phosphorescence: is not dependent on insolation; agency of the nervous system; influence of poisons. Microscopic structure of the luminous organs. Chemical nature of the phosphorescent substance. Conclusions as to the cause of the phosphorescence.

Phosphorescence of *animalcules;* of *putrescent fish;* of *the human body;* of *the perspiration;* of the *annelides* and *ophiuræ;* of *plants.*

Phosphorescence of Living Beings.—Living beings do not produce heat merely, many of them give out light also. Although the latter be not a general phenomenon proper to all organised beings, yet the numerous cases of it known are of the highest importance, and they show us a singular faculty of the living organism. We shall see in this lecture, in studying the best known cases of animal phosphorescence, that the phenomenon involves physico-chemical theories, so far as the general mode of its production is concerned; and that its exceptional character is one of those mysterious singularities which nature seems to have distributed amidst the immense variety of beings, almost without any previous attention to the animals on which she bestows them, as if merely for the purpose of constraining us to admire with humility the power of her creative skill.

Glow-worm.—I shall engage your attention for some time on the phosphorescence of a well-known insect, by giving you an account of the most conclusive experiments made some time since by Macaire and other natural philosophers, and recently by myself. The insect I speak of, is the *Lampyris Italica** or *Italian glow-worm,* commonly called *ver luisant* in France, and *lucciola* in Italy. It is a coleopterous insect living in the grass, and which shows itself after sunset in spring and summer. The two last segments of its body, which by day appear yellowish, are slightly luminous in the dusk, and during the night evolve a bright intermittent light.

The light sometimes ceases suddenly, either when the insects are gently touched, or at times when they have not been touched, and subsequently reappears again.

This fact led Macaire to suppose, that the phosphorescence was under the will of the animal. The cessation of the luminosity is certainly not effected by an opaque membrane, which it was supposed the insect drew over its rings, for no such membrane exists.

We shall find, in the course of this lecture, that everything leads us to assume that the phosphorescence is not continuous, because the cause that produces it is

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* The insect above noticed, under the name of *Lampyris Italica,* is by some authors referred to another genus. In Dejean's Catalogue it is called, *Colophotia Italica.* — J. P.

not persistent; and that we can explain the intermittence of the phenomenon.

In studying this subject, the observation which has always excited my astonishment is, that the yellowish matter contained in the terminal rings of the insect continues to emit light when separated from the body of the animal. If we kill one of the insects and crush it between the fingers, long streaks of light are perceived to issue from the yellowish matter. The phosphorescence continues for a greater or less period, according to a variety of circumstances, which we shall presently investigate. Indeed, this fact proves that the integrity and life of the animal are not essentially necessary for its production. In order to study the matter, thus separated from the body of the insect, I commenced by examining what influence heat, electricity, and gaseous media had on it, as those persons had done, who preceded me in this curious inquiry. At the same time, I also studied the influence of the same causes on an entire and living insect; and thus, by comparison, I believe I have adopted the best method of ascertaining the nature of the phenomenon.

Effect of Heat on the Glow-worm. — I placed several very lively and very luminous glow-worms in a glass tube immersed in water. A thermometer with a very small bulb was surrounded by these insects. I have repeatedly endeavoured to ascertain whether the thermometer thus placed acquired, in consequence, a higher temperature, but have never observed that it did. By slowly heating the water, I saw the intensity of the light increase up to + 30° Reaumur. [= 99·5° Fahr.] At about this temperature the intermittence ceased, the light became continuous, and, by applying more heat, acquired a red colour. At + 40° Reaumur [=122° Fahr.] the light entirely and finally ceased, and the animal died. By crushing the matter from the rings between the fingers, it no longer evolved light.

In experimenting with the posterior luminous segments, instead of entire glow-worms, I discovered no difference in the phenomena. These results confirm the experiments made by Macaire on entire glow-worms, placed in water which was gradually heated.

Effect of Cold.—I found some differences between my results and those obtained by this philosopher when the insects were submitted, in the same way, to the influence of cold. The tube being placed in ice, the light had not ceased even at the end of fifteen or twenty minutes, though it was more feeble, and not intermitting. When withdrawn from the tube and placed on the hand, the animals became as brilliant as ever. The same effect was obtained with the posterior luminous segments. The tube containing the glow-worms and thermometer being placed in a freezing mixture, whose temperature was — 5° Reaumur [= 20·75° Fahr.] the animals ceased to shine, and appeared motionless in about eight or ten minutes; but when they were withdrawn and placed on the hand, life returned, and with it light. If, during the time they are in the tube at 5° Reaumur [= 20·75° Fahr.]

their segments be broken by a pointed wire, a transient and very feeble light appears. This fact is likewise confirmed by the observation that their isolated posterior segments or luminous matter cease to shine at this temperature. If the luminous matter thus cooled be withdrawn and rewarmed, it recovers its brilliancy for an instant, and the light before becoming extinct acquires, as usual, a red colour when the heat has been too strong.

Effect of Carbonic Acid.—I put, at the same time, into two small equal-sized bell glasses ten glow-worms, and an equal number of segments detached from other similar insects. Then, after having filled the two glasses with mercury, I introduced some carbonic acid. In a few minutes, the light entirely disappeared, but without any remarkable difference being observed between the segments and the entire insects. When I introduced a little air, all recovered their luminosity; and, by adding some bubbles of oxygen, the effect took place more rapidly and brilliantly. Glow-worms, which appeared dead in carbonic acid, returned to life and motion on the introduction of oxygen. If thirty or forty minutes intervened, before the introduction of the air or oxygen, the insects neither returned to life nor re-acquired their phosphorescent quality; the segments alone, having remained much longer in the carbonic acid without being luminous, re-acquired their phosphorescence when oxygen was introduced.

Effect of Hydrogen.—When we used hydrogen, in place of carbonic acid, the insects, as well as their separated luminous parts, preserved their phosphorescence only for a time, which was somewhat longer than that stated for carbonic acid. The difference was scarcely perceptible with entire insects; but was more considerable with the detached luminous segments. In one instance, I saw the phosphorescence continue in hydrogen for twenty-five or thirty minutes. And even insects that did not glow, in hydrogen, returned to life and instantly re-acquired their phosphorescence when they were exposed to the air, or, better still, to oxygen gas; provided that not more than five or ten minutes had elapsed after the cessation of the light.

I invariably observed that, with the entire insects, the intermittance ceased before the light had altogether disappeared.

Some hours after the glow-worms, or their luminous segments, had ceased to shine, a feeble but very visible light was obtained by crushing them on the hand, but it was only momentary.

Effect of Atmospheric Air. — I now proceed to give you an account of the most conclusive experiments which I made whilst investigating the action of the insects, or their luminous segments only, on atmospheric air, and on oxygen. I placed in a small graduated bell-glass nine living glow-worms, and in another similar vessel, containing as much air, an equal number of detached segments. In twenty-four hours the insects no longer shone, though the detached segments were still feebly luminous. The air which remained under the glasses was analysed thirty-six hours after; and it was found that the oxygen had entirely disappeared, and was replaced by an equal volume of carbonic acid. In 11·8 cubic centim. of atmospheric air, wherein the entire insects had been put, we found 2·4 cubic centim. of carbonic acid. In the glass which contained the luminous sections, all the oxygen had not been absorbed.

Effect of Chlorine.—The entire insects remained lively and glowing in chlorine, quite as long as the separated segments; but when life and phosphorescence had disappeared, they were neither restored by introducing air or oxygen, nor by applying heat. The insects, and their segments even, when crushed, no longer evinced phosphorescence.

Glow-worms which have been left for twenty-four hours in atmospheric air contained in a bell-glass, became slightly luminous for a few moments when they were warmed by a lamp.

Effects of Oxygen.—I put some living and glowing worms into some bell-glasses filled with oxygen gas over mercury; they lived for about forty hours, and during the whole time continued to glow.

I placed ten luminous segments, taken from ten living glow-worms, in pure oxygen. The segments continued to be phosphorescent for four whole days, and we saw them luminous, even during the day, when we looked at them in a dark place. The gas left in the glass was one third carbonic acid, and two thirds oxygen.

I put some other luminous segments into this oxygen, after having deprived it of the carbonic acid by means of potash, and again observed the same result. The segments which were there during four days, emitted, after that time, no more light, even when warmed.

Here are the numbers deduced from some experiments:—

	Cubic Centim.	Cubic Centim.
Volume of oxygen gas, in which the entire glow-worms were placed		6·8
Volume of gas at the end of thirty hours		6·2
Carbonic acid, absorbable by potash	4·2	
Oxygen gas, not absorbable by potash	2·0	
	6·2	
Loss [ascribed to the absorption of carbonic acid by water on the bodies of the insects]		0·6

The residual gas was oxygen, which disappeared by a small piece of phosphorus, leaving only a very small bubble of air.

Other glow-worms were placed in 11·8 cubic centim. of atmospheric air. After thirty-six hours, the volume of air was unchanged, but it contained 2·4 cubic centim. of carbonic acid.

The phosphorescent segments of some glow-worms were put into 6 cubic centim. of oxygen; in twenty-four hours we analysed the gas, whose volume was reduced to 5·8 cubic centim., and we found that it contained 2 cubic centim. of carbonic acid, the remainder being oxygen. In all these experiments I invariably operated upon eight or ten segments taken from eight or ten different glow-worms.

Effects of Oxy-hydrogen Gas.—I also observed that, in a mixture of 9 parts of hydrogen, and 1 oxygen, these insects continued to live and shine, even after twelve hours experimenting. I found that about half of the oxygen was replaced by an equal volume of carbonic acid.

Effects of mixed Oxygen and Carbonic Acid.—In a mixture of 1 part oxygen, and 9 carbonic acid, I found that these insects did not continue glowing longer than two or three hours; and in twelve hours died. I proved that, the glow-worm can neither shine nor live long in a mixture of 2 parts carbonic acid, and 1 part oxygen; and that in this, also, a portion of the oxygen disappeared after the insects had been there for some time.

Carbonic acid gas appears to act on them deleteriously. Luminous segments, introduced into the preceding mixture, yielded the same results, in regard to the duration of the light, as entire insects; but with this exception, that the oxygen absorbed, and the carbonic acid emitted, were in much smaller quantities, and only about a fourth of what we obtain with entire animals. The volume of gas which disappeared during the experiment, is owing to the small quantity of water introduced on the body of the insect, and which dissolves the carbonic acid formed.

Mutilated Insects.—The observation of this remarkable fact, that glow-worms continue to live for several hours after being deprived of their luminous segments, induced me to make a curious experiment, the result of which agrees with that already stated.

I introduced twenty living and very brilliant glow-worms into a bell-glass, inverted over mercury, and containing 6·6 cubic centim. of pure oxygen gas. I carefully removed the luminous segments from twenty other living and very phosphorescent glow-worms, and put the insects thus mutilated into another bell-glass, also inverted over mercury, and containing 5·6 cubic centim. of pure oxygen gas. Lastly, the remaining twenty luminous segments of the last-mentioned insects, were placed in a third graduated bell-glass, with 9 cubic centim. of oxygen, in the same manner as the preceding. In ten hours I examined the three glasses; in all the volume of gas had diminished, and certainly on account of the formation of carbonic acid, which was afterwards absorbed either by the humidity of the insects, or by the film of water which covered the mercury. Thus, in the first, the gas was 6·2 cubic centim.; in the second, 5·4 cubic centim.; in the third, the volume had not sensibly lessened. The entire insects were yet alive, and glowing; the segments were equally phosphorescent; and the mutilated or halved-insects moved. In the first glass, after the absorption effected by potash, there remained 3·8 cubic centim. of oxygen; in the second, 3·7 cubic centim.; in the third, 8·2 cubic centim. The potash had consequently absorbed 2·8 cubic centim. of the carbonic acid produced by the entire insect; 1·9 cubic centim. of the carbonic acid proceeding from the insects deprived of the segments; and 0·8 cubic centim. of the acid yielded by the phosphorescent substance alone. In examining these numbers, it is curious to find that the two parts, into which the animal had been divided, should act separately with the same degree of intensity, as in the entire insect, and as if they possessed a life in common.*

I have repeated the experiment several times, and have always found, that the absorption effected by the entire insect surpassed, by a much larger quantity than the numbers cited, the amount of absorption of the demi-glow-worms and their luminous segments. I will relate another experiment, which led to the same results as the preceding. I introduced several glow-worms into a bell-glass, filled with water, and inverted over the hydro-pneumatic trough. In twenty minutes, the insects ceased to glow; but immediately after the introduction of some bubbles of air, they returned to life, and again became luminous. I repeated this several times, with the same insects. I repeated the experiment with water, which I had previously boiled for two hours. In this liquid, the insects evolved light during ten or twelve minutes only. It is remarkable, that with other liquids than those which act chemically upon the substance of the insect, the duration of the phosphorescence should be different. In alcohol and ether, the phosphorescence lasts a little longer than in water; in oil, on the contrary, its duration is less. It is necessary to proceed in the way I have here indicated, and not to confine oneself to

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* The reader will perceive that, while twenty entire glow-worms produced 2·8 cubic centimetres of carbonic acid, the separated parts of twenty other animals produced only 2·7 cubic centimetres (1·9 + 0·8) of this gas. The difference, therefore, is 0·1 of a cubic centimetre, and is probably referable to the absorption of a portion of the gas by moisture on the portions of the mutilated animals. — J. P.

placing the insects in the liquid contained in a capsule. In each of these last experiments, I believe that the duration of the phosphorescence ought to be, in part, attributed to the air, which always remains adherent to the insect.

I tried another experiment, which, I think, I ought to describe to you, before deducing from the preceding, their necessary consequences. I separated the segments of several very lively glow-worms, and then crushed and rubbed them in a small agate mortar. By treating them thus, the matter of the segments at first appeared very luminous; but after a few seconds, the phosphorescence diminished, and then entirely ceased. This result was accelerated, when the mortar was moderately warmed. I placed at the bottom of a bell-glass, one portion of the triturated substance of the segments, and at the moment when it ceased to shine, I filled the glass with mercury, inverted it over the trough, and introduced oxygen. On contact with this gas, I saw once, and only once, amidst numerous experiments which I made, a very faint light, but which ceased in an instant. In another experiment, in which the triturated matter also shone feebly, when the gas was introduced, the light continued for some time. In both of these cases, I analysed the gas forty-eight hours afterwards. Its volume had not varied, and the absorption by potash did not exceed 0·2 cubic centim. in 8 cubic centimetres of oxygen gas, in the experiment where the light had continued; and in the other there was no absorption; the oxygen remained apparently pure.

In another experiment, I heated twenty luminous segments to + 40° Reaumur [=122° Fahr.], by putting the tube, in which they were contained, into water of this temperature. The segments became red, and ceased to glow. I then filled the tube with mercury, and, inverting it over the trough, introduced oxygen. I perceived no light; and after four days the potash absorbed nothing. These segments had not evolved light; oxygen had not been absorbed; and consequently carbonic acid had not been produced.

Effects of Sulphuretted Hydrogen. — Some glow-worms, put into sulphuretted hydrogen, quickly ceased to glow and to live; and they did not afterwards become phosphorescent, by placing them in contact with oxygen, or by warming them. Some of the luminous segments evolved a very feeble light when they were crushed.

Effects of Rarefied Air. — I will describe, lastly, the experiment of putting these insects in highly rarefied air. I placed, in the closed extremity of a long glass tube, some entire glow-worms, and the luminous segments of others. I filled the tube with mercury, and inverted it over a trough, filled with the same liquid as in the construction of a barometer. The glow-worms and their segments were thus contained in a space where the air was very rarefied. The light ceased in the insects and in the segments almost at the same time; that is to say, the phosphorescence was extinguished in both in the course of two or three minutes, and, as usual, the intermittence first ceased. When I introduced air, immediately on the disappearance of the phosphorescence, this phenomena recommenced. In this case, I very distinctly saw all the glow-worms recover the faculty of motion which they had lost; so that, though they had ceased to shine in rarefied air, they were not dead. The same thing occurred on cooling them.

Conclusions. — These facts necessarily lead to the following conclusions, — conclusions which are either entirely new, or more rigorously deduced from experiment, than any that have hitherto been published: —

1st. The phosphorescence of the glow-worm may cease, without the insect being dead.

2ndly. There exists in this insect a matter which evolves light, without any appreciable heat; and the animal, to manifest this property, does not necessarily require either to be entire, or to possess life.

3rdly. Carbonic acid and hydrogen form a medium in which the phosphorescent matter of the glow-worm ceases to shine after an interval of time, not exceeding thirty or forty minutes, provided that the gases are pure.

4thly. In oxygen gas, the brilliancy of the phosphorescent matter is considerably greater than in atmospheric air, and the duration of the phosphorescence is nearly three times as long. This holds good with regard to the luminous segments only, as well as with the entire animal.

5thly. This phosphorescent matter, placed under conditions suitable for the emission of light, absorbs a portion of oxygen, which is replaced by an equal volume of carbonic acid.

6thly. This same substance, when deprived of its faculty of glowing, and then placed in contact with oxygen gas, no longer absorbs oxygen, or produces carbonic acid.

7thly. Oxygen mixed with either hydrogen or carbonic acid, in the proportion of 1 to 9, forms a medium in which the phosphorescence continues for several hours. We may, therefore, conclude, that it is in consequence of some alteration in the phosphorescent matter, that this ceases to glow after it has been for some days in pure oxygen, one portion only of which has been replaced by carbonic acid.

I examined the hydrogen in which I had kept several glow-worms for the space of twenty-four hours, and in which they had glowed for a few minutes only. The following is the result obtained when the gas was pure, and the experiment was conducted over mercury; the bell-glass being carefully filled by inverting it two or three times, in order to get rid of all the air adhering to the glow-worms: the volume of the gas was slightly augmented: with 8 cubic centimetres of hydrogen, I obtained an excess of 0·2 cubic centim. which was absorbed by potash. Thus, then, the insects produce carbonic acid, which must either be formed by the union of carbon with the oxygen remaining in the tracheæ, or exist ready formed in the animals. When the luminous segments alone are placed in hydrogen, with due precaution, they glow for a few seconds only and the gas is not altered.

8thly. Heat, within certain limits, augments the light of the phosphorescent matter; cold has a reverse effect.

9thly. When the heat is too strong, the phosphorescent matter becomes altered, as it also does when placed in the air or in any gas whatever for a certain length of time. This undoubtedly is the reason why these insects cannot live in all climates, and why they shine only during certain months of the year.

10thly. The phosphorescent matter, when thus altered, is no longer capable of emitting light or of becoming luminous.

These conclusions evidently prove the nature of the phenomenon: the production of light, by this insect, is essentially connected with the combination of oxygen with carbon, which is one of the elements of the phosphorescent matter.

Cause of the Phosphorescence.—We must now inquire how the phosphorescence is produced in the living animal: under what circumstances it varies: and what is the structure of the luminous substance and of the parts which surround it.

Not due to Insolation.—I placed some very lively and shining glow-worms in a tin box which closed accurately. I opened it twenty-four hours afterwards, it being then two hours after sunset. The insects appeared dead, but they still emitted a feeble light. By warming them in my hand, they began to move, and the light became more vivid.

After thirty hours more, passed in this box, some of the insects were dead, and no longer glowed; in others, slight phosphorescence was observed. This experiment supports the opinions of Beccaria, Mayer, and other philosophers, who regarded the phosphorescence of these insects as due to insolation.

But here is another experiment, the result of which is clear and satisfactory. In the same box, which was provided with a double bottom, I put, in one compartment, a great number of glow-worms, and in the other, a like quantity, intermixed with some fresh cut grass, gathered in the places where the insects had been found. At the expiration of twenty-four hours I examined them: what I have before related had happened to the first, but the others were still lively and glowing. When we opened the box during the day, in a dark place, we perceived their phosphorescence. To avoid prolixity I shall content myself with saying, that for nine days I preserved the glow-worms, with which the grass had been intermixed; and during this period they continued alive and emitted much light. Thus, then, when the insect is placed in its natural conditions with regard to temperature, humidity, &c., and continues to be nourished, the phosphorescent matter is preserved independent of solar action.

We conclude, therefore, from the preceding experiments, that the phosphorescent matter prepared by the animal, is preserved for some time luminous, although the animal be deprived of life; proving that life is not an indispensable condition of phosphorescence. By life, this substance is continually preserved with its properties entire, by the same process of nutrition which operates equally upon all parts of the animal.

Agency of the Nervous System. — I have not omitted to examine, what part the nervous system takes in the production of the phenomenon ; and I shall describe the experiments, made for this purpose, with all the necessary details.

If, immediately after a glow-worm is caught, we place it on its back, and examine it, we perceive that the posterior abdominal segments are reddish green. During the day this colour is not so distinct, and is yellowish ; and the same thing occurs with glow-worms which have been dead a short time. During the life of the insect, the segments become, from time to time, luminous, and more or less frequently. By attentive observation made on many insects, it has been discovered that sometimes the light does not appear at every part of the segments at the same time. If we slightly irritate any part of the insect, the light becomes for an instant visible. By touching one of the points of the segments, the light continues longer. If, at this moment, we cut off the head of the animal, the light soon diminishes, afterwards entirely ceases, and then the red colour of the membrane of the luminous segments is very perceptible. In this condition, we may strongly irritate the thorax of the insect without succeeding in producing phosphorescence. In order that this effect should take place, it is necessary to touch the luminous segments themselves; the irritated points then glow, and the light thus produced, goes on extending itself over the untouched portion of the segments. If we perform this experiment by putting the insect upon the stage of the microscope, we can judge still better of the production and diffusion of the light. In order to succeed in this experiment, it should be made in the dark, and no light should be allowed to fall on the object. We perceive an extremely rapid oscillatory movement in the parts of the phosphorescent matter, and at the same time they become luminous.

Influence of Poisons.—I have frequently tried the effect of nux vomica and opium, on the phosphorescence of these insects. For example, I dissolved 0·265 gramme of the extract of opium, or of the alcoholic extract of nux vomica, in 61 grammes of water, and placed the glow-worms in bell-glasses, filled with solutions thus prepared, and inverted over similar liquids. By proceeding thus, there was no contact with air. The results of a great number of experiments induce me to conclude, that the insects die, in the solution of nux vomica, eight or ten minutes sooner than they would in water. On the contrary, in the solution of opium, the phosphorescence continues eight or ten minutes longer than in water. I hope to be able to return to this object of our study, which I have now only glanced at, and which requires a greater number of experiments.

I will add, that those glow-worms which had ceased to shine in water, shone anew on contact with the air; whilst those which had been submitted to the action of nux vomica, or of opium, shone no more and died. Hence, the action of certain substances on phosphorescence is proved, though it is probable that they do not act by altering the phosphorescent matter.

I tried the effect of varnishing the abdomen only, of a great number of glow-worms with turpentine. I found that the light became weaker, and the scintillations fewer, but they did not entirely disappear.

Microscopic Structure of the Luminous Organ. — I examined the structure of the luminous organ by means of the microscope. On the removal of the luminous segments of the dorsal and abdominal membranes, there was perceived a yellowish, granular, globuliform matter, in which appeared groups of red globules, a great number of ramifications, and, moreover, a species of tubes which had the appearance of muscular fibre, but which, when closely examined, appeared hollow.

By looking at them at night, the light was seen to be emitted by the granular yellow matter; and when we compressed this between two glasses, the light was always observed on the edges of the examined portion. The abdominal membrane examined alone, after it has been washed several times in water in order to remove from it all the phosphorescent matter, is transparent, and furnished with a great number of hairs. The dorsal membrane, less transparent than the former, is likewise hairy, but it is also supplied, on its internal surface, with many tubes or tracheæ, which penetrate the phosphorescent matter. I must further add, that I never separated the abdomen of a glow-worm without finding under the last luminous ring but one, a bright red vesicle, which, viewed by the microscope, is found to be made up of a group of red globules. I have never met with this vesicle in other insects of the same genus, and no work on comparative anatomy mentions it. In my ignorance of this science I content myself with announcing to zoologists the presence of this body.*

Chemical Nature of the Phosphorescent Matter.—I will not now detain you longer than is absolutely necessary, in our examination of the chemical nature of the phosphorescent matter. This substance, taken from the living animal, has a remarkable odour, resembling that of the sweat of the feet. It is neither acid nor alkaline; it dries readily in the air, appears to coagulate on contact with acids diluted with water; does not per-

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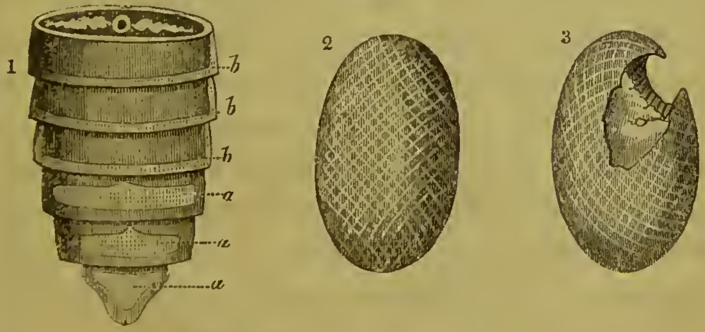
* I subjoin figures of the luminous organs in two species of *Lampyris*, viz. *L. lucida* and *L. noctiluca:* the latter is the common glow-worm of England.

Fig. 7.



Phosphorescent Organs of Glow-worms.

No. 1. is an enlarged view of the inferior surface of the abdomen of the *Lampyris noctiluca* after the integument had been removed.

a a a. Represent the three masses of luminous substances which are applied to the three last rings of the abdomen.

b b b. The arrangement of cellular or intestinal substance on the other abdominal rings, which gives the pale colour to the body of this insect.

Nos. 2. and 3. are the sacs of the common glow-worm, prodigiously magnified to show their structure. Fig. 3. is cut open to expose the luminous matter it contains. The coat of the sac is still seen to preserve its figure. The sacs are formed of two layers or membranes, each composed of a transparent silvery fibre. The sacs are more minute than the head of the smallest pin, and are placed on the last abdominal ring.—J. P ceptibly dissolve either in alcohol, or ether, or in weak alkaline solutions. It dissolves and becomes changed, in hot concentrated hydrochloric and sulphuric acids. By the employment of the last-mentioned acid, the solution does not become blue, which fact excludes the idea of the presence of albumen. Heated in a tube, it gives out the usual ammoniacal products. It does not present any obvious trace of phosphorus; of this fact I have assured myself, by calcining this matter several times with nitre in a platinum crucible, and by treating the dissolved residue with the tests which indicate the presence of the phosphates. From all that we have now stated, we can no longer regard the presence of phosphorus as the cause of the light in these insects. Perhaps by operating on a large number, we might succeed in discovering a slight trace of phosphorus, which is usually found in all organised substances.

From all these experiments, I conclude that carbonic acid is produced by the contact with oxygen of the phosphorescent matter alone, separated from the rest of the animal; that the light ceases to be produced when this gas is not present, and that by the contact of the latter, light and a volume of carbonic acid, equal to that of the oxygen consumed, are produced; and that the phosphorescent substance of this insect, when not luminous, does not act on oxygen.

It is, therefore, natural to suppose, that in the luminous segments of these animals, enveloped by transparent membranes, and by means of the numerous tracheæ discovered here and there in these animals, atmospheric oxygen is brought in contact with a substance, *sui generis*, principally composed of carbon, hydrogen, oxygen, and azote. The presence of a great number of dispersed blood-globules intermixed with the granular luminous matter, proves that these segments are the centre of a peculiar organ of secretion; and I believe that this red vesicle, which I have described as existing above the luminous segments, merits the attention of naturalists. Excitation and heat affect the phosphorescence, as they do all other phenomena of the animal economy, by directly favouring combustion; and in this way we account for the effects produced by some agents on the phosphorescent substance alone, when separated from the animal. The example of an organic substance which burns in the air by absorbing oxygen and emitting carbonic acid, is not new; this is the case with decaying wood, with oiled cotton, with very finely divided charcoal, and many other substances liable to spontaneous combustion. If, in the case which now engrosses our attention, the heat which ought to accompany the chemical combination be wanting, it may be easily explained. The quantity of carbonic acid disengaged from the luminous segments of each of these insects in a given time, is so small that the heat developed cannot accumulate there. The phosphorescence of wood, to which I alluded just now, as well as many other cases in which an emission of light accompanies chemical combinations, but which I need not farther notice, clearly prove that a disengagement of light may take place without any perceptible augmentation of temperature. Heat requires to be accumulated in order that its presence may be discovered by our instruments; and it is in this way we explain our inability to detect heat in the animals termed cold-blooded.

I have thought it my duty to enter thus fully into the peculiarities relative to the phosphorescence of the glow-worm, because I intend only slightly to allude to other cases of animal phosphorescence.

Phosphorescent Animalcules.—Every one knows, that during the night there is observed in the sea vast luminous tracts, which were formerly vaguely ascribed to the clashing of waves, to electricity, or to phosphuretted gases formed by the putrefaction of the mollusca. We now believe them to be owing to an immense number of phosphorescent microscopic animalcules. But no one knows what are the physico-chemical conditions under the influence of which these *infusoria* become phosphorescent.

Phosphorescence of Putrescent Fish.—There can be no doubt that fish, by putrefaction, become luminous; and from this circumstance may perhaps, in some cases, be produced the phosphorescence of the sea. The few experiments which I have made, prove that in a vacuum, or in carbonic acid, this phosphorescence ceases, but recommences in the air.

Phosphorescence of the Human Body.—In the annals of medicine, there exist well established facts of the appearance of flames upon the bodies of persons affected with certain diseases.

Phosphorescent Perspiration.—A phosphorescent perspiration of the feet has been spoken of, and it is curious to observe the analogy which exists between the odour of the phosphorescent substance of the glow-worm and the sweat of the feet. All these cases of phosphorescence remain unexplained.

Phosphorescence of Annelides and Ophiuræ.—I cannot conclude this lecture without mentioning to you the important observations recently made by Quatrefage, upon the phosphorescence of the *Annelides* and the *Ophiuræ*. This distinguished naturalist has observed with the microscope, that the phosphorescence of these animals belongs to the muscular fibre, is intermittent, and becomes more vivid when the fibre is irritated; and after repeated contractions, ceases for a certain time, being reproduced if the animal be left to repose.

Here is a point of analogy that you ought not to lose sight of. The life and the functions of muscles are accompanied by the disengagement of heat and of light, and these functions are immediately dependent on the nervous agent. It is very desirable that the observations of Quatrefage should be confirmed and varied.

Phosphorescence of Plants.—Botanists assert, that in many plants inflorescence is accompanied by phosphorescence. But this phenomenon is, also, too rare to be capable of being properly studied. During the period of inflorescence, oxygen is absorbed and carbonic acid disengaged; in a word, there is combustion, and this is the reason why in certain cases of inflorescence considerable heat is developed. Perhaps, also, some volatile oil, separated from the flower at the ordinary temperature, may be the cause of this light.

LECTURE IX.

ELECTRICAL CURRENT OF MUSCLES.

Argument. — The chemical actions going on in living beings develop electricity as well as heat and light.

The *galvanoscopic frog.*

Demonstration of the existence of an electrical current in the muscles of recently killed, as well as of living frogs and other animals.

The *muscular pile;* its construction; the direction of the current produced by it is from the internal to the external surface of the muscle; the intensity of its current is proportionate to the extent of the series.

The electric current of other tissues and organs.

Agency of the nervous system in the production of the muscular current. Influence of the organic conditions of the muscles; intensity of the current in starved, in well nourished, in inflamed, and in engorged muscles; influence of temperature on the current; modifications produced by poisons.

Piles formed with muscles and their connected nerves. Origin of the muscular current; Liebig's hypothesis; objection to it.

Heat and Light evolved in the Organism. — In the preceding lectures, I have clearly proved that in animals there is a constant production of heat, and in some cases, of light also. Guided by experiments and evident analogies, we have been forced to attribute the disengagement of heat and of light, in the living organism, to chemical actions which take place there; and the result of this investigation has been, the discovery of a fresh proof of the constancy of the general effects of the great forces of nature. The facts which will orm the subject of the present lecture, also lead us to he same conclusions.

Electricity evolved also.—It would be absurd to suppose that the chemical actions of living beings, all of which levelop heat, and often light, would not be accompanied y the production of electricity. This emission of elecricity, which we have now to demonstrate by all the vidence proper to chemical truths, will form the subject n which I am about to address you.

Here is a very simple and easily executed experiment, which proves the existence of an electrical curent, which is produced when we connect, by means of conducting body, two different parts of the same muscular mass, belonging either to a living animal or to one recently killed.

Galvanoscopic Frog.—A frog is prepared according o the usual method of Galvani; that is, we cut it hrough the middle of its pelvis, separate carefully all he muscles of the thigh, and divide one of the lumbar lexuses as it passes out of the vertebral column. We hen have a leg of the frog united to its long nervous lament, composed of the lumbar plexus, and of its rolongation in the thigh, that is to say, of the crural erve. The frog, thus prepared, and which I have alled the *galvanoscopic frog*, is very useful in researches n the electric current. For this purpose we introduce he claw of the frog into a glass tube covered with an nsulating varnish, take hold of the tube with the hand, nd afterwards bring any two parts of the body, whose lectric state we wish to examine, in contact with two different and sufficiently distant points of the nervous filament of the galvanoscopic frog.

Fig. 8.

The Galvanoscopic Frog.

If we take the precaution of not touching the body with any portion of the muscle of the leg, and if the limb be well insulated from the hand, we may be certain that the contraction which the galvanoscopic frog suffers, is due to a current produced in the body touched, and that the nerve only conducts it, and renders it evident by the contraction of the muscle.

Electric Current in Muscles.—Furnished with a frog, thus prepared, I take a living animal, a pigeon for example, slightly cut its pectoral muscle, after having carefully removed the integuments, and introduce into the wound the nerve of the galvanoscopic frog.

You observe the contraction of the frog. If you reflect on the arrangement, you will be satisfied that it is absolutely necessary to touch two distinct parts of the pectoral muscle of the pigeon, with two different parts of the nervous filament. If I apply the extremity of the nerve to the bottom of the wound, and another portion of the nerve to the lips of the wound, or, better still, to the external surface of the muscle, the frog continually contracts. This experiment clearly demontrates the presence of an electrical current, which circulates in the nerve, since it is necessary to form a circuit in which the nerve forms a part. If you have any doubt that the contractions of the frog are really excited by a current due to the different parts of the muscle of the animal, you will soon be convinced, by finding that no contractions are produced when I touch two different parts of the nerve with one liquid, or with a perfectly homogeneous conducting body.

Do not suppose that the blood is more apt than any other conducting liquid, to excite contractions in the muscle of the galvanoscopic frog. I let fall a drop of the blood of this same pigeon upon a glass plate, and place two distinct parts of this drop in communication with two points of the nerve of the frog, but it evinces no contraction.

It is useless to show you, that if I moisten either the nerve of the frog, or the different portions of the muscle of the pigeon, with a saline or acid solution, or, still better, with an alkaline one, the contractions of the frog are more energetic than in the former experiment. These solutions act chemically on the substance of the nerve, or of the muscle.

The phenomenon which you have witnessed in the pigeon, takes place in every other animal, whether warm or cold blooded.

I have recently proved, that the galvanoscopic frog gives the same signs when we operate upon a wound made in the muscle of a man.

Contractions are also produced in the frog when we bring the nerve in contact with a muscle separated from an animal. Here is a thigh of a frog, detached some time since from the body of the animal. I make an incision into the crural muscle, and connect the extremity of the nerve of the galvanoscopic frog with the bottom of the wound, and another point of this same nerve with the surface of the muscle. You immediately perceive that the frog suffers contractions; you will observe that it will also do so if I repeat this experiment with the thigh of the pigeon or rabbit, or with a piece of an eel. But if I continue the experiment, by renewing from time to time the galvanoscopic frog, we perceive that the phenomenon soon ceases, if we employ the muscles of the pigeon or of the rabbit, whilst it continues longer with those of the frog and the eel.

The contractions which you have seen excited in the galvanoscopic frog, already give you an idea of an electric current, which I shall call *muscular;* which, derived from the muscle of a living or recently killed animal, in which it is produced, circulates in the nerve of the frog.

But it is necessary to have recourse to the galvanometer to place beyond doubt the existence of this current, to discover its direction, and to determine its intensity relatively both to the condition of life or death, and to the position of the animal in the scale of beings; in a word, to study its laws.

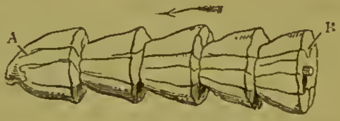
I expose the pectoral muscle of a living pigeon, I make a wound in it, and quickly convey the two platinum extremities of a very delicate galvanometer, the one to the external surface of the muscle, the other to the interior of the wound. You perceive that the needle instantly deviates from 15° to 20°, and even more; thus demonstrating the existence of a current, whose direction is from the internal part of the muscle to the surface of the same muscle. The needle soon comes back, and oftentimes returns to 0°. If I remove the extremities of the galvanometer from within the wound, and then a moment afterwards recommence the experiment, it sometimes, or rather, most frequently happens, that I obtain a new deviation in the direction of the first, but always more feeble. In some cases, however, the succeeding deviations are in the reverse lirection of the first ones. By repeating the experiment on the muscles of other animals, the first indication urnished by the galvanometer is obtained, in most instances, like that which we have witnessed; but it is ight to say that afterwards, in the succeeding experinents, the currents are often inverse to the first. Such fact, then, is not very clear: it does not rigorously rove the existence of the muscular current. If I had xperimented in like manner on a dead animal, you vould have seen, as usual, first, the signs of a current lirected from the internal to the external part of the nuscle, but less distinctly than in the living animal; iere, also, great uncertainty exists; our experiments are iot conclusive. There is, then, some defect in this node of proceeding, and every philosopher accusomed to manipulations with the galvanometer will erceive this defect, and will recognise the causes of it.

In one of my works, entitled, *Traité sur les Phénomènes Electro-physiologiques des Animaux,* I have dwelt with some prolixity on the manner of applying the galvanometer to the study of the electric phenomena of animals, but it would occupy too much time to repeat here what I have there stated.

I shall content myself with having shown you that I have succeeded in proving, by the aid of the galvanometer, the existence of the muscular current, and in discovering its fundamental laws.

Muscular Pile.—I take five or six frogs prepared after the manner (already mentioned) of Galvani; I cut them in halves, separate the thighs from the legs by disarticulation, and divide the thighs transversely into two parts. I thus obtain a certain number of the halves of thighs, from amongst which I select those only which belong to the lower portion; I arrange this series of demi-thighs upon a varnished tray, in which are some cup-shaped depressions or cavities. The annexed figure will show the arrangement.

Fig. 9.



The muscular Pile.

A. Positive end of the pile formed by the external surface of the muscle.
B. Negative end of the pile formed by the internal surface of the muscle.
The *arrow* indicates the direction of the current in the pile.

I first place one of these demi-thighs in such a manner that its external surface is contained in one of these cups. Next to this I place another, in such a position that its external surface is in contact with the internal surface of the first; and in this way, one after the other, all the demi-thighs are arranged in a row, touching one another, and with the same surface always turned in the same direction. The last demi-thigh, like the first of the series, should be placed in one of the cups of the tray, but by its internal surface.

Here, then, we have a pile of demi-thighs of frogs, of which one extremity is formed by the external surface of the muscle, the other by the internal surface. I fill the two cups with a weak saline solution, or even with distilled water; plunge into them the two extremities of the galvanometer, and immediately I observe a deviation of the needle, which before the immersion of these conductors was at 0°.

Thus, then, the presence of an electric current produced by a pile formed of the muscles of the frog is demonstrated by the galvanometer. Vary the experiments as much as you like; in lieu of the muscles of frogs, use the muscles of other animals, of fishes, birds, or mammals, and, provided that you keep the relative position of the muscular elements, before pointed out, namely the internal surface next to the external of the muscle, you will always obtain a deviation more or less great of the galvanometer needle: this deviation will indicate *constantly*, by its direction, the presence of a current proceeding in the pile from the internal to the external surface of the muscle.

I must observe that the intensity of the current is in proportion to the number of demi-thighs employed to form the pile. Here is a pile formed of ten demi-thighs; the variation of the needle is from 30° to 40°; here is another with six elements, and the needle marks from 10° to 20°; in a third, composed of four elements, it deviates only from 6° to 8° at the utmost. The increase of the intensity of the muscular current, in proportion to the number of the muscular elements employed to form the pile, is constant, whatever may be the animal from which the muscles are taken.

If, in place of arranging the elements in a straight line to form the pile, you dispose of them in such a way that they form a semicircle, and thus shorten the distance between the poles, you may close the circuit with the single nerve of the galvanoscopic frog, and by its contractions infer the existence of the electric current.

Currents in other Tissues. — I wished to ascertain whether the other tissues and organs of animals, the membranes, the nerves, the brain, the liver, and the lungs, denoted the presence of an electric current. I have invariably obtained very feeble signs of it. The heart alone gave indications of a very strong current; but, as you know, the heart is a muscle.

It is unnecessary for me to tell you that I tried the analogous experiments on the membranes, liver, &c., by forming the pile with portions of these tissues or organs, as in the case of the muscles, and that I took the same precautions.

Agency of the Nerves. — Thus the current, of which we have hitherto spoken, is principally demonstrated in the muscles. This property does not depend on the nervous system. Many of the experiments which I made, and which are related in my work, already alluded to, convinced me that if the nervous system which supplies the muscles be destroyed, the latter do not lose the property of manifesting the electrical current. I formed piles with muscles deprived of their nerves with every possible care; with other muscles taken from frogs, a considerable part of whose spinal marrow I had destroyed some days before with a red-hot iron, or which I had killed by opium; but no perceptible difference was manifested between the intensity of the current produced by these piles and that of the current caused by the same number of muscular elements taken from the entire frog.

If you continue experimenting, by means of the galvanometer, on a pile, which henceforth we shall call *muscular*, you will readily perceive that the deviations of the needle become more and more slight, and then cease entirely. And if you use piles formed of the muscles of animals belonging to different classes, you will observe that the signs of the current diminish the more rapidly, and disappear the sooner, in proportion as the animal which you have been using occupied a more elevated position in the scale of beings. Thus it happens that piles formed by the muscles of fishes, frogs, and eels, give, several hours after death, perceptible signs of the current; whilst those which are made with the muscles of birds and mammals, cease to do so at the end of a few moments. We have already noticed the uncertainty of the signs of the current, presented by the galvanometer, when the extremities of the wire of the instrument were put directly in contact with the muscles of the living animal. In this case, in order to be able to establish the facts in a more satisfactory manner, it becomes necessary to vary the mode of experimenting.

Here is an experiment which I have made, and which is free from all error; it is only the repetition, upon the living animal, of that which I made upon the demi-thighs of the frogs. You can easily understand how we manage, with great care, to confine, on the tray before spoken of, a certain number of living frogs, by fixing there the four legs by means of four nails, and by placing them thus one after the other; each one being deprived of the integuments of the thighs and the legs; and moreover, an incision being made in the muscle of one of their thighs.

The frogs being thus prepared, we easily succeed in putting the leg of one into contact with the interior of the muscles of the cut thigh of the next animal. In this way we form, with living frogs, the pile already described. The current which we then obtain is, as usual, directed from the interior to the external part of the muscle: its intensity is, with an equal number of elements, more considerable than with the muscles of dead frogs; and it more slowly becomes weaker.

When we connect the interior and the surface of the muscle, of a living or recently killed animal, by means of a conducting arc, the existence of an electric current is then rigorously demonstrated. This current is always directed from the interior to the exterior of the same muscle; its duration after death varies, and is much longer in cold-blooded animals than in those of a higher order. It exists without the direct influence of the nervous system, and it is not modified, even when we destroy the integrity of the latter.

Influence of the organic Conditions of Muscle.—It remains for me to notice the investigations I have undertaken with the view of discovering the influence which the organic conditions of the living muscle have upon this current.

When we examine the muscles of animals which have been kept without food, or in which the blood either circulates slowly, or is entirely interrupted, we see that the current has lost much of its intensity. The same effect is produced by employing frogs which have been left for some time in water, more or less deprived of air by ebullition.

If, on the contrary, the muscles have been for some time the seat of inflammation, or have been gorged with blood, or belong to animals that have been well fed, the muscular current shows more intensity, and continues for a longer period.

I have especially experimented upon frogs, because these animals are more capable of resisting the sufferings to which they are subjected, in these researches, than other animals.

If the muscles of which the pile is composed belong to frogs that have been submitted for a long time to a very low temperature, namely, to 0° centig. [=32° Fahr.], or even above 0°, the current will be found to be very feeble. In warm-blooded animals, the difference occasioned by a reduced temperature is less perceptible than in frogs. One result may perhaps at first surprise you. It is that the muscular current has the same intensity whether the pile be constructed of single demi-thighs of frogs, or of the same number of elements, each of which consists of two or more demi-thighs laid one on the other. In other words, the superficies of the elements has no influence on the intensity of the current. The same happens with piles formed of conductors of the second class, namely, with acid and alkaline solutions, which react on one another.

Influence of Poisons, &c.—I wished to see whether the action of poisons had any effect on the intensity and duration of the muscular current; and I found that the intensity of a current obtained from frogs poisoned by carbonic acid, hydrocyanic acid, and arseniuretted hydrogen, did not differ from that furnished by unpoisoned animals.

The influence of sulphuretted hydrogen on the intensity of the current, is, on the contrary, very marked; a fact which I have verified several times, both upon frogs and pigeons asphyxied and killed in this gas. A dead animal, in an atmosphere of sulphuretted hydrogen, almost entirely loses the property of manifesting the existence of the muscular current.

I have before told you, that in the muscles of frogs killed by narcotics, the current was as strong as in those which had not been destroyed in this manner.

Piles of Muscles with Nerves attached. — A word also on the results obtained by investigating the muscular current in muscles whose nerves are left entire, and thus submitted to experiment.

I formed some piles of the halves of frogs, in which, however, the muscles did not directly touch each other; the communications between them being established by the nervous filaments. I invariably found that the direction of the current was not changed, its intensity alone being diminished. In all, according as the contacts took place by the nervous filament above the incision in the thigh, or by the filament of the leg which was left connected with the thigh, the direction of the current being the same, the current was from the nerve towards the muscular element, sometimes in the contrary direction: in other words, the nerve having no influence upon the direction, always acted by representing the electric condition of the surface of the muscle, whether internal or external, with which it was in contact.

In these cases the current was weakened by the imperfect conducting power of the nerve, and if in place of the latter, we employ a cotton thread soaked in distilled water, the results are identical with those obtained by using the muscles with the nerve.

Pile of living Pigeons. — I may add that I have recently succeeded in forming a muscular pile with living pigeons, similar to that made of living frogs. In comparing these piles with one another, I found that the first signs of the muscular current were stronger in the pile of pigeons than in the frog pile. The difference is, in fact, greater when we consider that in pigeons the resistance of the circuit is much more considerable than in that of frogs. I have proved that the muscular current always becomes more rapidly weak, and ceases sooner, with pigeons than with frogs.

Muscular Piles in Gases. — Lastly, I have to state that the current produced by a certain number of muscular elements had the same intensity, and was of the same duration, when the elements were placed in hydrogen, oxygen, carbonic acid, and air more or less rarefied.

Sources of the muscular Current. — From all that we have stated in this lecture, it follows that the existence of an electric current in the muscles has been well demonstrated, and that its principal laws are established. The origin of this current resides in the electric conditions which are produced by the chemical actions of the nutrition of the muscle. The blood charged with oxygen, and the muscular fibre, which becomes transformed on contact with this liquid, compose the elements of a pile: they are the liquid acid and zinc. In the normal condition of the muscle, there can only be molecular currents produced by the formation and destruction of opposite electrical conditions in the same points; but if a great number of points of the muscular fibre be put, by means of a good conductor, in communication with others of a different nature, which do not suffer the same chemical action on the part of the blood, the electric current should then circulate. It is this fact, furnished to us by experiment, which proves at the same time the development of electricity in the living muscle, and the impossibility for the electric current to circulate in the masses of this muscle in the natural condition.

Liebig's Hypothesis untenable. — Liebig, finding a free acid in the substance of the muscle, and knowing that the blood and lymph are alkaline, fancied that he could explain the muscular current by saying that it is due to the combination of the acid with the alkali of the blood. But it is obvious that, on this hypothesis, the laws of the muscular current which we have given, cannot be understood. Weak acid and alkaline solutions are found in the tissues of animals where there is no electrical current. [The muscular current, whose direction from the interior to the surface of the muscle is constant, whose intensity and duration varies in a constant manner in mammals, birds, reptiles, &c., and which is destroyed by sulphuretted hydrogen, and by want of respiration, does not admit of so vague an explanation, and one so little founded in fact.]*

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* The passage within brackets is a translation from a notice by Matteucci in the *Comptes Rendus*, March 15. 1847. — J. P.

LECTURE X.

ELECTRIC FISHES. — PROPER CURRENT OF THE FROG.

Argument. — Electrical phenomena peculiar to certain animals.

Electric Fishes. Number of them known. The torpedo has been most frequently studied.

Torpedo; the shock produced by it; electrical phenomena of the discharge. Electric organs; direction of the current; physiological function of the discharge; influence of electricity, mechanical injury, heat, and chemical agents on the electric organ. Nervous system of the torpedo; the electric lobe of the brain. Analogies between muscular contractions and the discharge of the torpedo.

Gymnotus; Humboldt's description of the mode of catching electric eels; Faraday's experiments on the gymnotus; direction of the current.

General structure and composition of the electric organs of fishes; hypothesis of their action and of the influence of the nerves on them.

Silurus; position of its poles.

Proper current of the frog; discovered by Galvani; is a phenomenon belonging to all animals; its direction; has a common origin with the muscular current.

In the preceding lecture I have shown you that electricity is developed in the living muscular fibre, by the chemical actions going on there; and that by properly conducted experiments it may be rendered manifest. The muscular current is a general property of the living organism. To-day I proceed to bring under your consideration the development of electricity peculiar to certain animals.

Electric Fishes.—We are acquainted with five different ishes which are endowed with this property: the *Raia Torpedo**, the *Gymnotus electricus,* the *Silurus electricus,* he *Tetrodon electricus,* and the *Trichiurus electricus.* Two only of these, the torpedo and the gymnotus, have een carefully examined; and the first, in particular, aving been the object of numerous researches, will be he special subject of our lecture.

Electric Phenomena of the Torpedo.—If we grasp a iving torpedo with the hands, a strong shock is felt in he wrists and arms, like that produced by a voltaic ile of from a hundred to a hundred and fifty elenents, charged with salt and water. If we continue to old the animal between the hands, the shocks succeed ach other, sometimes with so much rapidity that it oon becomes impossible to sustain them; but after the apse of a certain time the animal loses its vivacity, and he shocks become less energetic, even when we may have aken the precaution to hold the animal in a vessel filled ith salt water. Direct contact with the animal is not equisite, as the shock is sufficiently strong to be felt ithout it. The [Neapolitan] fishermen are well acuainted with this fact, and learn the presence of the orpedo amongst the shoal in their nets by the shocks hich they experience, especially in the arms, when

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* Under the Linnean name of *Raia Torpedo* have been confounded veral distinct species now referred to the genus *Torpedo.* Three ecies of torpedo are found in the Italian seas; viz. *T. Galvani,* *. Narce,* and *T. Nobiliana.* The first two were included under the innean name of Raia Torpedo. The torpedo whose electrical organs ere described by Hunter was the *T. Galvani.*—J. P.

they wash the captured fishes by dashing bucketfuls of water over them. In the water containing the torpedo the shock is felt at considerable distances. The animal appears to be endowed with this faculty to enable it to kill the fishes on which it feeds.

Identity of the Power of the Torpedo and that of Electricity. — The earliest observers soon perceived the identity of the phenomenon presented by the torpedo with the electrical discharge: they found that when the animal was insulated, no shock was felt by touching it with sticks of sealing-wax, glass rods, &c.: but it was immediately felt when they employed, instead of resin or glass, water, wet cloths, or better still, metallic bodies. Walsh went still farther: he demonstrated by experiments, the accuracy of which are generally admitted at the present day, that the two opposite surfaces of the body of the torpedo are the poles, at which the opposite electricities are found at the moment of the discharge. It follows, therefore, that the greatest possible shock is obtained by connecting the belly and the back of the fish, by means of a conductor, which may be the body of the observer. At one time it was thought that, in order to obtain this shock, it was sufficient to touch, with a conductor, any part whatever of the back or the belly of the animal; and, consequently, that it was unnecessary to make the connection, which we have spoken of, between the two opposite surfaces of the fish. But it is now clearly proved that this condition is indispensable, and that if we succeed in getting a shock by touching the animal, at a single point, with a metallic conductor held in the hands, it must be in consequence of the torpedo not being insulated, whereby the circuit is completed through the ground and the body of the observer. If, however, the torpedo be insulated by placing it, with one of its surfaces, on a resinous plate, a slight shock is obtained when we touch the other surface with the finger. This phenomenon will be fully understood when we shall have explained to you the laws of the distribution of electricity, on the body of the animal, at the moment of its discharge.

Phenomena of the Shock. — The shock of the torpedo is accompanied by all the phenomena proper to the electric discharge or current. Frogs, prepared after Galvani's method, and arranged upon the body of the torpedo, suffer contractions at each shock which this animal gives, on being excited. The same effect takes place even when the frogs are put at some metres [yards] distant from the torpedo, provided that they, as well as the torpedo, are placed on a wet cloth. If the frog, prepared in the way stated, be held in the hand, and brought, by means of the extremity of its nerves, in ontact with a point of the body of the torpedo, it suffers ontractions at each shock from the fish ; but they cease when the torpedo is insulated, or when the frog is suspended by means of an insulating thread. Notwithtanding this precaution, contractions take place when a ong portion of its nervous filament is spread over the body of the torpedo. This fact is analogous to that of receiving the shock in the fingers when the torpedo is insulated.

When we distribute several frogs over many points of the surface of the torpedo, at first all of them contract at each discharge of the fish; but, in proportion as its vigour lessens, we perceive that those frogs which suffer contractions for the longest time are those placed upon the sides of the animal near the head: in other words, the points which preserved the faculty of making the frogs contract for the longest period, are those which correspond to two peculiar organs, situated laterally and symmetrically towards the cephalic extremity of the fish. When the two extremities of the platinum wires of a moderately sensible galvanometer are placed in contact with the back and belly of the torpedo, and the animal is irritated in order to obtain the discharge, we observe, that at the moment when the frogs contract, the needle of the galvanometer deviates suddenly, then immediately returns back, oscillates, and stands at 0°, even though we continue to keep the circuit closed. When a fresh discharge from the fish occurs, the same phenomenon is repeated. By the aid of this instrument we are enabled to prove that, in the discharge of the torpedo, the current is directed in the galvanometer from the back to the belly of the fish, so that the back represents the positive pole, and the belly the negative pole of the pile. If, by means of the extremity of the wires of the galvanometer, we examine different parts of the body of the torpedo at the moment when the discharge takes place, we perceive, in a manner still more evident than when frogs are used, that, at the commencement of the experiment, the signs of the current are obtained, by establishing the circuit between any part of the back and of the belly; but when the animal has become weakened, it is necessary for those parts of the body which correspond to the points called the *electric organs*, to be touched, in order that the existence of the current may be made manifest.

Fig. 10.

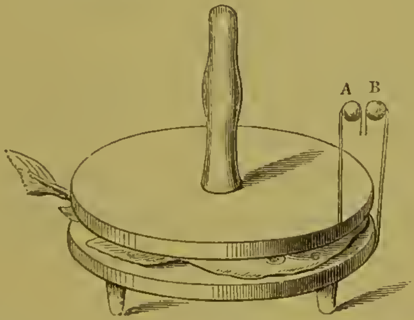
Torpedo with one of its Electric Organs exposed.

It is curious to observe that, by simultaneously touching two points of either the abdominal or dorsal surface f one of these organs, that the signs of the current are perceptible, but less evidently so than when the circuit as been established between the two opposite surfaces. In order to produce the deviation of the needle, by touching with the galvanometer wires two points belonging to he same surface of the fish, it is indispensable that one of the wires should be in contact with a part near the periphery of the electric organ, and that the other should occupy a point almost diametrically opposite to that of the first. We then have signs of the current which always passes in the galvanometer, from the wire nearest to the median line of the animal to that which is the most distant from it: we also obtain them with the galvanometer, by keeping one of the wires in contact with the abdominal or dorsal surface of one of these organs, and by introducing the other into the interior of the organ itself. The current is invariably directed from the wire which touches the dorsal surface, or that which is nearest to it, to the other wire. These facts explain the feeble shock experienced by touching with the finger the insulated torpedo.

If, instead of the conductor of the galvanometer, we use a metallic wire, one portion of which is bent into a spiral, with an [unmagnetised] steel needle introduced within it, and then touch the two surfaces of the torpedo with the extremities of the wire, the discharge magnetises the needle. The direction of the magnetism produced by the discharge of the fish is constant; that is to say, it is the same as that indicated by the galvanometer, whatever may be the thickness of the wire of the spiral, the length of the circuit, the diameter of the spiral itself, and the length, thickness, or temper of the steel needle.

If we insulate the torpedo, placing on each of its surfaces a disc of platinum, upon each of which has been placed a piece of paper of the same size, moistened with a solution of iodide of potassium; and, lastly, close the circuit by effecting a communication between the discs by means of a platinum wire, we soon find that at each discharge of the fish, a reddish yellow spot is formed around the extremity of the platinum wire, which touches the piece of paper placed on the platinum of the ventral surface. The paper on the platinum disc in contact with the back of the animal, is coloured also, but more feebly. The solution which impregnates the paper is, therefore, decomposed by the current of the torpedo, and iodine is evolved at the positive pole.

Fig. 11.



Torpedo placed between the Sole and Cover of an Electrophorus.

We may also succeed in observing the spark at the moment of the discharge. The apparatus employed to ffect this is very simple: place a very lively torpedo pon a large metallic plate, like that of a perfectly nsulated electrophorus, and then put above the fish a disc having an insulating handle. Each of the two arts of the apparatus should be furnished with a me-allic wire or rod, to the upper extremity of which is attached, by means of gum, a piece of gold leaf; the two leaves, consequently, hang downwards. We arrange the plates so as to bring the two leaves near to each other. By compressing the fish by means of the upper plate, and by bringing the two gold leaves almost in contact, we frequently see the spark pass from one to the other; but we can readily understand, that the phenomenon is one which is difficult to be seen; and that to succeed in its production, it is necessary to have, at the moment of the discharge, the two gold leaves at the proper distance for the spark to pass.

We succeed more easily if we substitute for the gold leaves a steel file, which is connected with one of the discs, and on which we rub a metallic wire which communicates with the other disc.

All the phenomena of the shock of the torpedo must, therefore, be attributed to an electrical current. The apparatus which produces it, consists of two peculiar organs, called the *electrical organs of the torpedo.* Each of the surfaces of these organs possesses an opposite electrical condition; the dorsal surface is positive, the abdominal surface negative. The discharge of the animal is voluntary, and all external irritation acts upon the electric organ by the intervention of the will only. In fact, as the discharge would traverse the animal's own body if there did not exist exterior circuits or conductors to receive it, it follows, either that the animal will no more produce it, or would immediately cease to do so, if it were out of water, or if it were either not touched or touched by insulated bodies.

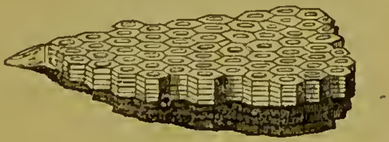
It is, then, not without reason that nature has placed the animals endowed with this property in a liquid conductor.

The properties of the current of the torpedo resemble those of the electric current, properly so called, more than those of the discharge of the Leyden phial.

Let us now examine the discharge of the torpedo as a physiological function, and consequently study the influence exercised on it by the different parts of the organ itself; and by those parts which surround it or which have some connection with it, as well as by the circumstances which affect the state of life of the nimal.

If we carefully and rapidly remove, from a very ively torpedo, one of its electric organs, by separating t from the cartilages and integuments which cover and ncompass it, but leaving uninjured the great nervous runks which are distributed to it, we readily perceive hat all its different parts, the integuments, cartilages, c., have no influence upon the discharge.

Fig. 12.



Electric organ of the Torpedo.

Indeed, if we cover the separated organ with pre-ared frogs, and then apply the conductors of the gal-anometer upon the two surfaces, and irritate the erves, we observe that the frogs contract, and the needle deviates, thus indicating a current which circulates, as usual, in the galvanometer from the dorsal to the abdominal surface of the organ.

By proceeding in this way, we observe another very curious phenomenon, namely, that the discharge is obtained sometimes in one portion of the electric organ, and sometimes in another. For this purpose it is sufficient to irritate separately each of the nerves of the same organ; and we find that all the frogs arranged thereon do not contract, but only some of them, viz. those which occupy the point where the irritated nerve ramifies. We can obtain these discharges for a short period only. Nevertheless, if we irritate the nerves by passing an electric current through them, the separated organ retains the faculty of giving a discharge for a greater length of time. The current which traverses the nerve of the electric organ of the torpedo, obeys the same laws as those which regulate its action upon muscles. At the moment that the current begins to circulate in the nerve of this organ, it excites the discharge; if it continue to traverse it, the discharge no longer takes place, but we can renew it by interrupting the current.

Whilst the organ is very fresh, and just separated from the living animal, the effects described belong to the *direct* current (that is to say, to a current which proceeds in the direction of the ramification of the nerve) as well as to the *inverse* current. But in proportion as the action of the current becomes weaker, the phenomena change; that is to say, the direct current excites the discharge only at its entrance, and the inverse current only at the moment of its interruption. The same result takes place, as we shall hereafter find, when the current acts on the mixed nerves and excites the contraction of the muscles.

It should also be observed, that in order to excite the discharge, the current must be made to act nearer and nearer the peripheral extremity of the nerves, in proportion as the vitality of the separated electric organ grows weaker. It also follows, from these facts, that the circulation of the blood is not absolutely necessary to the electric discharge, since the organ preserves this faculty even when separated from the animal, when deprived of blood, and when the circulation in it no longer takes place.

The discharge has been found to continue after the parenchyma of the organ has been pierced and cut in various directions, even when the organ had been completely separated from the torpedo; but it ceases when we coagulate the albumen, of which it is in a great part composed, by plunging it into boiling water, or by bringing in contact with it an acid.

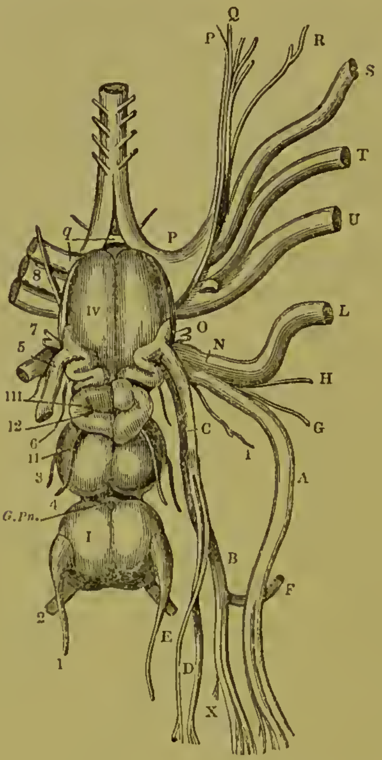
All these facts prove the influence of the will of the animal over the discharge,—an influence which is exercised by means of the nerves supplied to the organ.

These nerves, then, are neither nerves of sensation nor of motion. They possess no other function than that of exciting and bringing into action the organ in which they are distributed.

It was important to ascertain what influence the brain exercised over the discharge. For this purpose I exposed the brain of a living torpedo, by making a

Brain and Cerebral Nerves of the Torpedo.

Fig. 13.



I. Cerebrum.
II. Optic lobes.
III. Cerebellum.
IV. Medulla oblongata and electric lobes.

1. Olfactory nerves.
2. Optic nerves.
3. Motores Oculorum.
4. Pathetici.
5. Trifacial nerves.
6. Abducentes.
7. Auditory nerves.
8. Pneumo-gastric nerves.

A, B, C, D, E, F, G, H, I, N, X. Different ramifications of the trifacial nerve.
P, Q, R, S, T, U. Ramifications of the pneumo-gastric nerve.
L. Branch of the trifacial distributed in the electric organ.
U, T, S. Branches of the pneumo-gastric nerve distributed in the electric organ.
R. Twig, a part of which is distributed in the electric organ.
Q. Lateral and recurrent nerve, furnished with a ganglion at its base.
P. Branch distributed to the œsophagus and stomach.
G. Pn. Pineal gland.

horizontal incision of its aponeurotic case, and arranged the prepared frogs and the galvanometer upon the body, in order to perceive how, and at what moment the discharge took place.

When we irritate the first lobes of the brain (the olfactory lobes) there is no discharge, nor is there if we do the same with the optic lobes, and with the cerebellum. These three protuberances of the brain may be removed without the torpedo being deprived of the faculty of giving the discharge.

A fourth part only of the brain remains, which I have named the *electric lobe.* This can scarcely be touched before shocks take place; and according as we touch the right or left side, the corresponding organ gives them. We may remove all the other lobes of the brain without affecting the electrical function; but if the fourth lobe be torn, the function is permanently destroyed, although the others be left untouched.

A fact not less extraordinary is, that when even the torpedo ceases to give the discharges, if we irritate this electrical lobe, they recommence afresh; and if we wound it, still more violent shocks ensue, which, in some few instances, I have found to have an inverse direction to that which is usual.

To complete the examination of the phenomena presented by the torpedo, I ought to add that this fish ceases to manifest its electrical properties when plunged into water at about 0° centig. [=32° Fahr.]; but it reacquires them when put into water at a temperature of + 15° or 20° centig. [=59° Fahr or 68°]. We may repeat these alternatives a certain number of times upon the same animal.

In water, heated to about + 30° centig. [=86° Fahr.], the torpedo soon ceases to live, and dies while giving a great number of violent discharges.

When it is frequently irritated while in water, especially by compressing it about the eyes, it gives numerous shocks; and then ceases to do so, even though we continue to excite it. After a certain period of repose its faculties return.

Narcotic poisons, strychnia, and morphia, administered to these animals in large doses, quickly kill them, after exciting a great number of violent and rapid discharges. In small doses these poisons over-excite the torpedo, and in this state the slightest irritation procures shocks. I have known a shock produced by giving a blow to the table on which the animal was placed. Touched at the tail, it is immediately obtained; but if we divide the spinal marrow, the parts situated below the section are no longer able to give it: it is then a discharge produced by a *reflex action* on the spinal marrow.

The analogies between muscular contractions and the discharge of the torpedo are complete: what destroys, augments, and modifies, the one, acts equally upon the other.

Electric Phenomena of the Gymnotus.—Respecting the gymnotus, another electric fish found in some of the lakes of South America, I have but a few words to say, as the animal has been but little studied. I egret I cannot here quote a long passage from the vork of the celebrated Humboldt, in which he describes he method of capturing the electric eels as adopted by he South American Indians. They drive their horses nd mules into the muddy lakes where these fishes live. These commence the contest by giving very violent and ery numerous discharges to the horses and mules; and ot unfrequently kill them. After a long fight, the ymnoti, exhausted by fatigue, float on the water, and pproaching the margin of the pools, are easily captured y the hunters by means of harpoons attached to long ords.

The observations of Humboldt have proved, that the ischarges of this fish, like those of the torpedo, take lace without any muscular movement being necessary; nd that when the brain is removed this phenomenon no nger occurs, even though the spinal marrow be irritated. he influence possessed by the different parts of the rain on the electrical phenomena, requires to be more refully and attentively studied than it has hitherto een. The mode of catching the fish proves, that the scharge is voluntary, that the function becomes weakned by being exercised too often, and that it is restored y repose.

Faraday, who made some experiments upon a living ymnotus in London, succeeded in obtaining, from the scharge of this fish, all the phenomena of the electric urrent; namely, the spark, electro-chemical decomsition, the action upon the magnetic needle, &c. urthermore, he compared the shock given by this fish to that of a battery of Leyden jars charged to its highest degree; and he concluded, from his experiments, that a single medium discharge of this animal, is equal to that of a battery of fifteen jars, containing 3500 English square inches, charged to its highest degree. It cannot, therefore, be a matter of surprise that a horse sinks under a number of successive discharges given by the gymnotus.

The most important result which Faraday obtained, was that respecting the direction of the discharge. The cephalic extremity is the positive, and the opposite extremity is the negative pole; so that the current circulates in the galvanometer from the head towards the tail of the animal. This arrangement explains the stratagem employed by the animal when giving the shock for the purpose of killing a fish; it coils itself so that its victim is enclosed in the concavity formed by its body.

I have myself recently made some experiments upon a gymnotus, which had lived for several months in the palace of the King at Naples; and have verified all the facts observed by Faraday. The only important and new result which I obtained was, that the fish possessed the power of discharging voluntarily either the whole or only a part of its organ. Fresh researches, however, are necessary to substantiate fully this fact. We know nothing of the other electrical fishes; and I can, therefore, only mention their names to you.

What does the organ of the electrical fishes consist of? What electrical apparatus is analogous to this organ? It is very difficult to answer these questions satisfactorily. The electric organ of the torpedo is composed of from 400 to 500 prismatic masses, comparable to grains of rice, placed side by side, each of which is composed of superimposed vesicles. From this general arrangement, the entire organ resembles a honey-comb. Each of the component prisms present a certain number of diaphragms, which divide it perpendicularly to its axis, and which, in fact, are nothing more than the aponeurotic walls of the neighbouring vesicular masses. Nervous ramifications, consisting entirely of primitive nerve fibres, are distributed over hese walls or diaphragms. Savi, Robin, and Wagner, have studied this structure.

The great resemblance, or, to speak more accurately, he identity of structure of all these vesicles, leads us o assume that they are the true elementary organ of he electric apparatus: the truth of this hypothesis is lso demonstrated by the identity of their composition; or all are filled with the same dense liquid, composed f about $\frac{9}{10}$ of water and $\frac{1}{10}$ of albumen, with a little ommon salt. Experiment proves directly, that each of hese vesicles forms the elementary organ of the electric pparatus. I have removed from a living torpedo a ortion of one of its prisms, about the size of the head f a large pin, and I placed it in contact with the nerve f the galvanoscopic frog, and frequently observed contractions produced in the frog, on pricking the fragment f the prism with a piece of glass, or any other pointed ody. Now, if you consider that each of the prisms is composed of a very large number of vesicles or elementary organs, that Hunter counted 470 prisms in each organ of the torpedo, you will understand that the discharge, being proportional to the number of vesicles, must necessarily be very strong.

The electric organ, then, is a true multiplying apparatus.

Volta supposed that it was a pile which the animal itself rendered active by compressing its organ, and thus establishing the contacts between the latter and the skin. But the experiments which we have described to you, in no way confirm this hypothesis. It has been lately stated, that the electric organ was analogous to an electro-magnetic coil, and that the discharge was a phenomenon of *extra-current* or of *induction.*

If we assume, what microscopic observation proves, that in each vesicle there exists a nervous filament, which here divides, it is difficult to discover the analogy between the electric apparatus of the torpedo and an electro-dynamic coil.

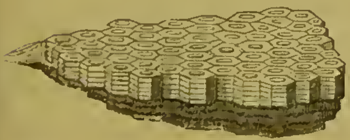
Let us advance an hypothesis, which we shall hereafter find to be supported by facts, or, at least, by powerful analogies.

Suppose, that every time the nervous irritation reaches one of the elementary vesicles of the organ of the torpedo, that the two electricities separate. Heat, which acts on the tourmaline, and on some crystallised metals, separates the two electricities: chemical action operates in the same way; as do also the mechanical actions, friction, and pressure. Suppose, that nervous irritation acts in this way on the vesicle of the electric organ. The identity of structure, and the arrangement of each of the vesicles, is such that each of the prisms becomes a pile, but only for the infinitely small period of the duration of the irritation; and, consequently, the entire organ will be a multiplying apparatus, which will remain charged only for an instant, since it is surrounded by conducting bodies. The discharge will take place partly outside the surrounding medium and partly in the interior of the organ: but so much more outside as the medium will be a better conductor than the interior of the organ. Yet it may be remembered that we have shown, by experiment, that this discharge really takes place internally.

It results from this hypothesis, that the opposite electrical conditions ought always to be found at the long extremity of the prisms; and that their intensity

Fig. 14.

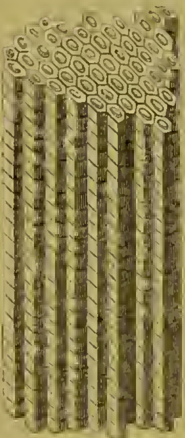
Dorsal surface (positive).



Ventral surface (negative).

Electric Organ of the Torpedo

Cephalic extremity (positive).



Caudal extremity (negative).

Electric Organ of the Gymnotus.

should be proportional to the length of these prisms; that is, to the number of cells of which each is composed. It is important to remark, that these hypotheses are confirmed by experiment.

In fact, the relative position of the poles in the gymnotus, correspond to those of the poles of the torpedo as regards the extremity of the prisms. In the first of these fishes, the prisms are extended along the axis of the body of the animal, that is, from the head towards the tail, or *vice versâ;* in the second, on the contrary, the prisms have their extremities in contact with the back and the belly. Hence, then, in the gymnotus, the poles are the head and the tail, and in the torpedo we find them at the back and the belly.

Silurus.—It remains for us to examine the direction of the current of the silurus; if we are to judge from the structure only of the organ in this fish, we must conclude that two poles are, as in the gymnotus, at the head and the tail.

The intensity of the electric discharges, is the strongest at the points of the organ nearest to the mesial line; there, also, the height of the prisms, and the number of nervous filaments are the greatest.

Microscopic anatomy may also render a great service to physics, by studying the electric organ of fishes, and particularly by establishing exactly the distribution of the nervous filaments in the elementary organ or cell. This cell appears to be largest in the silurus, and, therefore, it is in this fish that the structure should be studied.

What happens in the electric organ, is certainly anagous to electric induction: the constancy of the irection of the discharge, indicates a determinate irection in the action of the nervous force; and there ppears some foundation for this supposition, when we onsider that the excitation of the fourth lobe, and of he electrical nerves of the torpedo has no other effect han that of producing the discharge.

Proper Current of the Frog.—Lastly, I must menion to you another phenomenon of animal electricity hich has hitherto offered, by its speciality, some anagy to those we have observed in the electric fishes. refer now to the *current proper to the frog.*

Galvani discovered, and all philosophers after him ave observed, that a frog, prepared according to his sual method, contracts when we bring the lumbar erves in contact with the muscles of the thigh or leg. Nobili was the first to study this phenomenon by the id of the galvanometer. Here is his fundamental exeriment: a frog prepared in the usual manner is placed etween two small glasses containing distilled water, in uch a manner that on one side the lumbar nerves, on he other, the legs, are immersed in the liquid. Matters eing thus arranged, the circuit is closed by plunging nto the two glasses the two platinum extremities of a galvanometer. Observe the needle; it deviates, and rom 0° where it was, it reaches 5°, 10°, and even 15°. You see that the direction of deviation indicates a current circulating in the frog from the legs to the nerve; that is to say, from the legs to the upper part of the animal.

The signs of the current are augmented in intensity if, in place of using a single frog, I form a pile with a number of them.

This arrangement is very easily understood: I employ the varnished tray before spoken of, when treating of the muscular current. I place on it some frogs, prepared in such a way that the nerves of the first animal touch the legs of the second, and the nerves of the second the legs of the third, and so on. I thus have a pile, one extremity of which is formed by the legs, and the other by the nerves. I plunge the two poles of this pile into two cavities of the tray, which contain either a very weak saline solution or distilled water. Into these I also put the two extremities of the wires of the galvanometer. You see that the needle deviates, and indicates precisely, as in the experiment of Nobili, the existence of a very energetic current which circulates from the legs to the nerves in each of the frogs which form the pile. I have repeated and varied in a thousand different ways this experiment, which has enabled me to ascertain that the variation of the needle is proportional to the number of frogs composing the pile; that it is more considerable when we employ an alkaline or saline solution, or, better still, an acid solution, than when we employ distilled water; that, whatever be the liquid employed, the direction of the current is constant, and circulates always in the pile, from the feet to the upper part of the frog.

On repeating these experiments, you will observe that at the moment when the galvanometer indicates the presence and the direction of the current, the frogs contract.

The contractions are analogous to those observed by Galvani; they take place whenever we complete the circuit with a conducting body, as, for example, with a wick of cotton, or a piece of paper moistened with water, or any conducting liquid, provided that the arrangement be such that the conducting substance communicates on one side with the nerves, and on the other with the muscles of the animal. These contractions are also observed at the moment when we interrupt the circuit.

This current was at first called the *current of the frog*; but for this name I afterwards substituted another, that of the *proper current of the frog*, because until recently it was in the frog alone that we could recognise its existence.

I have endeavoured to ascertain what part of the inferior extremity of this animal was necessary to the production of the current, or what influence the different parts of the limb had upon it. I will show you an experiment which will solve these questions.

Here are two piles opposed to each other, each formed of the same number of elements. One of them is composed of six frogs, prepared according to the method of Galvani, the other of six legs only, the thighs and spinal nerves being removed. The six elements of the first touch the six of the other; but their arrangement is inverse, so that at the point of junction there come in contact, on one side, nerves, and on the other, the upper end of the leg. Thus the two piles are opposed: I put the wires of a galvanometer in connection with the two extremities of the two piles, and I obtain no signs of a differential current.

The proper current of the frog has, then, for the organic element, the leg only.

Recently, by studying more attentively the proper current, I have satisfied myself that it is a phenomenon which appertains to all animals. Here is the enunciation of the fact: in every muscle endowed with life, in which the tendinous extremities are not equally disposed, there exists a current directed from the tendon to the muscle, in the interior of the latter. All animals have some muscles, in which one tendinous extremity is narrower than the other; and which, at one part, forms a kind of cord, and at the other, becomes broader and ribbon-like. In the frog, and many other animals, the gastrocnemius has this character: in birds, the pectoral muscle presents this arrangement. When we form a pile with these muscles, we find that a current circulates in the muscle, from the tendinous extremity to the muscular surface.

In arranging this pile, we must carefully avoid exposing the internal part of the muscle, and we must especially place one element in contact with another, in such a manner that the tendinous extremity touches the surface of the muscle, and never its interior: indeed, the latter ought to be as far as possible from the endon. Without this precaution there will be, in the ircuit, the muscular current which, being directed rom the interior to the surface, would have a direction recisely the reverse of that of the proper current. The existence of the proper current of the frog, in all nimals in the way described, was found at the same ime by M. Cima, by M. Boy-Raymond, at Berlin, nd by myself.

Having thus ascertained the conditions on which the roper current depends, I think that I may generalise s origin, and connect it with the muscular current. his community of origin is principally demonstrated y the identity of action which the different circumstances that modify the organism and life of animals, xercise upon the muscular current. In fact, whether e current be muscular or proper, the action exercised n it by heat, narcotics, sulphuretted hydrogen, and e degree of integrity of the nervous system is the ame.

Anatomists, and especially Bowman, have lately emonstrated, that the elementary muscular fibres are mmediately continuous with the tendinous fibres, and at the sarcolemma which invests the muscle, ceases ruptly where the tendon begins. We may, then, ith some probability, consider the tendon as being the same electric condition as the interior of the uscle; and, therefore, when we form, by means of a od conductor, a circuit or communication between e tendon and the sarcolemma, we put into circulation portion of the muscular current.

LECTURE XI.

PHYSIOLOGICAL ACTION OF GRAVITY, LIGHT, AND CALORIC.

Argument. — 1. *Action of Gravity.* Experiments of Hunter and Knight on the influence of gravity on the germination of seeds.
2. *Action of Light.* Influence of light on the development, and on the colours, of animals. Influence of light on the respiration of plants; illustrative fact observed in the process of the Daguerreotype. Influence of light on the germination of seeds, and on the direction of the roots of plants.
3. *Action of Caloric.* A certain degree of heat is essential to vitality. Actions of contact take place only at certain temperatures; these probably have some agency in the phenomena of living beings, especially in fecundation. Influence of temperature on the life of frogs. Relation between respiration and temperature. Capability of man and other mammals to bear extremes of heat and cold. Refrigerating process of the animal body.

We have hitherto spoken of the development of heat, electricity, and light, in organised living beings; but we must now study their action upon these bodies.

Action of Gravity.—I think it unnecessary to say that, in speaking of the action of gravity upon living beings, I do not mean to notice that which is exercised upon bodies generally; which makes them, when left to themselves, fall to the earth, and which causes them to press on the surface which supports them, and to maintain themselves *in equilibrio* when their centre of gravity is supported or suspended.

But I wish to speak of a peculiar phenomenon which presents itself in the development of vegetables, and in which it is impossible not to recognise the effect of gravity.

In general, the seeds of all vegetables germinate and shoot, by manifesting the tendency which their roots have to descend, and their stems to ascend. Experience proves, that the opposite direction which these parts of the plant take, is owing neither to the moisture of the soil, nor to the action of light or of atmospheric air. The roots continue to descend, and the stems to ascend, even when their natural position is inverted; that is to say, when the latter is placed in contact with the earth, and the former is submitted to the action of light. We are indebted to Knight for some ingenious experiments, which, if they do not entirely clear up this subject, at least have demonstrated the existence of one of the causes which preside over this phenomenon.

Hunter was the first who observed, that if a barrel filled with earth, in the centre of which were some beans, be rotated, for several days, horizontally, the roots pointed in a direction parallel to the axis of rotation.

Knight fixed some garden beans on the circumference of a wheel, supplied them with moisture, and kept the wheel revolving for a considerable time. He found, that when the wheel was vertical, the radicles or roots of the young plants pointed towards the cir-

circumference, and the stems towards the centre of the wheel; but when the wheel was horizontal, the roots and the stems pointed obliquely, the roots being always directed towards the circumference.

By considering Knight's experiment in connection with the first one quoted, and which demonstrates that the direction of stems and roots is under the influence of gravity, it follows, that in the second experiment these point obliquely, in order to place themselves between the horizontal position which tends to make them acquire the centrifugal force, and the vertical, which is natural to them, and which they take in their ordinary conditions.

It is evident that, in order to find an explanation of the facts discovered by Hunter and Knight, we must admit: —

1st. A more or less liquid condition of the new parts of the young plant;

2ndly. A different density in the different parts of the latter; and,

3rdly, That the denser parts of the new plant are directed, at least in the first stage of germination, towards the roots.

From these conclusions it follows that, in the case of the vertical wheel, the parts of the young plant, being submitted to the action of the centrifugal force only, developed themselves by having their densest parts, namely, their roots, at the circumference; while, in the case of the horizontal wheel, they took an intermediate position, between that which the centrifugal force impressed on them, and that which they would have acquired if they had been under the influence of gravity only.

Dutrochet, without denying the influence of gravity upon the ordinary direction of roots and stems, admits, nevertheless, a second cause for this phenomenon. It may depend on the unequal development of the cellular system of the roots and stems, and on the different turgescence which endosmose produces in the cells of this system.

Action of Light on Animals.—Let us now speak of light.

We know nothing, or scarcely any thing, respecting the action which this agent exercises upon animals. Edwards has proved, that the ova of the frog are more rapidly developed in the sun than in darkness; and also, that tadpoles more quickly and completely changed into frogs under the same condition.

The colours of animals are brighter in proportion to the intensity of the light to which they are submitted. It has been asserted, that the quantity of carbonic acid exhaled by the skin of an animal, is augmented by the action of the solar rays. But not knowing which of the rays of the sun produce these effects, we do not know whether these effects are due to the chemical action of the rays, however probable this may otherwise appear.

Action of Light on Vegetables.—The action of light on vegetables, although still obscure, is better known with respect to its laws, and exercises a great influence upon the life of these beings. It has been proved, that the respiration of a plant, namely the decomposition of carbonic acid effected by the green parts, the fixation of carbon, and the exhalation of oxygen, take place only under the influence of solar light: in darkness the plant, on the contrary, absorbs oxygen and emits carbonic acid. In light, vegetables become coloured, and their tissues harden;[1] whilst, in darkness, they lose their colours, and their stems elongate and become soft. A very vivid artificial light acts like that of the sun, although in a much feebler degree. We possess only a single fact capable of explaining this singular action of the sun. It has been observed, in executing images with the Daguerreotype, that the green parts of vegetables, and, in general, all green bodies, are not represented, being the contrary to what takes place with objects of other colours. Now, since it is well established that, in the formation of images, by the well-known process of Daguerre, these are owing to the influence of the chemical rays of solar light, we are compelled to assume that the green parts are not produced, because they entirely absorb the rays. A very natural conclusion to draw from this fact is, that the production of the green matter in vegetables, and the extraordinary property with which this substance is endowed, of decomposing carbonic acid under the influence of light, of appropriating to itself the carbon and of exhaling the oxygen, take place under the chemical action of the solar rays. Nevertheless, it follows from some of Draper's experiments, that the luminous rays, properly so called, those which act more especially on the retina, viz. the yellow rays, are those under whose influence principally the green matter of vegetables decomposes carbonic acid. With respect to the absorption of oxygen and the exhalation of carbonic acid in darkness, it is admitted, that these are effected independently of the condition of life.

You perceive, by the little which I have been enabled to state to you respecting this very important subject, how very limited our knowledge is respecting it. What is really the immediate chemical principle which acts thus in plants, and which is capable of accomplishing a chemical action, whose intensity has no parallel in the most energetic ordinary chemical affinities? What share does the organism take in this action?

In a balloon, filled with water acidulated with carbonic acid, I exposed to the light some leaves which had undergone a very strong trituration, and I obtained no trace of oxygen, whilst, in another similar apparatus, in which the leaves were uninjured, I soon discovered its presence. I may also add, that there is a great number of green vegetable parts, containing a substance analogous to that of the leaves, and having no action upon carbonic acid in solar light. It is very desirable that these experiments should be extended and varied, in order to establish the influence of organsation on the respiration of plants.

The influence of the luminous rays on germination has been of late spoken of. Some observers have asserted, that the violet, or chemical rays, promote it;

whilst others maintain an opposite opinion. This contradiction is a fresh proof of the necessity of having recourse to more exact experiments. It is not difficult to discover the source of the different results obtained by experimentalists, since they have not employed the simple rays of the solar spectrum; but the coloured rays obtained by the passage of solar light through glasses of different colours. Now, in general, a solar ray which traverses coloured glass, is not deprived of all the rays which possess colours different to that presented by itself.

A curious effect of light upon vegetables is seen in the tendency which certain roots have to avoid it; and, on the contrary, which others evince to seek it. The roots of many plants belonging to the family *Crucifera*, do the former; those of *Allium Cepa*, the latter. According to Dutrochet, the intimate structure of the cortex of roots, is different according as they seek or avoid the light; and from this difference results the tendency which they evince to direct themselves one way or the other. Generally, in the barks of young plants, the utricles are largest in the median layers of its thickness, and decrease in size as they approach both towards the interior and the surface; but in some cases this decrease is less towards the outer layers; while in others, it is less towards the inner ones. Under the influence of solar light and heat, the plant transpires and the utricles yield up the water they contain. It follows, therefore, that the roots direct themselves towards the light, when the structure of the internal layer of the bark is denser than that of the external; and the reverse effect takes place when the external layers possess a greater density.

Action of Caloric on organised Beings.—I have, in the last place, to notice the influence which heat exercises over living organised beings.

A suitable temperature is a condition essential to life. The possibility of living, is indeed comprised within certain limits of temperature, beyond which there are no examples of the development and preservation either of animals or vegetables. With respect to the general mode of action of heat, it will be sufficient to say, that all the physico-chemical phenomena of living bodies can be produced within those limits of temperature, which are also the limits of vegetable and animal life.

We now know, that the different actions of contact take place only at a certain temperature, and we must not forget that these actions intervene in a great number of the phenomena of living beings; and the little which we know of these actions gives us an imperfect notion only of all the uses which yet remain to be made of them.

The fecundation and germination of plants occur at a certain temperature only, and the actions of contact play an important part in these mysterious phenomena.

Independently of this general mode of action of heat on living beings, we must more particularly study its influence upon animals.

From that classical work, entitled, *On the Influence* *of Physical Agents upon Life*, I shall draw the most important discoveries that have been made on this subject, and to which I shall confine my notice.

Edwards, when studying the life of frogs, in river water at different temperatures, saw that at 0° centig. [= 32° Fahr.] these animals lived eight hours; at a temperature of + 10° [= 50° Fahr.] they lived only six hours; at + 16° centig. [= 60°·8 Fahr.] two hours; at + 22° centig. [= 71°·6 Fahr.] from seventy to thirty-five minutes; at + 32° [= 89°·6 Fahr.] from thirty to twelve minutes; and at + 42° centig. [= 107°·6 Fahr.] death was instantaneous.

The very great influence exercised by slight variations of temperature upon the life of frogs, cannot be ascribed to the different quantity of air dissolved by the water at these different temperatures. We know, indeed, that this varies very little in different seasons, and yet we have seen that the differences of temperature of the year produce very marked effects upon the life of frogs immersed in water.

Edwards found that the quantity of air which these animals respire is greater in proportion to the higher temperature of the medium in which they live; so that the quantity which is usually dissolved in water, even when constantly renewed, is not sufficient for them if the temperature be at all elevated. Frogs, then, only live in water at very low temperatures; except they can come to the surface, and respire atmospheric air. We observe, with fish, analogous phenomena. Immersed in a certain quantity of water containing air in solution, ut which is not in contact with the atmosphere, the uration of their life is longer in proportion as the temperature of the water is lower.

We have already described an experiment of this ind, made on a torpedo contained in water at + 28° entig. [= 82°·4 Fahr.]; it soon died, in giving a eries of strong shocks; and, on the contrary, lived a ng time in cold water, giving few and feeble discharges.

The relation found between the respiration and the emperature of the medium, in which the animals, we re now speaking of, live, is a further proof of the hemical nature of this function.

Man, and mammals in general, are able to support a emperature much higher than that which is proper to hem. The observation of Tillet and Duhamel is well nown. They saw a young girl remain for twelve inutes in an oven, the temperature of which was 128° entig. [= 262°·4 Fahr.]. Delaroche and Berger introduced rabbits, cats, and several other vertebrated nimals, into an oven heated to from + 56° to + 65° entig [= 132°·8 to 149° Fahr.]. The animals died at he expiration of some minutes. These observers concluded, from a great number of experiments, that vertebrated animals, when exposed to a dry atmosphere eated to + 45° centig. [= 113° Fahr.], are near he extreme limit of temperature in which they are apable of living. It appears, then, that man alone s endowed with the faculty of supporting a higher emperature; indeed, besides the instance already mentioned, there exists others, of the truth of which no doubt exists.

Dobson tells us of a young man who remained for twenty minutes in an oven heated to + 98·88° centig. [= 210° Fahr.]; his pulse, which was usually sixty-five, was, when he came out, a hundred and sixty-four. Berger supported, for seven minutes, an atmosphere at + 109° centig. [= 228°·2 Fahr.], and Blagden was inclosed in one heated to +127° centig. [=260° ·6 Fahr.].

But it is different if the heated air be saturated with aqueous vapour. Berger could remain only twelve minutes in a vapour bath whose temperature was raised from +45°·25 to 53°·75 centig. [=113°·45 to 128°·75 Fahr.]. The temperature which a man can support when in heated water, is still less than that which he is capable of enduring in a vapour bath. We shall soon see what are the causes of this difference.

It was important to ascertain the variations in the temperature of animals exposed to different degrees of heat. If we limit ourselves to the ordinary variations of temperature of climates and seasons, the heat of the human body is not perceptibly modified. The numerous experiments of Dr. John Davy on this point, give only very slight differences. Franklin was the first to observe, that the temperature of his body was +35°·55 centig. [= 96° Fahr.], whilst the air was at +37°·77 centig. [= 100° Fahr.]. The conclusion drawn from this fact was, that warm-blooded animals had the faculty of remaining in a degree of heat below that of the medium in which they exist. Nevertheless, t was necessary to ascertain whether, if placed in a emperature much higher than that which is natural to man, the temperature of his body did not undergo some variations. Delaroche and Berger found an increase of 5° centig. [= 9° Fahr.] in the temperature of one of hem, who had remained for eight minutes in a chamber heated to +86° centig. [=186°·8 Fahr.]. The same experimentalists have repeated their trials upon mammals and birds, and ascertained that the exposure of these animals to a hot and dry air produced an elevation in heir temperature, but that it cannot exceed 7° or 8° centig. [= 12°·6 to 14°·4 Fahr.] without causing death.

An elementary knowledge of physics suffices to explain the effects of the exterior temperature on the heat of animals. The formation of aqueous vapour, which constantly escapes by the skin of an animal, is a permanent cause of refrigeration for it. This fact explains why in hot and dry air the temperature of the animal is not so high as when the air is loaded with vapour.

There exists, then, in the animal, a constant source f heat, and a constant cause of refrigeration, and an lmost invariable temperature is maintained, notwithstanding the variations which take place in the exterior media, whether colder or hotter than itself, since the ause of cooling is more energetic in proportion as the emperature is higher, and *vice versâ*.

Edwards tried a great many experiments with the iew of determining whether there existed any difference in the refrigeration going on in an animal, by its immersion in an atmosphere colder than its own, according as this was moist or dry; and the conclusion was, that it was the same in both cases. If we consider that in moist air heat ought to be diffused more easily than in dry air, we may explain the result which Edwards obtained, by saying that the refrigeration produced by the more considerable evaporation which takes place in dry air, can be compensated for by the loss of heat effected by contact with moist air. But there is, on the contrary, a very considerable difference in the cooling of an animal, according as the atmosphere is calm or agitated. When it is tranquil, and at a temperature below that of our own body, we lose heat by evaporation, by contact with air, and by radiation. The presence and nature of the gas and its agitation, have no perceptible influence on the loss by radiation; but this is not the case with the loss occasioned by evaporation or by contact with the air, which is considerably augmented by the motion of the air. These results are evidently the consequence of the physical laws of the cooling of bodies in the air, and of the effects of evaporation. Parry relates, that he has often supported a temperature of — 17°·77 centig. [= 0° Fahr.], without suffering therefrom, when the atmosphere was calm; whilst a cold of — 6°·66 centig. [=20° Fahr.] was very annoying, when accompanied by even a slight wind. And the surgeon, who accompanied Captain Parry in his celebrated expedition, relates, that in a calmair, the sensation produced by a temperature of

46°·11 centig. [= — 51° Fahr.] might be compared to that which they experienced at — 17°·77 centig. [=0° Fahr.] with a breeze. It follows from this observation, that a certain agitation of the air will produce a sensation of cold equivalent to the effect of a fall of 29°·6 of the centigrade scale [=53°·28 of Fahr.].*

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* There is, I suspect, a typographical error in one, at least, of the temperatures referred to. Captain Parry (*Journal of a Voyage for the Discovery of a North-West Passage*, 1821, p. 145.), alluding to a temperature of — 55° Fahr. [=48°·3 centig.] observes, that "not the slightest inconvenience was suffered, from exposure to the open air, by a person well clothed, as long as the weather was perfectly calm; but in walking against a very light air of wind, a smarting sensation was experienced all over the face, accompanied by a pain in the middle of the forehead, which soon became rather severe." Is this the passage to which Matteucci refers? — J. P.

LECTURES XII. and XIII.

PHYSIOLOGICAL ACTION OF THE ELECTRIC CURRENT.

Argument. — Effects of statical electricity on living beings. Galvani's hypothesis of animal electricity. Convulsions produced in living and recently killed animals by the electric current.

Action of direct and inverse currents on the *sciatic nerve* of a frog. Two periods in the action of the current. *Reflex movements* caused by the current. Effects of the current on the nerves of recently killed animals.

Action of the current on *muscular fibre.* The contractility of the fibre is inherent.

Voltaic alternatives. Contractions renewed in the muscles of a frog by reversing the current.

Action of the current on *poisoned animals.* Further evidence that the muscular fibre contracts under the influence of the current, independently of the nerve.

Action of the current on a nerve to which a ligature has been applied.

Opposite effects of the direct and inverse current. Brequet's apparatus for measuring the contraction caused by the current. General conclusions. Effect of repose on a nerve which has been submitted to the electric current.

Theory of the action of the electric current on the nerves.

Effects of the electric current on the *brain,* on the columns of the *spinal marrow,* on the *roots of the spinal nerves,* on the *nerves of sensation,* and on the *ganglionic nerves.*

Effects of the interrupted current on the excitability of nerves; it more speedily exhausts the nerves than the continued current. Masson's apparatus.

Therapeutical uses of the electric current. Employment of it in paralysis and in tetanus; rules for its application. It is useless in the treatment of urinary calculi and cataract. Proposed employment of it in aneurism.

In this lecture I shall examine the physiological action of electricity.

Effects of Statical Electricity.—I need not dwell long on the effects of statical electricity on animals and vegetables; for though in old works on physics we find some strange and wonderful effects attributed to it, more accurate observations have completely disproved these statements. An animal or a plant, insulated and electrified by means of the electric machine, has hitherto presented no phenomenon peculiar to them, or different from those offered by inorganic bodies when submitted to the same influence. But this is not the case with respect to the action of the electrical discharge on animals.

Effects of Dynamical Electricity.—This subject is of the highest importance, and I am anxious to make you acquainted with every particular regarding it.

In a memoir, found among the manuscripts of Galvani, and bearing this title, in his own handwriting, *Experiments on the Electricity of the Metals,* dated 20th September, 1786, a fact is mentioned which certainly has had as much influence on the advancement of science, as any of the discoveries of Galileo and Newton. It is, that contractions are excited in a frog, recently killed and prepared according to the usual method of Galvani, when we touch its muscles and nerves with an arc composed of two different metals.

I shall not stop to explain to you how Galvani interpreted these facts, by assuming the existence of an *animal electricity* which the metallic arc merely discharged. After Volta had demonstrated, by the condensing electrometer, that the two electricities were separated by the effect of the contact of two metals, the idea of Galvani's animal electricity was abandoned, and it was generally supposed that the contractions observed in the frog, were simply the effects of the passage through the nerves, of electricity developed by the two metals. The two last lectures have taught you in what animal electricity really consists; and you must be convinced that Galvani's assumption was not erroneous, since a great number of facts discovered by him are certainly due to the electricity developed in, and proper to, animals.

The contractions excited in the frog, or in any other animal living or recently killed, when one of its nerves is traversed by the electric current, are quite independent of all animal electricity. It is by the examination of this first fact, that we shall commence our study of the action of electricity upon animals.

For some years after the discoveries of Galvani and Volta, every journal and every work teemed with particulars relating to them. The convulsions and the leaps observed in recently killed animals that are submitted to a sufficiently powerful electric current, at first gave hopes of the possibility of the restoration of life. This illusion, of course, soon disappeared, and science withdrew within the proper limits of its domains. Valli, Lehot, Humboldt, Aldini, Marianini, and Nobili, have subsequently studied the physiological action of the electric current.

As I cannot possibly here relate all their experiments, I must limit myself to a notice of those matters which, in the present state of science, appear to be best established.

Effects of the Current on the Sciatic Nerves.—In this rabbit, which you observe is firmly fixed by its four paws to the table, I expose the sciatic nerve in both thighs. I separate it as much as possible from the surrounding parts, then wipe it with some unsized paper, and introduce beneath it a band of gummed taffeta to insulate it completely from the neighbouring tissues. You perceive the effect produced when I transmit the current from a pile of ten elements along the nerve, by applying the two conductors at a little distance from each other, in such a manner that its direction is from the central part of the nervous system to the periphery of the nerve (that is, in the direction of the ramifications of the nerve). At the moment when I close the circuit, all the muscles of the thigh contract, the animal utters loud cries, its back becomes forcibly bent, and its ears are agitated.

These phenomena recur when I change the respective position of the electrodes; that is, when I cause the current to pass in the reverse way to that which it previously did, and direct it from the periphery to the nervous centres.

The effects which you observed at the moment when I closed the circuit, are repeated when I open it, by interrupting the communication of the conductors with the nerve, both when the current is in the first direction (*direct* current), and when it is in the second, or opposite direction (*inverse* current).

But during the time that the circuit is closed, whatever may be the direction in which the current is passing, the animal no longer presents any of these phenomena. We shall soon see what kind of action ought to be attributed to the latter during the time of its passage along the nerves.

If the current be applied to the nerve in such a way that it passes across the nerve instead of along it, the contractions are more feeble; and even entirely cease when the experiment is conducted in such a manner that all the current passes normally in the nerve.

In repeating these experiments upon different rabbits we have remarked that, in general, the signs of pain evinced by the animal are more violent at the commencement of the passage of the inverse current, and that the contractions are stronger and more obvious at the commencement of the direct current.

Whatever be the direction of the current in the nerves, it gives rise, both at its commencement and at its interruption, to analogous phenomena; but we constantly observe, that the most violent contractions are those which are excited during the first moments of the passage of the direct current. Marianini observed, that if a man close the circuit of a pile composed of a certain number of elements, by touching one pole with one hand, and the opposite pole with the other hand, the strongest shock is always felt in that arm in which the direct current circulates.

If we continue to experiment upon the same animal, all these phenomena more or less rapidly cease, according to the greater or less energy of the current, and the animal gives no further evidence of the passage of it. If the animal be then left undisturbed for some time, or, if we augment the force of the battery, the previous phenomena reappear.

But it is important to follow carefully the phenomena which take place in proportion as the action of the current upon the animal is prolonged, and before they completely cease. You will observe that, when the direct current is interrupted, the contractions of the inferior muscles (those which are placed below that part of the nerve to which it is applied) become more feeble, whereas they continue in the muscles of the back, and the agitation, and often the cries, of the animal continue. We see, also, that for the first few moments of the passage of the current, its effects are limited to contractions of the inferior muscles. When the current is reversed, the contractions of the muscles of the back, the movements of the ears, and the cries, are not manifested except at the moment of closing the circuit; while the contractions of the inferior muscles are scarcely perceptible. But the opposite effect takes place when we interrupt the circuit: that is to say, the contractions of these latter muscles continue, whilst those of the back, and the movements of the ears disappear, and the animal ceases to utter cries.

Two Periods in the Action of the Current on the

Nerves.—We must, therefore, divide into two different periods the action of the electric current on the nerves of a living animal. In the first, the irritation of the nerve is transmitted in all directions towards its central part, as well as to its periphery, both at the commencement and cessation of its action, and independently of the direction of the current. In the second period, the excitation of the nerve is transmitted towards its periphery, during the first moments of the action of the direct current, and at the instant of the interruption of the inverse current: on the contrary, when the direct current is interrupted, or when the circuit of the inverse one is being closed, the irritation of the nerve is transmitted towards the brain.

I may express the whole of these results in more simple terms: the current acts in the direction in which it is transmitted, when it begins to circulate in the nerve, and in the opposite direction when it ceases to circulate.

Before we proceed further, we must inquire how the electric current can occasion contractions of the muscles of the back and of the head, by acting, as we have seen in the preceding experiments, on nerves which are not distributed to them.

If you divide the spinal marrow of a rabbit transversely, and cause an electric current to pass along the crural nerve, you will observe that the contractions are, in this case, confined to the muscles situated below the point where the spinal marrow was divided; and if this was effected near its inferior extremity, no contractions will occur in the muscles situated above the excited nerve.

Reflex Movements. — The contractions excited in the muscles situated above the irritated nerve by an electric current, are then *reflex movements.* The excitation of this nerve is transmitted to the spinal marrow, and the latter, by a reflected action, produces contractions of muscles not supplied by the nerve irritated by the current. We may, therefore, say, in the language of Dr. Marshal Hall and other modern physiologists, that the electric excitation of a nerve which was at first centripetal, is transformed into a centrifugal one.

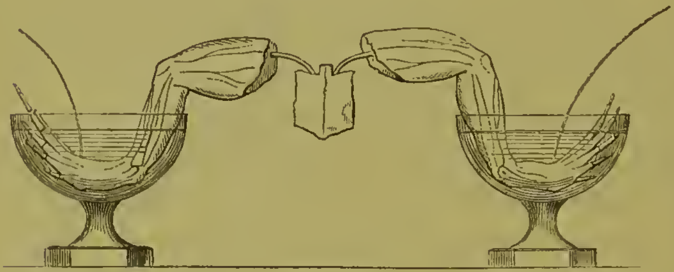
Effects on dead Animals.—Hitherto I have demonstrated the laws of the action of the electric current on the nerves of a living animal. I must now speak of this action on the nerves of animals which have been recently killed.

By submitting recently killed rabbits, prepared as in the preceding experiments, to the influence of a single element, we obtain the contraction of the inferior muscles, at the moment when the circuit of the direct current is closed, and when that of the inverse current is interrupted. By acting with a more powerful battery, the contractions of the same muscles take place as well when the current begins to circulate as when it ceases, whichever its direction may be. After it has continued to pass for a certain time, contractions no longer ensue, except at the commencement of the direct, or at the cessation of the inverse current.

These phenomena may be verified on all animals, but they are most easily shown in the frog.

I have here one of these animals, prepared according to Galvani's usual method, and from which the bones of the pelvis, and the lumbar vertebræ have been removed. This frog is placed astride with one foot in one glass full of water, and the other foot in another glass of water.

Fig. 15.



Experiment to illustrate the Action of a direct and an inverse Current on the Nerves of a Frog.

When I plunge the two conductors of a pile into these glasses, you will at first observe that the frog will leap out; and if we retain it forcibly in its place, contractions take place in both legs both when opening and closing the circuit, and, consequently, in the limb in which the current is direct, as well as in that in which it is inverse. But if we continue the experiment, we soon perceive the alteration already described; that is to say, at the moment when the circuit is completed, one limb only contracts, namely, that in which the current is direct; while, on the contrary, when we interrupt the circuit, the contraction takes place in the other limb, namely, in the one traversed by the inverse current. This series of phenomena is manifested, more or less speedily, according to the strength of the current, and the vivacity of the animal, but it is never absent. Thus, then, the frog is not only a galvanoscope of extreme sensibility, but it is also an instrument which may perform the office of a galvanometer, and like this, indicate the direction of the current which circulates in a portion of its nerves.

It was Marianini who first observed that contractions were obtained at the interruption of the circuit, and not at the moment when it was closed. To succeed with this experiment, it is necessary to introduce a frog into the circuit of a pile, and to close this by touching one pole with one hand, and plunging the fingers of the other into the liquid in which one of the extremities of the frog are also immersed. At first the current which circulates is very feeble, in consequence of the bad conducting power of the hand, but it goes on increasing in proportion as the fingers imbibe more of the liquid; and the frog does not suffer any effect at the commencement of the current, but only at its interruption.

Effects on the Muscles.—We have hitherto caused the current to act on the nerves of animals, and have determined the laws of this action. We have also examined the case in which it circulates in the entire animal, by traversing at the same time nerves and muscles. It remains now for me to notice the action of the current on the muscular fibre alone.

The difficulty surrounding such an investigation may be easily conceived; for we cannot with certainty remove every trace of the nervous substance, even when we remove from a muscle all its nervous filaments visible to the naked eye, as well as those which are only perceptible by the aid of a magnifying glass. However, it is upon a muscle thus deprived, as carefully as possible of its nervous filaments, that we are obliged to operate, and the following are the results obtained: —

By passing the current from twenty to thirty elements through the pectoral muscle of a pigeon, from which the nerves have been removed, as just mentioned, we always observe, that contraction takes place when we close the circuit. This contraction, moreover, lasts but for an instant, and appears to consist in a momentary shortening of the fibres. Whatever may be the direction of the current in relation to that of the muscular fibres, the phenomenon is the same. If the circuit be kept closed, and the passage of the current through the muscle be continued, the contractions reappear on opening the circuit. They are weaker than when we close the circuit; and if the passage has been prolonged for some time, they are entirely absent at the interruption of the circuit.

In general, the contractions obtained by acting on the muscle at the closure of the circuit, are more persistent than those which take place at the opening of it. The latter re-appear when the intensity of the current is augmented.

We may, therefore, conclude, that when the electric current acts on a muscular mass deprived of its visible nervous filaments, it excites contractions therein, both when the circuit is closed and when it is interrupted, whatever may be the direction of the current relatively to that of the muscular fibres; and, also, that the contractions which take place when the circuit is opened are the first to disappear.

If the discharge be that of a Leyden bottle, which is made to pass across a muscle,—for example, the gastroenemius of a frog,—it is curious to observe that the muscle contracts and continues in this state.

It now remains for me to notice the various circumstances which modify the action of the current on the nerves and muscles of living, or recently killed animals.

The *voltaic alternatives*, of which I am now about to speak, are the result of the same passage of the current in the nerve. Observe in what this phenomenon consists. We put a frog, prepared in the usual way, across two small glasses, containing pure, or slightly saline water, in such a way that the spinal marrow is immersed in one glass, and the legs in the other. We then close the circuit. If we allow the current to circulate for a certain time, say twenty or thirty minutes, according to the strength of the current, and then open it and close it again, no further contractions are obtained. But by reversing the direction of the current, the contractions re-appear; and they cease again, more speedily than in the preceding one, when the passage of the current has been prolonged. By again reversing the direction of the current, that is, by re-establishing it as it was at the commencement of the experiment, the contractions re-appear. These alternations may be repeated a certain number of times on the same animal. The intervals of time between the passage of the two currents, depend on the intensity of the current and the vivacity of the animal.

It is easy to prove that the diminution of the excitability of the nerve from the passage of the current, is principally manifested in the portion traversed by the latter. Suppose we have passed a current through the nerve of a frog, prepared after Galvani's method, and have prolonged the action until the contractions have ceased; if we then apply the conductors to a portion of the nerve more distant from the brain than that on which we first acted, we soon observe the contractions re-appear according to the laws already laid down. By continuing these experiments, exposing successive portions of the nerve more distant from the brain, similar results are obtained. We may therefore say, that the excitability of the nerve, roused by the current, retires towards the periphery according as its vitality becomes extinct. When we act on a living animal in the way described, we find that the signs of pain evinced when an electric current traverses its nerves, are also manifested when we act on those parts of the latter, nearer and nearer to the brain, according as its vivacity diminishes by the prolonged passage of the current. In both cases, it is the excitability of the nerve which becomes weakened by the passage of the current; and, as when a muscular mass is traversed by the latter, it is certain that the whole, or at least the greater part of the current passes, not by nervous filaments, but by the muscles, which are better conductors, it is natural that these filaments should preserve their excitability, and that they should be found also excitable by the current.

Effects of the Current on poisoned Animals.—It was important that the action of the current upon poisoned animals should be examined. For this purpose I made a considerable number of experiments, the principal results of which I shall now state.

The different methods of proceeding to ascertain the effects of various toxicological agents upon the excitability of the nerves to the passage of the electrical current may be reduced to two: one consists in ascertaining the number of elements necessary to excite contractions both in poisoned and in uninjured frogs; the other, and preferable method, is to compare the time required for the passage of a given current to destroy entirely the nervous excitability of a poisoned animal, and of another animal killed in the usual way.

Animals which have perished in hydrogen, azote, carbonic acid, and chlorine, and are submitted to the passage of the electric current through their nerves, present no difference from other dead animals which have not been subjected to the action of these gases. But this is not the case with those killed by hydrocyanic acid, or by the repeated discharges of a large battery through the spinal marrow. In these cases the current furnished by one, or even many elements, applied upon the nerves of an animal excites no contraction there, or if there be any they are very slight, and the passage of the current for a few seconds is sufficient to destroy them entirely. The muscles, however, when submitted to this same current, give very evident signs of contraction; thus proving what I have before mentioned, that the muscular fibre must possess the property of contracting, under the influence of the current, independently of the nerve.

Lastly, if the animals on which we act by the electric current have died in sulphuretted hydrogen, we never obtain contractions unless very powerful currents are employed, and even then the contractions soon cease; and that, whether we act on the nerves or on the muscles.

Effect of tying the Nerve.—I wish also to show you some differences observed in the physiological action of the current when the nerve on which we act has been tied. I expose and isolate the crural nerve of a rabbit, put a ligature on the nerve at about its middle, and afterwards transmit the current through the part above the ligature, namely, towards the brain. I obtain contractions of the back and signs of pain, both when I open the circuit and when I close it, whatever may be the direction of the current. Very soon, these effects are produced only at the commencement of the passage of the inverse current, and at the cessation of the direct one. If, on the contrary, I transmit the current below the ligature, I first obtain contractions of the leg, whether I open or close the direct or inverse current, and very soon, as usual, contractions cease, except at the first moment of the direct current, and at the termination of the inverse one. From these facts, then, it appears that a ligature on a nerve produces no other influence than that of insulating the effects of the current; that is, of producing the effects of its action on the nervous centres, separately from those which it has when acting upon the extremities of nerves. It is unnecessary to add, that if we operate on a dead animal, we obtain no signs indicative of pain.

In order to avoid errors in repeating these experiments, care must be taken to insulate the nerve completely from the moist parts which surround it, and to apply the ligature tightly. The best way of proceeding is to use a frog prepared in the usual manner, and then to suspend it by its nerve with a silk thread. In this way we are sure that no moist part around the nerve can divert any portion of the current; but if we neglect these precautions, a certain portion of the current may pass above or below the ligature, and thus confuse the results.

When we apply the two poles, one above, and the other below the ligature, as the current is not interrupted, but only weakened, it follows that the phenomena will be the same as if there was no ligature, except that they will be more feeble.

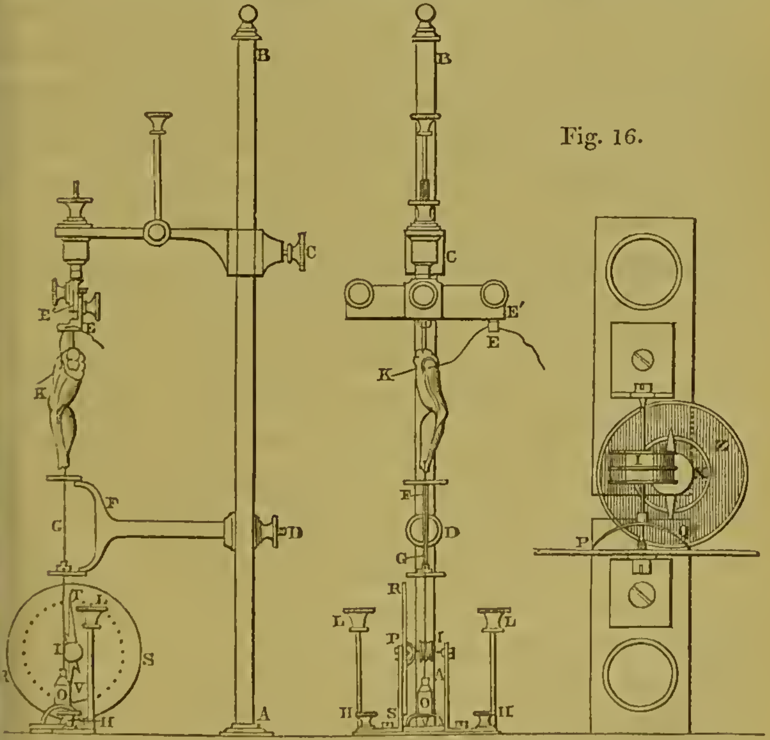
Different Effects of the Direct and Inverse Current.—I have hitherto purposely refrained from noticing here, the difference which exists in the loss of excitability produced by the passage of the electric current in the nerve, according to its direction. When we transmit the current in a frog, prepared in the manner I have described, and placed across two small glasses, we at first obtain contractions in the limbs both at the commencement and at the termination of the passage of the current, whatever be its direction. The second period of excitability, already described, soon follows, and in this there is contraction in the limb traversed by the inverse current, only at its cessation, and in that traversed by the direct current only at its commencement.

Let us now examine some phenomena which present themselves, when we continue the passage of the current. All contraction disappears, after a certain time, in the limb traversed by the direct current, and we see contractions continue in that limb only which is submitted to the action of the inverse current, when the latter is interrupted. This result, which may be obtained either on a living or a dead frog, which may be also produced by causing the current to act on the nerves only, evidently proves that the excitability of a nerve is much more weakened by the passage of the direct current, than by that of the inverse one. I shall here show you some facts relating to this subject, which appear to me clearly to prove that not only the inverse current affects less the excitability of the nerve than the direct one, but that it acts in a directly opposite way; namely, that whilst the direct current diminishes the excitability, the inverse current augments it.

If the nerve be traversed for several hours, say three or four, by the inverse current, it frequently happens that at the interruption of the circuit, the limb suffers a very violent contraction which lasts for a certain number of seconds, and might, therefore, be termed tetanic. This phenomenon ceases when we again close the circuit; but it is important to observe, that at the moment when we thus close it, there is a fresh contraction, after which the limb returns to its natural condition. This contraction, which occurs when we close the circuit of the inverse current, does not exist longer than the first moments of the experiment, and it has reappeared after the very prolonged action of the same current.

It was desirable to ascertain whether the contractions obtained on opening the circuit of the inverse current, increased within certain limits, in proportion to the time the current remained closed. To ascertain this it

Fig. 16.



Three views of Bréguet's Apparatus for measuring the Contraction of Muscles produced by the Electric Current.

was necessary to measure the contraction, and this I did by means of an apparatus contrived by Bréguet.

This apparatus consists of a solid brass support, A B, fixed upon a wooden stand in which slide two pieces of metal, C D, capable of being fixed in different places by means of screws of pressure (*vis de pression*). The piece of metal, C, is furnished with a vice, E, in which is to be held the morsel of spinal marrow of the prepared frog, and is fastened there by three screws. The other piece, F, of a fork-shape, is provided with a hole in each extremity of the fork, in which a very fine wire, G, is fixed or regulated. At one end of the wire is a fork, to which is affixed the claw of a frog; the other end of the wire is attached to a silken thread, which winds round the little pulley, I. Upon this another thread of silk is wound in contrary sense to the former, and to this is attached a small leaden weight, O. The axis of the pulley is furnished with a kind of double index, P Q, in the form of a semicircle. The axis is fixed upon two pivots, which admit of being more or less approximated. One of these pivots is the centre of a circle, R S, which bears a division. A long ivory needle, T V, is attached to this pivot; it is very light, and turns with the slightest possible touch. The use of this ivory index is obvious. In effect, when this index is brought in contact with the semicircular one, P Q, which is attached to the axis of the pulley, and the pulley is put in motion, the movement is communicated to the ivory index, and this latter will stop at the point at which it arrives in its gyration, even when the pulley is brought back to its former position by the little weight. It must be allowed, that without such an index as the one described, it would have been impossible to have judged of the extent of the movement of the pulley produced by the contraction, on account of its short duration. The weight I have been in the habit of using is 0·600 gramme [= 9·266 grs. troy], sufficient to allow of the limb returning to its position after the contractions have ceased; a heavier weight than this would stretch the nerve too much.

The following is a description of the manner in which I pass the current. In every case it is always a half frog, deprived of the muscles and bones of the pelvis, which is used for this experiment. The half frog is thus reduced to a portion of spinal marrow, which is held in the vice, the nervous filament, the thigh and the leg, minus the claw, which is cut off. The little hook of the wire, G, is inserted between the bone and the tendo-achillis. Lastly, a gilded steel needle is thrust into the muscles of the thigh, as near as possible to the insertion of the nerve; and to this needle is soldered a very fine copper wire, K, covered over with silk, which is fixed to the piece of ivory, E. It is quite clear that in order to pass the current through the nerve, nothing more is wanting than to touch the support, A B, with one pole of the pile, in any point whatever, and the wire which is soldered to the steel needle with the other pole. In all my experiments I made use of a Wheatstone pile, the elements of which, as every one knows, are formed of an amalgam of zinc, contained in a cylinder of wood immersed in a solution of sulphate of copper, in which the copper of the pile dips.

I cannot here give you all the details of the numerous experiments I have made with this instrument, for the purpose of determining the force of the contraction excited by the electric current in different cases. But here are the general conclusions at which I have arrived: —

1st. The contraction excited by the electric current, transmitted along a nerve in the direction of its ramification, and which we call *direct*, is always more energetic than that which this same current produces when passing along the nerve in the opposite direction.

2dly. The direct current weakens and rapidly destroys the excitability of a nerve; whilst the passage of the inverse current augments it within certain limits.

3dly. To produce these effects, the action on the nerve, of the direct as well as of the inverse current, ought to be continued for a certain time, which will be longer in proportion as the excitability of the nerve is weaker.

It is very easy to prove, by experiment, the most important of these conclusions; that is to say, that when the direct current traverses the lumbar nerve of a frog for twenty or thirty minutes, there are no further contractions, either when interrupting or closing the circuit: on the contrary, the contraction obtained by opening the circuit of the inverse current, after many hours' passage, scarcely differs from that which occurred at first, when the nerve was endowed with great excitability. This difference in the excitability of a nerve, according as it has been submitted to the passage of a direct or inverse current, can be observed whatever may have been the manner in which the nerve is stimulated. When we operate with the inverse current on a very excitable nerve, and one which has never before been submitted to the passage of the current, it is impossible to discover any difference between the contraction excited by the opening of the circuit of this current after the passage has continued for one second, and that which occurs after the passage has been continued for ten or twenty seconds. It does, however, exist: but to appreciate it, it is necessary to proceed more rapidly. If the passage of the inverse current be limited to a small fraction of a second, we find, on opening the circuit, a weaker contraction than that which is obtained after the current has circulated for one or more seconds. It is very easy to prove this, by closing the circuit by the aid of a wheel furnished with a metallic tooth, and to which we attach one of the wires of the pile during its rotation. When the nerve has lost a part of its excitability, we then readily perceive that the contraction manifested on opening the circuit, increases proportionally to the time that the circuit has been closed. The greatest effect of the passage is obtained at the end of fifteen or twenty seconds. It is needless to say, that these effects do not continue to increase on a dead animal.

Influence of Repose.—Finally, it remains for me to notice the influence produced by repose on a nerve which has been submitted to the action of the current. If the nerve has been traversed by the direct current, repose restores a portion of its excitability; if it has been traversed by the inverse current, it loses by repose a part of that excitability which it had acquired under the influence of the current. When the nerve is very irritable, a very short repose suffices to restore the excitability lost by the action of the direct current; it is the same with the augmentation occasioned by the inverse current; almost as soon as it is interrupted, the nerve returns to its normal condition. In proportion as the excitability diminishes, the duration of repose necessary for giving or arousing the excitability acquired under the passage of the current, augments.

I must here add, that the relation between the muscular contractions and the current is established in a more intimate manner, by measuring this contraction and comparing it with the quantity of electricity which excites it. I have lately proved, by employing Bréguet's apparatus before mentioned, that this contraction is proportionate to the quantity of electricity which excites it.

Theory of the Action of the Current on the Nerves.— With the knowledge of these facts, all of which I have recently established by a great number of experiments, I hope to be enabled to give a very simple theory of the action of the electric current on the nerves, and of the phenomena which it produces in animals.

No experiment demonstrates that the electric current excites muscular contraction during its passage in the nerves. This passage only modifies the excitability of the nerve. Contraction is constantly produced by the effect of the electric discharge, properly so called, namely, by the neutralisation of the two opposite electrical conditions accumulated at the poles, and which give the spark. Every one knows, that when we close the circuit of a pile, as well as when we open it, there is a spark. Under precisely the same circumstances the current always excites contractions. It is sufficient to have once seen the contractions of a frog excited by touching its nerve with the armatures of a Leyden bottle, which has already been discharged several times by a metallic arc, to be convinced how excessively feeble is the discharge capable of producing this effect. A bottle which, as we have stated, has been already discharged many times, can yet produce fifteen or twenty contractions in a frog.

On the discharge of the bottle, we likewise see that the contraction of the limb, traversed by the inverse discharge, first ceases, whilst that which is provoked by the direct discharge continues.

There is no difficulty, therefore, in understanding why, when the excitability of the nerve is lessened, the spark produced by the interruption of the direct current should excite no farther contractions. With the inverse current we obtain the contraction by the spark at the opening of the circuit, because, in the interval of its passage, the excitability of the nerve has increased. This augmentation disappears immediately the current ceases to act, and this is the reason why the spark no longer excites contractions when we subsequently again close the inverse circuit.

With these notions we can also understand the fact of the voltaic alternatives: when a nerve has been for some time traversed by the direct current, and has lost its excitability, it no longer suffers contraction, notwithstanding that there may be a spark at the closure, and at the interruption of the current. The inverse current restores to the nerve a portion of its excitability, and its contraction reappears when we open the circuit. If from the action of the inverse current we return to that of the direct current, the contractions which are obtained during the short time that the nerve preserves the excitability acquired by the passage of the inverse current, will be more energetic, inasmuch as we have seen that the direct discharge produces on a nerve, endowed with a certain degree of excitability, a stronger contraction than the inverse one.

Effects on other Parts of the Nervous System. — To complete this lecture, it only remains for me to speak of the effects which the electric current produces when applied to different parts of the brain, to the nerves of sensation, to the roots of the spinal nerves, and to the ganglionic nerves. I regret that a subject so important has not hitherto been properly investigated. We may say that every thing yet remains to be done: the few words which I can say to you on the subject will prove it.

Effects on the Brain.—I applied the conductors of a pile, formed of several elements, to the cerebral hemispheres, and to the cerebellum of the brains of a living animal; and I introduced the conductors into the very substance of these organs, without perceiving either convulsions or signs of pain. But by bringing the conductors in contact with the *tubercula quadrigemini*, the *crura cerebri*, and *medulla oblongata*, we obtain very violent convulsions throughout the body, and the animal gives signs of suffering.

Effects on the Spinal Marrow and the Roots of its Nerves. — In conjunction with Longet, I examined the action of the electric current upon the roots of the spinal nerves, and on the fasciculi of the spinal marrow. The following are the results obtained. With the anterior roots, which are for motion, there was, as usual, in the first period, contractions produced both when we closed and when we interrupted the circuit, whatever was the direction of the current. In the second period of excitability, we obtained, by acting upon the anterior roots, the opposite effect to that which took place upon the mixed nerves; the inverse current excited contractions in the first moments of its passage, and none when it ceased; the direct current, on the contrary, produced them when it was interrupted, and not when we closed it. It is unnecessary to add, that contractions were never produced when we acted upon the posterior roots, provided that we had divided the anterior ones. The anterior fasciculi of the spinal marrow offered the same phenomena as the corresponding roots. These differences appear to me of the very highest importance.

I have recently found that a mixed nerve, after having been submitted to a great number of successive discharges, such as can be obtained with an electro-magnetic machine, presents, *for a certain time*, the phenomena of the anterior roots now described.

This study, I repeat, will be of the highest importance for the physics of the nervous system; and the facts related lead us to assume that the differences obtained with different nerves are due rather to a difference of structure than to a different state of the nervous fluid.

Effects on the Nerves of Sensation.—Let us now pass to the nerves of sensation. Magendie caused the current to pass through the optic nerve of a living animal without obtaining contractions or symptoms of pain. In operating on himself, by touching with the extremities of a pile, formed by a single element, the ear and the eye, or the ear and the tongue, or the eye and the tongue, he obtained sensations of sound, flashes of light and a peculiar taste. These effects could only depend on an action exercised by the current on the sensorial nerves of these organs, and not on the contractions excited in the muscles dependent on them In fact, a very feeble current, insufficient to excite the slightest muscular movement, is capable of acting upon the senses. The peculiar taste cannot be attributed to the impression exercised upon the tongue by substances produced by the decomposition of the salts of the saliva effected by the pile; for a very feeble current, and one that is unable to cause this decomposition, is yet sufficiently strong to give rise to the electric sensation upon the tongue.

Effects on the Ganglionic Nerves.—A few words, in the last place, on the action of the current on the nerves of the ganglionic system. For the little that we know on this subject we are indebted to the celebrated Humboldt.

When we transmit a current through the heart of a recently killed animal, a few instants after pulsations have ceased, we observe that this organ recovers its usual movements some time after the passage of the electric current; and that these movements are preserved for a certain time after the organ has been withdrawn from the action of the current.

If, instead of waiting until the natural movements of the heart have entirely ceased, we transmit the current when they are merely becoming weaker, we then perceive that when the latter has acted upon the heart for some time, the movements become more frequent, and the augmented frequency continues for several seconds after the current has been interrupted.

The same effects take place with the vermicular movement of the intestines when these organs are subjected to the influence of the current.

If we reflect on the importance which the ganglionic system possesses in the performance of the *organic functions* of animals, we can easily understand how very insufficient are the researches hitherto made on this subject.

The difference of action exercised by the current upon the nerves of the life of *relation*, and on those of *organic* life, is very marked. In the former, its effects are manifested only in the first and the last moments of its application; in the latter, on the contrary, they are slow to appear, continue during the passage of the current, and even persist after it has been interrupted.

Effects of the Interrupted Current. — Having now examined the influence exercised upon the irritability of nerves by the passage of the continued current, it remains for us to examine the effects produced by the current interrupted and re-established at short intervals, in such a way that its action is very frequently repeated upon the nerve.

For this purpose I fix to the table, by means of small nails, a frog prepared in the usual manner. I connect one of the conductors of the pile with one of the nails, and with the other conductor I touch, many times successively, another nail, thus establishing and interrupting successively, the circuit, in a very short time. We see the frog violently extending its limbs, as if affected with tetanic convulsions, whether the current which thus traverses it by jerks be either direct or inverse.

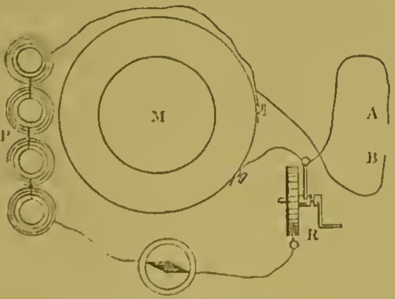
The excitability of the nerves of a frog, thus *tetanised* by the passage of the electric current, is very feeble when compared with that of another frog upon which a continued current has been made to act. I have often repeated this comparative experiment by submitting two frogs, prepared alike, the one to the action of the continued current produced by forty-five elements, and the other to that of a pile of the same force, but in which the current was interrupted and re-established at short intervals.

In each of these cases the experiment lasted ten or fifteen minutes. By submitting afterwards, the lumbar nerves of these frogs to the passage of the direct current, I observed that a greater number of elements was required to cause contraction in the animal which had been previously submitted to the interrupted current, than in that which had been subjected to the direct one. I also satisfied myself of the difference of the excitability of these two frogs, by causing a continued current to act upon them at the same time; the diminution was constantly greater in the one which had been submitted to the influence of the interrupted current.

Marianini, also, found, that by comparing two frogs, one of which had been traversed by a continued current always in the same direction, and the other by a current transmitted sometimes in one and sometimes in the other direction, that the excitability of the nerves, was less exhausted in the first animal, than in the second one.

This great diminution of excitability in the nerves, or, to speak more precisely, in the nervous force, induced by the passage of the current renewed at very short intervals, has been clearly demonstrated by the

Fig. 17.



Masson's Apparatus.

experiments of Masson. The apparatus by which this philosopher succeeded in transmitting the electric current a great number of times through an animal, and interrupting it for very short intervals, consisted of a metallic wheel, R, supported on a metallic axis, and turned, by means of a handle, upon two amalgamated cushions. One of these was in communication with one of the poles of a pile, P, whilst the second pole was in contact with a wire, which, after being wound spirally around a cylinder of soft iron, M, terminated in a fixed metallic plate applied to the teeth of the wheel.

When we revolve the wheel, the circuit closes every time the plate touches one of the teeth, and the circuit is interrupted when one of the non-metallic spaces comes in contact with it. By applying the moistened hands to the two extremities, A and B, of the conductor when the wheel revolves, a series of violent shocks is obtained. If the movements of the wheel be sufficiently rapid, these successive passages produce a very painful tension in the arms; the experimenter cannot let go the conductors, but involuntarily grasps them with great force.

Masson, by means of this apparatus, and a pile, formed of a small number of elements, succeeded in killing a cat in five or six minutes.

He found that these shocks, and this spasmodic tension, disappeared by communicating a great rapidity of the wheel. Pouillet proved, that when the duration of the interval between the two passages of the current is about $\frac{1}{300}$ of a second, the interruptions were no longer perceptible, and the effect produced was the same as with a continued current.

I have here a rabbit, which I shall submit to the passage of a current by means of Masson's wheel. One of the conductors of the pile has been introduced into its mouth, the other communicates with the muscles of the back. Although the pile consists only of ten elements, the animal dies within a few seconds. These powerful effects must certainly be ascribed to the great loss of nervous power effected in a very brief space of time.

These results appear to me to explain readily Weber's observation, that the pulsation of the heart ceases when the interrupted current is made to act on its nerves.

Therapeutical Use of the Current.—I cannot conclude this lecture without stating some of the therapeutical applications made of the electric current, and founded upon the scientific principles, which I have made known to you.

In Paralysis.—Abstraction being made of all purely theoretical ideas, and independently of all hypothesis of the nervous force, we may admit that, in certain cases of paralysis, the nerves undergo an alteration analogous to that which they would suffer if they had been subjected to a continued passage of the electric current. We have seen that, in order to restore to a nerve the excitability lost by the passage of this current, it is necessary to subject it to the action of the inverse current.

I must add, in favour of the efficacy of the therapeutic use of this current, that a limb, although paralysed, constantly suffers some contractions when it is submitted, either to the passage of a current or to the action of electric discharges; and these contractions favour the restoration of the functions of the muscles. Experiment confirms these ideas: divide the two sciatic nerves of a living frog, allow one of the two limbs to remain quiet for ten, fifteen, or twenty days, and submit the other, two or three times a day, to the action of the current. The latter will continue to contract, whilst the other will fail to give any contractions when the current is applied to it.

I am anxious to state to you some rules which I consider as important in the application of the current to the treatment of paralysis. You should always commence by employing a very weak current. This precaution seems to me now more important than I formerly believed it to be, having seen one paralytic patient seized with true tetanic convulsions under the action of a current furnished by a single element. Take care never to continue the current for too long a period, especially if it be energetic. Apply the interrupted current in preference to the continued one; but after *twenty* or *thirty* shocks, at the most, allow the patient to have a few moments' repose. Both practice and theory seem to prove, that the interrupted current is more useful than the continued one.

A pile, with Masson's wheel, or, still better, the electro-magnetic machine, is the most convenient apparatus for this purpose. Electro-magnetic apparatus are now constructed, in which the interruptions of the current are made without the necessity of an assistant.

We may employ, as conductors, two ribands of sheet lead or copper. The extremities, which are placed in contact with the skin, should be covered with cloth moistened with salt and water. In some cases, it is useful to employ, as extremities of the conductors, acupuncture needles.

The number of authentic cases of paralysis cured by the electrical treatment, is already sufficiently great to encourage physicians and patients to persevere in its use. Perseverance indeed is indispensable in the application of the electric current, for without it, successful results are impossible.

In Tetanus.—The use of the electric current has been suggested in another malady, namely, tetanus. I believe I am the first who has attempted its application to man.

The principles on which is founded its employment for the cure of this disease are the following. A current which circulates by jerks in an animal during a certain time, produces tetanic convulsions; the direct current, if continued sufficiently long, produces, on the contrary, paralysis. From this it was concluded, that the continued passage of the latter, in a tetanised limb, would destroy this condition, by producing a state more or less allied to paralysis. The truth of this conclusion is demonstrated by facts. In operating upon frogs which have been tetanised by narcotics or hydrocyanic acid, we observe a fit of tetanus cease under the influence of the prolonged passage of a direct current. The frogs die without presenting those convulsions, which are observed take place when these animals have not been submitted to the direct current.

The effects produced by the application of the electric current in a case of tetanus, which I published in the *Bibliothèque Universelle* (May, 1838), appeared to me in some degree to prove the truth of the scientific principles which I have explained to you. During the passage of the current, the patient experienced no violent convulsions; he was able to open and shut his mouth; and circulation and perspiration appeared to be re-established. Unfortunately, this amendment did not continue long; the disease being occasioned, and kept up by the introduction of foreign bodies into the muscles of the leg. Perhaps more satisfactory results from the electric current may be expected, in cases where tetanus has not been caused by a traumatic injury: moreover, we ought already to be thankful in being able to lessen the sufferings to which this dreadful disease gives rise.

In Urinary Calculi.—Finally, it has lately been proposed to dissolve vesical calculi, and resolve cataract by the electric current. It is sufficient, however, to remember, that the substances which compose urinary calculi are insoluble in water, to be convinced that such an application has no basis to rest on.

In Cataract.—As for cataract, I would remark, that by changing the position of the poles of a current, which has been made to pass through an albuminous liquor, we never find that the albumen coagulated at the negative pole is redissolved at the positive pole. It is possible, therefore, to create a cataract, but impossible to destroy it.

In Aneurism.—Petrequin, of Lyons, has recently proposed the use of galvano-acupuncture for curing certain aneurisms. This application appears to be founded on the property which the electric current possesses, of coagulating the serum of the blood, and, consequently, of partially filling up the aneurismal sac.

LECTURES XIV. AND XV.

NERVOUS FORCE.

ARGUMENT. — Characters of the nervous force; the cerebro-spinal system, and its action; the ganglionic system, and its action.

Effects produced by electric irritation of the nerves, compared with those caused by other stimulants.

Analogy between the nervous force and the electric current. No evidence of an electric current in the nerves of a living animal. The nerves do not present the necessary conditions for such a current. Relation of the nervous force to electricity. Induced contraction; illustrative experiments; facts favourable to the opinion, that it is caused by an electric discharge from contracting muscles; disproof of Liebig's hypothesis of muscular contraction; failure to detect the development of electricity during contraction. New investigations of the induced contraction; it is only excited by a contracting muscle. Influence of substances interposed between the contracting muscle and the nerve of the galvanoscopic frog. Hypothesis to explain induced contraction.

Production of nervous force: mechanical power obtained by the conversion of chemical action into heat, electricity, and nervous force.

IT may, perhaps, appear strange and almost rash for me to notice the nervous force, or agent, in a course of lectures on the physical phenomena of living beings; but I hope to prove, by the considerations which will follow, that the subject is not out of place here; and that if, in treatises on physics, the chapter consecrated to the general analogies existing between caloric, electricity, and light, is the most important, and in some respects the most philosophical, so likewise will this lecture possess analogous advantages, at least in its high importance.

Characters of the Nervous Force. — I shall commence by briefly explaining the characters of the nervous force and its laws, according to the present state of physiological science.

Cerebro-spinal System. — All animals having a certain degree of development, possess organs, by which they are enabled to influence their muscles, and by the intervention of which they perceive external actions. These organs constitute the cerebro-spinal nervous system, which is principally composed of an infinite number of ramifications, disseminated throughout the body of the animal, and uniting in a central mass, constituted by the brain and spinal marrow.

If we divide one of these ramifications in a living animal, and afterwards touch, with either a red-hot iron or potash, or wound with a needle, or pull with pincers, the portion which remains in communication with the cerebro-spinal axis, the animal manifests evident signs of pain; but if we apply these irritants below the section or ligature of the nerve, no signs of pain are exhibited, and we perceive, merely, contractions in the muscles to which the irritated nerve is distributed. If we excite the uninjured nerve, both pain and contraction simultaneously occur. Lastly, if we tie the nerve in two places, and then irritate the part included between the two ligatures, we produce neither pain nor contraction. The nerve, then, has no other office than that of transmitting and propagating the action of the stimulant applied to it. This double action consists in a sensation carried to the brain, and in a muscular contraction communicated to the muscles.

The physiologists, Bell, Magendie, Müller, Pandyza, and others, have discovered that there are some nerves which, when excited, merely produce muscular contractions, and others which, when submitted to the like irritation, only provoke pain. The anterior and posterior spinal nerves, and some other nervous branches, possess a single function only.

Flourens, Longet, and other physiologists, have also distinguished in the nervous centres, some parts which preside over sensations only, and others, again, exclusively devoted to movements.

A nervous fasciculus is made up of a great number of filaments, every one of which is separately able to transmit the influence of the will, or of some stimulant, without the other filaments with which it is in contact, participating in the action.

Ganglionic System. — Besides the cerebro-spinal, there exists a second nervous system, which, notwithstanding its numerous connection with the first, does not, when irritated, excite either movements or sensations. It is the ganglionic nervous system, composed of ramifications, chiefly distributed to the apparatus of organic life. These ramifications gradually unite, interlace with each other, and have in their interstices a globular matter, which seems also to exist in the central masses.

In this system, irritations, manifested by certain peculiar movements, excited chiefly in the intestines, are slowly propagated, and continue to be so, even when the irritating action has been removed. A muscle, which has been deprived for a certain time of all communication with the centres or ganglia, of this system, loses the property of contracting under the influence of irritation of its cerebro-spinal nerves.

These few words concerning the nervous action will, I hope, be sufficient to make you understand the importance of the results to which we are come on the physiological action of the electric current.

Peculiarities of Electric Irritation of the Nerves. — I consider it necessary to give here a summary of the principal differences which have been ascertained experimentally, between the effects produced by electric irritation of the nerves, and those determined by other stimulating agents, such as heat, chemical and mechanical actions, &c. The following are these different distinctions: —

1. Electricity is the only irritant which can excite, at one time sensation, and at another contraction, according to the direction in which it traverses a nerve.

2. The electric current alone, in passing transversely across a nerve, produces no phenomena due to the excitability of the nerve.

3. The electric current has no effect on the nerves, that is, it neither causes contraction nor sensation, when its action on the nerve is prolonged.

4. The electric current alone can modify the excitability of a nerve, and even rapidly destroy it, when the current circulates in a certain direction, and can pre-

serve or augment the excitability, when passing in the opposite direction.

5. Lastly, of all irritating agents, the electric current is the only one which possesses, for a long space of time, the power of reviving the excitability of the nerve, when it has become very much enfeebled in respect to other stimulants.

Analogy between the Electric and Nervous Forces.— These differences between the action which the electric current exercises on the nerves, and that of other irritants, evidently show that the first is more simple than the others. Hence arises the analogy between the nervous force and the electric current, which the earliest observers of galvanism faintly perceived.

But ought we, from this analogy, to conclude that the nervous force is merely an electric current? Let us be cautious in assuming such an inference, which is too often adopted as one of the best demonstrated experimental truths.

Let us, in the first place, inquire whether with the instruments, which physical science furnishes us, the electrical current can be discovered in the nerves of a living animal? Can this current exist there, and if it does, would it be under the requisite conditions to be endowed with the characters of the nervous force?

The muscular electric current, on which we have dwelt at some length in one of the preceding lectures, is shown, by experiment, to owe its origin to chemical actions going on in the muscles. We have seen that this current exists in the integrant parts of the muscles, as well as between the molecules of two bodies which enter into combination; that it circulates there without any regularity, and one might say that, as Ampere imagined, it took place in magnetisable bodies, and that it is only by an experimental arrangement that we can discover the presence of this current. We have also shown, that the nerves have no direct influence over the production of this current, and that their office is limited, in experiments on the muscular current, to that of a slightly conducting body, communicating with certain parts of the muscle.

No Electric Current in the Nerves.—It was important to search for the presence of an electric current in the nerves of a living animal. I shall refrain from noticing here all the experiments which have been undertaken with this object in view, and which have terminated in the announcement, at one time, that this current did exist — at another time that it did not exist. The most conscientious and best established conclusion is this:—*In the present state of science, and with the means of experimenting which we at present possess, no sign of the electric current is found in the nerves of living animals.*

Some persons have asserted, that steel needles introduced into the muscles perpendicularly to the direction of their fibres, become magnetic, especially at the moment when the muscles contract. From this it has been oncluded, that there existed an electric current in he nerves, and that the circuit was established as in spiral or an electro-dynamic cylinder.

I have repeated these experiments, by introducing steel or iron needles into the muscles of living animals, and in all directions relative to their fibres. In order to convince myself of the magnetisation of the needles, thus plunged into the muscles, I made use of those of a very good astatic system, and even of those of the sideroscope of Lebaillif. I never obtained an affirmative result. I placed the recently prepared thigh and leg of a frog, in the interior of a spiral of varnished copper wire, the extremities of which were connected with those of a second smaller one, in which there was a soft iron wire. I afterwards irritated the nerve of the frog, observing, at the same time, if an induced current transversed the spiral, and magnetised the iron wire. All my researches were fruitless.

I likewise tried the effect of introducing into an exposed nerve of a living animal, the conductors of a very delicate galvanometer by two points, as far apart as possible. I operated upon animals under the influence of certain narcotic poisons, and I excited strong muscular contractions in them, at the moment when I placed the two wires of the galvanometer in the nerve; but I must confess that, whenever the experiment was well made, I never obtained evident and constant traces of the electric current.

At the school of Alfort I made, in conjunction with Longet, an experiment of this kind upon a horse. We employed a very delicate galvanometer; the nerve was exposed for a considerable extent of its course, and I could traverse it with the platinum extremities of the galvanometer, by passing from a distance of 2 or 3 centimetres to that of 15 or 20. We never obtained distinct signs of the derived current, and in a constant direction, even when the muscles of the animal were violently contracted.

Lastly, I may add that, from what we know of the properties of electricity, and of the laws of its propagation, it is impossible to conceive the existence of a current circulating in the nerves. In order that an electrical current should pass from one extremity of the nervous system to the other, it would be necessary to compare the nerve to a metallic wire varnished or otherwise insulated, an assumption which is not in accordance with fact. An electric current which, subjected to the will, would set out from the brain to reach the muscles, by traversing the nerves, could not be stopped in its course by the ligature of the nerve; whereas, we well know, that the propagation of the nervous force is prevented by that proceeding. Lastly, ts circulation in the nerves requires that the nervous ystem should form a closed circuit; but the labours of anatomists are very far from having proved such an arrangement, especially in the ultimate ramifications in he muscles, where it would be especially necessary.

I have often tried an experiment, which, had it given me a positive result, would have proved, in an indirect manner, that the nervous system forms a complete chain for the electric current. I exposed a nerve in a living animal at two distant parts of its course, namely, at the top of the thigh, and at the lower exremity of the leg. I introduced the latter into a spiral, imilar to that which I described a few minutes ago, and which was put into communication with a second much smaller spiral, containing within it a soft iron cylinder. Through the nerve, thus prepared, I passed the electric current, but never observed constant signs of the induced current in the spiral, which would certainly have occurred if the current had traversed this species of spiral which, it is presumed, is formed by the nervous ramifications distributed over the muscles.

Let us conclude, then, that the electric current does not naturally exist in the nerves of a living animal. The laws of its propagation require conditions which are not found fulfilled in the nervous system; the propagation of its force is interrupted by causes which could not produce a similar effect upon the electric current.

Relations between the Nervous Force and Electricity.—This unknown force of the nervous system is, therefore, not electricity, and still less is it the electric current. But what connexion exists between it and electricity, or the electrical current?

In order to reply to these questions, I will here sum up, in a few words, the only positive result that my lengthened investigations of electro-physiological phenomena of animals have permitted me to deduce.

There exists, between electricity and the nervous force, an analogy which, if it does not possess the same degree of evidence, is, however, of the same kind as those analogies which we know to exist between caloric, light, and electricity. We have seen, when speaking of the phenomena presented by electrical fishes, that the faculty which they possess of producing electricity is obedient to the nervous system. There is, then, in these animals a peculiar organic structure, such an arrangement of parts that, by an act of the nervous force they can develop the electrical fluid. You remember the identity of causes and circumstances which excite and modify muscular contractions, and this function is proper to these animals. You have seen, that in them the property which they have of giving the discharge is under the immediate dependence of the functions of the nervous system, as well, also, as is the faculty which the muscles have of contracting.*

A crystal of tourmaline, when heated, develops electricity, and from this fact we assume, that between caloric and electricity there exists a more or less intimate relation. The phenomena which we have observed in electrical fishes, prove that a link of the same nature unites the nervous force and electricity. Electricity is not the nervous force, nor is caloric electricity. The latter is derived from caloric in consequence of the form of the integral molecules of the tourmaline; the nervous force is transformed into electricity under the influence of the peculiar structure of the organs of the electric fishes.

How does Electricity excite Nervous Phenomena.—Let us examine, lastly, how electricity can excite nervous phenomena. The excitability of the nerves can also be awakened, and sensations and muscular

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* Mr. Grove (*On the Correlation of the Physical Forces*, p. 49, 1846) has suggested, that "muscular force, animal and vegetable heat, &c., might, and at some time will, be shown to have similar definite correlations" to those which he has shown to exist between the forces of the inorganic world.

movements determined, by other agents as well as by electricity; namely, by heat, mechanical and chemical actions. Ought we to conclude from these facts, that mechanical, chemical, and calorific actions, affect the nerves after they have been transformed into the electric current? We have no evidence in favour of such an hypothesis; if, however, notwithstanding this, we would assume an analogous change, there would perhaps be some appearance of probability of it in the case of chemical actions; but none for mechanical and calorific actions. There are no circumstances, in fact, under which we obtain a current by merely dividing a body. It is impossible to establish a comparison between a muscle and a thermo-electric body. In all these actions, we can only see various causes of molecular movement.

We may, however, ask ourselves this question,—Does the cause of nervous phenomena reside in these molecular movements of the substance of the nerves, or is it owing to a disturbance in the equilibrium of the ether, distributed in the nerves? Is this disturbance the consequence of a particular movement of the ether, which should constitute what we call the nervous fluid?

We can make no satisfactory reply to these important questions; the facts which are necessary to enable us to resolve them are wanting, and will remain so, perhaps, for a long time to come. Yet, if it be sometimes allowable, in scientific matters, to express, not only convictions, but even doubts, I will not hesitate to tell you that I do not consider it impossible to interpret nervous phenomena, by the mere movement of the ponderable molecules of nerves.

But rather than stop to develop hypotheses, I believe it will be more useful, before leaving the subject of the nervous force, to dwell for some time upon two classes of phenomena, or of the researches connected therewith. One relates to the fact which I discovered, and named *contraction by induction*, or *induced contraction;* the other relates to the development of the nervous force.

Induced Contractions. — By the term contraction by induction, or induced contraction, has been expressed, in England, a physiological fact which I discovered some years ago. I shall henceforth employ this name, which has the advantage of expressing briefly the phenomenon, and of specifying in some respect its nature.

I shall commence by briefly stating in what this phenomenon consists, and by detailing the principle researches which I at first made to determine its laws.

Fig. 18.

Experiment illustrative of Induced Contraction.

Having prepared a galvanoscopic frog, we place its nerve upon one or both thighs of a frog placed in the ordinary manner: then, by applying the two poles of a pile to the lumbar plexus of this frog, we observe that when the muscles of the thighs contract, convulsions simultaneously occur in the galvanoscopic claw, whose nerve rests upon the thighs in contraction.

I have also ascertained, that this phenomenon likewise occurs when we place the nerve of the galvanoscopic frog upon the muscles of the thigh of a rabbit, and make these contract by means of a current acting upon the nerve which ramifies in the thigh. I have also observed contractions of the galvanoscopic frog, without the employment of the electric current to excite the contractions of the muscle, producing the induced contraction; the action of some other stimulant, applied to the spinal marrow, or the lumbar plexuses, being substituted.

Lastly, I have repeated these experiments by placing very fine layers of different substances between the nerve of the galvanoscopic frog and the muscular surface where the induced contraction is developed. A leaf of gold, or a very thin and insulating lamina of mica, or glazed paper, being interposed, prevents the production of the phenomenon; that is to say, the contractions by induction do not occur in the galvanoscopic frog; but, on the contrary, a leaf of fine paper, moistened with water, does not prevent the occurrence of this contraction.

Conclusions.—From the whole of these facts we are authorised to conclude, 1st. That we cannot consider the induced contractions of the galvanoscopic frog, to be due to the electric current.

2d. And that we might, on the contrary, assume, that an electric discharge happens during the contraction of the muscle.

I have tried a great number of experiments with the view of supporting, by facts, this explanation of induced contractions. With this aim, I formed a pile of entire frogs, and closed the circuit with the two extremities of the galvanometer. When the needle had become stationary, I touched the nerve of the frogs, with a solution of potash, in order to excite contractions. By operating in this manner I have often seen the deviation of the needle increase several degrees, and afterwards return to 0°. When the frogs had been touched several times with potash, or were so weakened that, on submitting them again to the stimulating action of the alkali, no more contractions ensued, it usually happened that no further deviation of the needle could be obtained.

Finally, by moistening the nerves of frogs arranged in a pile, with acid or saline solutions, the deviation of the needle not only did not increase, but rapidly diminished.

These facts, on which I have dwelt at some length, would appear at first sight favourable to the idea, that contractions by induction are the effect of an electric discharge which accompanies the act of muscular contraction; nevertheless, I did not venture to affirm, from the outset, that the question was completely solved.

Moreover, the phenomenon of induced contraction, has always appeared to me as being of very great importance; and I could not, therefore, resist entering into a complete investigation of it. I have latterly studied it with all possible attention, and I believe, with some success. I hope, on account of the interest which this subject presents, you will excuse the minuteness with which I shall detail to you my numerous experiments.

Before proceeding to new researches on the fundamental fact of induced contraction, I wished to re-examine and vary the experiments of which I have already given a sketch, and which I had made with the view of discovering whether electricity was developed during the contraction of a muscle. It was necessary, therefore, to operate with piles formed of a greater number of elements than those which I had previously employed, in order to obtain a constant and a greater deviation; consequently, I believed that a muscular pile was more suitable than one of frogs.

The Muscular more Energetic than the Proper Current. — Since my recent experiments, there can no longer be a doubt, that with an equal number of elements, taken from the same frogs, the muscular current is more energetic than the proper current. I have lately shown that when by defective nutrition, by the effect of a very low temperature, or by the action of sulphuretted hydrogen, the muscular and proper currents are weakened in the frog, the diminution is greater for the second than for the first; indeed, when forming a pile with half-frogs, prepared by cutting the thighs in halves, I found a differential current more or less considerable, but always in the direction of the muscular current. It is only with very lively frogs, by dividing the thigh very high up, and by leaving only a small part of the surface of the interior of the muscle exposed, that we find no sign of the differential current, or, that it exists, in the direction of the proper current. Such was the fact which I discovered in my first experiments, and which, since my more recent ones, I can explain in a more satisfactory manner, by considering that in leaving the thigh nearly entire, we have two elements, namely, the muscles of the leg and those of the thigh, which give a current in the same direction; whilst the muscular element which furnished the current in the contrary direction is a single one.

Is Electricity evolved during Contraction? — To return to our principal subject, I may observe, that I have employed a muscular pile in order to ascertain whether there was a development of electricity during the contraction of a muscle. But seeing that in order to excite the latter I was compelled to moisten the muscles with acid, saline, or, better still, alkaline solutions, I thought it right first to direct my attention to the action of these liquids on the muscular current.

With this object I took eight frogs from among a very great number, and prepared them in the usual manner, by making with them sixteen elements, or half-thighs. I closed the circuit; the needle oscillated to 90°, and stopped at 22°. I formed another similar pile, but with this difference, that I washed the half-thighs several times in pure water, and afterwards dried them: I obtained the same result. Sixteen other elements like the latter were put, for some seconds, in a weak solution of sulphuric acid, then washed repeatedly until they no longer reddened the tincture of litmus. The pile being formed, and the circuit completed, the direction of the current obtained was that of the muscular current, but only from 6° to 7° at the first deviation, and the needle stopped at 0°. I rapidly divided the half thighs with scissors, in order to renew the internal surface of the muscle; and the pile, thus renewed, gave rise to one first deviation, which was also weaker than that before indicated.

Being induced to think that the effect of the acid solution upon the muscular elements, had been that of diminishing the conducting power, I made a muscular pile with eight half thighs taken from untouched frogs, to which halves I added four thighs taken from others also untouched. I obtained a current of 46°. Instead of employing four entire frogs, I used four also entire, but which had been plunged in sulphuric acid, and then washed: the current was 44°. The conducting power had not, therefore, been modified in the muscular masses which had been subjected to the acid solution.

In order to have more absolute certainty of the accuracy of this result, I made the experiment, which I am about to describe, by using, in order to prolong the current, not entire thighs but eight halves of thighs, washed with the acid solution, and which I then had re-united upon their internal surfaces in such a manner as to form a pile altogether similar to the preceding: the result was the same.

I likewise repeated this experiment, by using a concentrated solution of potash, in order to immerse for a few instants the muscular elements, or the half-thighs; the latter were then washed in pure water, in order to remove every trace of alkali. With the pile, thus formed, of sixteen elements, and the circuit being closed, I obtained a current of 10° to 12° in the direction of the muscular current, and a definite, not appreciable, deviation. I renewed the interior of the muscle and recomposed the pile; but the result did not vary. In this case, also, the conducting power had not changed. Consequently, acid and alkaline solutions acted, as I had found water to act when at a high temperature.

I made another experiment of this kind, which I relate merely to show you that it accords perfectly with those before mentioned. Sixteen half-thighs were left for some seconds in water at + 50° centig. [= 122° Fahr.]. These elements were then withdrawn from the water, washed in cold water, and formed into a pile, the circuit of which I completed, and obtained 12° at the first deviation, in the direction of the muscular current, and afterwards the needle stopped at 0°. The pile was re-formed, the internal surfaces of the muscles renewed, and the signs of the current were exactly the same as those which I had at first obtained. In this case, also, I am convinced that the conducting power was not sensibly modified by the action of the warm water.

I may add, also, that the muscular current is not diminished by repeatedly washing the muscles in pure water, at the ordinary temperature. I have many times observed the same deviation, sometimes a trifle stronger, sometimes a trifle weaker, by means of a pile of a certain number of elements, or halves of thighs, washed or not washed in pure water.

This experiment confutes Liebig's hypothesis relative to the muscular current.

A very concentrated solution of common salt, into which the muscular elements are plunged for a few seconds, equally diminishes, in a very perceptible manner, the signs of the current. Thus, whilst sixteen ordinary elements give a first deviation which may amount to 90°, and a definitive one of 20° to 22°, we obtain, on the contrary, if the elements have been plunged into a solution of common salt, and afterwards washed, a deviation of about 60° only at first, and afterwards the needle stops at 8° or 10°. We shall remark the agreement between this result and that to which Dumas has recently arrived, when investigating the influence of certain salts on the arterialisation of the blood.

We are, then, led to conclude, that the effect of these alkaline, acid, or very concentrated saline solutions, is to destroy in the muscular elements those conditions necessary to the development of electricity.

This conclusion is in no way opposed to the origin ve have assigned to this current. But since, by the action of acid or alkaline solutions, the signs of the muscular current either cease, or are very much weakened, it remains for us to explain how, in the preceding experiments, there has been no diminution of the current in a pile of entire frogs, when these have been touched at certain points with alkaline solutions, while it is immediately manifested when they re washed in an acid solution. In fact, you have remarked that, when we employ alkali, there is, in many instances, a remarkable augmentation of deviation, although of short duration, in the first contractions excited. With acids, on the contrary, the deviation immediately diminishes, but appears again some instants after.

Let us endeavour to give an account of these phenomena. But first, I should describe the experiments which I have made in a most careful manner, with the view of discovering if there be a development of electricity during muscular contraction.

I prepare several frogs according to the usual manner f Galvani. I then remove their legs, disjointing them with the greatest care. I have, thus, two thighs of a rog, united to a portion of the spinal marrow; I cut one of the thighs in half, and prepare, in the same manner, a certain number of elements all alike, and ormed of an entire thigh, of a portion of spinal marrow, nd of a half-thigh. You can easily understand how I compose a muscular pile with these elements, by applyng the external part of the entire thigh to the internal part of the divided thigh of the following element. Afterwards I plunge the two extremities of the galvanometer into the liquid, in which the two extremities of the pile terminate. By means of a slight modification in the two extremities of the galvanometer, I have no occasion to hold them with the hands in order to keep the circuit closed.

I have repeated this experiment many times, by employing piles of a dozen, sixteen, or twenty elements. The first deviation, as well as the permanent one, is sometimes weaker than that obtained with piles composed of a like number of half-thighs. This difference must be ascribed principally to the greater length and resistance offered by the circuit. In all these cases, after having allowed the needle to become stationary, indicating a deviation which, in my different experiments, varied from 10° to 12° or 15°, I rapidly touch the lumbar plexus of the elements of the pile with a concentrated solution of potash; excepting, however, the two last, lest the solution should reach the liquid in which the extremities of the galvanometer are plunged. The muscular contractions became manifest after the application of the alkali, and continued for some instants, without scarcely ever being sufficiently strong to destroy the contact, by separating the elements from each other. *During these contractions, if the experiment be made without any interruption or change in the circuit, the needle of the galvanometer undergoes no variation.*

In some cases, however, I have seen the needle descend, in others augment, 2° or 3° · but these variations are uncertain; they are wanting in the greater number of cases, and are almost always due to some too *brusque* movements in the elements, in consequence of which the contacts are deranged.

We therefore conclude, that direct experiment replies negatively to the question which we put whether there was a development of electricity during muscular contraction.

There now remains to be explained the phenomena presented by the proper current, when we employ entire frogs, and which consist of the almost constant occurrence of signs of augmentation, when we, for the first time, touch the lumbar plexuses of the frogs with potash; whereas, on the contrary, when we apply an acid solution to them, the needle immediately descends. have repeated and varied for this purpose my first experiments, and the following is the way in which these differences may be explained.

Whatever be the form of the muscular elements which we use to make the pile, that is, whether they be made with entire frogs, with half-thighs, or such as we have escribed, when we moisten the surface of the muscular lements with an acid, or alkaline solution, it invariably appens, whether there be or be not contractions, that he deviation diminishes, and that the needle returns to °, where it stops, if either the application of the alkali e repeated, or the solution employed be too concentrated.

This effect is analogous to that already described, and which the muscular elements present when they have been plunged for a few seconds into acid, or alkaline solutions.

In our mode of experimenting, we excite the contractions in the muscles by touching with alkali those points which are out of the circuit, and which do not constitute parts of the electromotive element.

In piles formed of entire frogs, with which we most frequently succeed in obtaining, in a transient manner, signs of augmentation in the current, by touching the lumbar plexus alone with alkali, we touch with alkali the parts which really constitute the electromotive element, and even in these cases we never succeed in obtaining the signs of the current, if we moisten the whole of the muscular surface.

I may also add, that if we employ an acid solution, taking care to touch with a pencil the lumbar plexus only, and not the muscles of the thighs or of the legs, the deviation is not weakened; and notwithstanding the contractions excited, are less strong than those produced by the alkali, there is no augmentation of deviation. In order to cause the needle to descend, it is necessary to touch the surface of the muscles with an acid. This same effect takes place with the alkali, and it is, I repeat, in accordance with the experiments, already related, upon the muscular elements which have been immersed in acid or alkaline solutions.

It is, then, only with the pile of entire frogs, or by touching with alkali the lumbar plexuses alone, that we often obtain a slight augmentation of deviation; and this phenomenon appears when operating in the same manner with acids. By taking into consideration all the experiments which I have mentioned, this result cannot be regarded as contrary to the negative reply that we have given, in an absolute manner, to the question of the development of electricity during muscular contraction.

However little may have been the attention paid to the exposition of all these facts, it is impossible not to perceive how great is the difficulty which we encounter when we attempt to explain, in the particular case alluded to, why the alkali produces an augmentation of deviation in the proper current of the pile formed of entire frogs. I am inclined to think that, as the alkali excites in the muscles stronger and more permanent contractions than those produced by acids, the contraction ought in most cases to render the contacts between the elements more perfect, and, consequently, the interior conducting power of the pile ought to be augmented. Indeed, in the pile formed of entire frogs, the contact between the elements is always imperfectly established, and we constantly observe great differences in the intensity of the current produced by these same elements, according to the greater or less care with which they are arranged.

Whatever may be the explanation given of the slight augmentation manifested in the intensity of the proper current, by touching the lumbar plexus of frogs with alkali, and thereby exciting muscular contraction, it is certain that this fact alone cannot prove that electricity is developed during the muscular current; the more so, as those before mentioned lead us to the conclusion that this development does not occur.

Experiments on Induced Contractions.—I pass now to the exposition of the new and numerous experiments made upon the phenomenon of induced contractions. However extended, I do not think that I ought to pass them over in silence, on account of the great importance of the principal fact which they are designed to illustrate.

It is sufficient to have seen once the phenomenon of induced contraction obtained without having excited the inducing contractions* by means of the electric current, to be convinced that this is not the direct cause of induced contractions. If, after having placed the nerve of the galvanoscopic frog upon the muscles of another prepared in the usual way, we rapidly tear the spinal marrow of the latter, either with a pair of scissors, or a piece of glass, or any other substance, it rarely happens that induced contractions are wanting. We are able to say as much of the induced contractions excited by a species of tetanus, which the frog suffers when it has been for a long time submitted to the passage of the inverse current at the time of opening the circuit.

We must, therefore, confine ourselves to saying that the easiest method of observing the induced contraction is that of exciting the inducteous contraction by the

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* Henceforth I shall call, by way of analogy, the contraction *inducing* or *inducteous*, in which the muscles are in contact with the nerve of the galvanoscopic frog in which the induced contraction is developed. *Note by Matteucci.*

passage of a current in the lumbar plexuses of the frog prepared in the ordinary manner.

On this ground I have generally made use of the current to excite contractions in my experiments. I have taken every precaution to prevent the galvanoscopic frog, or the thighs of the inducteous frog, from carrying off a portion of the same current. The most sure method is that which consists in almost completely filling a common plate with turpentine, and placing the frog thereupon. It is needless to add that this substance should be sufficiently thick to prevent the frog from being submerged. Precaution also should be taken in preparing the galvanoscopic frog, to detach from the nerves all the muscular shreds, and to cleanse them thoroughly from blood, by means of blotting paper.

Whatever be the arrangement of the nerve of the galvanoscopic frog, relatively to the muscular fibres of the inducteous thighs, the phenomenon of induced contraction is always manifested: thus, in some cases, I have stretched this nerve parallel to the fibres of the muscles: in others I have arranged them normally to these same fibres; or, lastly, I have folded them zig-zag, and yet induced contractions have been constantly produced in every case without any perceptible differences. They are also obtained by disposing the nerve of the frog upon the gastrocnemius muscle of the leg.

I have also tried the effect of washing the frog, in which I had excited the inducteous contractions, many times in pure water, in order to remove all traces of blood, or other secreted liquids, which might be on the surface of its muscles; and the induced contractions were still equally manifested.

I removed with a razor, or, better still, with scissors, a layer of muscular substance, and then placed the nerve of the galvanoscopic frog upon the internal surface of the muscles, and still obtained induced contraction.

The same phenomenon is also produced by disposing the nerve of the galvanoscopic frog upon the muscle, in such a manner that the extremity of the nerve is folded upon the nerve itself, and forms a kind of closed circuit.

I also endeavoured to satisfy myself whether these induced contractions would continue when the nerve of the galvanoscopic frog had not been cut. For this purpose I prepared a frog, taking care to preserve the integrity of the nerve, in the following way: after having skinned the animal, I removed the abdominal viscera, then the bones and muscles of the pelvis, and lastly, those of the thigh, taking care to leave the nerve of the thigh entire. I then prepared another frog in the usual way, and placed it upon the turpentine, as already described. Afterwards I put the nerve of the galvanoscopic frog, thus prepared, upon the thighs of the second. By exciting muscular contractions in the latter, we obtained induced contractions like those when we employed the galvanoscopic frog; and, moreover, we observed simultaneous contractions in the muscle of the back, and in the other leg. We shall have occasion hereafter to return to this experiment, and therefore, for the present, we shall content ourselves with stating, that the induced contractions are also manifested when the nerve, placed upon the muscles in contraction, is untouched.

By using the frog, thus prepared, I have studied induced contractions, employing such an arrangement that the nerve which is in contact with the muscle in contraction is already excited, either by a current or some other stimulant. For this purpose I either included the galvanoscopic frog in the circuit of a voltaic pair, or applied a drop of a very weak alkaline solution upon the nerve. Every time that the *inducting* muscles contracted, there was invariably induced contraction, whether the nerve by which this last was excited was previously irritated or not; and, consequently, when even the muscle on which this contraction took place was already contracted. Notwithstanding this contraction of the galvanoscopic frog, there is no difficulty in perceiving the induced contraction which ensues.

Numerous experiments, equally simple, prove, that whatever may be the manner in which the nerve of the inducting muscle be excited, if the contraction be absent, the induction is equally so. I shall confine myself to the relation of some of the principal ones. If the nerves have been divided at two or three places in the inducting muscles, in order to prevent the contraction taking place, induction is constantly absent.

If, without cutting the nerves, we make incisions into all the tendinous extremities of the muscles of the thigh, and if, moreover, we make some transverse incisions in the same muscles, avoiding the nervous cords, both the inducing and induced contractions are wanting.

By carefully dividing all the muscles of the leg of a frog, we may expose the nervous filament which traverses it. If we irritate this nerve with either the current or any other stimulant, after having spread the nerve of the galvanoscopic frog upon the muscles of the thigh, when the latter suffers no contraction, induced contraction is also wanting.

When experimenting upon rabbits and dogs, I have been able to operate with the electric current upon the nervous filaments distributed to the kidneys, stomach, and intestines; the nerve of the galvanoscopic frog was, during the experiment, stretched upon these different organs, and in an analogous position to that which it had when it was placed upon the muscles. I never obtained any sign of induced contraction.

I also sought to discover whether induced contraction occurred when the nerve of the galvanoscopic frog was placed on that which is irritated. For this purpose two galvanoscopic frogs were prepared, and the nerve of the first was placed upon that of the second, at points very near to the leg. In order not to omit any precaution in this experiment, we placed the two frogs upon turpentine, and afterwards irritated, either with the current or with some other stimulant, the upper parts of the nerve of the frog, which I shall continue to call inducteous. The galvanoscopic frog manifested no induced contractions; whilst, on the contrary, it showed them immediately, if its nerve was disposed upon the gastrocnemius of the other frog. It is needless to add, that in making use of the current in order to excite the inducteous contraction, we must never put the conductors of the pile in contact or in proximity with the nerve of the galvanoscopic frog. We may conclude from this experiment, that an irritated nerve, and within which the unknown principle which excites the contraction in the muscle and the sensation in the brain is certainly propagated, does not act upon the nerve of the galvanoscopic frog placed in contact with it.

I will also relate the following experiment: I exposed with all possible care the brain of a frog, prepared in the ordinary method, and spread upon this organ the nerve of the galvanoscopic frog. In several experiments made in this way, I applied, to the lumbar plexuses, sometimes the direct current sometimes the inverse one; in others, I touched the plexuses with potash, and invariably obtained contractions in the inferior members, and convulsions of the back. Yet the galvanoscopic frog, whose nerve rested upon the brain, never manifested induced contractions.

This same experiment has been tried, and with the like results, by applying the nerve of the galvanoscopic frog upon the spinal marrow, the brain, and different parts of dogs and rabbits: it is useless to say, that during the experiments, we irritated the animal in various parts, in order to be quite certain that the nervous action was propagated and reached the nervous centres.

The induced contractions are, then, excited only by a muscle in contraction.

I wished to examine whether these induced contractions became weakened, by provoking them by means of a muscle in which the contraction was also induced. In a word, I have sought for induced contraction of the first, second, and third order, &c. To do so, I prepared several galvanoscopic frogs, and one only in the ordinary way, I then arranged them in the following manner:—I spread the nerve of a galvanoscopic frog upon the muscles of the thighs of the entire frog; then upon the muscles of the leg of the galvanoscopic frog, I spread the nerve of another galvanoscopic frog, and so on. The whole apparatus was placed upon turpentine. By exciting contractions in the whole frog, by means of the current passed through the lumbar plexuses, I frequently saw three galvanoscopic frogs contract at the same time, and nearly all of them with equal vivacity. This effect was constantly produced in two; but I have never seen it in four. These, then, are induced contractions of the first, second, and third order.

Before deducing from the facts which I have now made known, the conclusions which may be drawn from them, there remains for me to relate to you the numerous experiments which I have made, with the view of discovering what influence is exercised upon induced contraction, by bodies interposed between the contracting muscle and the nerve of the galvanoscopic frog.

In my first experiments upon induced contractions, I had remarked that, by spreading a leaf of gold (such as is used for gilding) upon the inducting muscles, and afterward placing upon one of the muscles, covered over with this small layer of gold, the nerve of the galvanoscopic frog, the induced contraction was no longer produced. In order to render this experiment successful, the muscle must be completely covered with gold; and it does not succeed, after one or two contractions, by which the metallic leaf has become torn.

I have also observed, that a leaf of glazed paper, placed between the muscle and the nerve, likewise prevented this same contraction. On the contrary, filtering paper, moistened with water, or with the serous liquid which moistens the surface of the muscles, when placed between this latter and the nerve of the galvanoscopic frog, offers no obstacle to the production of these induced contractions.

Our knowledge respecting the influence of interposed bodies on this phenomenon, was at first confined to the observation of these three facts. I have recently endeavoured to extend and vary the experiments. The mode of experimenting which I adopted, consists in preparing a frog, after the manner of Galvani, and placing it on turpentine; at the same time an assistant prepared other galvanoscopic frogs, the nerves of which I placed upon the muscles of the thighs of the first frog. I always used, in order to excite the contractions, a small pile of Faraday's of five elements immersed in pure water, and the conductors of which are varnished and covered with silk.

No other liquid among the number with which I experimented, prevented the induced contraction from taking place. The liquids which I employed, and through which the phenomenon appeared, were water, slightly acidulated or salt water, serum, olive oil, diluted alcohol, resinous spirit varnish, and oil of turpentine. I generally let fall a few drops of the liquid I wished to try, upon the muscle, and I moistened with the same liquid the nerve of the galvanoscopic frog. The induced contraction was also produced when we interposed a piece of filtering paper, dipped in the same liquid, between the muscle and the nerve.

The slight conducting power of some of these liquids, such as oil, spirit of turpentine, varnish, &c., leads me to suspect that the induced contraction was not destroyed by the interposition of a perfectly insulating body.

I have satisfied myself, in fact, that through even very thin layers of these liquids, the proper current and the muscular current are not propagated. It will be, doubtless, remembered, that when we take a galvanoscopic frog in the hand, and put its nerve in contact with moistened paper, which is in some way in communication with the ground, contractions are produced. Tho same phenomenon is observed by touching the muscles of a frog or other animal, which communicates with the ground, with the nerve of the galvanoscopic frog. In all these cases, it is the proper current which circulates through the experimentor, the ground, the body touched, and the frogs; but if we plunge the nerve of the latter in oil, oil of turpentine or varnish, the slight layer of these liquids which remains adherent is sufficient to interrupt the circulation of the proper current.

There is no doubt, then, that if the induced contraction be propagated through a layer of one of these badly conducting liquids, it is not owing to a current, which, taking its source in the contracting muscle, would be able to diffuse itself in the nerve of the galvanoscopic frog.

Nevertheless, in considering the vast importance of these experiments for the theory of this phenomenon, I was anxious to try the effect of interposing between the muscle in contraction and the nerve of the galvanoscopic frog, a worse conductor than those just mentioned. The body which appeared to me fit for employment in these experiments, is the almost solid Venice turpentine, rendered more or less liquid by the addition of a small quantity of oil of turpentine. I varnished the thighs of a frog with this mixture, and placed some of it on the nerve of the galvanoscopic frog. After having arranged the experiment in the usual way, I found that the *induced contraction continued.*

To demonstrate the non-conducting power of the mixture, I hasten to add, that if, in order to excite the contractions, I applied one pole of the pile upon the insulating layer, well spread, without penetrating to the muscle itself, and with the other pole touched the leg of the galvanoscopic frog, no contraction was excited in the animal. These experiments evidently prove, therefore, that induced contractions exist through an insulating layer, capable of intercepting not only the proper or muscular currents, but even that of the pile which excites the inducing contraction.

If the insulating mixture exceed certain limits in its thickness, and if it have not a suitable degree of liquidity, the induced contraction does not occur. It is impossible, however, to state what degree of thickness and fluidity the layer ought to possess to yield this result. It is sufficient for me to have fully shown that, in some circumstances, we obtained induced contraction when we have interposed, between the muscle and the nerve, an insulating layer, which certainly impedes the propagation of the proper, or muscular currents, as well as an ordinary voltaic current.

Finally, I may state that I never succeeded in obtaining induced contraction, by interposing between the nerve and the muscle a solid body, whatever its nature and however slight its thickness might be. I have employed for this purpose very thin layers of mica, sulphate of lime, of gold, sized paper, and the leaves of plants, but the phenomenon was not produced with any of them. A very curious fact, and which, from its consequences, I believe to be important, is that of the existence of the induced contraction through the skin of the inducing muscles of the frog. This experiment constantly succeeds, whether the inducing contraction be excited by means of the electric current, or by any other stimulant applied to the lumbar plexus of the inducing frog.

Hypotheses of Induced Contractions. — After having thus enumerated a long series of experiments relative to the circumstances which intervene in producing, modifying, or destroying, the phenomenon of induced contraction, one would hope that, with such a collection of facts, it would be easy to give the physical theory of the phenomenon. Unfortunately, I believe that such is not possible, and this doubt obliges me to discuss minutely the different hypotheses which can be formed to explain induced contractions.

1st, It is sufficient to have once seen this phenomenon produced by provoking the inducing contractions with any mechanical stimulant, to be convinced that the electric current, employed to excite the contraction, is not propagated to the nerve of the frog, and takes no part in the phenomenon.* How can the induced contraction of the second and third order be understood? In what way can we explain the fact, that the induced contraction is wanting, even when the current has been, as usual, applied upon the lumbar plexuses, and that only because by the incision of the nerves in the thigh, we have abolished, or greatly diminished, the inducing contraction? Why does the induced con-

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* From excessive precaution, I have often tried to obtain the induced contraction by exciting the inducing ones by lacerating the spinal marrow with a piece of glass. The induced contraction took place as if the inducing had been excited by the current or any other stimulant. *Note by Matteucci.*

traction fail when we apply the same current in the nerve below the thigh, in which case the muscles of the thigh do not contract? Why, when we operate with the current upon the lumbar plexuses of a frog, already weakened to such a degree that it only excites contractions at the commencement of the passage of the direct current, or at the moment of the interruption of the inverse one; why, I say, is there, in this case alone, induced contraction? It is useless to continue to reconcile the objections we can make to the interpretations of this phenomenon, by having recourse to a diffusion of current to produce the inducing contractions, a diffusion which we can in no way comprehend physically.

2dly, We might suppose that the induced contraction is the effect of a mechanical stimulant, namely, of the contraction of one of the inducing muscles, which thus gives a shock to the galvanoscopic frog.

I have tried a great number of times, by using very sensitive galvanoscopic frogs, to excite, by all possible means, some movements in the muscles of the thighs, without causing the galvanoscopic frog to contract. If the true cause of this phenomenon really resides in this shock, how can we explain the cessation of the induced contraction, occasioned by the interposition of a very thin leaf of gold, or mica, between the nerve and the muscle? I have very frequently tried the effect of applying the nerve of the galvanoscopic frog upon plates of metal or glass, upon tense membranes, and upon vibrating catgut strings, and have never seen contractions nanifested in the galvanoscopic frog. It is not then the hock of the contracting muscle against the nerve of the alvanoscopic frog which is the cause of induced conraction.

3dly, In some extremely rare cases contraction in the alvanoscopic frog is produced when we extend its nerve ipon the thigh of the second frog, where both are perectly insulated. But whenever this anomaly presents tself we never fail to discover the cause. It sometimes epends on some portion of the internal part of the nuscle being exposed; sometimes on some portion of he muscle being left attached to the nerve of the galanoscopic frog, and touching the nerve when we place t on the thigh. It appears to me, also, that these conractions are sometimes manifested when we touch the endinous extremities, and the surface of the muscles of he thigh, with two points of the nerve of the galvanscopic frog. I begin by telling you, that induced ontraction is invariably obtained in all cases where, from ll the precautions that have been taken, there exists no ircumstances capable of exciting contraction in the galanoscopic frog. We also know, that by dividing with cissors the muscular surface of the thighs, and renderng it by this means perfectly smooth, the contraction y induction takes place, when we apply the nerve of he galvanoscopic frog upon the fresh muscular surface. t is also produced through the skin of the frog, and ven by placing layers of insulating liquids between the nuscle and the nerve. And we have seen that the inulation produced by them was capable of intercepting the propagation of the proper or muscular currents. How can we suppose now, from these facts, that the induced contraction takes its origin from the circumstances which we have before enumerated, even by admitting that they are rendered stronger, or are excited, by the muscular contraction?

These circumstances can only arise from a phenomenon of the muscular or the proper current which ought to traverse the nerve of the galvanoscopic frog, even when the latter is surrounded by a layer of an insulating substance, which we have seen cannot be the case.

4thly, The first idea which presented itself, to explain induced contraction, was to admit that there was a development of electricity accompanying muscular contraction. There is a disengagement of heat during the contraction; there may be even cases where light might accompany it, according to the observations of Quatrefages, observations which deserve, on account of their great importance, to be repeated in order to study more fully all their peculiarities.

Hence, therefore, a certain degree of analogy would authorise us to regard as probable the production of electricity during the muscular contraction. Moreover, the few experiments which I made when I discovered induced contraction, would be equally explained, and in as satisfactory a manner, by this hypothesis. An insulating body, such as a leaf of mica, or glazed paper, prevents, by its interposition, the induced contraction from taking place. The like result occurs when a leaf of gold, discharging perfectly the electricity which we presume is developed during contraction, prevents the nerve from being traversed by it.

Notwithstanding these first suppositions, which we made in the hope of being able to furnish a simple explanation of induced contraction, and at the same time a demonstration of the existence of a very important fact respecting muscular contraction, we are now compelled to abandon entirely this view, since it is disproved by experiment.

At the commencement of this lecture I related to you many experiments which I had made, with the view of ascertaining whether there was any augmentation in the energy of the muscular or the proper current during the act of contraction. All my efforts were useless, and I was obliged to conclude that experiment did not prove that the signs of the proper or muscular current acquired a further degree of intensity during muscular contraction.

We might believe in the development of electricity independently of the proper and muscular currents. But, how can we suppose such a fact, when we see that the induced contraction is transmitted through certain insulating substances, such as turpentine, oil, &c., whilst it no longer does so if we employ very thin leaves of mica. We might suspect that electricity, developed during muscular contraction, acted by influence. In this hypothesis, we can understand why turpentine offered no obstacle to the passage of the contraction by induction; but the other fact, that with an extremely thin plate of mica, the same result does not happen, makes it become doubly inexplicable. I have tried the effect of covering a galvanoscopic frog, placed on a glass plate, with a plate of mica; the discharge from a Leyden bottle, passed between the knobs of the exitor upon the mica plate, and contractions were excited in the galvanoscopic frog. I shall not now stop to analyse this fact: it is sufficient for the present to show, that induced contraction through the mica ought to have occurred, if the cause of the phenomenon resided in an electric discharge, or was the result of the latter. I shall conclude by adding, that I have endeavoured repeatedly, but always unsuccessfully, to excite contractions in the frog by holding the nerve of the galvanoscopic frog in proximity with it, or even in contact with a metallic conductor traversed by an electric current. In order to find out the most favourable conditions, and in order that the circuit by induction should be formed and completed in the frog, I prepared the latter in such a manner that a long nervous filament, that is, one of the lumbar plexuses, and its prolongation in the thigh were exposed. The remaining portion of the body was left entire, and its two legs touched. I suspended the frog by silken cords, in an horizontal position, and its nervous filament being in contact and parallel with the voltaic conductor, which was varnished. When all these precautions are taken, in order that the frog may be perfectly insulated, we never observe contraction excited in the latter, at the commencement, at the opening, or at the closure of the circuit of the pile. It must be remarked, that in this experiment the circuit by induction may take place completely in the frog. I employed Bunsen's pile of ten elements, without obtaining any other result.

From all this, it appears, that there is no experimental evidence in favour of the explanation of the phenomenon of induced contraction, by the assumption of the development of electricity during muscular contraction.

We are, then, still ignorant of the cause of muscular contraction, and all that we know of this phenomenon are the following particulars: it is produced, even when acting at great distances from the muscle, upon the nerve, whose ramifications it receives; the integrity of the nervous filament from the point where the excitation takes place to the muscle itself is indispensable; this transmission is effected with such rapidity, that we are compelled to compare it to that of electricity, light, and radiant calorie, propagating itself through various media; what modifies, augments, or destroys the accomplishment of the chemical physico-phenomena of the nutrition of the muscle, has an analogous action upon its contractability, provoked by any influence acting upon the nerves; and, lastly, in the laws of the contraction of a muscle, we find an analogy with the physical laws of elasticity.

The fact of induced contraction will, then, be a phenomenon of induction of this unknown force, which circulates in the nerves and produces muscular contraction.

By assuming as well-proved, that the phenomenon of induced contractions could not be satisfactorily explained by having recourse to the electric current, as I believe I have completely placed beyond doubt, it appears to me that we cannot, in speaking of a fact as simple as that of induced contraction, assume any other interpretation than that which we have given. The induced contraction will be a new phenomenon of the nervous force, a phenomenon of which we have already given the principal laws. To me it would seem most rational henceforth to call that *muscular induction*, which we have hitherto termed induced contraction.

A muscle in contraction exercises an inductive action upon a living nerve. After all, I am compelled to say that, recently, I have resumed the examination of the induced current, by considering it as due to a very feeble electric discharge analogous to that of the Leyden jar. After having seen that excessively feeble discharges produce contractions in frogs, knowing that the presence of these discharges cannot be detected either by the galvanometer or any other instrument, but only by the frog, it appeared worth while to ascertain whether a very slight discharge of the Leyden phial, traversing a muscle, acted on the nerve of the galvanoscopic frog, and under the same laws that we have found induced contraction to do. I must admit that, notwithstanding a great number of endeavours, I have not been able to discover any differences. I wish, then, to tell you frankly, that until new facts are obtained with regard to induced contraction, we cannot decide whether it be due to a nervous induction, or be the effect of an electric discharge occurring during contraction.

If we could succeed by experiment in proving the truth of the latter hypothesis, we should have made a grand step in the analogies between muscular contraction and the electric function of fishes.

Production of Nervous Force.—In conclusion, let us say a few words on the production of the nervous force. Although it be true that we possess no knowledge of it except in living animals, and, consequently, want the apparatus to accumulate it and study its laws, out of the animal itself, yet we should not abandon all physical analogies in the investigations which we make relative to its mode of production.

Whenever a movement occurs, or the effect of force is manifested, we are certain that some transformation of matter must have taken place. Wherever a force is exerted, a chemical action precedes it. Caloric, electricity, and light, furnish us, at every instant, with evidence of this truth. But putting aside all analogies, let us examine the conditions under which the development, of what we call nervous force, takes place in animals. A man or animal, after a long walk, having put his machine into motion, is fatigued, and needs repose and nourishment. Although it be true that facts are wanting to establish an intimate and really scientific connection between the effects of labour, repose, and nourishment, on the one hand, and of the loss and reparation of the nervous force on the other, yet we cannot abstain from discussing these facts, complicated though they be, on the principles of mechanics and general physics.

Muscular exercise, of whatever kind, is constantly followed by a loss of power, and as we see the animal machine recover its aptitude for exercise, after having obtained food and rest, we must admit that the force necessary to muscular action may arise from the chemical actions of nutrition; inasmuch as, by means of the latter, and of repose, this force is reproduced and accumulates in the nervous system. Interrupt for a certain time the sanguineous circulation in a muscle, and soon this becomes incapable of contracting; but with the return of blood the muscular force revives. In animals, where circulation and respiration are very active, the development of muscular force is more considerable.

But among the numerous chemical actions that occur in animals, which is the one that gives rise to the force which is put in action during muscular contraction? It is impossible to give a satisfactory answer to this question.

Physiologists now admit that heat is produced by the combustion of fatty matters, and principally by that of bodies into which fecula is transformed during digestion: the nervous force may be due to chemical actions which take place during the changes which the neutral azotized substances of the tissues undergo. But I know of no experiment which directly proves this difference of origin, between heat and the nervous force.

Of all chemical actions of which the animal is the seat, the only one which we perfectly know, and which we have even measured, is that which produces carbonic acid. On the average, man converts and exhales, in the form of carbonic acid, 10 to 15 grammes of carbon per hour.

Setting out with these data, we shall endeavour to compare the nervous force which results from this chemical action, by representing the mechanical work done by a man in the space of one day, with the quantity of work that this same action could produce in the same space of time, either by means of heat, or by electricity developed by it. In other words, it is possible to ascertain whether we obtain with steam-engines, or electro-magnetic apparatus, and by means of a determined chemical action, a mechanical effect equal to, or different from, that which is produced when this same action takes place within an animal.

But before commencing this investigation, I cannot refrain from observing, that, in order to establish this comparison, it is necessary to assume one of the two following hypotheses: we now know that heat, electricity, and nervous force, are developed in animals, and we assume that the causes of their production reside in the chemical actions of nutrition. But we may suppose that they are produced in certain constant quantities, and independently one of the other; or, indeed, that from a certain chemical action there never follows but a certain quantity of force, whatever be the form in which it is manifested.

In order to make myself better understood, I will give an example: zinc burns in oxygen, producing light and heat; this same zinc can be oxydised by decomposing water and developing only heat, or heat and electricity, if we touch it with a platinum wire. Suppose, now, that we could transform these forces into a certain quantity of mechanical work done by them, we might say that the sum of these quantities is the same in every case, and that when one happens to fail, the other is substituted for it, by a relative quantity drawn from their mechanical equivalents. But it might equally happen, that they were developed independently of each other. Experiment would reply in favour of this latter opinion. I measured the heat disengaged by zinc oxydised by decomposing water, and I repeated this experiment by having, in addition, the disengagement of the electric current; the heat was invariably the same, and nearly equal to that which zinc produces when oxydising by burning in oxygen.

We might then consider all the chemical action of carbon which combines with oxygen in animals, as the cause of nervous force, independently of the heat and electricity that it may produce; and we would ask whether this action, taking place in the animal itself, determines an effect analogous to, or different from, that which it would determine, if it occurred in a steam-engine or in an electro-magnetic apparatus.

While travelling on one occasion with the celebrated Robert Stephenson, we were obliged to send a man on foot forty miles. I asked Mr. Stephenson what quantity of carbon was necessary to transport a man forty miles by a locomotive. He replied, about 5 kilogrammes [about 11 lbs. avoirdupoise].

The person we had despatched accomplished his journey, by walking, in less than ten hours, consuming by his respiration a quantity of carbon not exceeding 150 grammes, that is about $\frac{1}{34}$ of the quantity which would have been necessary if this transit had been effected by a locomotive. M. Dumas has calculated how much carbon would be burnt in a steam-engine, in conveying a man from the level of the sea to the summit of Mount Blanc. The quantity would be from 1000 to 1200 grammes; but a man accomplishes this feat by a two days' march, and consumes only 300 grammes. The difference in the second example is not so great as in the first; because the useful result which we obtain from a stationary steam-engine, is much more considerable than that from a locomotive. It is equally true that the difference is very great, and that the work produced from nervous force derived from a certain chemical action, is much greater than that which this same action produces when converted into heat.

I can show you in another way the great advantage which results from the transformation of chemical action into nervous force in an animal.

I endeavoured to measure the amount of mechanical work obtained, when applying to the nerves of a frog a current produced during the oxydisation of a given quantity of zinc in a pile. Here are the numbers obtained: three milligrammes of zinc, oxydising in one day, furnish a current which, if we suppose that it could be continually applied to the nerves of a frog, would produce a muscular power equal to 5·419 kilogrammes, raised to one metre in height during the same interval of time. It is probable that these numbers are far from being accurate, and I intend hereafter to repeat these experiments: it is, however, certain, that the causes of error are all one way, and tend to represent as much smaller than it really is, the effect produced by the three milligrammes of zinc.

The same quantity of zinc burnt, would yield a quantity of heat, which employed in forming steam, would execute work equivalent only to 0^{k}, 8304, raised to one metre.

Finally, the current produced by the three milligrammes of zinc was applied to an electro-magnetic machine; and in this case we obtained 0^{k}, 96, raised to one metre.

Everything, then, leads us to the conclusion that the mechanical work developed by chemical action, and transformed into nervous force, in an animal, is very great; and that in all the machines which man has invented he is always, and will be perhaps for a long time to come, far from attaining that degree of perfection which exists in those machines which we know not how to imitate and can only admire.*

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* In the *Comptes Rendus* for the 15th of March, 1847, Matteucci has drawn up the following summary of his hypothetical views respecting the nervous force: —

1. The nervous fluid is produced by the chemical actions of nutrition.

2. This fluid, developed principally in the muscles, is diffused there, and being endowed with a repulsive force between its parts, like the

electric fluid, retains the elements of the muscular fibre in a state of repulsion analogous to that presented by electrified bodies.

3. When this nervous fluid ceases to be free in the muscle, the elements of the muscular fibre mutually attract each other, as we see happens in cadaveric rigidity.

4. This nervous fluid enters continually into the nerves, and from them passes to the brain, assuming in these bodies a new state which is no longer that of the free fluid: in this manner it passes from the muscle to the nerve. According to the quantity of this fluid which ceases to be free in the muscle, the contraction is more or less strong.

5. This state is that of the nervous current, or species of discharge which proceeds from the nervous extremities to the brain, and returns in the contrary direction, by the act of the will.

6. When this discharge takes place, muscular contraction ought to take place, the fluid ceasing to be free in the muscles.

7. This discharge of nervous fluid, acting as in the electric fish, explains the induced contraction ; in both cases, and by the same disposition of parts, the nervous current produces a species of electric polarisation of the elements, either of the muscular, or of the electric apparatus : the difference of effects will be due to a different number of elements, to their dimensions, &c.

8. The electric current impedes the nervous discharge, if it be directed in the contrary direction ; as in the direct current : the nervous fluid not being able to enter and accumulate in the nerve, this loses its excitability. The contrary takes place for the inverse current, which goes in the same direction as that of the nervous discharge : the nervous fluid is found accumulated in the nerve, and its excitability is augmented. — J. P.

LECTURE XVI.

MUSCULAR CONTRACTION. — ANIMAL MECHANICS.

Argument. — *Muscular contraction:* agents which affect it; influence of the nervous force, velocity with which it is propagated. Analogy between the structure of muscular fibres and of the electric organs of fishes. Phenomena of muscular contraction. Schwann's observations on the degrees of muscular force evinced during contraction. Hypothesis of muscular contraction.

Locomotion of animals. The locomotive organs may, in general, be reduced to a system of levers. Discovery of the brothers, Weber, that the lower limbs oscillate during progression. Influence of atmospheric pressure on the coxo-femoral articulation.

Passive organs of locomotion: composition and structure of bone. Relation of the length and thickness of bones to their resisting power.

Muscular power. Velocity and extent of motion, how gained. Borelli's principles for estimating the force of muscles. The fore arm is a lever of the third kind. Standing on one foot is effected by a lever of the second kind. Estimated strength of the muscles of the fore arm.

The essential part of the mechanism of locomotion is either elongation or shortening.

In the preceding lecture we were engaged in giving an exposition of the laws of the nervous force, and in investigating the causes of its development; as far, at least, as is allowable in a course of lectures, from which vague notions, and purely hypothetical deductions, have been carefully excluded.

Muscular contraction and its result, locomotion, are the effects of the force which we have now to study; and in treating of these subjects, we shall confine ourselves to that which is more positive, and best established.

Muscular Contraction, how induced.—Volition, mechanical actions, heat, and electricity, determine muscular contraction by their action upon the nerves; for this effect does not take place if the nerves be tied, or if their structure be in any way altered. There is, then, evidently a force propagated along the nervous filament to the muscular fibre. We are, likewise, compelled to admit, that the fibre possesses the aptitude for, and the property of, contracting under the action of the nervous force; and we cannot explain why, for so long a time, there prevailed, and still prevail in physiology, theories, either exclusively ascribing the contraction of muscles to the action of the nervous force, and denying to the muscular fibre the power of contracting *per se;* or asserting that the power of contraction resides in the fibre and is independent of the nervous force. In the same manner, that elasticity is the property of bodies by means of which molecules are capable of being put into vibration; so is it necessary that some impulses should be communicated to these molecules, in order to throw them into vibration.

Velocity of the Nervous Force.—The velocity with which the nervous force is propagated is very great, and may be compared to that of light and electricity. I would observe, however, that without being able to deny, that its velocity is as great as that of the two latter agents, we are in want of experiments to support this supposition. The distances to which we are accustomed to observe the nervous force propagated, are very short, and we ought not to be very much surprised at the velocity of its propagation.

When we observe a muscle at the moment of its contraction, we soon perceive that its longitudinal fibres become shorter, and augment in diameter. Such is the result of the numerous observations of Foderà, Edwards, Weber, &c.

Experiment shows that the volume of a muscle does not sensibly alter during contraction. This vessel contains a torpedo and a prepared frog; it is filled with water, and closed with a cork perforated by a narrow tube, in which the liquid rises. Two insulated, varnished wires penetrate the vessel, and when brought into communication with the poles of a pile, excite contractions both in the torpedo and frog; but you perceive that no change has taken place in the height of the column of the liquid, which is precisely the same now, that it was before the contractions.

Structure of Muscular Fibre. — According to the observations of modern micographers, it appears, that muscular fibre is composed of a great number of cells or globules (discs) arranged in piles, according to the longitudinal direction of the fibres; and that from the union of these piles muscular fasciculi are formed; so that a muscle appears to be susceptible of a kind of cleavage, both longitudinally and transversely. This structure has, therefore, a great analogy with that of the electric organ of fishes; and it is very remarkable that, with respect to the general circumstances of the two phenomena, the same laws preside over the discharge of electric fishes and muscular contraction.

In the act of contraction, the globules, or, to speak more accurately, the transverse striæ of muscular fibres, approach one another; the distances between them diminish, and the thickness of the fibres augments: hence the volume of the muscle remains sensibly the same.

Contraction partial. — Bowman states, that active contraction never occurs in the whole of an elementary muscular fibre at once; but, according to this skilful anatomist, the contraction is partial, and is propagated during a certain very short, but yet appreciable interval of time; so that in a fibre in a state of contraction, there must be some points at rest, and others brought nearer together.

I am inclined to think that these appearances exist only in muscular fibres which are torn, and of which, consequently, one at least of their extremities is free. But, even according to Bowman's opinion, muscular contraction must determine the locomotion of a limb, for the inactive parts of the muscle, would be placed under the same circumstances as the tendinous extremities which are devoid of contractility.

Power of Muscles at different Periods of Contraction. — Schwann has made some important researches on the variations of the muscular force, according to the different degrees of contraction of a muscle; but I shall restrict myself to a notice of the principal result which he obtained. The force displayed during contraction, is always proportional to the length of the muscle at the different periods of contraction: so that this force, which is very great in the beginning, when the contraction commences, diminishes in proportion to the shortening of the muscle, and vanishes when the contraction has attained its maximum. A muscle which contracts may, consequently, be compared to an elastic thread stretched by a weight, and which, when this is removed, resumes its primitive length with a force invariably proportional to the weight which it supported, and to the elastic elongation it had undergone. This result is a refutation of the assumption, often made, that muscular contraction is due to the reciprocal attraction of the globules, or elementary particles which compose the muscular fibre. If this were true, the force exhibited by the muscle ought to augment during contraction: on the contrary, the result obtained by Schwann might be explained by saying, that contraction is produced by the instantaneous cessation of a repulsion, assumed to exist between the discs, and excited the instant previously.

Hypothesis of Muscular Contraction. — I take this opportunity of saying a few words respecting an hypothesis which has engaged my attention for some time, and which is supported by a great number of facts and some well founded analogies. The contraction of a muscle may be assumed to consist, at first, in a repulsion existing between the elementary parts of the muscular fibre for a very short period, and to which succeeds, in virtue of its proper elasticity, the return of the fibre, or, as it is commonly said, the muscular contraction. Nervous action would thus produce repulsion, which, by the dispersion, or the instantaneous loss of this force, must be followed by contraction. Fancy a string of globules, or discs, kept in their places by as many interposed springs; an electric discharge communicated to this system produces, at first, repulsion between the globules, assuming that these only were capable of being electrified. The repulsion will go on augmenting in the direction of the globules situated at the two extremities of the string. The electricity being dissipated, the globules return to their natural position, which they will at first pass beyond by the action of the interposed springs.

Locomotion of Animals.—I shall follow up these brief generalities on muscular contraction, by an exposition of the mechanism relating to the locomotion of animals. I do not propose, in the present course of lectures, to enter into a minute description of the various ways in which this function is effected in the different parts of the body of an animal, and in different animals; but must confine myself to a few general principles, sufficient to prove that the theories of mechanics, properly so called, are the foundation of the apparatus, or organs of locomotion of animals.

All the locomotive organs of animals may be in general reduced to a system of different kinds of levers, of which the length, the resistance, and the weight, are suitably combined; and to these levers are applied, in various ways, muscular fasciculi. Air, water, and earth, are the media where these movements take place; and which furnish fixed points, or points of support. The theories concerning the composition of forces, the centre of gravity, levers, and the resistance of media, necessarily apply, therefore, as well to animal machines, as to any machine employed in the arts.

Oscillation of the Lower Extremities in Walking.—We are indebted to the brothers, Weber, for an important discovery in the theory of walking and running in man, and of which I must not leave you ignorant. It consists in having demonstrated, by experiments, that the lower limbs, when put in motion, oscillate in a pendulum-like manner around the trunk, by the action of gravity. Limbs of different lengths, both of living men and of bodies, were made to oscillate; and in every case it was found that the durations of these oscillations were proportional to the square roots of the lengths of the oscillating limbs. These movements, then, are effected independently of our will; a fact which explains the perfect regularity with which the steps succeed each other in a child as well as in an adult, in the idiot as well as in him whose will and sensibility have received full development. The action of the muscles, therefore, is little or nothing in the execution of these movements. The leg raised and then left to itself, accomplishes the step by the sole influence of gravity. The head of the thigh-bone suffers a very slight friction only in rotating in the cotyloid cavity, where it is retained by atmospheric pressure, which thus assists in accomplishing these movements. The whole weight of the limb does not press against the sides of the hip joint: the head of the thigh-bone remains fixed in this cavity by atmospheric pressure, and hence the effect of gravity upon the member is destroyed. For this fact we are indebted to the researches of the brothers, Weber, who have demonstrated this mode of action of atmospheric pressure. The tendons and the muscles which connect the thighs to the trunk were cut across in a suspended subject, without the limb suffering the least change in its position; but when a hole was made into the cotyloid cavity, the limb immediately fell, and it could be made to fall, or be prevented from doing so, merely by opening or keeping closed this aperture. By calculating the pressure exercised by the atmosphere against the plane section of the cotyloid cavity, we find that it is equivalent to 11·970 kilogrammes [about 26 lbs. 6 oz. avoirdupoise]; that is to say, it slightly exceeds the mean weight of the leg.

Passive Organs of Locomotion.—The passive organs of locomotion, like every other part of the human machine, present a constant application of the principles of mechanics, in order to realise a very complicated result. Suppose we said to an engineer, we want a movable column composed of a certain number of cylindrical pieces, united together by their extremities, and of which the length must be variable: the thickness of the column should be such that it can support the weight with which it will be loaded, and it should be capable of resisting lateral shocks. The extremities of the different pieces of the column shall so terminate, that the forces to put it in motion may be applied there; and, finally, the column must be capable of executing a great number of partial movements without deranging its simplicity and elegance. The engineer would certainly consider it a problem difficult, if not impossible, to solve.

Bones. — The bones are formed of a mixture of gelatine, and of phosphate and carbonate of lime in different proportions. If these vary, great changes in the degree of tenacity and elasticity of the bones in consequence take place. Independently of their composition, the more or less fibrous, and more or less spongy, texture of bones, serve to modify their weight and resistance. These properties also vary according to the dimensions of the bones. Euler proved long since, that the weights, which cylindrical or prismatic rods can sustain without yielding, are in the inverse ratio to the square roots of their length, provided that their nature and section remain constant. If we express by 1, 2, 3, 4, &c., the lengths of the homogeneous rods, the weights that they can support without bending are expressed by the numbers 1, $\frac{1}{4}$, $\frac{1}{9}$, $\frac{1}{16}$, &c. Hence, the bones must have different lengths, according to the different efforts which they are required to sustain.

Salidcé demonstrated long since, that within certain limits of thickness of the osseous wall, the resistance which bones oppose to fracture, against a force applied laterally, is greater in a hollow cylinder of large diameter than in a solid cylinder, and consequently one of smaller diameter. All bones are constructed so as to give the necessary resistance, without greatly increasing the weight.

Muscular Force. — Let us, in the last place, speak of muscular power, a subject, which I must admit is not more advanced now than it was a century ago, when Borelli began to study it. In general, we observe that, in the employment of muscles in locomotion, their arrangement is so combined as to give the greatest possible velocity and extent of motion, without sacrificing the simplicity, harmony, and elegance of the different parts of the human machine. In every possible case, the following conditions are united to realise these results:

1st. The oblique insertion of the muscular fibres in the tendon.

2d. The obliquity of the direction of the tendon to the axis to which it is attached, and on which it must act.

3d. The proximity of the points of insertion of the tendons to the articulation of the bones, which serve as points of support.

The principles established by Borelli for calculating the force of the different muscles of the same animal, and which principles are at the present time generally admitted, are as follows: —

1st. Two muscles composed of the same number of fibres, and consequently of equal thickness, can raise a given weight to heights which are proportional to the lengths of the muscles; or rather, they can raise to a given height weights which are proportional to their lengths.

2d. If two muscles be of equal length, they can raise to a given height weights proportional to their thickness. In a word, the volume of a muscle determines the weight it can raise; and, on the contrary, the height to which the weight can be raised varies according to the length of the muscle. In more general terms, the mechanical work of a muscle varies according to its length and thickness. It is unnecessary to say that we omit mentioning here an element which is not susceptible of being measured *a priori*, namely the intensity of the nervous force, which varies with the will.

It is easy to understand how the extent of motion of the bone depends on the different obliquity with which the muscular fibres are inserted in the tendon, and how it must increase in proportion as the fibres are the more obliquely attached.

We may also observe that, according to the different obliquity with which the muscle is made to act upon the moveable bone, a greater or less loss of power must ensue.

Such is the most general arrangement of the motor organs of animals, and we can readily conceive the reason, when we consider that the bodies of both man and animals generally, would have a monstrous form if the muscles acted normally upon the bones. In order to diminish a portion of the loss of the force which the muscles suffer in consequence of the obliquity of their insertion on the bones, the latter terminate by spherical enlargements, and the tendons glide over these, in proceeding to attach themselves inferiorly to the bone to be put in motion.

Lastly, let us remark, that on the relative position of the points of support (*fulcra* or *props*), and of the points of application of the resistance (or *weight*), and of the power (or *force*) in the levers of the animal economy, depend the well-known relations between the spaces traversed by the power, and the resistance, and the absolute forces which they represent. In general, the levers of the human body are of the third kind, so that the arm of the lever of resistance surpasses that of the power. The fore-arm offers us an example of a lever of this kind; indeed, its point of support is at the elbow-joint, its resistance is the weight of th arm, which we presume to be applied at its middle, and its power is the flexor muscles, fixed at the extremity of the bones of the fore-arm. We need only compare the relative points of application, of the power, and of the resistance, with the point of support, to obtain numbers expressing the ratios of the movements of the power and of the resistance, such as they are furnished by theory. The extremity of the fore-arm traverses an arc much greater than that described by the flexor muscles; the first accomplishes its movement with a velocity of 974 millimetres in the second; the other, on the contrary, with a velocity of 81 centimetres.

In cases where it is necessary to make an equilibrium to a greater force, by means of a smaller one, the lever of the second kind is employed; we have an example of this in man, when standing on one foot, which thus supports the whole weight of the body.

Borelli endeavoured to value the force of a great number of muscles, and deduced, from the numbers found in his experiments, the amount of force lost in most muscular movements, in order to obtain velocity. We shall notice one only of the cases examined by Borelli; that being sufficient to explain his mode of calculating the forces of muscles. The weight of the fore-arm of man is about 2 kilog., which we may consider as applied at the middle of the fore-arm; or, what is the same thing, we may assume that this weight is 1 kilog., applied at twice the distance from the point of support, namely, the hand. It is known that a man can support with his extended arm a weight of about 13 kilog., and consequently the total resistance to which the equilibrium should be made is 14 kilog. But the power of the muscles for the arm of the lever has a length which we know to be about the twentieth part of that of the fore-arm; therefore, the value of x will be 280, or the product of 14 multiplied by 20, so that to carry a weight of 14 kilog. with the hand, the flexor muscles ought to make an effort equal to 280 kilogrammes.

The generalities which we have now explained suffice to enable you to comprehend the mechanism of the different movements of animals. If I had wished to speak with all possible minuteness of the theory of walking, of running, of swimming, of flying, &c., the subject would have passed beyond the limits, not only of a single lecture, but even of an entire course.

In all cases, whatever be the animal and the manner of its progression, the essential part of its mechanism of locomotion invariably consists in an elongation or a contraction, movements which the two branches of the arc or arcs, formed by certain parts of the animal, execute. Under some circumstances, these arcs are formed by the body of the animal, which then becomes vermiform, or arched, as in animals which swim or crawl. In other cases the movements of extension and of flexion result from the successive approach and retirement which the two sides of an angle formed by the limbs of the animal undergo. One of these sides always contains the point of support, or both of them, according to the medium in which the animal moves. Air, water, and the ground, offer resistance to the parts of an animal which strike against, or press upon, these media, and the movement is produced in the same way that the progression of steam-boats and of locomotives is effected, by the impulse of the paddles against the water, and of the friction of the wheels upon the rails.

LECTURE XVII.

CIRCULATION OF BLOOD.

Argument. — Circulatory apparatus; the heart; bloodvessels. Mechanism of the circulation. The blood; its general properties; quantity in the body. Number of pulsations. Velocity of the circulation; experiments of Hering, of Matteucci and Piria, of Poiseuille, and of Hales. Pressure on the blood in the vessels; Poiseuille's hæmo-dynamometer; results obtained with it. Motion of the blood in the capillaries. Motive forces of the circulation; muscular contraction of the heart; elastic contraction of the arteries. Pulmonary circulation. Concluding observations.

One of the most important effects of the nervous force is the motion which it communicates to the blood in animals; for it is now admitted by all physiologists that muscular contraction is the principal agent concerned in the circulation of the blood. In order to carry out the intentions of this course, I propose, in the present lecture, to demonstrate to you that the apparatus which puts the blood in motion, by muscular contraction, possesses all the conditions of an hydraulic instrument; and I trust that I shall be able to prove to you that the simplest and most elementary laws of the movements of liquids are applied, to obtain, by the passage of the blood through the various organs and different parts of the body, the numerous effects necessary to the development and preservation of the animal.

I regret that I am unable to treat of this subject in the extended manner which it deserves, in consequence of being confined within the narrow limits which I feel obliged to impose on myself in these lectures. I shall, therefore, restrict myself to an exposition of those experimental results which are best established, and which are necessary for the theory of this function.

Circulatory Apparatus.—The apparatus for the circulation of the blood may, in its simplest form, be reduced to a system of canals or tubes, forming a kind of complete circle, invested at some point with a muscular substance to develope the force necessary for putting the blood into motion. This apparatus becomes necessarily more complicated in proportion as we ascend in the scale of beings; and, whilst, in the lower animals, the nutritive liquid fills a vast system of lacunæ, which constitutes the whole of their organisation, and is endowed with a slow and irregular motion; in the higher animals, on the contrary, the blood circulates in a system of canals of a peculiar organisation, in a fixed direction, and with a constant, but more or less considerable rapidity. The apparatus is complete when, for the execution of this function, two orders of vessels are employed whose structure is very different, and which communicate with each other after having divided into a great number of ramifications of a constantly decreasing diameter. In one order of vessels the blood travels from the great trunks towards the smaller ones; and in the other it moves in an opposite direction. At the point where these two systems arise there is a peculiar organ called the *heart.* The small tubes which altogether form the extremities of the arteries and the veins, are called *capillary vessels.* And as these two orders of tubes terminate by the opposed extremity in the cavities of the heart, the vascular apparatus may be correctly said to form a complete circle, which the blood traverses, returning incessantly to its starting point.

I cannot pass over in silence various particulars relating to the structure of the circulatory apparatus; but I shall notice them in the briefest manner possible; and as we ought to study the function in its most perfect and complicated state, the few anatomical points which I shall here notice will relate to the human body.

Heart.—The heart of man is a conical or pyramidal cavity, formed by a kind of muscular sac divided into two parts, each composed of two cavities, placed one above the other; one of these is called the *ventricle,* the other the *auricle.* The two ventricles of the heart occupy the lower portion; and their cavities are much larger than those of the auricles placed above them. The left auricle and ventricle belong to the apparatus for the circulation of arterial blood; while the right auricle and ventricle belong to that for venous blood. The ventricles have thicker walls than the auricles, especially the left ventricle, from whence the blood is propelled into the arteries, and into all parts of the body. The right ventricle and auricle communicate with each other by an opening called the *auriculo-ventricular* orifice; to the two sides of which is attached an annular membrane, whose inner border is floating, and to which are fixed some tendinous cords. The latter are attached to some muscular bundles, or *fleshy columns,* which arise from the inferior walls of the ventricle, and proceed towards the orifice: this membrane is called the *tricuspid valve.* Another one, analogous to this, and named *mitral valve,* exists at the orifice by which the left auricle and ventricle communicate with each other. It differs from the former by its greater solidity and more considerable resistance, which are capable of overcoming its tendinous filaments. The orifices of both the left and right ventricles, through which the blood escapes from the heart, are furnished with another kind of valve of a very different construction to those already described. They are called *semilunar valves,* and are formed of three portions of membranes, which have, as their name indicates, a semicircular form; on one side they are adherent to the wall of the orifice, and on the other are free, so that when they are depressed, they present the form of three triangles, having for a common summit the centre of the vessel, and for a base, its circumference. They are pressed against the sides of the vessel, when a liquid column is driven out of the heart; but, on the contrary, when a liquid attempts to pass back from the vessel to the heart, they immediately expand, and close the orifice. Thus then they allow the blood contained in the heart to escape by the vessels, and prevent the blood contained in the vessels from returning into the heart.

Blood-vessels.—Lastly, we shall say a few words respecting the blood-vessels. In the arteries, we remark a middle membrane, very thick, and composed of circular fibres of a peculiar nature, called *the yellow elastic tissue.* To this the arteries owe their elasticity. The membrane, composed of this tissue, is placed between the internal or serous membrane and the external membrane, composed of condensed cellular tissue. Three tissues likewise form the sides or walls of the veins; but in these vessels the yellow elastic tissue is less obvious; the internal membrane is enveloped by a thin layer of slight longitudinal fibres, of a loose texture; and the external membrane is of a cellular nature, and very resisting. The internal coat is thin, smooth, resisting, and extensible. It forms in the cavities of some of the venous trunks numerous folds or *valves,* which are so placed that they can diminish or even entirely obliterate the diameter of the vessel, when a liquid column moves in a direction contrary to that of the course of the blood within them.

The Circulation.—In a few words we shall describe the manner in which the circulation is effected. To give you a correct notion of the way in which this function is performed, it is sufficient to expose the heart of a rabbit or other mammal, and we shall then see the alternate contraction and dilatation of the ventricles and auricles. When the right ventricle dilates, the right auricle contracts, and *vice versâ:* the same phenomenon takes place in the other side of the heart; so that the movements of the homonymous parts take place simulneously, and are alternate with those of parts having different name. At the moment of the contraction of ne of these cavities, the blood which it contains is expelled, and the direction which it takes is regulated by he arrangement and action of the valves placed at the rifice of this cavity. At the moment of the contraction f the right auricle the blood is forced towards the auriculo-ventricular orifice, and backward into the *venæ avæ.* At this instant the ventricle dilates, the orifice pens, and very nearly the whole of the blood rushes nto it; a small quantity only regurgitating into the enæ cavæ. To these movements succeed the dilatation f the auricle, and the more energetic contraction of his ventricle. The blood which filled the ventricle is xpelled from this cavity: two orifices present themselves, that of the pulmonary artery, and the auriculo-ventricular orifice. The latter would allow it to return nto the auricle, whose contraction has just ceased, but for he action of the tricuspid valve, which opposes it, and hose structure is well adapted for this purpose. The lood, pressed on by the walls of the contracting ventricle, distends the membrane forming this valve, which ields until it becomes perpendicular to the axis of he ventricle, when the orifice becomes almost completely closed. But the fleshy columns, by contracting so, prevent the valve from turning over into the uricle, and retain it at the orifice. On the same inciple, valves in all our hydraulic machines are constructed. The semi-lunar valves, placed at the orifice the pulmonary artery, on the contrary, yield to the impulse of the blood, and leave this orifice free; hence the blood escapes into the artery in consequence of the construction of the valves, which open from within outwards.

The arrangement of the valves, in the left cavities of the heart, is similar to that in the right cavities just noticed. When the left auricle dilates, the blood from the pulmonary vein enters it; and at the moment when it contracts, which circumstance coincides with that of the dilatation of the ventricle, the blood is propelled into the latter, and from this into the aorta, in consequence of the contraction of the ventricle.

Two successive sounds reach the ear when applied to the chest, corresponding to the two successive movements of the heart: the auricles dilate together; and the ventricles likewise.

By contraction of the heart we mean that of the ventricles: their contraction is called *systole,* their dilation *diastole.*

We have thus sketched the principal features of the mechanism of the circulation. As for the details, they are not within the limits of the present course, and, therefore, it is not our province to explain them to you. I would have even suppressed what little I have said on this subject, had I not considered it absolutely necessary to precede the study of the phenomena of the circulation of the blood by an exposition of these elementary anatomical facts.

The Blood.—The liquid which circulates in the vessels is of a vermilion red colour, in the arteries, and of a dark red in the veins. It is slightly alkaline, has a specific weight of 1·0527 to 1·057, and holds in suspension globules of a greater or less diameter. In most mammals these globules are circular discs; while in birds, reptiles, and fishes, they are elliptical. This liquid constitutes the blood. The quantity of it varies in different animals, and there appears to exist a certain ratio between its weight and that of the animal. Valentin has pointed out an ingenious method of determining its total quantity: it consists in first ascertaining the percentage quantities of water and solid matters contained in the blood drawn from an animal by a small bleeding. A given weight of water is then to be injected into one of these blood-vessels, and again we must ascertain the percentage composition of the blood thus diluted, that is, the proportion between the water and solid constituents. It is easy to perceive how, with these data, we may obtain the amount of the total quantity of the blood.

It is assumed that man contains, on an average, from 12 to 15 kilogrammes [about 30½ to 40⅕ troy pounds] of blood. The ratio which exists between the weight of the blood and that of the man will be about 1 to 5.

Number of Pulsations. — The heart of an adult man contracts from seventy to seventy-five times in a minute; but the number of pulsations varies according to the age, sex, temperament, and idiosyncrasies of individuals, the species of animal, and the pressure of the atmosphere. Thus in the new-born infant the number of pulsations is from a hundred and thirty to a hundred and forty; in fishes, from twenty to twenty-four; in the frog, sixty; in birds, from a hundred to a hundred and forty. Parrot ascertained that his own pulse was a hundred and ten, at an elevation of 4000 metres [=13123 English feet] above the level of the sea, while the number, at the level of the sea, was only seventy.

Velocity of the Circulation.—Let us now speak of the velocity of the movement of the blood. The researches made on this subject may be divided into two classes: first, it is important to determine how much time the blood occupies in traversing the whole system; afterwards we must examine the velocity with which the blood traverses certain parts of this circle; in short, with what speed it moves in the arteries, the capillaries, and the veins.

We are indebted to Hering for some of the experiments on the first class of these investigations. A solution of ferrocyanide of potassium was injected into the jugular veins of a horse, and at the same instant the blood which escaped from the opposite jugular was collected. It was received in numbered vessels, which were changed successively at equal intervals, by counting with a chronometer the number of seconds which elapsed from the commencement of the experiment to the moment when the blood was collected in the last vessel.

In an experiment which I had occasion to make with Professor Piria, and which was performed on a horse, he blood of one jugular was collected at the moment vhen the ferrocyanide was injected into the other, and he receiver was changed every five seconds. We found hat the blood which escaped twenty or twenty-five econds after the injection, contained traces of the ferocyanide. These numbers agree with those of Poieuille and of Hering.

Poiseuille, when repeating these experiments, first scertained that, notwithstanding the introduction of he ferrocyanide, neither the number of pulsations, ior the force of the heart's contractions varied. But, ike Hering, he discovered traces of the ferrocyanide in rom twenty or twenty-five seconds after its introluction.

I cannot refrain from relating some of Poiseuille's xperiments, made for the purpose of ascertaining the nfluence which certain substances mixed with the lood had on the velocity of its circulation. He found n every case, that the number of pulsations, and the orce of the contractions, were unaltered. When a olution of acetate of ammonia was injected along with he ferrocyanide the latter was detected in about ighteen seconds; and nitrate of potash gave an anaogous result, but extending this interval to twenty econds. On the contrary, when a little alcohol was dded to the ferrocyanide injected into the jugulars, the atter did not escape from the opposite vessel until fter forty or forty-five seconds. The influence of hese substances upon the rapidity of the circulation nerits especial attention, for it is connected with a fact entirely within the domain of molecular physics. Poiseuille ascertained, in an important investigation on the passage of water, serum, &c., in capillary tubes, that these substances acted there absolutely in the same way as in the sanguineous circulation. It is not, however, to be supposed that it is by this kind of influence that many other substances introduced into the blood, act; for a great number of them exercise their influence upon the nervous force, and through this on the contraction of the muscular fibre of the heart. Thus, a small quantity of an infusion of coffee injected into the veins of a dog, instantly augments the force of the heart's contraction, while a solution of opium diminishes its energy.

The rapidity of the circulation, that is, the time which a molecule of blood occupies in passing from the right to the left ventricle, seems, at first sight, to have been very accurately ascertained by the before-mentioned experiments of Hering, and the numbers given by him are very generally adopted. But it appears to me easy to show that his method is liable to several errors, and that his results are far from expressing the duration of the circulation. If, in place of introducing into the vein a liquid solution, the presence of which we must afterwards detect in the opposite vessel, we could cause a small solid body, of a density equal to that of the blood, to pass there, and which would accompany the blood when traversing the capillaries and the whole circulatory system, the period which elapsed, from the moment of its introduction into one jugular to its appearance in the other, would be precisely the time required. But we must bear in mind that, if a solution susceptible of mixing with another be poured into any part of the latter, we soon find it in the entire mass, even supposing that it be much more considerable than that of the first, and without any motion. Two solutions, capable of mixing, diffuse themselves, and rapidly mix in consequence of the effects of chemical action, aided by the physical properties of the liquids. Hence, it is not necessary, for the kind of diffusion of which we are now speaking, that the ferrocyanide should have traversed the entire circulatory circle. It must be especially observed, that it is impossible to introduce a liquid solution into the veins without propelling it by a pump, and employing a force which, it is obvious, is considerable, since it must overcome the pressure exercised by the blood, so that the mixture of this solution with the sanguineous mass is promoted, more or less, according to the degree of force employed.

In consequence of these objections, I cannot admit the accuracy of the numbers given to show the time in which the blood accomplishes its entire circulation; and this method appears to me to be incapable of furnishing correct results.

Moreover, there exists another mode of experimenting, which the celebrated Hales first applied to the investigation of the velocity of the circulation. It is based on a principle essentially accurate, and consists in deducing the velocity from the capacity of the ventricles, and from the number of pulsations of the heart in a given time.

Hales measured the capacity of the left ventricle of a horse, and of several other animals, by taking a cast of the ventricle, by pouring melted wax into it, and allowing it to solidify therein. He found, that the capacity of the left ventricle of a mare was equal to 10 cubic inches; and the weight of 1 cubic inch of blood being 267·7 grains, it follows that the total weight of the blood contained in the ventricle was about 6 ounces avoirdupoise. Assuming that, at each pulsation, the ventricle completely empties itself, 6 ounces of blood would be expelled at each contraction, and seventy-two of these contractions would be necessary to effect the complete circulation of the 36 pounds (troy) of blood which the horse contains. His result is very different from that obtained in the experiments of Hering, relative to the velocity of the circulation. For, if the heart of a horse made only sixteen pulsations in twenty-five seconds, there could not escape from the ventricle, in that interval of time, more than 8 pounds (troy) of blood. It would be difficult to account for such a very great discrepancy in these results, if we assumed Hering's numbers to be correct.

We shall here quote the numbers given by Hales for the time required for the completion of the circulation in man. Assuming seventy-five pulsations per minute, from 24 to 30 pounds for the weight of the total mass of blood, and 2 ounces to be about the quantity thrown out at each contraction of the left ventricle, a hundred and ninety-two pulsations would be necessary to make the entire mass circulate; that is, about two minutes and a half.

It is, however, but right to observe, that we cannot assume that all the blood of the ventricle would be expelled at each pulsation; consequently, the numbers given must always be below the true ones.

It remains now for us to speak of the velocity of the blood in the different vessels of the circulatory system. Assuming the section of the orifice of the left ventricle to be equal to that of the aorta, and that the sum of the sections of the different branches into which it divides, is also the same, it follows that if the same quantity of blood pass every where in the same interval of time, the velocity will be the same in every vessel. But the sections of the arterial and venous trunks are not really equal to those of their ramifications. The most simple observation proves, on the contrary, that the sum of the sections of the small vessels is more considerable than that of the trunks. Look at the heart of an ox, in which we have divided the arterial and venous trunks, the orifice of the aorta is about 28 millimetres, while that of one of its trunks is about 20, and the other 16: the venæ cavæ have a total diameter of 76 millimetres. The well-known law of Castelli ought to be applied, in order to obtain the velocity of the blood at different parts of its course: this velocity will always be in the inverse ratio of the sections. If we could accurately estimate the ratios which exist between the sections of the various vessels in which the blood circulates, it would be easy to determine what would be the velocity in all the vessels; the quantity of blood thrown out of the left ventricle being known, as well as the time employed in expelling it.

I shall content myself with showing you by one illustration, how we may determine the velocity with which the blood circulates in the aorta: I adopt the numbers given by Hales. The quantity of blood expelled from the left ventricle of the heart of a horse, is about 10 cubic inches; the area of the aorta is 1·036 square inches; the fraction $\frac{10}{1\cdot036}=9\cdot64$ expresses the length of the cylinder of blood which is formed in passing through the orifice of the aorta at each systole of the ventricle; and, as the heart of the horse makes thirty-six pulsations per minute, that is, 2160 per hour, it follows that a column of blood [2160 × 9·54 inches or] 1735 feet long will be thrown into a vessel of the calibre of the aorta every hour. Then, estimating the systole to continue only one third of the interval between the pulsations, the velocity will be found to be thrice as much, namely, 86 feet per minute.

Hales, who studied the phenomena of the circulation with so much sagacity and skill, endeavoured to determine experimentally the velocity of the blood in the capillary vessels. For this purpose, he introduced warm water into the descending aorta of a dog, by means of a tube thrust into the artery. The pressure exercised by the column of liquid, was nearly equal to that which the blood experiences in this vessel [from the contraction of the heart]. The intestines having been slit open from end to end, the water was seen to ooze out, drop by drop, from the capillaries. Hales varied the experiment, by successively dividing the vessels nearer and nearer to the aorta; and, at the same time, measured the water which passed off, in a given time, by the various capillaries, whose diameter he knew. The pressure of the column of liquid was kept constantly equal. Here are some of the numbers obtained: 342 cubic inches escaped from the capillary vessels in 400 seconds; the same quantity passed off by the mesenteric arteries in 140 seconds; and by the crural arteries in 20 seconds.

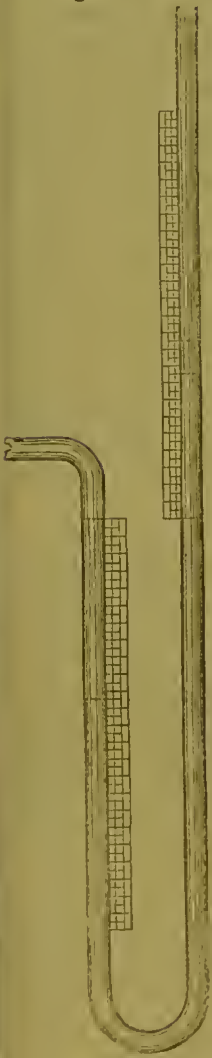
Although the numbers may be far from expressing the absolute velocity of the circulation in different vessels, they are, nevertheless, sufficient to prove that the velocity diminishes in proportion to the distance from the heart, and to the smallness of the sections of the vessels. Notwithstanding the remarkable augmentation in the sum of the sections of the branches, in comparison with those of the trunks, it is certain that the velocity of the circulation is diminished, and becomes much less than that which it would have been if the partial sections had been united and formed into a single vessel. This diminution of velocity is occasioned by the friction of the fluid against the sides of the vessels, by the large folds, the numerous curves, and the resistance of the liquid column put in motion. On account of the great number of anastomoses, between the ultimate arterial trunks and the extremities of the arterial and venous capillaries, and which are especially remarkable in the lungs, the velocity of the blood suffers less diminution than it would otherwise do. In this way the lengths of the small tubes are diminished as much as possible, and precisely in proportion as the sum of the sections of the ramifications exceeds the sections of the trunk from which they arise.

A beautiful object of experimental physiology, is the microscopic examination of the capillary circulation of the lungs of the salamander, or that of the mesentery and the claw of the frog. We perceive the globules of blood moving with more or less rapidity within small vessels, with a velocity which varies according as the section of the vessels is greater or less.

Pressure on the Blood.—I must now speak of the pressure which the blood sustains in the vessels in which it circulates. The investigation of this subject has engaged the attention of physiologists of all times, and they have pursued different ways in their experiments. Such, also, has been the case with respect to the investigation of the force with which the left ventricle contracts. Borelli, Bernouilli, and Keil, obtained numbers which differ very considerably from each other. Thus, Borelli estimates the force of the heart as equivalent to that of 180,000 pounds; while Keil, on the contrary, calculates it at only 5 ounces.

Hales was also the first who made accurate experiments for measuring the pressure of the blood in the arteries; but it is to Poiseuille that we are indebted for the most complete investigation of this subject. I shall confine myself to an exposition of his principal conclusions.

Fig. 19.



Poiseuille's Hæmo-dynamometer.

The hæmo-dynamometer of Poiseuille consists of a kind of glass manometer, whose short, horizontal arm is placed in a brass tube, which is afterwards introduced into the artery of the living animal. In order to prevent the coagulation of the blood, which obstructs the portion of the tube situated between the artery and the column of mercury, Poiseuille first fills this with a solution of carbonate of soda. We then see the column of mercury in the long vertical arm rise, and remain at a height which continues constant throughout the experiment. The difference in the level of the two columns of mercury, indicates the pressure exercised by the blood against the corresponding section of the wall of the vessel into which the tube of the apparatus is introduced. Poiseuille made a great number of experiments upon the arteries of different animals, and upon various arteries of the same animal; and the most important fact which he demonstrated is, that the pressure of the blood in the arteries is the same, whatever be the part of the arterial system, the diameter of the vessel, its distance from the heart, and the position of the branch experimented on, in relation to the trunk from which it is derived. Thus, Poiseuille found that the pressure obtained, by applying the hæmo-dynamometer to the carotid artery of a dog, at a distance of 180 millimetres from the heart, is the same as that of the aorta at 370 millimetres: the diameter of the aorta being 9 millimetres, and that of the carotid 4 millimetres. In both cases the pressure was measured by a column of mercury 84 millimetres in height. This experiment, made on a horse, gave the same result, when performed on two arteries, the diameter of one of which was five times greater than that of the other: the pressure made by both was equal to that of a column of mercury of 146 millimetres. It is curious to observe, that the pressure, in different animals, has no relation to their weight.

This result, which it is right to have had demonstrated by experiment, ought not, however, to astonish us when we reflect that it is a necessary consequence of the principle of the equality of pressure of liquids. The impulse given by the column of blood expelled from the left ventricle, to that contained in the aorta, is immediately propagated equally to the whole mass of blood, filling both the large and small arteries.

It is this impulse, which takes place at the moment when the ventricle propels the blood into an artery and into all its branches, that produces the well-known phenomenon of the *pulse*, and which we know to be isochronous with the contraction of the ventricle.

Adopting the numbers given by the experiments of Poiseuille, we have then the pressure supported by the walls of the heart and of the arteries. This pressure is always equal to the weight of a column of mercury, which has for its base the area of the artery or the superficies of the ventricle, and for its height, that obtained by means of the hæmo-dynamometer. From these data, Poiseuille calculated that at the moment of the contraction of the heart the blood is propelled into the aorta of a man, twenty-nine years of age*, exercised against the column of liquid which fills it, supposing this to be at rest—a pressure equal to 1·971779 kilogrammes [= about $5\frac{1}{4}$ lbs. troy]. In the radial artery this pressure is no more than 15·35 grammes [about 137 grains troy]. By knowing exactly the internal surface of the left ventricle, at the moment of contraction, we can easily calculate the pressure exercised upon these walls at this moment.

Among the most important results of Poiseuille's experiments, I would also mention that which shows the constancy of the variations in the height of the column of mercury in the hæmo-dynamometer, during the respiratory movements. This height is invariably greater during expiration, than during inspiration; and this difference is observed in large as well as in small arteries; though it is more or less considerable in different animals.

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* It is assumed that the column of mercury raised in the hæmo-dynamometer, applied to man, should be 160 millimetres, and the diameter of the aorta 34 millimetres.—*Note by Matteucci.*

It is proper, also, that I should draw your attention to the fact, that the height of the column of the hæmodynamometer likewise varies according to the position of the animal. I have always remarked, that when this instrument is introduced into the carotid, the column of mercury rises several millimetres when we raise the animal by its hind part, and falls when we place it in an opposite position. The cause of this difference is evident. We must, therefore, presume, that in all Poiseuille's experiments, in which he compared the pressure of the blood in different vessels, the animal was kept in the same position.

I cannot pass over in silence the dilatation of arteries at every pulsation. We are indebted to Poiseuille for proving beyond doubt the existence of this phenomenon, which has so great an influence upon the circulation. He laid bare a certain portion of the carotid artery of a living horse, and enclosed it in a metallic tube filled with water. This tube had an opening closed by cork, in the centre of which was fixed a small glass tube. At every contraction of the left ventricle the liquid rose in the tube, and fell again when the contraction ceased. Thus, then, after the dilatation of the artery, occasioned by the impulse of the blood, the arterial coats return to their former state in virtue of their elasticity. Poiseuille endeavoured to measure this elastic force displayed by the arterial coats; and without adopting his conclusion, that the force of contraction of the coat exceeds that of the dilating force, it is certain that, when the contraction of the ventricle, the principal force which puts the blood in motion, ceases, the elasticity of the arterial coats, which recover themselves, also propels the blood, and thus adds to the force of the heart.

Lastly, I must say a few words respecting the researches which this physiologist, already so frequently quoted, has made on the movement of the blood in the capillaries. Poiseuille observed, in a great number of experiments, that the motion of the blood in the vessels ceases when the heart is raised or bound, and that this movement continued only for a few minutes, on account of the diminution of volume, and of the kind of contraction which the elastic coats of the vessels suffer when the blood ceases to be propelled by the heart. By the aid of the microscope there was seen an immovable layer of serum, adherent to the coats of the vessel; and the liquid blood thus moves in this tube, formed by its own substance. Poiseuille examined the passage of the same liquid, both in a glass capillary tube, and in a capillary blood-vessel of a living or dead animal, and found that it followed the same laws in all cases. This fact assuredly proves, that in these various cases the liquid really circulates in a tube always formed of the same matter; that is to say, of an immovable and adherent liquid layer, and which is the same as the liquid which flows through, whatever may be the material of the tube. It is curious to observe that the capillary circulation continues uninfluenced either by a vacuum or by a pressure of eight or ten atmospheres.

Having explained to you, as far as the limits of this course will permit me, the most accurate and conclusive experiments upon the various questions relating to the sanguineous circulation, we have now all the necessary elements for giving an account of the mechanism of this function.

I consider it useless to detain you longer for the purpose of experimentally demonstrating, that the contraction of the heart and the elasticity of the coats of the vessels, especially of the arteries, are the principal powers of the circulation. If we tie an artery in a living animal, the vessel almost entirely empties itself of blood, and the circulation continues in it only for a short space of time. The contrary happens with the ligature of a vein; for in that case the blood soon accumulates, and the vein swells below the ligature. I shall confine myself to the relation of a single experiment by Magendie. The crural artery and vein of a dog were laid bare, and a ligature applied to the vein: an incision was then made below the ligature, and a jet of blood escaped. When the artery was pressed between the fingers, in order to prevent the passage of the blood within it, the jet of venous blood diminished, and was ultimately stopped completely; but when the pressure was removed, the jet re-appeared. These phenomena could be reproduced several times. The conclusion drawn from this fact is evident: the blood traverses the capillaries, and circulates in the veins by the sole forces which have propelled it into the arteries, namely, by the contraction of the left ventricle, and that of the arterial coats, which are the chief powers, the only ones, in fact, of the circulation.

The influence which atmospheric pressure exercises upon this function is very limited. I have already mentioned, that at each experiment the column of mercury of the hæmo-dynamometer, applied to the artery, rises; and at each inspiration it falls. Poiseuille observed the same phenomenon, and under the same circumstances, in large venous trunks: thus, in the jugulars the column of mercury rises during expiration, and falls during inspiration. These phenomena do not occur when we make the experiment upon venous trunks distant from the thoracic cavity. We easily understand that when the latter dilates, the pressure of the atmosphere must compress the veins, and thus, by means of valves, so placed in these vessels as to impede the return of this liquid, it assists in making the blood move towards the heart. On the contrary, during expiration, when the thoracic cavity is contracted, all the vessels contained within it are simultaneously compressed. Experiment has shown, that the variations of the pressure of the blood in the arteries and the veins, correspond to the respiratory movements, and cease to manifest themselves in the sanguineous trunks situated beyond this cavity.

The muscular contraction of the heart, and the contraction of the arterial coats, are then the principal motive powers of the sanguineous circulation. The combination of these two forces in the mechanism of this function is so perfect, that the movement of an intermitting jet, produced by the alternate contractions of the heart, is transformed into a continued movement by the elastic force with which the arterial coats are endowed. By this force the arteries, which were at first dilated, recover themselves, and consequently propel the blood forward, at the moment when the contraction, which has thrown the blood into the arteries and caused them to dilate, ceases.

Let us imagine, then, a circular system of tubes of different diameters, and having elastic sides; and that the two extremities or apertures of this system open into two cavities separated from each other, and of which the walls can approximate and separate like those of a pair of bellows. When we fill this tube with a liquid, and rapidly close one of these cavities by depressing its movable wall, the liquid which it contains is urged into the tube, and thus pushes onward the liquid column. This advances by a movement which soon becomes uniform, and is rapidly communicated to the entire mass. In the mean time, the other part of the bellows opens; the liquid which filled the tube in the opposite extreme, advances also, and is easily projected into the dilated cavity. If the walls of the tube were not elastic, the movements would be intermittent, and would cease as soon as the bellows could no longer open; but they become continuous, because the walls are endowed with that property which begins to be exercised precisely at the moment when the bellows shuts, and the action lasts during the whole time that they remain at rest. The function which I have described, by supposing it to take place with a pair of bellows, is that which is performed by the heart. The walls of the left ventricle thus approach each other, contract with a great rapidity, which can be determined when we know the exact duration of the contraction and the quantity of blood expelled. The capacity of the ventricle is thus diminished, and a certain quantity of blood thrown into the aorta, and a movement communicated to it which is propagated throughout the system. At the instant that the arteries dilate, the right ventricle opens, and the blood enters it. The contraction of the left ventricle ceasing, the arteries return to their original condition, and again propel the blood forward.

Pulmonary Circulation.—We have passed over in silence all that which relates to the pulmonary circulation, this being produced by the same causes, and under the same laws as the circulation in the rest of the body.

Conclusion.—Thus, then, by the arrangement of the various parts of the sanguineous system, is solved, by a very simple mechanism, and conformably to the physical laws of the movements of fluids, a very complicated hydraulic problem: namely, that of the continued distribution of the same liquid in a system of tubes, having different diameters in different parts of the body, and, consequently, with very variable velocities, and always in relation to the functions of these parts, and all this by the simple alternating impulsion given to this fluid by the sudden contraction of a species of sac, which makes a part of the tube itself.

LECTURE XVIII.

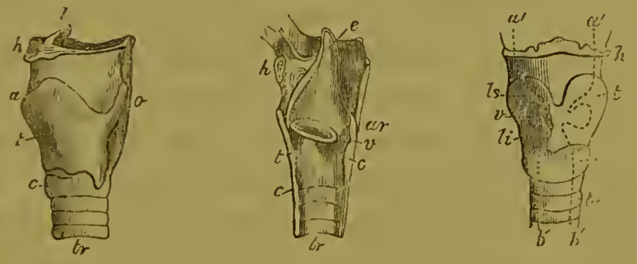
VOCAL APPARATUS. THE VOICE.

Argument. — Description of the human vocal organ. Experiments demonstrative of the seat and mechanism of the voice: structure of the vocal cords. Analogies between the human voice and musical instruments. Description of reed-instruments. The human vocal organ is a reed-instrument with membranous lips. Results of Müller's experiments on the larynx. Qualities of the human voice. The human vocal organ is infinitely superior to any musical instrument. Artificial caoutchouc larynx.

After the lectures on the nervous force and muscular contraction, I must immediately proceed to the consideration of the production of sound in animals; which here, as in all other cases, is caused by a vibratory movement. In the vocal organ this movement is produced by the contraction of muscles, and of parts subjected to their influence, and from them, therefore, the voice takes its origin.

Organ of Voice in Man.—In order that I may be enabled to explain at sufficient length, the theory of the vocal organ of man and animals, I must not pass over in silence the description of its constituent parts. It is easy to prove, experimentally, the position occupied by this apparatus. A very superficial examination shows us that the voice is produced when air is expelled from the lungs; and every one knows that it is impossible to articulate sounds when we close the mouth and nose. It is, therefore, evident, that the vocal organ resides in a certain portion of the tube which proceeds from the bronchi and terminates in the mouth. To determine more accurately its position, we have only to observe that if an opening be accidentally or purposely made in the trachea of a man, below the *larynx*, it is impossible to produce a sound; but, if we close the opening, the power of producing the voice immediately returns. If, however, the opening be made above the larynx, the voice remains as before. In birds the organ of voice is situated at the bifurcation of the trachea; that is to say,

Fig. 20.



Side View. Vertical Section. Front View.

Three Views of the Human Larynx.

a. The *pomum Adami* (projection formed by the thyroid cartilage).
ar. Arytenoid cartilage.
a' a' b' b'. Dotted line indicating the outline of the inner wall of the larynx.
c. Cricoid cartilage,
e. Epiglottis.
h. Os hyoides.
l. Body of os hyoides, which gives attachment to the base of the tongue.
li. Inferior ligaments of the glottis, or vocal cords.
ls. Superior ligaments of the glottis.
o. Posterior side of the larynx in contact with the œsophagus.
t. Thyroid cartilage: it is attached to the os hyoides by membranes.
tr. Trachea.
v. Ventricle of the glottis (formed by the interval between the vocal cords and the superior ligaments of the glottis).

it occupies a lower situation in them than in mammals; and, hence, after cutting off the head of a bird, we may still succeed in obtaining sounds by the compression of the thorax.

We shall describe the vocal organ of man as being the most complicated and the most perfect. The trachea is a species of tube formed of cartilaginous rings, separated from each other by membranous and flexible rings. The lower end of this pipe divides into two branches, which subsequently ramify in the parenchyma of the lungs, somewhat like the branches of a tree. The upper part of the pipe opens in the buccal cavity, and is terminated by the larynx, the true organ of voice. We may regard the larynx as a continuation of the trachea, but with this difference, that the portion of the tube, composing it, is broader, and is attached to the *os hyoides.* It consists of four cartilages; namely, the *cricoid,* the *thyroid,* and the *two arytenoid.* They are of very different shapes, are articulated one to the other, and are united to the upper ring of the trachea. Several muscles are attached to the larynx, and by their contraction the entire larynx, or some of the cartilages composing it, are moved. The mucous membrane lining the larynx, forms, at about the middle of this organ, two large lateral folds directed transversely, and which have the appearance of a button hole: these folds are called the *vocal cords,* or the *inferior ligaments of the glottis.* Above them we find two other folds, analogous to the preceding, and which are termed the *superior ligaments of the glottis.* The cavities formed by this arrangement, between the superior and inferior ligaments, have received the name of *ventricles of the larynx.* The chink directed from before backwards, and comprised between the two vocal cords, is called the *glottis.* Lastly, above this chink, we observe a fibro-cartilaginous tongue-like body, fixed by its base beneath the root of the tongue ; and which in the act of deglutition is inclined downwards, so as to cover the glottis, but during expiration is placed obliquely : this body is the *epiglottis.* The dimensions of the glottis are as follows : its length is from 25 to 30 millimetres ; the space between the two lips, which is extremely small in front, is from 7 to 8 millimetres posteriorly : these lips, moreover, are capable of being brought nearer together, so as to touch. The depth of the ventricle is from 25 to 30 millimetres, and their greatest possible height 15 millimetres. The superior walls of the ventricles are so close together that they form a kind of second glottis, at from 15 to 18 millimetres above the other one.

Seat of the Voice. — The air expelled from the lungs without any extraordinary effort easily traverses the larynx without producing sound. From the time of Galen, an experiment has been known which shows, that to articulate a sound, it is absolutely necessary to contract the muscles of the larynx. This experiment consists in wounding the laryngeal nerves, or in effecting their complete division on both sides of the trachea : n this case, paralysis of the larynx is complete, and the power of forming sounds is entirely destroyed. By another experiment, we can ascertain still more exactly the part of the organ which is the most essential for the production of the voice. If we remove the superior ligaments of the glottis, the voice continues, though it becomes more feeble; but if we divide or injure the inferior ligaments, namely, the vocal cords, the voice is completely destroyed. Müller ascertained that it is easy to obtain sounds from the larynx of the dead human subject, by forcing air through the trachea; provided that the inferior ligaments of the glottis be rendered in some degree tense, and the aperture of the glottis narrowed. According to this learned physiologist, the experiment likewise succeeds when all the parts above the glottis, such as the epiglottis, the superior ligaments, and the ventricles of the larynx, are removed. If the vocal cords alone remain, and the chink between them be narrow, sound is always produced. Magendie laid bare the glottis in living animals, and saw that the vocal cords were thrown into vibration, when cries were uttered. I propose to show you one of Longet's experiments, to prove the part which the vocal cords take in the production of the voice. I have here a rabbit, whose larynx has been laid bare; and you observe that the animal cries whenever we pinch any part of its body. You perceive that, at the same time, the crico-thyroid muscles contract. By the contraction of these muscles the vocal cords are rendered tense, and brought nearer together. After having divided the nerves going to these muscles, their contraction can no longer be effected; and if we then pinch the animal it no longer cries, and we can hear only some very grave and hoarse sounds. We may, therefore, conclude that the glottis constitutes the organ of the voice; that the trachea may be compared to the bellows' pipe of an organ; and that the upper part of the larynx, and all the parts situated above the epiglottis, including the cavities of the mouth and nose, form the upper tube of this instrument, which serves merely to modify the sound, as may be easily shown by effecting some alteration in this cavity.

Vocal Cords. — The vocal cords are formed of that elastic tissue, which is remarkable for its yellow colour, for the arrangement of its fibres, and for forming the middle coat of the arteries, and a great number of ligaments. It is decidedly the most elastic of all the tissues of the human body. The movements of the cartilaginous pieces of the larynx vary the degree of tension of the vocal cords, and the transverse and oblique diameter of the glottis. In general, the latter becomes narrower when it emits sounds. The vocal cords, either as cords or as stretched membranes, attached at one side, evolve sounds when a column of air causes them to vibrate; and these sounds necessarily vary according to the tension and length of the cords, and the force of the current of air. A current of air traversing with a certain rapidity an orifice whose diameter is variable, may also, independently of the elasticity of the lips of the orifice, produce different sounds, as in the so-called wind musical instruments. The tube which rises above the glottis, and which consists of the ventricles, the pharynx, and the mouth, may modify the intensity and even the tone of the sound produced in the glottis. Finally, we may consider the larynx as a cylindrical cavity, having two orifices in the centre of its bases. Assuming that a current of air traverses this cavity with different degrees of rapidity, that the diameters of these orifices are variable, and that the lips of its orifices, which are endowed with a variable tension, are elastic, we can easily conceive that a great variety of sounds may be produced by this apparatus.

Analogy of the Organ of Voice to Musical Instruments. —These considerations explain why the organ of the human voice, and the voice of animals, has been compared at one time to a stringed, at another to a wind instrument; sometimes to a reed instrument; and, lastly, by Savart, to the bird-call. I must pass over in silence the long and minute explanation of the reasons assigned by the various authors in support of their different opinions concerning this organ. I do this the more willingly because I think we shall find, at the end of this lecture, that these theories of the voice are not so opposed to each other as their authors have imagined.

Let us be guided, as usual, by experiment. All parts of the larynx may be removed without destroying the voice, provided that the vocal cords are preserved: these latter, then, are the indispensable elements of the apparatus; and if we consider that they may undergo a more or less considerable degree of tension, by varying the opening left between their edges, we cannot but consider this essential part of the vocal organ as a reed instrument, with a membranous tongue of a peculiar construction. The other parts of the larynx, as well as the entire superior cavity of the mouth, form the pipe of the reed instrument; and the inferior part of the trachea constitutes the ordinary tube which proceeds from the bellows. We are indebted to Weber, but more especially to Müller, for observations best fitted for demonstrating the correctness of this manner of viewing the organ of voice in man. I shall endeavour to give you, as briefly as possible, an analysis of these investigations.

Reed Instruments.—Allow me first, however, to show you some general facts on sounds produced by reeds. When a rectangular metallic plate is fixed by one extremity, on the edges of an opening almost equal in extent to that of the plate, we have then a reed instrument, provided that the piece which contains the opening has a cylindrical or other form, and closes the orifice of a pipe into which a current of air is propelled. The *mouth-harmonicon* is the simplest instrument of this kind. A sound is also produced when a current of air is propelled, by means of a tube, against a small metallic tongue fixed in some way, but not mounted on the chink. Some experiments seem to show that the pitch of the sounds thus produced by these tongues, is independent both of the intensity of the current, and of the chemical nature of the air or gas: these circumstances merely vary the intensity of the sound. Some experiments, again, seem to have' proved that, in order to vary the acuity and the gravity of a sound, the thickness of the tongues requires to be altered. We may explain in two different ways the sounds produced by a metallic tongue: it may be said, that the latter vibrates like an elastic rod, and that these vibrations are the cause of the sound; or rather that the tongue, forced from the opening, in which it is fixed by the current of air, returns to its former position in consequence of its elasticity; and thus, by its position of equilibrium being disturbed, undergoes oscillations which give rise to impulses of the air, as in the *sirène,* or in Savart's machine. It must, however, be observed, that the sounds furnished when the tongue is made to vibrate by striking it, are never so strong or so distinct as those produced by the impulse of a current of air. Hence, therefore, the second explanation of the sound of the tongue appears more probable than the first. It may, indeed, be replied, that by striking the tongue, we merely shake it, but not long enough to produce a uniform and durable vibratory movement. I do not, however, see why we should refuse to admit the simultaneous existence of both these causes of sound; for the tongue would easily, by its transversal vibrations, place itself in unison with the sound produced by the vibrations excited in the air.

When a sounding tube is added to the reed, we have a reed instrument such as is generally met with; and sounds thus obtained are very different, both in tone and intensity, from those produced by the [reed or] tongue alone. The sound is neither that of the reed nor of the tube alone; but both become modified, and accord together. I must here explain to you the results obtained by Weber upon this subject. The pipe added to the reed may render the sound of the latter more grave, but never more acute; and the lowering of the pitch thus produced never exceeds an octave. By lengthening the tube, we may again raise the sound to the fundamental primitive pitch of the reed; and by increasing still farther the length of the tube, the pitch again becomes lowered; but for this purpose it should be shorter than the first time. The length which it is necessary the tube should have, in order to lower the pitch to any given point, constantly depends on the relation which exists between the number of the vibrations which the reed and the tube separately make: thus the sound sinks gradually as the tube is lengthened, until the column of air reaches such a length that it alone would produce the same fundamental sound as the reed itself would give. By again lengthening the tube, the sound sinks to about a fourth, until its length is double that of the column of air, which would give the same sound as the reed. At this point the pitch again rises to the fundamental note of the latter.

Let us now devote our attention to the sounds of membranous reeds, and especially of those which, by their shape, have great analogy with the glottis, and which consist of a lamina of caoutchouc, having an opening in their middle, and which are fixed at the edges of a tube, into which the air is propelled. It is with apparatus of this sort that attempts have been made to construct an artificial larynx. Let us confine ourselves to the determination of the differences which exist between membranous and metallic reeds. Membranous reeds produce sounds which may be greatly modified, by varying their tension. By compressing the membrane, the sound becomes more acute. The principal difference in these two cases consists in this, that membranous reeds produce sounds which are more acute, in proportion as the force of the blast in the tube is greater; whilst the reverse holds good with metallic reeds. Müller has endeavoured to determine what influence is exerted by the *porte-vent* and body of the tube adjusted on the membranous reed. But we must acknowledge, as does the author himself, that this subject has not hitherto been satisfactorily elucidated. The modifications produced in the sounds of the reeds, by the adjustment of a tube or porte-vent, are considerable. Experiments prove that, according to the lengths given to them, the sound is at one time raised, at another lowered. I shall relate to you one of the numerous experiments of Müller: with a tube of 6 inches in length, the fundamental sound was *ré* 4; this became *mi-dièse* 4, when a tube of 4 inches was added below the tongue; and with a tube of 4½ inches it rose to *ré-dièse* 4; with a tube of 8½ inches it fell to *ut-dièse* 4, and rose again to *ré* 4 with a tube of 27½ inches.

The apparatus with membranous reeds constitutes the largest number of wind instruments; and we must include among them the trumpet and French horn.

Indeed, an experienced performer embraces three octaves by merely varying the tension of his lips, without modifying the length of the column of air.

The Human Vocal Organ is a Reed Instrument. — I believe that these introductory observations respecting membranous reeds, will enable you to form a clear notion of the production of the human voice. The organ is essentially a reed-apparatus, formed by two membranous lips.

Müller's Experiments. — The experiment of Müller, which I am about to quote, will render this conclusion evident: He fixed the larynx of a human subject upon a board, after having removed all the parts above the inferior ligaments. Then, by means of a hook, he attached a thin cord to the angle of the thyroid cartilage, immediately above the vocal cords. The string passed over a pulley, and was connected with a scale, loaded with weights. By employing different weights, the cartilage was pulled, and the vocal cords stretched. A wooden tube was introduced into the trachea, to blow through. The following are the principal results which Müller obtained with this apparatus: —

1st. When the glottis was sufficiently narrowed, and the inferior ligaments stretched, he obtained clear and full tones, which approximated to those of the human voice. An artificial larynx, made either with bands of the middle coat of arteries, or with caoutchouc, yields similar results.

2nd. By altering the tension of the vocal cords, the notes rise with the tension to an extent of about two octaves; and when the tension is very considerable, the sounds become disagreeable and whistling.

3rd. Sounds produced when the vocal cords are but slightly stretched, differ in their intensity, but not in their tone, according as the glottis is more or less contracted; when the vocal cords touch, the sound is as strong and as full as it can possibly be.

4th. If the force of the current of air be increased, the tension of the vocal cords remaining equal, the sound rises a fifth, or even more.

5th. The parts of the larynx, and all the rest of the tube situated above the vocal cords, seem to act in the apparatus of the human voice, like tubes adjusted upon the reeds; and we remark in the whole, that possibility of compensation which is always desired in musical instruments, and by means of which the tones remain the same, notwithstanding great differences in the intensity of the cause of sound.

Compensation. — Weber discovered a method of constructing a compensating reed-tube, in such a way that the sound has always the same purity for the *piano* as for the *forte*, notwithstanding the great changes in the force of the blast. The note of a reed-pipe may be raised by increasing the force of the current of air, and may be lowered by means of the tongue. Hence, we may conceive the possibility of compensating these effects by means of a certain length of the column of air.

Qualities of the Voice. — As for the *force* or *strength of the voice*, it evidently depends in part on the aptitude of the vocal cords for vibration ; and, in part, on the fitness of the membranes and cartilages of the larynx, as well as of the pectoral, nasal, and buccal cavities, for resonance. The *peculiar timbre* of voice, which each person possesses, and its imperfections, evidently depend on the differences of these resonances, or on different aptitudes for vibration which the parts of the organ possess. The *intensity* of the voice, or, as it is generally called, the *volume of the voice*, results, in part, from the force with which the air is driven from the lungs, and from the size of the thoracic cavity ; and, in part, from the facility with which the vocal cords and the other parts of the larynx are able to vibrate. These modifications explain the difference which exists between the male and female voice. To the great resounding cavities in communication with the larynx, must be attributed the faculty which several species of monkeys have of giving vent to very shrill and deafening cries.

Superiority of the Human Vocal Organ. — In reflecting upon all that we have said whilst studying the human voice, we cannot withold our feeling of admiration at the wondrous skill displayed in the construction of the organ producing it. No instrument which we possess approaches it in perfection. Some wind instruments can only run the octave, and pass without gradation from one note to another ; while in stringed instruments it is impossible to prolong the sound. The organ with two registers, which is formed both with reed-pipes and mouth-pipes, gives sounds which resemble somewhat those of the human voice ; but these advantages are only obtained by means of a great number of tubes, and much complication. In the vocal organ, on the contrary, this infinite variety of sound is obtained by means of a very simple apparatus.

Artificial Larynx. — I have seen in the museum of King's College, London, a caoutchouc larynx, modelled from a human larynx, to the different parts of which threads are attached, in order to enable its walls to be stretched more or less at pleasure, and to vary the capacity of the tube. By means of a current of air, the force of which can be modified, a certain number of sounds are obtained which, for their timbre and purity, closely resemble those of the human voice.

LECTURE XIX.

HEARING.

ARGUMENT. — Modes of exciting the sensation of sound. Structure of the ear. Propagation of sonorous waves through the organ of hearing. Uses of the various parts of the ear. Physical properties of sound.

A LUMINOUS sensation is the invariable effect of any excitation of the retina or optic nerve; and, in like manner, excitation of the acoustic nerve, however it may be effected, always gives rise to the sensation proper to this nerve. Thus, when speaking of electricity, I told you that a peculiar sound was heard when one of the poles of a voltaic pile was applied to the ear, the circuit being closed. Some substances, principally narcotics, when introduced into the organism, also produce sensations of sound.

In most cases, however, sound is produced by vibratory movements, communicated to elastic bodies, and propagated, by means of the air or other media, to the acoustic nerve. In this way the function of hearing is usually exercised.

In special treatises on Acoustics, experiments are described which tend to show that the cause of sound, the difference between grave and acute tones, and the intensity and timbre of sound, depend on the velocity and amplitude of these vibratory movements. In that department of physical science, you will also find laid down the laws of the propagation of vibrations in air, liquids, and solids. We must, however, presume, that you have already acquired this knowledge; and we purpose devoting the present lecture to the study of the sense of hearing, and more especially of the propagation of the sonorous vibrations through the different parts of the ear.

Structure of the Ear.—But we must not pass over in complete silence the structure of the ear, which varies extremely in different animals. In some it is reduced to an apparatus of the greatest possible simplicity; namely, to a special nerve, whose peripheric extremity is expanded in a liquid contained in a cavity of variable form, situated sometimes in the osseous wall of the skull, sometimes having membraniform envelopes. We shall give a particular description of the human ear, because it is to this that we must devote our especial attention, on account of its complication and perfection.

The exterior part of this organ, called the *pinna* or *auricle* of the ear, is of a fibro-cartilaginous nature, very pliant and elastic, and has the greatest portion of its surface free. It is, in a manner, an expansion of the external auditory passage. The form of the pinna varies greatly in the higher animals: thus, in man, although it presents a number of folds, we may consider it as normally implanted in connection with the auditory tube. In the horse, the ass, &c., it consists of a species of cone, which arises from this tube. In these animals the pinna is generally movable; whilst in man its motions are very limited.

The *external auditory passage,* or *meatus auditorius externus,* excavated in the temporal bone, terminates at a short distance from the surface, where it is truncated obliquely to its axis. A thin, and very elastic membrane, called the *membrane of the tympanum,* closes the passage. The *tympanum* is a cavity, in great part osseous, which has five openings: one formed by the extremity of the external auditory passage, and closed by the membrane of the tympanum; two opposite to this, one situated above and called the *oval window* (fenestra ovalis), and the other the *round window* (fenestra rotunda); both openings are closed by membrane. Superiorly and posteriorly the tympanum communicates by a large irregular opening with the mastoid cells. Lastly, in the lower part of the tympanum there exists a fifth opening, namely, that of the Eustachian tube, which communicates with the upper part of the pharynx.

Within and across the tympanum is fixed a chain of small bones; which, from the analogy of their forms, are called the *hammer* (malleus), the *anvil* (incus), the *orbicular bone* (os orbiculare), and the *stirrup* (stapes). The hammer is attached parallel to the membrane of the tympanum, like a solid ray going from the circumference to the centre of the latter. One extremity ouches the anvil, which is connected with the orbicular bone, and the latter with the stirrup, which terminates at the fenestra ovalis. Several muscles effect slight movements in the chain, shortening and lengthening it, and thus varying the degree of tension of the membranes upon which it presses.

Beyond, or on the inner side of the tympanum, in the substance of the petrous bone, is situated what is called the *labyrinth*, or *internal ear;* formed of different cavities, which communicate with each other, and are distinguished by the names of the *vestibule*, the *semicircular canals*, and the *cochlea*. The vestibule occupies the central part, and communicates with the tympanum by the fenestra ovalis, and with the three semicircular canals, which are expanded at their extremities, forming ampullæ. The cochlea, so called because it is curved spirally, like the shell of a snail, communicates with the interior of the vestibule, and is separated from the tympanum by the fenestra rotunda. The vestibule and the cochlea contain a fluid called the *liquor of Cotunnius*, in which float the filaments of the acoustic nerve. Such is a succinct enumeration of the principal parts of the ear of man, and the higher animals. We shall examine the propagation of the sonorous waves through these parts, in order to deduce therefrom the physical theory of hearing.

Propagation of Sound through the Ear.—The ear is composed of several membranes, of osseous parts, of air, and of a liquid. The vibrations are propagated through these bodies, all of which transmit sound in consequence of the vibratory state excited in them by that of sonorous bodies. Each portion of the ear thus takes part in the function which this organ has to fulfil. But in what manner do the different parts act? Acoustics cannot give a completely satisfactory reply to this question; but by the aid of comparative anatomy, of experimental physiology, and of pathology, we may succeed in determining the different degrees of importance possessed by each part in the function of hearing, and, hence, can determine in what degree they respectively contribute to the perfection and delicacy of the ear. The external and middle parts of the ear are wanting in a great number of animals, which, nevertheless, are considered to be endowed with a perfect sense of hearing. Thus, in birds there is no vestige of the pinna; in reptiles the external auditory passage is wanting; and in fishes the ear is reduced to the internal part alone. But the part which is present in every case, in which hearing can be effected, is the vestibule; in other words, a membranous sac, filled with a liquor in which the extremities or ramifications of the auditory nerve are contained. Any other mode of termination of this nerve would have been less advantageous for the exercise of this faculty. The nerve being reduced to some very small filaments, diffused in a liquid, the points of contact are in this way multiplied as much as possible. The structure of the nerve thus approaches more to that of the liquid in which it is placed. These ramifications, scattered in the fluid mass, are expanded in every direction, and are thus directed normally with their extremities towards the vibratory movements, which are propagated to the fluid by the walls of the spherical cavity in which it is contained. We know that vibrations are propagated in liquids, as in membranes and in all elastic bodies, which divide into vibrating parts separated by nodal lines. In liquids, numerous vibratory movements can likewise be propagated, and co-exist without interfering with one another. Some experiments of Cagnard-Latour, seem also to prove that vibrations are propagated more readily from the solid walls of a cavity to the liquid contained in it, if small solid bodies are dispersed through the latter, either floating or fixed to the walls. The points of contact between the solid and the liquid are thus rendered more numerous, and the directions in which the vibrations can be propagated, in a direct line from one to the other of these media, are multiplied by the inequalities presented by these surfaces. These small solid bodies are met with in the liquid which fills the labyrinth of the ears of certain fishes. We may, therefore, conclude, that the vestibule, or, more particularly the mode in which the acoustic nerve terminates in all animals, is of great importance in the function of hearing, and is explicable by the laws of acoustics.

The undulations of the air, produced by a sonorous body, may excite the auditory nerve, by being transmitted either through the osseous parts of the skull in which the air is contained, or through the column of air contained in the external auditory passage. If we carefully close the external auditory passages, we can still hear very well the ticking of a watch, held between the teeth, or applied to any part of the head. Some persons afflicted with hardness of hearing, manage to hear distinctly by applying to the auditory passage, or by holding between the teeth, a wooden rod fixed to the centre of a reservoir of air, placed in front of the sounding body. The stethoscope employed by physicians acts principally as a solid cylinder, by which a great number of points of contact are established between the sounding body and the ear applied to it.

The column of air contained in the external auditory passage, also vibrates in consequence of the sonorous undulations excited in the air. Experiment proves, that a sound produced in a solid body is much more distinct to our ear if transmitted by means of a solid immediately in contact with the ear, or through the intervention of another similarly interposed body, all other conditions being equal. If, on the contrary, the sound be produced by sonorous undulations primarily excited in the air, as in all wind instruments, it becomes the more distinct in proportion as the quantity of air in communication with the auditory passage, is, within certain limits, more considerable. The ear-trumpet acts on this principle. When these two modes of propagation are combined in the same organ, it cannot be doubted but that they powerfully concur in perfecting the function. These principles are applicable to the explanation of the use of the pinna, and of the external auditory passage. We perceive in these parts, an instrument analogous to an ear trumpet, which, by its peculiar form, collects and reflects towards the axis of the canal, a greater number of sonorous waves; and which, at the same time, acts like a reservoir of air, and increases the sound. All stringed and wind instruments yield a more intense sound by the effect of the presence of this reservoir. Savart's bell, which, after having been put in vibration, is brought near an air receiver, and the monster tuning fork of Marloye, placed on a large air-chest, place beyond doubt the effects produced by the resonance of masses of air which surround sonorous bodies. An analogous experiment can be made by bringing a tuning fork near to the ear, or introducing it within the mouth: in the latter case, the sound acquires a great intensity. In all these instances, the sound is augmented by sonorous waves reflected by the walls of the receivers; and, at the same time, by vibrations communicated to the mass of air, and to the walls of the reservoir. In all cases, however, it is essential that all should vibrate in unison with the primitive note, in order that the sound should be increased.

In animals in which the pinna is movable, the analogy of this part of the ear with the ear-trumpet is very evident. Thus we see the animal, when pursued, direct the opening of the pinna backwards; and, on the contrary, when pursuing its prey, it turns the same part forwards. In man these movements are wanting; and the external form of his ear is also very different from that of other animals. Hence, it is difficult for us to understand the use of the pinna, and we might quote a number of instances in which hearing has been but very slightly affected by its absence. Considering man's usual position, and the mobility and elegance of his head, it does not appear possible to endow the pinna with the power of motion without giving to this organ the shape of an ear-trumpet; and all of you, I suspect, would be shocked at the idea of being thus transformed into a kind of mythological monster. Let us add, that, for the same reasons, the pinna could not be mobile. So far from this, it is formed of an elastic cartilage, the plane of which is for the most part parallel to that of the membrane of the tympanum; and, consequently, normal to the axis of the external auditory passage. According to the laws of the propagation of vibratory movements, this is the best arrangement it could have for receiving the sonorous undulations, which, thus striking this membrane perpendicularly, are propagated more readily into the interior of the ear, either by the solid walls of the auditory passage, or by the column of air which it contains. It is well known that when we wish to hear distinctly, we place the head in such a position that the pinna receives the sonorous waves normally. By the vibrations which sound excites in the external membrane of the ear, or by those produced in the columns of air contained in the auditory passage, the vibratory movement reaches the membrane of the tympanum, which is stretched over the internal orifice of this tube. Why, it may be asked, has the tympanum been added? Why is not the apparatus so arranged that the vestibule or sac, in which the acoustic nerve is contained, should be in contact with the membrane of the tympanum? We have no hesitation in replying that hearing could be effected without this tympanum, as is the case in many species of animals; and, as has been said, with some men, in whom this middle ear has been deficient, either from disease or from natural conformation. It is obvious, however, on physical grounds, that the middle part of the human ear renders this organ more perfect and less exposed to undergo alterations.

Let us, in the first place, speak of the manner in which membranes vibrate. Savart has shown that, when they are properly stretched, or put in proximity with an organ pipe, or any stringed instrument producing a sound, they vibrate as if they were in contact with the sonorous bodies; and if they be covered with sand, we obtain the division of these membranes into vibrating parts, separated by the ordinary nodal lines. At each variation of sound, new arrangements appear upon the membrane, and these divisions are more easily and quickly produced upon membranes than upon plates of metal or glass. Savart has likewise shown, that membranes alone present the peculiarity of dividing in different ways under the influence of the same sound; all of them having the same form, dimension, and tension. Lastly, we may add, that by varying the degree of tension of a membrane, its manner of dividing and vibrating for the same sound, is changed. From all these facts, for which we are indebted to Savart, we may infer that, in order to propagate sound in the interior of the ear, and in order to modify at pleasure the intensity of the vibratory movements, it is useful to close the auditory passage with a tense membrane, and to add to the interior of the organ an apparatus adapted for propagating vibrations to parts in contact with the acoustic nerve, and which, at the same time, can at will vary the tension of the membrane of the tympanum.

Here is a common ear-trumpet, on the aperture of which is fixed a membrane; upon this membrane a small wooden rod is glued, as in the experiments of Savart and Müller. By means of this, the tension of the membrane can be easily altered. If I listen to a sound by applying the trumpet to my ear, I perceive a great difference in its intensity, according as the membrane is rendered more or less tense: when it is very much so, the sound is remarkably weaker than that obtained by relaxing the membrane. In the first case, perhaps, the membrane is more easily divided into vibrating segments, which produce sounds in unison with the primitive note, but not so strong. In the human ear we can vary the tension of the membrane in two different ways: either by the chain of ossicles (the effect produced by the wooden stick in our preceding experiment); or by modifying the density and elasticity of the air contained in the tympanum. This latter method, which is not the natural one, could only be effected by a certain degree of dexterity, and by making a violent effort. It is important, then, to have in the middle ear a free and constant communication between it and the exterior air: this is the office of the Eustachian tube, which opens at one extremity into the tympanum, and at the other into the fauces. In this way the air of the tympanum has a constant degree of moisture, while its elasticity is the same as that of atmospheric air. The physical properties of the membrane of the tympanum, as well as of the membranes of the fenestræ, rotunda, and ovalis, are thus preserved.

If we close the nose and mouth, and then enlarge, as much as possible, the thoracic cavity, we produce a temporary deafness; and the same effect is obtained by means of a strong and sustained expiration. This deafness continues for a few seconds, and is most effectually got rid of by an act of deglutition. In the one case, the air of the tympanum is less dense; in the other, more dense than the exterior air; while the membrane undergoes a different tension in the two cases,—in the one being drawn inwards, in the other being forced outwards. Wollaston, who first observed these facts, remarked that in the first case he was deaf to grave sounds; while, in the other case, he was deaf only to acute ones. Thus he states that he could not hear distinctly the loud noise of a carriage at a certain distance, though he readily heard the noise produced by striking the end of the nail upon a table. I think I have given a sufficiently plausible reason for this difference. An experiment analogous to the preceding one, may be made by means of a trumpet prepared with the membrane fixed over its opening. By rendering this very tense, all loud sounds were indistinctly heard, whilst, on the contrary, the ticking of a watch was rendered stronger. Cases are mentioned of individuals who cannot hear the ordinary voice, and who can only hold conversation in the midst of a great noise. In these cases it must be admitted, that the sensibility for grave and strong sounds may continue. This can be understood only to a certain extent, by assuming that the membrane is deprived of the power of varying the tension under different circumstances.

The internal muscle of the hammer (*malleus*) and that of the stirrup (*stapes*) serve to modify the tension of the membrane of the tympanum, in obedience to the will. Müller assumes that these muscles are excited to contract by a reflex action, as the muscular fibres of the iris are by a strong light. Under the influence of a prolonged and violent noise, the stirrup and the hammer, by contracting their muscles, render tense the membrane upon which they are fixed, and thus give rise to a temporary, and, under the circumstances, a useful deafness.

What is the function of the entire chain of ossicles? Why are there four connected bones together rather than a single one? Why not suppress the foramen rotundum, or the foramen ovale, or even both of them, by applying the vestibule upon the membrane of the tympanum? It is impossible, with our present acoustical knowledge, to answer these questions satisfactorily. The chain of ossicles, besides having the faculty of varying the tension of the membranes upon which its extremities are fixed, also fulfils in the ear the same office as the bridge in stringed instruments, and the cylinder of a drum. If we sprinkle sand on one of the membranes of a drum we observe that the sand vibrates, and forms itself into divisions when we make the other membrane sound; and this propagation varies according to the arrangements of the cylinder. The chain of ossicles is a kind of music-bridge suspended in the tympanum, which not only propagates the vibrations from one wall to the other, but receives also impulses of air upon the various parts of its surface. The membrane of the fenestra ovalis, which communicates with the vestibule, where the nerve is, has its elasticity and tension better protected by being placed in an air cavity contained in the interior of the ear, than it would be if it were in direct contact with the atmosphere. It is, undoubtedly, for the purpose of giving to the ear a more varied and extended mode of action, that it is provided with two openings, supplied with stretched membranes in the internal part of the ear, and in proximity with the acoustic nerve; one of the openings being free, and the other in contact with the membrane of the tympanum, through the intermedium of the chain of ossicles, and thus rendered susceptible of different degrees of tension. We must thereby obtain a greater compass in the function of the ear. These replies to the questions which we put, doubtless, are not the only ones that we may one day be able to give; but since the organ can exist and perform its function without this chain of ossicles, and without either the membrane or the tympanum, we must admit that these parts are not essential to the ear, and, that they serve only to render it more perfect, and for its conservation.

I shall make but a few remarks on the semicircular canals and the cochlea. It is generally supposed, that the vibration excited and transmitted through the solid walls, in which the ear is contained, are transmitted by these walls to the acoustic nerve.

I shall rapidly pass over the physical characters of sound, the comparison of different sounds, and the limits of appreciable sounds. Any impulse or excitation communicated to the acoustic nerve, as mentioned at the commencement of this lecture, produces a sonorous sensation. By the word *sound,* we strictly understand a sensation which is preserved uniform for a certain time, and which is susceptible of being measured and compared. Sound differs, then, from a mere *noise,* inasmuch as the latter is the effect of a single shock, or of a series of shocks which are repeated without any regularity; whilst the sonorous sensation is that which we experience when the acoustic nerve receives a certain number of successive shakings, separated from each other by a certain and constant interval of time. It is this which takes place with Savart's wheel, with Cagnard Latour's sirène, or by vibrations of a stretched cord, producing corresponding undulations in the air, which reach the ear and strike the acoustic nerve, producing a number of impulses in a given time which belong to the movements of the sonorous body. The acuity or gravity of a sound, is more or less due to the great rapidity with which the vibrations succeed each other. The intensity depends on the extent of excursions of the vibrating parts.

Wollaston and Savart have endeavoured to determine the limits within which sounds remain perceptible, or cease to be so, to our ear, by acquiring too great an acuity or gravity. Savart showed, that these limits were much more considerable than had been previously supposed; and that, in order to perceive, either very acute or very grave sounds, it is requisite merely to increase the intensity. By making a long bar of iron pass, with a certain rapidity, through a longitudinal chink, which it almost completely filled, we obtain a very intense sound when this bar goes and comes seven or eight times in a minute; and, as at each passage of the bar there is a compression of the air, to which a rarefaction succeeds, the undulations which constitute the sound are only to the extent of forty or fifty per second. If, on the contrary, we employ a toothed wheel of very large diameter, and hold an elastic plate in contact with the teeth when the wheel is rotated, we perceive a very acute sound when it has twenty-four thousand impulses per second, in which case the sound is formed of forty-eight thousand undulations. How complicated must be the organ of hearing, when we reflect that its sensibility is preserved within limits so far apart, and that its principal parts must vibrate in unison with sounds which vary from fourteen to forty-eight thousand vibrations per second.

According to the definition given of sound, we explain without difficulty how it happens that, by the coexistence of two sounds, whose vibrations stand to each other in a simple ratio, we hear a graver sound. When this takes place, there are some moments in which the shakings, produced by the two sounds, coincide upon the acoustic nerve; and, if these coincidences be sufficiently near and regularly repeated, we have the sensation of a very grave sound. When these coincidences are very rare, which happens with sounds almost in unison with each other, we have no other sensation than that of the well-known phenomenon of *beats*, observed for the first time by Tartini.

Not being able to speak of all that relates to the act of conscience, awakened through the excitation of the acoustic nerve, I cannot stop at the theories of music. If the sounds that we hear simultaneously are, by the relative number of their vibrations, in simple ratio to each other, we then experience those most agreeable sensations which we call *harmonics;* and the contrary effect takes place when these relations do not exist. Experiment teaches us, that *harmonic sounds* are obtained simultaneously by touching a thick cord, extended so as to make it render the fundamental sound due to the vibration of its entire length. We therefore conclude, that the cord divides itself into a certain number of parts, which vibrate separately and at the same time. We also know, that by having several cords together, if their lengths are in simple ratios, it is sufficient to cause one to vibrate, in order that the others should render the sound proper to their lengths. We may therefore assume, that the membrane of the tympanum, the membrane of the fenestra ovalis, and perhaps, also, the extremities of the acoustic nerve, may be the seat of harmonic sounds; and, that the elasticity of these parts is not opposed to these movements. The contrary should hold good for sounds which we call discords.

LECTURE XX.

VISION.

ARGUMENT. — Two parts or stages in the process of sensation, viz. the action of external agents, and the perception of impressions.

Vision is effected by means of the physical agent called light. Necessity for an optical apparatus to form images of exterior objects. Three modes of forming images: the camera obscura; the mosaic dioptric instrument; and the concentrating dioptric instrument.

Structure of the human eye: its membranes and humours; dimensions of its various parts.

Mechanism of vision. Action of the eye on the rays of light. Image formed on the retina. Adaptation of the eye to vision at different distances: hypotheses to account for it: Sturm's explanation. Presbyopia and Myopia. Achromatism of the eye. Cause of erect vision with inverted images. Idea of distance and size of objects. Single vision. Wheatstone's observations on binocular vision: his stereoscope. Duration of impressions on the retina. Ocular spectra: accidental colours.

Two Stages in Sensation.—THE function of every apparatus of sensation is composed of two distinct parts :

1st, external objects produce in the sensorial nerves a modification which is peculiar and specific for each sense;

2dly, this modification is transmitted to the brain, where the impression is received and transformed into a perception of the external object.

Causes of Vision.—In vision, and in hearing, our relations to external objects are the same. When the sun rises above the horizon and we *see* it, there necessarily exists some sensible relation between that luminary and our eyes. It must be by the physical agent called *light*, that the sun produces an impression on our eyes. In this lecture we shall examine the way in which the rays of light, emanating from external objects, reach the retina and excite it.

If, in the dark, you press or strike the eye, a vivid, but indistinct luminous sensation is produced. If the pressure be made by means of a small body on a limited extent of the eye, the sensation will be equally limited, and you will be conscious of its limit; that is, of the degree of pressure exercised upon the compressed point.

Optical Apparatus required to form Images.—If the surface of the retina were presented naked to the luminous object, without any optical apparatus being placed in front of it, it is obvious that every point of it would be stimulated at the same time, by all the rays which proceed from the object in every direction. And if thousands of those rays, with their divers colours, simultaneously presented themselves before the eyes, they would all at the same time give rise to the impression of their light upon the retina, which, however, would not have an exact and definite sensation of any of them.

The problem to solve to obtain vision, consisted, therefore, in placing before the retina an optical apparatus by which the rays of light, emanating from the various parts of an object, should separately reach distinct parts of the retina, and in a given order.

1. *The Camera Obscura.*—The camera obscura is the most simple apparatus which we possess for obtaining these effects. Fancy a diaphragm with a small aperture in its centre, placed before the retina: the rays proceeding from one extremity of the object, passing through the aperture, will excite a certain point of the retina; and the same will take place with all the other parts of the same object, which will excite a corresponding number of parts of the retina. The smaller the aperture, the more defined will be the image, but its light will be proportionately fainter: this, perhaps, is the reason that, in nature there is no apparatus for vision analogous to the camera obscura.

2. *The Mosaic Dioptric Instrument.*—Müller has described a very curious arrangement found in the eyes of some insects. Imagine that in front of the retina, and perpendicularly to its surface, there is placed an immense number of small cones filled with a transparent matter, and whose sides are invested by a black and opaque pigment, capable of absorbing all the rays which do not traverse the cone in a direction parallel to its axis. A transparent and convex membrane forms the external surface, which is also the base of the cone. In the summit or apex of the cone is fixed the extremity of the nervous fibre, which, according to Wagner, is prolonged into the interior of the cones, on whose sides it is expanded. It is easy to conceive how distinct vision may be obtained by means of this arrangement. Of all the luminous rays which emanate in every direction from an object, and fall upon every point of the surface of the eye, those

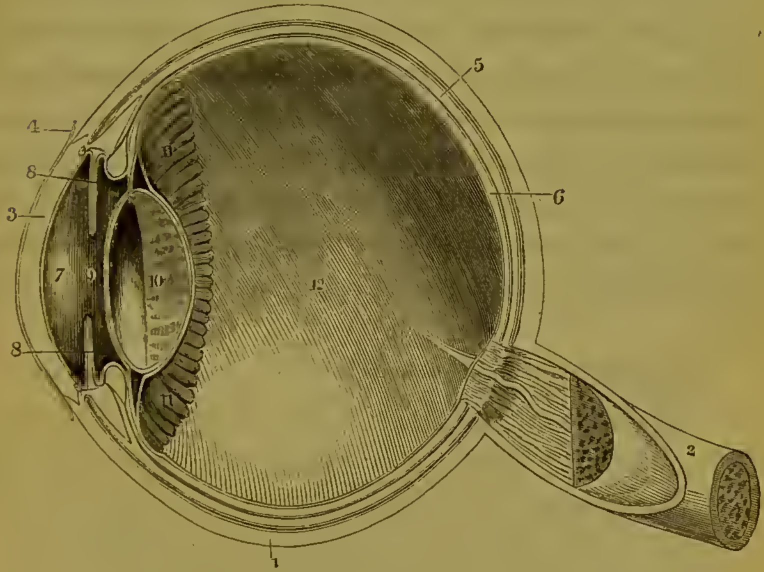
only which proceed from a determinate point of the object, and which traverse one of the cones parallel to its axis, can reach the retina. The distinctness of the image will depend, then, on the number of cones arranged on the nervous surface; whilst the intensity of the image must always diminish with their number. We can also understand how, with this apparatus, the extent of the field of vision may be increased, by augmenting the convexity of the spherical segment representing the eye.

3. *The Concentrating Dioptric Instrument.*—Lastly, to obtain an apparatus of vision still more perfect than the one just described, it was necessary so to construct it that the distinctness of the image should not be obtained at the expense of its brightness; and, that rays which fall very obliquely on the surface of the eye should be made to converge upon the retina. Such is the construction of the eye of man, and of all the higher animals.

Structure of the Human Eye.—Let us now describe the form of the eye, and of the parts composing it. This organ is contained in a cavity called the *orbit.* Its form, which is almost spherical, is preserved by an exterior coat formed of a fibrous dense membrane, which, at its posterior part, is opaque, and is called the *sclerotic coat,* or the *opaque cornea;* but at its anterior part is transparent, has an augmented curvature, and takes the name of the *transparent cornea.*

In the circle formed by the junction of the transparent with the opaque cornea, is extended and fixed an-opaque membrane called the *iris*, which gives the colour to the eye, and is composed of muscular fibres, one portion of which is circular, while the other radiates from the centre to the circumference. In its centre, the iris is pierced by a circular opening called the *pupil*, whose diameter is variable according to the intensity of the light. Behind the iris is placed the *crystalline humour* or *lens*, a solid, lenticular, transparent body, invested by its own proper membrane. On the internal

Fig. 21.



Horizontal Section of the Right Human Eye.

1. Sclerotica.
2. Optic nerve.
3. Cornea.
4. Conjunctiva, extending over the cornea, and detached to line the eyelids.
5. Choroid.
6. Retina.
7. Aqueous humour.
8. Iris.
9. Pupil.
10. Crystalline lens.
11. Ciliary processes.
12. Vitreous humour.

surface of the sclerotic, is a dark membrane called the *choroid:* this is lined by another, termed the *retina,* which is semi-transparent and thin, and is formed by the expansion of the medullary portion of the optic nerve placed at the bottom of the orbit. The eye is divided by the crystalline lens into two distinct portions. The anterior one, in which the iris floats, is filled with a liquid called *aqueous humour,* which is very similar to water, and contains traces of common salt. The posterior part contains a much denser liquid termed the *vitreous humour.* When carefully examined, the crystalline is found to consist of concentric layers, whose consistence and refracting power increase from the circumference to the centre. Lastly, the line, according to which the axis of the figure of the eye is directed, in the act of distinct vision, is called the *optic axis.* The following are the mean dimensions of the different parts of the human eye: —

	Millimetres.
Radius of curvature of the sclerotica	10 to 11
Radius of curvature of the transparent cornea	7 — 8
Diameter of the iris	11 — 12
Diameter of the pupil	3 — 7
Thickness of the transparent cornea	1
Distance of the pupil from the cornea	2
Anterior radius of the crystalline lens	7 — 8
Posterior radius of the crystalline lens	5 — 6
Diameter of the crystalline lens	10
Thickness of the crystalline lens	5
Length of the optic axis	22 — 24

The numbers which represent the indices of refraction for the media of the eye, are as follows: the index of refraction of the aqueous humour differs but little from that of water, which is 1·336; that of the aqueous humour is 1·337; of the vitreous humour 1·3394; of the surface of the crystalline lens 1·3767; of the centre 1·399; and the mean, 1·3839.

Eyes of other Animals.—Some differences are observed between the eyes of animals and those of man. In some birds, the crystalline is almost spherical; and, in all, the transparent cornea is very convex. In fishes, on the contrary, the cornea is almost plane. The choroid, also, presents very different colours in different animals.

Action of the Eye on Light.—Although the description of the eye, which I have here given you, is as summary as possible, yet, by its aid, I trust that you will easily comprehend, in a general way, the progress of the rays of light through the eye, by considering that this organ is constituted by a system of spherical lenses, which produce the convergence of the luminous rays.

Fig. 22.

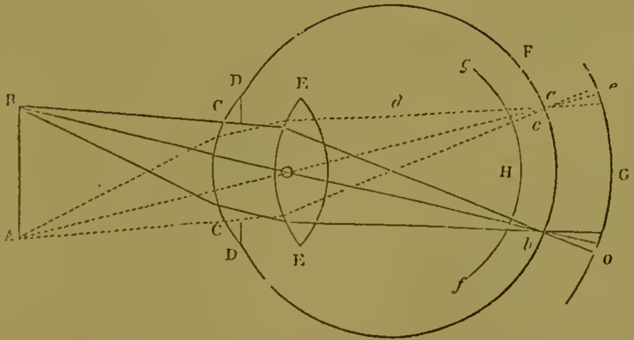


Diagram explanatory of the Mechanism of Vision.

Most of the rays of the luminous cones emitted by the points A and B of the object, traverse the transparent cornea C C, and enter the aqueous humour contained between the cornea and the surface of the crystalline lens: they thus undergo a first refraction, bending towards the rays which enter parallel to the axis of the eye. It is easy to calculate the convergence of these rays, when the convexity of the cornea, and the refracting power of the aqueous humour, have been ascertained. These rays arrive at the crystalline, which is a real double convex lens, and suffer here a further deviation and inclination towards the axis of the eye. Lastly, they undergo a third refraction in the same direction, when passing out of the crystalline lens, and at the moment of their entrance into the vitreous humour. The route followed by the rays emanating from A and B is indicated in figure 22.: their focus is at *a* and *b*: and, if F be the retina, *a* and *b* correspond to the points A and B of the object. If we suppose the retina at H or at G, the points *c* and *f*, as well as *e* and *o*, will be the circles in which the image of A and B will be diffused. Vision, therefore, is distinct when the retina is exactly at such a distance from the crystalline lens, that the focus of the rays is formed upon it. But in order to obtain this result, it is necessary to intercept all the rays which would fall near the margin of the crystalline, and which would have their focus at a different point from that of the rays which traverse the central part of the lens. This is the most important function of the iris and the pupil, which produce the effect of a diaphragm, provided with an opening susceptible of great variations in its diameter. For the same purpose, the crystalline should be denser and more convex, in proportion as the medium in which vision is effected is denser; as is the case with fishes.

Lastly, the whole internal part of the eye, and especially the posterior surface of the retina, is covered with a black pigment, which absorbs all the rays, which would otherwise be again reflected within, and thus disturb the distinctness of the image. All optical instruments have this arrangement: thus, the tubes of telescopes and of microscopes are blackened in the interior.

Seat of the Image formed.—A very simple experiment serves to show that images are formed at the bottom of the eye, upon the retina. It consists in placing, in a dark room, the eye of an Albino rabbit before the flame of a candle placed at a proper distance: as the sclerotica in these eyes is semi-transparent, we can distinctly see the image of the flame inverted upon this membrane. Here is the eye of an ox, whose sclerotica has been pared and rendered almost transparent: each of you can see the image of the flame, which I hold before it, inverted upon this membrane. If we calculate by means of the formula for convex lenses, taking into consideration the dimensions and refracting powers of the different parts of the eye, we find that if an object be placed at about 30 centimetres [11·8 English inches] from the eye, its rays at this distance fall upon the eye with the necessary degree of divergence for them to converge to a focus upon the retina. From all this, therefore, it is natural to conclude that vision, that is, the sensation of a body which transmits luminous rays to our eye, is owing to the modification effected in the retina by the luminous rays brought together upon all the points of this membrane, where the image of the body is formed; and to the transmission of this modification to the seat of perception, by means of the optic nerve. In whatever way the retina is excited, the sensation experienced is always that of light: thus the passage of electricity, a blow, and pressure of the eye, and, consequently, that of the retina, produce luminous impressions there; so that the nerves of the senses, when excited, are each susceptible of one determinate kind of sensation only. The retina, upon which images of luminous objects are formed, is less energetically affected by points whose light is less intense; is more affected by the more illuminated points, and is unaffected by the dark points. If images were not formed upon the retina, and if the eye was composed of this membrane only, without the apparatus of lenses, vision could not be distinct: all would be reduced to distinguishing the alternatives of day and night, of light and darkness. But by means of this apparatus, the action of light is limited to a certain portion of the retina, which exactly represents by its form that of the luminous object. It is, then, an essential condition of vision, that the image should be formed upon the retina, and that the focus of the luminous rays should be found upon this membrane. I ought also to add, that it has been proved by a curious experiment, for which we are indebted to Mariotte, that vision does not take place with equal distinctness when the image is formed upon different parts of the retina. If we place upon a horizontal black plane [as a sheet of black paper], and on the same line three small white discs [wafers for example], 5 or 6 centimetres [about 2 inches] apart, and look at them vertically, in such a position that the eye is distant from them 12 or 15 centimetres [about 5 or 6 inches], and that the nose of the observer is vertical to the middle disc: then, by shutting one eye, and looking with the other at one of the lateral discs, the disc placed under the open eye ceases to be visible. This disc becomes visible by varying the distance at which it was at first placed; and when this has taken place, if we shut the eye which had previously been opened, and open that which had been closed, and fix it on the middle disc, we shall no longer see that which is vertical to it. The point of the retina on which is formed the image of the disc, which becomes invisible, corresponds, in these different positions, to the base of the optic nerve.

Adaptation of the Eye to different Distances.—It is, then, clearly proved, that for vision to be perfectly distinct, our eye should be placed in such a manner that the image should be formed upon the sensible points of the retina, in the smallest possible dimensions, and with sufficient intensity.

This being admitted, let us now see how these conditions can be constantly fulfilled, the distance at which we can see objects being variable. We perceive a star as distinctly as an object placed but a few centimetres off: all that is required for the object to be distinctly seen is, that its size, and, consequently, the intensity of its light, should increase in proportion to its distance. The image of a luminous object recedes further from, or advances nearer to, a lens, according as the body is brought nearer to, or is carried further away from, the opposite side of the lens. It is, therefore, certain, that the eye, by an act of volition, becomes so modified as to see at different distances. In fact, if we look at a body, a black spot for instance, made upon a glass, placing it at different distances from the eye, we shall have a confused image of the objects more or less distant from the spot, whilst the latter will be very distinctly seen in every position, and however remote. With a healthy eye, vision is effected without either effort or fatigue at a distance of about 30 centimetres [=11·8 inches], but not for greater or less distances. This is the reason why it is no longer distinct after the eye has been looking at an object very near to it for several hours.

In order to explain the property which this organ possesses, of seeing objects placed at variable distances, we must adopt one or the other of the following two hypotheses: either we must assume that the retina communicates to the brain the distinct sensation of a luminous object, not only when its rays are collected into a single one, as when they reach it from a distance of about 30 centimetres [=11·8 inches], but also when they are collected in a small, very limited, circular space; or we must suppose, that the curvature of the transparent cornea and of the crystalline lens, can be varied for different distances, and that the crystalline lens can change its position; that is to say, augment or shorten its distance from the retina in different cases. According to the calculations of Olbers, it would be requisite, in order to have equally distinct vision at very different distances, namely, from 4 inches to an infinite distance, that the interval between the crystalline lens and the retina could vary at least $\frac{1}{10}$th of an inch, supposing the convexity of the cornea and of the crystalline lens to remain constant. The same result would be obtained, if we were to suppose that the convexity of the crystalline lens and the cornea varied, while the distance of the crystalline from the retina remained the same. Olbers also found, that vision would be distinct within the limits mentioned, if the radius of the cornea were capable of change, to the extent of about the $\frac{3}{10}$ths of an inch.

An experiment made by Scheiner shows, that there are cases in which the image of an object appears to the same eye sometimes double, at others single. If, by means of a needle, we make in a piece of paper two holes, at a less distance from each other than the diameter of the pupil, and if we look through them with one eye only, at a certain distance the object will appear single; but at a greater or less distance than this, it will appear double. If we close one of the holes, one of the two images will disappear. In the first instance, the two luminous fasciculi evidently meet upon the retina; while, in the two other cases, the retina is more or less removed from their point of intersection. By looking at the object directly, vision would be distinct at these different distances; the eye, therefore, must undergo some modification in order to bring about this result.

We ought, then, to be able to find in the intimate structure of the eye, some peculiarities calculated to explain the faculty we possess of seeing distinctly at every distance. The different media of the eye have been long compared to an apparatus composed of lenses terminated by regular surfaces, and having all their axes on the same line represented by the axes of the eye itself. On this hypothesis, all the luminous rays emanating from any point of an object ought to be concentrated at a single point called the focus; and in order that the vision might be distinct, it would be necessary that the retina should be so placed that the different foci, corresponding to the different points of an exterior object, should be formed at its surface. But as the place at which the image is formed by refraction, approaches or recedes from the refracting apparatus, just as the object itself recedes from or approaches it, we ought to find in the structure of the eye some means of remedying this shifting of the image, by which the distinctness of the impression produced upon the retina itself may be maintained. For this purpose several hypotheses have been advanced.

1st. It has been thought that the transparent cornea might vary its curvature so as to remedy this change of place of the image. But observation has proved that this curvature was invariable.

2dly. Some physiologists fancied that the crystalline lens had the power of contracting, and that the curvatures of its two surfaces could change, so as to keep the image constantly upon the retina. Everything proves that this is pure hypothesis.

3dly. Setting out from this fact, that the pupil dilates when the object recedes, and proportionately contracts as the object approaches, natural philosophers have supposed that the vision of distant objects was effected by means of the rays traversing the less refracting marginal portion of the crystalline; whilst the act of vision at short distances was performed exclusively by means of the rays passing through the more refracting layers of the centre of the crystalline lens. In this manner the image would be always distinctly formed at the surface of the retina. It is needless to show here, that this explanation is at least very far from being complete.

4thly. Some physiologists have had recourse to a change of place of the crystalline in the interior of the globe of the eye, in order to explain vision at every distance. But there is no evidence that such a movement of the lens is effected, and it is difficult to conceive the possibility of it.

5thly. Finally, it has been thought that the contraction of the muscles of the eye, and the consequent pressure on this organ, were sufficient to lengthen or shorten its axis at will; and, consequently, to change the position of the retina, and incessantly restore it to a suitable position for receiving the distinct image of the external object. This, also, is a pure hypothesis, which has nothing to support it.

The distinctness of vision at every distance remained, then, inexplicable, and seemed to have escaped all the researches of physiologists and philosophers, when Sturm placed the question on its true ground, and clearly showed why all previous attempts to explain it had failed.

According to Chossat's measurement of the eye of an ox, the anterior surface of the cornea is a segment of an ellipsoid of revolution about the major axis of the ellipse, which represents the horizontal section of this cornea; and the faces of the crystalline are segments of ellipsoids of revolution about the lesser axes of their generating ellipses; the axes of these two ellipses not exactly coinciding in length. Moreover, the axes of these three generating ellipses of the surfaces of the refracting media of the eye, coincide neither with the axis of the eye nor with each other.

It follows, then, that in place of comparing the optical apparatus of the eye to a system of spherical lenses, whose axes were blended, we ought, according to Sturm, to consider the organ as "composed of several refracting media, separated by surfaces which are neither exactly spherical, nor even of revolution or symmetrical about a common axis."

Sturm, studying the problem in all its generalities, has shown that, with a like composition of the eye, the fasciculus of luminous rays, transmitted to the cornea by a point placed on the prolongation of the axis of the eye, could not be so refracted that all the rays could converge towards a single focus; but the following is what happens:—

Fig. 23.

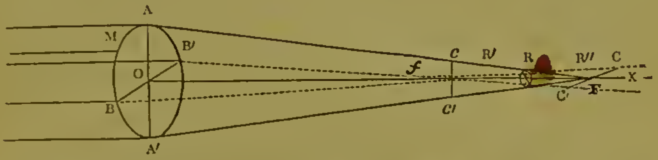


Diagram Explanatory of Sturm's Hypothesis of Vision at different Distances.

Let the circle A B A′ B′ represent the aperture of the pupil, and O X the axis of the eye: then suppose that a fasciculus of rays, parallel to the axis, falls upon the cornea.

It will have there two planes, A O A′ and B O B′, perpendicular to each other, so that all the luminous rays contained in the plane A O A′, will converge towards the axis in one focus only, F, and all the rays contained in the plane B O B′, will be concentrated towards the axis in one point *f*. Let us call the distance F*f*, the *focal interval*.

At the point F draw a perpendicular, C C′, at the axis, to the rays B*f* and B′*f* produced. At the point *f*, also draw a perpendicular *cc′* at the axis, to the extreme reflected rays A F and A′F.

If, now, we consider a luminous ray of light traversing the pupil at any point M, situated beyond the planes A O A′, B O B′, this ray will no longer meet the axis of the eye, but will be refracted so as to rest at the same time upon the line cfc' and upon the line CFC'. Hence, it follows that:

The luminous fasciculus which falls on the surface of the cornea parallel to the axis will be so refracted, that in the whole extent of the focal interval, fF, it will form a very narrow and very concentrated fasciculus, surrounding the axis on all sides, and terminating very near it by a twisted surface (*surface gauche*).

It is within the focal interval, between the points f and F, at the point R, for instance, that the retina is placed. Hence the refracted fasciculus depicts, on the surface of the retina, a very narrow elliptic surface around the axis, and on which all the rays which have traversed the opening of the pupil meet.

It follows, then, that a luminous point, placed before the eye, does not meet upon the retina at a single point, but upon a very small surface, proceeding from the meeting of the retina and of the fasciculus concentrated about the axis in the focal interval fF.

Let us suppose, now, that the exterior point recedes from or approaches the eye, the entire focal interval fF will at the same time change its place, so that the retina which at first was at R will be at R″, or at R′, being always contained between the points f and F. Hence, it follows, that this retina will be always met by the concentrated fasciculus around the axis in the focal interval, and that the surface of intersection of this fasciculus, and of the nervous membrane, will be very slightly modified, in order that the impression may not be sensibly altered, and that the perception may preserve all its distinctness.

That which we have said of an isolated luminous point, is applicable to each of the points of the illuminated object, placed before the eye; and it is easy to understand how this new theory of the path of the luminous rays through the refracting media of this organ, accounts for such an important fact, and one which seemed difficult to explain, namely, that of the *distinctness of vision at every distance.*

Long-sightedness.—Presbytæ, or long-sighted persons, see objects distinctly at the distance of two or three feet. In their eyes the cornea is less convex than that in a perfect eye; and, in fact, this defect of sight comes on with old age, and follows the general diminution of the secretions of all the tissues. By this flattening of the cornea, the focal interval of the rays, which emanate from the point of distinct vision of the sound eye, is thrown behind the retina, and, therefore, long-sighted persons are under the necessity of increasing the distance of an object, in order that its image may be formed on this membrane. Those who suffer from this defect, usually have the pupil but little dilated, as if a continual effort was made to use the centre only of the crystalline lens, namely, the most refracting portion. To correct this defect, they are obliged to employ convex lenses, which diminish the divergence of the rays before they enter the eye. By this means the rays, emanating from an object placed at the point of ordinary vision, are bent by these lenses and brought into the direction they would have if the object were situated at the distance at which a long-sighted person saw distinctly.

Short-sightedness. — The other defect of the eye, *myopia,* or short-sightedness, arises from an opposite cause; namely, a too great curvature of the transparent cornea. In this case, the rays proceeding from the point where ordinary vision occurs, form their focal interval in front of the retina; and, hence, short-sighted persons employ diverging or concave eye-glasses. These lenses increase the divergence of the rays before they enter the eye; and, in consequence, an object placed at the distance of normal vision, is seen under the divergence which it would have for a short-sighted person, if it were brought near to the eye. Convergent and divergent meniscuses, or the *periscopic* lenses of Wollaston, correct these defects more effectually than ordinary lenses. The thickness of these meniscuses being necessarily less than that of the eye-glasses commonly used, they absorb a smaller quantity of light, and the objects preserve more distinctness.

Achromatism of the Eye. — The achromatism of the eye, which is perfect for objects situated at the distance of distinct vision, is owing to the circumstance that the fasciculus which meets the retina, within the focal interval, being contracted around the axis, contains rays of every colour in a space too narrow for the coloured bands to be distinctly formed.

We know, indeed, that if the spaces which separate images of different colours or intensity upon the retina are very small, these images cannot be separately perceived; the sensation which we experience being the result of the simultaneous impression of neighbouring images.

If, then, we cannot point out exactly the cause of the achromatism of the eye, we cannot, on the other hand, deny that there is in the structure of its lenticular apparatus, that variety of curvatures, and of refractive and dispersive power of the media, which are the general conditions observed in the structure of achromatic optical apparatus.

How do we appreciate the position, the distance, and the size,—in a word, the mode of existence of an object, and its relations to surrounding bodies? What is the office of the two eyes?

What we have hitherto said, has been for the purpose of proving that the image of an object is formed upon the retina, and that it is distinct, but is inverted, as regards the object itself; and that this double effect is produced whatever may be the distance between the object and the eye. Nevertheless, this image is not yet the sensation; this only takes place when the modification experienced by the retina, has been transmitted to the seat of perception by means of the optic nerve.

Cause of erect Vision.—But how does vision result from this modification made upon the retina by the rays which external objects transmit there? The first question that presents itself to our notice, without occupying ourselves with the metaphysical part of the subject, is, that of the position of objects. It has been repeatedly said, by way of explaining the inversion of the images as regards the objects they represent, that we see the produced images in an inverted position. To see objects in the inverted position of their images, is what we call seeing objects erect. In appreciating the position of bodies and their erection, we merely compare the position of their different parts with that of surrounding bodies. Without this, the words *reversed* and *erected* as applied to objects, would be devoid of all meaning. To us, a man is in the upright position when his feet are towards the earth, and his head in the part most distant from it; and the reversed image which he forms upon the retina, in no way deranges the respective position of the various parts of the man in regard to the earth. In the image, the feet are equally nearer the earth than the head. If an object presents itself to us really reversed, relatively to the position in which we are accustomed to see it, we consider that it has acquired the reversed position because its image upon the retina is equally so, by comparison, with that which we ourselves hold, and to that in which we usually see it. We know that a man, and that each of us, has his, or our, feet upon the ground: but when we see in the image formed upon the retina by a dancer, that his head touches the ground, we know that we see it in a reversed position.

Judgment of Distance and Size.—We judge of the distance and magnitude of objects in many ways: if they were placed constantly at one distance, and always equally illuminated, we should be able to measure their size by that of the image painted on the retina. The dimension of this image is, in general, proportional to the visual angle made by two right lines drawn from the extremities of the object to the centre of the retina: this we call the *apparent size.* In judging of the distance of objects, we have perception, 1st, of the movements which the eye makes in order that the luminous cone, which the object sends to the pupil, and which is more or less divergent according to the distance, may form its focus upon the retina; 2ndly. Of the movements by which we bring the optic axes of the two eyes more or less near one another, in order to make them converge upon an object placed at different distances. But this latter means of appreciating distances can no longer serve us when the bodies are at great distances, for then the two axes become almost parallel, and we are subject to optical illusions. Thus, the two rows of trees of a long avenue appear to approach nearer to each other in proportion as they are farther off; and the lateral walls of a long gallery, also, present this appearance.

The intensity of the light which we receive from an object, and which we know diminishes with the distance, assists us also in judging of the distance; but such a judgment is rendered uncertain by atmospheric variations, which continually modify the quantity of light received by the object. Lastly, in forming an opinion respecting the real magnitude of objects which are more or less distant from us, we depend partly on our estimate of their distance, and partly on their apparent magnitude, which is measured by the size of the images produced on the retina. The errors that we frequently commit when estimating distance, are one source of the illusions in judging of real magnitude: these are most frequently made in the dark, as in the phantasmagoria.

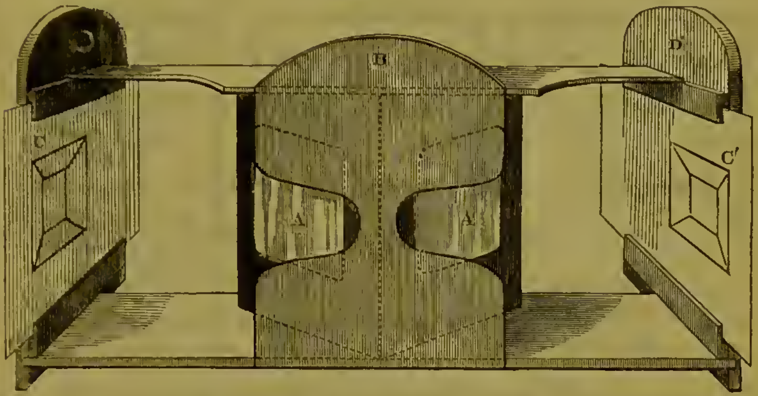
Single Vision.—What is the use of the two eyes in the act of vision? Whilst the object is situated very far from us, the images formed in the two eyes are identical, and vision is effected as with one eye, the optical axes being then sensibly parallel. The single impression produced by a body seen with two eyes is, in this case, the result of an intellectual act, which, from custom, we execute with an inconceivable rapidity. We do not perceive two objects, although the image be double, because experience has taught us that this object is single in every case where two identical representations are produced upon two parts of the retina, which are necessarily correspondent, in order that vision may be distinct. It is the same with the organ of touch. If we place all the fingers of one hand on a ball, we do not experience the sensation of five balls, but only of one. If we look at a body with two eyes, and then compress the globe of one of them so as to change the position of the image on the retina, and alter the axis of one of the two eyes, the body instantly appears double. This is the cause of *strabismus* or *squinting*. An analogous phenomenon is also produced with the sense of touch. To effect this, cross the fore and middle finger of one hand, and with their extremities touch a ball: you will experience an illusion, and fancy that you touch two different balls instead of one.

Wollaston supposed that the unity of vision was attributable to an anatomical cause. This philosopher thought that the two optic nerves, at the point where they unite, in passing out from the brain, and afterwards separate and proceed to the eyes, divide in such a way that each nerve forms half of both retinæ. From this semi-decussation of the optic nerves, it happens that the right side of both retinæ is formed by the divison of one nerve, and the left side, by the division of the other nerve; so that all images of objects out of the optic axis, are perceived by one and the same nerve in both eyes, and the two excited nerves, consequently, furnish a single and complete image. By this anatomical arrangement would be explained the phenomenon which Wollaston and Arago observed in their own persons, after long study, namely, that of seeing only half of every object. We must, however, admit, that at first, anatomical observations did not confirm this opinion; and that we may also oppose to it the fact of the single sensation of sound by the two ears, by means of the two acoustic nerves, which are perfectly distinct from each other in their passage to the brain.

Identical images painted upon the retina of two eyes by a distant object, are such that there is no difference between the perception produced by solid bodies in sculpture, or in relief, and a drawing made on a flat surface, in which the rules of perspective are followed. A picture representing objects which we are accustomed to see at a certain distance, if it be properly illuminated in its different parts, gives us the perfect image of the original, so that the illusion is complete; of this we have an example in the *diorama*. But this is no longer the case when the object is at a very short distance from the eye. We are indebted to Wheatstone for a series of extremely ingenious experiments upon this subject. When a solid body, a cube for instance, is very near the eyes, its projection on the retina of each eye forms there two different images; they resemble each other so slightly, that if we supposed them drawn, we could scarcely, by looking at them, recognise that they belonged to the same body. Notwithstanding this difference, we see a single object: we must then conclude, from these facts, that the perception in relief may be produced by the simultaneous impression of two images formed in each eye; in other words, to see objects as they are, becomes an illusion. Notwithstanding the observations of Wheatstone, we must, however, assume, that a single eye is capable of estimating the solidity of bodies, as is seen daily in persons who have lost one. Experience, custom, and the other senses, assist in correcting this defect.

The Stereoscope.—By looking at the same time at the images of two drawings, obtained by copying the two projections of a solid body upon the retinæ of the two eyes, Wheatstone succeeded in producing the same sensation as that which would have been produced by the solid body. When the observation is made in such a way that the images of the two drawings are formed in the same manner, and upon the same points of the retina which the two projections of the solid occupy, the illusion is complete, and it is impossible to believe that we have before the eyes only paintings made upon a plane.

Fig. 24.

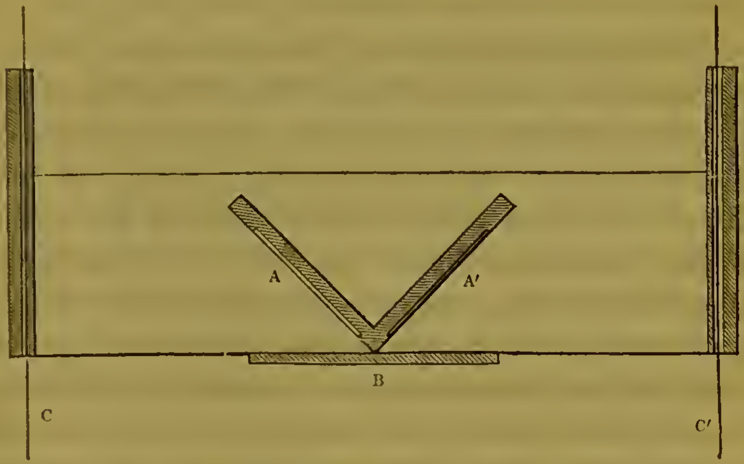


Front View of Wheatstone's Stereoscope.

A A′. Two plane mirrors, whose backs form an angle of 90° with each other.
B. Upright, against which the common edge of the mirrors is fixed.
C C′. Two drawings which slide in grooves made in the pannels D D′.

The observer must place his eyes as near as possible to the mirrors, the right eye before the right-hand mirror, and the left eye before the left-hand mirror.

Fig. 25.

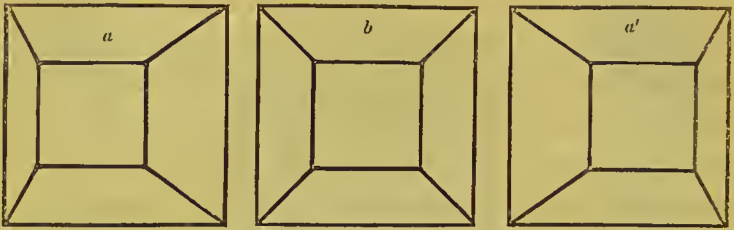


Plan of Wheatstone's Stereoscope.

(The letters refer to the same object as in the preceding figure).

Wheatstone has given the name of *stereoscope* to the instrument by the aid of which this illusion is produced.

Fig. 26.



a a′. Outline figures placed in the Stereoscope: *a* being seen by the left eye, and *a′* by the right eye.
b. Outline of the figure seen in the Stereoscope by the simultaneous perception of the figures *a a′*.

It consists of two inclined mirrors, upon which are formed by reflection the images of the two paintings representing the projections of a solid body in both eyes. The two images are observed by applying the eyes to two openings which allow the two images formed on the mirrors to be seen.

Duration of Impressions.—Among the most curious phenomena of vision, is that of the continuance of impressions received on the retina. Observe a burning coal when being whirled round: if the rotation be sufficiently rapid, you fancy you see a circle of fire. It is evident that this illusion can only be explained by assuming that the sensation produced by the luminous body remains for a certain time, which can be estimated by the interval necessary for the coal, in revolving, to return again to the same point; so that we see it simultaneously at all the points which it successively traverses. The apparent augmentation of volume which a cord undergoes during vibration,—the disappearance of the spokes of a wheel which revolves with great rapidity,—the luminous train of shooting stars,—and the white appearance of a revolving disc, on which the seven colours of the spectrum are painted, are phenomena depending on the same cause; that is, the duration of impressions on the retina. If light were instantaneous all these phenomena would cease. Look steadfastly for some seconds at any luminous body, close the eyes and you will still see it.

In order to ascertain the length of this duration, Aimé contrived to revolve, in opposite directions, two discs fixed on one axis. One of them was perforated with a great number of holes, in the form of equal and symmetrically disposed sectors; the other had but one such hole. When a fasciculus of light was made to fall on this apparatus while in motion, in a dark place, the eye which looks along the common axis of the two discs, perceives sometimes one illuminated sector, whose position is variable and dependent on the coincidence of the single aperture of the second disc with each of those of the first; sometimes two, sometimes three, or even more, and, lastly, one disc of light.

These different impressions depend on the rapidity of rotation. There is only one sector if the velocity of motion be such, that the second coincidence takes place when the impression made upon the retina by the first has ceased. There are two sectors if the impression continues until the second coincidence takes place, and so on. It thus becomes easy by this apparatus, to determine the duration of the impressions upon the retina. Some very ingenious apparatus, which likewise serves to amuse children, have been constructed on the same principle as that of Aimé's instrument just described. Around a circle is placed a number of figures of men, exactly alike in dress and shape, and all represented in motion. They are arranged successively, one after the other, each representing a different but successive position of a given exercise, as, for example, sawing, playing the violin, dancing, &c. The first circle is seen through the slits of a second one. By turning both upon one axis, the eye receives the impression of each of the positions of the figure, at the moment of the passage of the corresponding opening, and preserves the impression of the preceding until the following one takes place. There results from this *persistence,* an effect similar to that which would be produced by the object in motion.

Plateau, who long studied this subject, discovered that, in order to produce a complete impression, the light ought to act for a certain time; that the total duration of the impression is the same for all the colours, and may be approximatively estimated at 0″·34; that the interval of time, during which the impression preserves the same intensity, is as much more considerable as the light is more moderated; that it is different for rays of different colours; for instance, it is longer for the blue than for the red and white light; and that, in fact, the total duration of the impressions is so much the more persistent as the light is the more intense and its action less prolonged. A cannon-ball does not leave the impression of its passage like a shooting star, because the intensity of its light is less.

Ocular Spectra.—Besides the persistence of impression received by the retina, other phenomena occur to us by no means less curious, when we have an object fixed for a certain time. Look at a disc of any colour placed in the middle of a piece of black card, after having fixed your eyes on it for a certain time remove them rapidly to a white ground, or cover them with a cloth, and you will then apparently see a disc, in form similar to the first, but having a colour complementary to it. Thus, if the disc be red, the image will be green; if yellow, it will appear violet; and if white, it will look grey. These apparent sensations have received the name of *accidental colours.* Plateau seems to have demonstrated that these images cease, after having presented a singular series of phenomena; thus it appeared, that after a certain time they vanish, and give place to an image which has really the colour of the object; this second disappears, and that of the complementary colour returns. These images become weaker after having undergone this series of alternations.

Accidental colours form, with each other, combinations like real ones. The following fact puts this curious observation beyond a doubt:—place upon a black ground two squares of paper, one violet, for example, the other orange, both having black points in their centres. Carry the eyes alternately from one point to the other, and after each passage shut the eyes, and you will fancy you see three squares: one yellow, which is complementary to violet; one blue, complementary to the orange; and a third, between the two others, of a green colour, which is the precise shade formed by the yellow and blue. In this experiment the primitive impressions upon the retina, are only the superposition of the two partial impressions which they would manifest if we had observed only one black point; but as the direction of the optic axes differs according as we look at one or the other of these points, it follows that the points of the retina which receive these two partial impressions are not symmetrical. From the juxtaposition of the two squares it results, that the accidental image of the orange colour for the first partial impression, is superposed on that of the violet for the second. The intermediate square, which we perceive when closing the eyes, is owing to this superposition; consequently, we must conclude that the accidental yellow and blue form the green, as real blue and yellow would do. We should arrive at the same conclusion, whatever were the colours of the two squares. Yet we observe a difference when they are tinted with complementary colours; in this case the intermediate square, arising from the superposition of the accidental images, is black, and not white, as takes place in the mixture of two real colours.

Accidental colours combine with real ones exactly as the latter would do with each other. To be convinced of this, we need only observe an image accidentally coloured, not upon a white cardboard but on a coloured one; the image has no longer the complementary colour, but that which results from the mixture of this colour with that of the cardboard on which it is fixed.

Finally, I am anxious to say a few words on the accidental colours formed around objects at the same instant at which we fix them. If we look for a certain time at a coloured object, placed in the middle of a white cardboard, we see upon the edges a fringe of complementary colour. Observe a strip of white paper pasted on a coloured leaf, and place it near a window, in order that it may receive the greatest possible amount of light; the strip will soon appear to have acquired the complementary colour to that of the leaf. All white bodies, when powerfully illuminated, seem to be larger than black objects, although in reality their dimensions are the same. This experiment succeeds as well if we employ two similar discs, the one black, placed upon a white ground, the other white on a black ground, the latter appears to be larger than the other. All these facts evidently prove, that each impression produced upon the retina is accompanied by an accidental fringe, so that the excitation is extended beyond the points of the retina, where the image is formed.

Important applications of these principles are made in the arts in which coloured objects are employed. If the colours which are neighbouring ones, and which mutually influence each other, are complementary, there follows from their reciprocal action a greater brilliancy; the black and the white become, the one more brilliant, the other more deep. On the contrary, all those which are near each other in the series of the seven colours, weaken and injure each other when placed side by side. Pictures, carpets, tinted papers, and decorations in general, sometimes present bad effects, when the reciprocal influences of neighbouring colours have not been attended to in their construetoin. We are indebted to M. Chevreul for a work on this subject, which is remarkable and complete.

I cannot pass silently over the ingenious theory by means of which Plateau has attempted to explain all the phenomena now alluded to. According to him, when the retina has been impressed and agitated by light, emanating from an object, and the cause of the excitation has ceased, the retina returns to its normal position, after a series of decreasing oscillations. The conditions through which it successively passes during the continuation of these oscillations, produce opposed sensations. There is opposition between the black and the white, and in general between the effects produced by two complementary colours. In fact, two accidental complementary colours produce, by their superposition, black; that is to say, no effect. During the continuation of the excitation of the retina, the points of the latter, which are near to those upon which the image is formed, likewise suffer oscillations; which, being identical with those produced upon a tense membrane, ought to be in a direction opposed to the first, just as we know the vibrations of two neighbouring *concamerations* [arches] of a vibrating plate are in an opposite direction. There is, then, a neighbouring fringe which produces the effect of a complementary colour, or that of an opposite condition.

In a word, a portion of the retina being disturbed from its normal state, and the cause of the disturbance having ceased, it returns to a state of repose by a series of oscillations, which vary in their directions and in their intensity with the time. The movement communicated to it is propagated to all the neighbouring parts by a series of oscillations, which also vary in intensity and direction, according to their distance from the place of the direct impression.

THE END.

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