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Title Critiques and Addresses

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CRITIQUES AND ADDRESSES

CRITIQUES

AND

ADDRESSES

BY

THOMAS HENRY HUXLEY, LL.D., F.R.S.

London
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PREFACE.

THE "Critiques and Addresses" gathered together in this volume, like the "Lay Sermons, Addresses, and Reviews," published three years ago, deal chiefly with educational, scientific, and philosophical subjects; and, in fact, indicate the high-water mark of the various tides of occupation by which I have been carried along since the beginning of the year 1870.

In the end of that year, a confidence in my powers of work, which, unfortunately, has not been justified by events, led me to allow myself to be brought forward as a candidate for a seat on the London School Board. Thanks to the energy of my supporters I was elected, and took my share in the work of that body during the critical first year of its existence. Then my health gave way, and I was obliged to resign my place among colleagues whose large practical knowledge of the business of primary education, and whose self-sacrificing zeal in the discharge of the onerous and thankless duties thrown upon them by the Legislature, made it

a pleasure to work with them, even though my position was usually that of a member of the minority.

I mention these circumstances in order to account for (I had almost said to apologize for) the existence of the two papers which head the present series, and which are more or less political, both in the lower and in the higher senses of that word.

The question of the expediency of any form of State Education is, in fact, a question of those higher politics which lie above the region in which Tories, Whigs, and Radicals "delight to bark and bite." In discussing it in my address on "Administrative Nihilism," I found myself, to my profound regret, led to diverge very widely (though even more perhaps in seeming than in reality) from the opinions of a man of genius to whom I am bound by the twofold tie of the respect due to a profound philosopher and the affection given to a very old friend. But had I no other means of knowing the fact, the kindly geniality of Mr. Herbert Spencer's reply[1] assures me that the tie to which I refer will bear a much heavier strain than I have put, or ever intend to put, upon it, and I rather rejoice that I have been the means of calling forth so vigorous a piece of argumentative writing. Nor is this disinterested joy at an attack upon myself diminished by the circumstance, that, in all humility, but in all sincerity, I think it may be repulsed.

Mr. Spencer complains that I have first misinterpreted, and then miscalled, the doctrine of which he is so able

[1] "Specialized Administration;" *Fortnightly Review*, December 1871.

an expositor. It would grieve me very much if I were really open to this charge. But what are the facts? I define this doctrine as follows:—

"Those who hold these views support them by two lines of argument. They enforce them deductively by arguing from an assumed axiom, that the State has no right to do anything but protect its subjects from aggression. The State is simply a policeman, and its duty, neither more nor less than to prevent robbery and murder and enforce contracts. It is not to promote good, nor even to do anything to prevent evil, except by the enforcement of penalties upon those who have been guilty of obvious and tangible assaults upon purse or person. And, according to this view, the proper form of government is neither a monarchy, an aristocracy, nor a democracy, but an *astynomocracy*, or police government. On the other hand, these views are supported *à posteriori* by an induction from observation, which professes to show that whatever is done by a Government beyond these negative limits, is not only sure to be done badly, but to be done much worse than private enterprise would have done the same thing."

I was filled with surprised regret when I learned from the conclusion of the article on "Specialized Administration," that this statement is held by Mr. Spencer to be a misinterpretation of his views. Perhaps I ought to be still more sorry to be obliged to declare myself, even now, unable to discover where my misinterpretation lies, or in what respect my presentation of Mr. Spencer's views differs from his own most recent version of them. As the passage cited above shows, I have carefully defined the sense in which I use the terms which I employ, and, therefore, I am not greatly concerned to defend the abstract appropriateness of the terms themselves. And when

Mr. Spencer maintains the only proper functions of Government to be those which are comprehensible under the description of "Negatively regulative control," I may suggest that the difference between such "Negative Administration" and "Administrative Nihilism," in the sense defined by me, is not easily discernible.

Having, as I hope, relieved myself from the suspicion of having misunderstood or misrepresented Mr. Spencer's views, I might, if I could forget that I am writing a preface, proceed to the discussion of the parallel which he elaborates, with much knowledge and power, between the physiological and the social organisms. But this is not the place for a controversy involving so many technicalities, and I content myself with one remark, namely, that the whole course of modern physiological discovery tends to show, with more and more clearness, that the vascular system, or apparatus for distributing commodities in the animal organism, is eminently under the control of the cerebro-spinal nervous centres—a fact which, unless I am again mistaken, is contrary to one of Mr. Spencer's fundamental assumptions. In the animal organism, Government does meddle with trade, and even goes so far as to tamper a good deal with the currency.

In the same number of the *Fortnightly Review* as that which contains Mr. Spencer's essay, Miss Helen Taylor assails me—though, I am bound to admit, more in sorrow than in anger—for what she terms, my "New Attack on Toleration." It is I, this time,

who may complain of misinterpretation, if the greater part of Miss Taylor's article (with which I entirely sympathise) is supposed to be applicable to my "intolerance." Let us have full toleration, by all means, upon all questions in which there is room for doubt, or which cannot be distinctly proved to affect the welfare of mankind. But when Miss Taylor has shown what basis exists for criminal legislation, except the clear right of mankind not to tolerate that which is demonstrably contrary to the welfare of society, I will admit that such demonstration ought only to be believed in by the "curates and old women" to whom she refers. Recent events have not weakened the conviction I expressed in a much-abused speech at the London School Board, that Ultramontanism is demonstrably the enemy of society; and must be met with resistance, merely passive if possible, but active if necessary, by "the whole power of the State."

Next in order, it seems proper that I should briefly refer to my friend Mr. Mivart's onslaught upon my criticism of Mr. Darwin's critics, himself among the number, which will be found in this volume. In "Evolution and its Consequences"[1] I am accused of misrepresentation, misquotation, misunderstanding, and numerous other negative and positive literary and scientific sins; and much subtle ingenuity is expended by Mr. Mivart in attempting to extricate himself from the position in which my exposition of the real

[1] *Contemporary Review*, January 1872.

opinions of Father Suarez has placed him. So much more, in fact, has Mr. Mivart's ingenuity impressed me than any other feature of his reply, that I shall take the liberty of re-stating the main issue between us; and, for the present, leaving that issue alone to the judgment of the public.

In his book on the "Genesis of Species" Mr. Mivart, after discussing the opinions of sundry Catholic writers of authority, among whom he especially includes St. Augustin, St. Thomas Aquinas, and the Jesuit Suarez, proceeds to say: "It is then evident that ancient and most venerable theological authorities distinctly assert *derivative* creation, and thus their teachings harmonize with all that modern science can possibly require."[1] By the "derivative creation" of organic forms, Mr. Mivart understands, "that God created them by conferring on the material world the power to evolve them under suitable conditions."

On the contrary, I proved by evidence, which Mr. Mivart does not venture to impugn, that Suarez, in his "Tractatus de Opere sex Dierum," expressly rejects St. Augustin's and St. Thomas' views; that he vehemently advocates the literal interpretation of the account of the creation given in the Book of Genesis; and that he treats with utter scorn the notion that the Almighty could have used the language of that Book, unless He meant it to be taken literally.

Mr. Mivart, therefore, either has read Suarez and has totally misrepresented him—a hypothesis which, I hope I need hardly say, I do not for a moment en-

tertain: or, he has got his information at second hand, and has himself been deceived. But in that case, it is surely an imprudence on his part, to reproach me with having "read Suarez *ad hoc*, and evidently without the guidance of anyone familiar with that author." No doubt, in the matter of guidance, Mr. Mivart has the advantage of me. Nevertheless, the guides who supplied him with his references to Suarez' "Metaphysica," while they left him in ignorance of the existence of the "Tractatus," are guides with whose services it might be better to dispense; leaders who wilfully shut their eyes, being even more liable to lodge one in a ditch, than blind leaders.

At the time when the essay on "Methods and Results of Ethnology" was written, I had not met with a passage in Professor Max Müller's "Last Results of Turanian Researches"[1] which shows so appositely, that the profoundest study of philology leads to conclusions respecting the relation of Ethnology with Philology, similar to those at which I had arrived in approaching the question from the Anatomist's side, that I cannot refrain from quoting it:

"Nor should we, in our phonological studies, either expect or desire more than general hints from physical ethnology. The proper and rational connection between the two sciences is that of mutual advice and suggestion, but nothing more. Much of the confusion of terms and indistinctness of principles, both in Ethnology and Phonology, are due to the combined study of these heterogeneous sciences.

[1] Bunsen's "Outlines of the Philosophy of Universal History," vol. i. p. 349. 1854.

Ethnological race and phonological race are not commensurate, except in ante-historical times, or perhaps at the very dawn of history. With the migration of tribes, their wars, their colonies, their conquests and alliances, which, if we may judge from their effects, must have been much more violent in the ethnic, than even in the political, period of history, it is impossible to imagine that race and language should continue to run parallel. The physiologist should pursue his own science unconcerned about language."

It is further desirable to remark that the statements in this Essay respecting the forms of Native American crania need rectification. On this point, I refer the reader who is interested in the subject to my paper "On the Form of the Cranium among the Patagonians and the Fuegians" published in the *Journal of Anatomy and Physiology* for 1868.

If the problem discussed in my address to the British Association in 1870 has not yet received its solution, it is not because the champions of Abiogenesis have been idle, or wanting in confidence. But every new assertion on their side has been met by a counter assertion; and though the public may have been led to believe that so much noise must indicate rapid progress, one way or the other, an impartial critic will admit, with sorrow, that the question has been "marking time" rather than marching. In mere sound, these two processes are not so very different.

LONDON, *April* 1873.

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CRITIQUES AND ADDRESSES.

I.

ADMINISTRATIVE NIHILISM.

AN ADDRESS TO THE MEMBERS OF THE MIDLAND INSTITUTE, OCTOBER 9TH, 1871.)

To me, and, as I trust, to the great majority of those whom I address, the great attempt to educate the people of England which has just been set afoot, is one of the most satisfactory and hopeful events in our modern history. But it is impossible, even if it were desirable, to shut our eyes to the fact, that there is a minority, not inconsiderable in numbers, nor deficient in supporters of weight and authority, in whose judgment all this legislation is a step in the wrong direction, false in principle, and consequently sure to produce evil in practice.

The arguments employed by these objectors are of two kinds. The first is what I will venture to term the caste argument; for, if logically carried out, it would end in the separation of the people of this country into castes, as permanent and as sharply defined, if not as numerous, as those of India. It is maintained that the whole fabric of society will be destroyed if the poor, as well as the rich, are educated; that anything like sound and good education will only make them discontented with their station and raise hopes which, in the great

majority of cases, will be bitterly disappointed. It is said: There must be hewers of wood and drawers of water, scavengers and coalheavers, day labourers and domestic servants, or the work of society will come to a standstill. But, if you educate and refine everybody, nobody will be content to assume these functions, and all the world will want to be gentlemen and ladies.

One hears this argument most frequently from the representatives of the well-to-do middle class; and, coming from them, it strikes me as peculiarly inconsistent, as the one thing they admire, strive after, and advise their own children to do, is to get on in the world, and, if possible, rise out of the class in which they were born into that above them. Society needs grocers and merchants as much as it needs coalheavers; but if a merchant accumulates wealth and works his way to a baronetcy, or if the son of a greengrocer becomes a lord chancellor, or an archbishop, or, as a successful soldier, wins a peerage, all the world admires them; and looks with pride upon the social system which renders such achievements possible. Nobody suggests that there is anything wrong in *their* being discontented with *their* station; or that, in *their* cases society suffers by men of ability reaching the positions for which nature has fitted them.

But there are better replies than those of the *tu quoque* sort to the caste argument. In the first place, it is not true that education, as such, unfits men for rough and laborious, or even disgusting, occupations. The life of a sailor is rougher and harder than that of nine landsmen out of ten, and yet, as every ship's captain knows, no sailor was ever the worse for possessing a trained intelligence. The life of a medical practitioner, especially in the country, is harder and more laborious than that of most artisans, and he is constantly obliged

to do things which, in point of pleasantness, cannot be ranked above scavengering—yet he always ought to be, and he frequently is, a highly educated man. In the second place, though it may be granted that the words of the catechism, which require a man to do his duty in the station to which it has pleased God to call him, give an admirable definition of our obligation to ourselves and to society; yet the question remains, how is any given person to find out what is the particular station to which it has pleased God to call him? A new-born infant does not come into the world labelled scavenger, shopkeeper, bishop, or duke. One mass of red pulp is just like another to all outward appearance. And it is only by finding out what his faculties are good for, and seeking, not for the sake of gratifying a paltry vanity, but as the highest duty to himself and to his fellow-men, to put himself into the position in which they can attain their full development, that the man discovers his true station. That which is to be lamented, I fancy, is not that society should do its utmost to help capacity to ascend from the lower strata to the higher, but that it has no machinery by which to facilitate the descent of incapacity from the higher strata to the lower. In that noble romance, the "Republic" (which is now, thanks to the Master of Balliol, as intelligible to us all, as if it had been written in our mother tongue), Plato makes Socrates say that he should like to inculcate upon the citizens of his ideal state just one "royal lie."

"'Citizens,' we shall say to them in our tale—'You are brothers, yet God has framed you differently. Some of you have the power of command, and these he has composed of gold, wherefore also they have the greatest honour; others of silver, to be auxiliaries; others again, who are to be husbandmen and craftsmen, he has made of brass and iron; and the species will generally be preserved in the children. But as you are of the same original family, a golden parent will sometimes have a silver son, or a silver parent a golden son. And God

proclaims to the rulers, as a first principle, that before all they should watch over their offspring, and see what elements mingle with their nature; for if the son of a golden or silver parent has an admixture of brass and iron, then nature orders a transposition of ranks, and the eye of the ruler must not be pitiful towards his child because he has to descend in the scale and become a husbandman or artisan; just as there may be others sprung from the artisan class, who are raised to honour, and become guardians and auxiliaries. For an oracle says that when a man of brass or iron guards the State, it will then be destroyed.'" [1]

Time, whose tooth gnaws away everything else, is powerless against truth; and the lapse of more than two thousand years has not weakened the force of these wise words. Nor is it necessary that, as Plato suggests, society should provide functionaries expressly charged with the performance of the difficult duty of picking out the men of brass from those of silver and gold. Educate, and the latter will certainly rise to the top; remove all those artificial props by which the brass and iron folk are kept at the top, and, by a law as sure as that of gravitation, they will gradually sink to the bottom. We have all known noble lords who would have been coachmen, or gamekeepers, or billiard-markers, if they had not been kept afloat by our social corks; we have all known men among the lowest ranks, of whom everyone has said, "What might not that man have become, if he had only had a little education?"

And who that attends, even in the most superficial way, to the conditions upon which the stability of modern society—and especially of a society like ours, in which recent legislation has placed sovereign authority in the hands of the masses, whenever they are united enough to wield their power—can doubt that every man of high natural ability, who is both ignorant and miser-

[1] "The Dialogues of Plato." Translated into English, with Analysis and Introduction, by B. Jowett, M.A. Vol. ii. p. 243.

able, is as great a danger to society as a rocket without a stick is to the people who fire it? Misery is a match that never goes out; genius, as an explosive power, beats gunpowder hollow; and if knowledge, which should give that power guidance, is wanting, the chances are not small that the rocket will simply run a-muck among friends and foes. What gives force to the socialistic movement which is now stirring European society to its depths, but a determination on the part of the naturally able men among the proletariat, to put an end, somehow or other, to the misery and degradation in which a large proportion of their fellows are steeped? The question, whether the means by which they purpose to achieve this end are adequate or not, is at this moment the most important of all political questions—and it is beside my present purpose to discuss it. All I desire to point out is, that if the chance of the controversy being decided calmly and rationally, and not by passion and force, looks miserably small to an impartial bystander, the reason is that not one in ten thousand of those who constitute the ultimate court of appeal, by which questions of the utmost difficulty, as well as of the most momentous gravity, will have to be decided, is prepared by education to comprehend the real nature of the suit brought before their tribunal.

Finally, as to the ladies and gentlemen question, all I can say is, would that every woman-child born into this world were trained to be a lady, and every man-child a gentleman! But then I do not use those much-abused words by way of distinguishing people who wear fine clothes, and live in fine houses, and talk aristocratic slang, from those who go about in fustian, and live in back slums, and talk gutter slang. Some inborn plebeian blindness, in fact, prevents me from understanding what advantage the former have over the latter. I have never

even been able to understand why pigeon-shooting at Hurlingham should be refined and polite, while a rat-killing match in Whitechapel is low; or why "What a lark" should be coarse, when one hears "How awfully jolly" drop from the most refined lips twenty times in an evening.

Thoughtfulness for others, generosity, modesty, and self-respect, are the qualities which make a real gentleman, or lady, as distinguished from the veneered article which commonly goes by that name. I by no means wish to express any sentimental preference for Lazarus against Dives, but, on the face of the matter, one does not see why the practice of these virtues should be more difficult in one state of life than another; and any one who has had a wide experience among all sorts and conditions of men, will, I think, agree with me that they are as common in the lower ranks of life as in the higher.

Leaving the caste argument aside then, as inconsistent with the practice of those who employ it, as devoid of any justification in theory, and as utterly mischievous if its logical consequences were carried out, let us turn to the other class of objectors. To these opponents, the Education Act is only one of a number of pieces of legislation to which they object on principle; and they include under like condemnation the Vaccination Act, the Contagious Diseases Act, and all other sanitary Acts; all attempts on the part of the State to prevent adulteration, or to regulate injurious trades; all legislative interference with anything that bears directly or indirectly on commerce, such as shipping, harbours, railways, roads, cab-fares, and the carriage of letters; and all attempts to promote the spread of knowledge by the establishment of teaching bodies, examining bodies, libraries, or museums, or by the sending out of scientific expeditions; all endeavours to advance art by the

establishment of schools of design, or picture galleries; or by spending money upon an architectural public building when a brick box would answer the purpose. According to their views, not a shilling of public money must be bestowed upon a public park or pleasure-ground; not sixpence upon the relief of starvation, or the cure of disease. Those who hold these views support them by two lines of argument. They enforce them deductively by arguing from an assumed axiom, that the State has no right to do anything but protect its subjects from aggression. The State is simply a policeman, and its duty is neither more nor less than to prevent robbery and murder and enforce contracts. It is not to promote good, nor even to do anything to prevent evil, except by the enforcement of penalties upon those who have been guilty of obvious and tangible assaults upon purses or persons. And, according to this view, the proper form of government is neither a monarchy, an aristocracy, nor a democracy, but an *astynomocracy*, or police government. On the other hand, these views are supported *à posteriori*, by an induction from observation, which professes to show that whatever is done by a Government beyond these negative limits, is not only sure to be done badly, but to be done much worse than private enterprise would have done the same thing.

I am by no means clear as to the truth of the latter proposition. It is generally supported by statements which prove clearly enough that the State does a great many things very badly. But this is really beside the question. The State lives in a glass house; we see what it tries to do, and all its failures, partial or total, are made the most of. But private enterprise is sheltered under good opaque bricks and mortar. The public rarely knows what it tries to do, and only hears of failures when they are gross and patent to all the world. Who

is to say how private enterprise would come out if it tried its hand at State work? Those who have had most experience of joint-stock companies and their management, will probably be least inclined to believe in the innate superiority of private enterprise over State management. If continental bureaucracy and centralization be fraught with multitudinous evils, surely English beadleocracy and parochial obstruction are not altogether lovely. If it be said that, as a matter of political experience, it is found to be for the best interests, including the healthy and free development, of a people, that the State should restrict itself to what is absolutely necessary, and should leave to the voluntary efforts of individuals as much as voluntary effort can be got to do, nothing can be more just. But, on the other hand, it seems to me that nothing can be less justifiable than the dogmatic assertion that State interference, beyond the limits of home and foreign police, must, under all circumstances, do harm.

Suppose, however, for the sake of argument, that we accept the proposition that the functions of the State may be properly summed up in the one great negative commandment,—"Thou shalt not allow any man to interfere with the liberty of any other man,"—I am unable to see that the logical consequence is any such restriction of the power of Government, as its supporters imply. If my next-door neighbour chooses to have his drains in such a state as to create a poisonous atmosphere, which I breathe at the risk of typhus and diphtheria, he restricts my just freedom to live just as much as if he went about with a pistol, threatening my life; if he is to be allowed to let his children go unvaccinated, he might as well be allowed to leave strychnine lozenges about in the way of mine; and if he brings them up untaught and untrained to earn their living, he

is doing his best to restrict my freedom, by increasing the burden of taxation for the support of gaols and workhouses, which I have to pay.

The higher the state of civilization, the more completely do the actions of one member of the social body influence all the rest, and the less possible is it for any one man to do a wrong thing without interfering, more or less, with the freedom of all his fellow-citizens. So that, even upon the narrowest view of the functions of the State, it must be admitted to have wider powers than the advocates of the police theory are disposed to admit.

It is urged, I am aware, that if the right of the State to step beyond the assigned limits is admitted at all, there is no stopping; and that the principle which justifies the State in enforcing vaccination or education, will also justify it in prescribing my religious belief, or my mode of carrying on my trade or profession; in determining the number of courses I have for dinner, or the pattern of my waistcoat.

But surely the answer is obvious that, on similar grounds, the right of a man to eat when he is hungry might be disputed, because if you once allow that he may eat at all, there is no stopping him until he gorges himself, and suffers all the ills of a surfeit. In practice, the man leaves off when reason tells him he has had enough; and, in a properly organized State, the Government, being nothing but the corporate reason of the community, will soon find out when State interference has been carried far enough. And, so far as my acquaintance with those who carry on the business of Government goes, I must say that I find them far less eager to interfere with the people, than the people are to be interfered with. And the reason is obvious. The people are keenly sensible of particular evils, and, like a man suffering from pain, desire an immediate remedy.

The statesman, on the other hand, is like the physician, who knows that he can stop the pain at once by an opiate; but who also knows that the opiate may do more harm than good in the long run. In three cases out of four the wisest thing he can do is to wait, and leave the case to nature. But in the fourth case, in which the symptoms are unmistakable, and the cause of the disease distinctly known, prompt remedy saves a life. Is the fact that a wise physician will give as little medicine as possible any argument for his abstaining from giving any at all?

But the argument may be met directly. It may be granted that the State, or corporate authority of the people, might with perfect propriety order my religion, or my waistcoat, if as good grounds could be assigned for such an order as for the command to educate my children. And this leads us to the question which lies at the root of the whole discussion—the question, namely, upon what foundation does the authority of the State rest, and how are the limits of that authority to be determined?

One of the oldest and profoundest of English philosophers, Hobbes of Malmesbury, writes thus:—

"The office of the sovereign, be it monarch or an assembly, consisteth in the end for which he was entrusted with the sovereign power, namely, the procuration of *the safety* of the people: to which he is obliged by the law of nature, and to render an account thereof to God, the author of that law, and to none but Him. But by safety, here, is not meant a bare preservation, but also all other contentments of life, which every man by lawful industry, without danger or hurt to the commonwealth, shall acquire to himself."

At first sight this may appear to be a statement of the police-theory of government, pure and simple; but it is not so. For Hobbes goes on to say:—

"And this is intended should be done, not by care applied to individuals, further than their protection from injuries, when they shall

complain; but by a general providence contained in public instruction both of doctrine and example; and in the making and executing of good laws to which individual persons may apply their own cases."[1]

To a witness of the civil war between Charles I. and the Parliament, it is not wonderful that the dissolution of the bonds of society which is involved in such strife should appear to be "the greatest evil that can happen in this life;" and all who have read the "Leviathan" know to what length Hobbes's anxiety for the preservation of the authority of the representative of the sovereign power, whatever its shape, leads him. But the justice of his conception of the duties of the sovereign power does not seem to me to be invalidated by his monstrous doctrines respecting the sacredness of that power.

To Hobbes, who lived during the break-up of the sovereign power by popular force, society appeared to be threatened by everything which weakened that power: but, to John Locke, who witnessed the evils which flow from the attempt of the sovereign power to destroy the rights of the people by fraud and violence, the danger lay in the other direction.

The safety of the representative of the sovereign power itself is to Locke a matter of very small moment, and he contemplates its abolition when it ceases to do its duty, and its replacement by another, as a matter of course. The great champion of the revolution of 1688 could do no less. Nor is it otherwise than natural that he should seek to limit, rather than to enlarge, the powers of the State, though in substance he entirely agrees with Hobbes's view of its duties:—

"But though men," says he, "when they enter into society, give up the equality, liberty, and executive power they had in the state of nature, into the hands of the society, to be so far disposed of by the Legislature as the good of society shall require; yet it being only with

[1] "Leviathan," Molesworth's ed. p. 322.

an intention in every one the better to preserve himself, his liberty and property (for no rational creature can be supposed to change his condition with an intention to be worse), the power of the society, or legislation, constituted by them can never be supposed to extend further than the common good, but is obliged to secure every one's property by providing against those three defects above mentioned, that made the state of nature so unsafe and uneasy. And so, whoever has the legislative or supreme power of any commonwealth, is bound to govern by established standing laws, promulgated and known to the people, and not by extemporary decrees; by indifferent and upright judges, who are to decide controversies by those laws: and to employ the force of the community at home only in the execution of such laws; or abroad, to prevent or redress foreign injuries, and secure the community from inroads and invasion. And all this to be directed to no other end than the peace, safety, and public good of the people." [1]

Just as in the case of Hobbes, so in that of Locke, it may at first sight appear from this passage that the latter philosopher's views of the functions of Government incline to the negative, rather than the positive, side. But a further study of Locke's writings will at once remove this misconception. In the famous "Letter concerning Toleration," Locke says:—

"The commonwealth seems to me to be a society of men constituted only for the procuring, preserving, and *advancing* their own civil interests.

"Civil interests I call life, liberty, health, and indolency of body; and the possession of outward things, such as money, lands, houses, furniture, and the like.

"It is the duty of the civil magistrate, by the impartial execution of equal laws, to secure unto all the people in general, and to every one of his subjects in particular, the just possession of those things belonging to this life.

". . . The whole jurisdiction of the magistrate reaches only to these civil concernments. . . . All civil power, right, and dominion, is bounded and confined to the only care of promoting these things."

Elsewhere in the same "Letter," Locke lays down the proposition that if the magistrate understand washing a

[1] Locke's Essay, "Of Civil Government," § 131.

child "to be profitable to the curing or preventing any disease that children are subject unto, and esteem the matter weighty enough to be taken care of by a law, in that case he may order it to be done."

Locke seems to differ most widely from Hobbes by his strong advocacy of a certain measure of toleration in religious matters. But the reason why the civil magistrate ought to leave religion alone is, according to Locke, simply this, that "true and saving religion consists in the inward persuasion of the mind." And since "such is the nature of the understanding that it cannot be compelled to the belief of anything by outward force," it is absurd to attempt to make men religious by compulsion. I cannot discover that Locke fathers the pet doctrine of modern Liberalism, that the toleration of error is a good thing in itself, and to be reckoned among the cardinal virtues; on the contrary, in this very "Letter on Toleration" he states in the clearest language that "No opinion contrary to human society, or to those moral rules which are necessary to the preservation of civil society, are to be tolerated by the magistrate." And the practical corollary which he draws from this proposition is that there ought to be no toleration for either Papists or Atheists.

After Locke's time the negative view of the functions of Government gradually grew in strength, until it obtained systematic and able expression in Wilhelm von Humboldt's "Ideen,"[1] the essence of which is the denial that the State has a right to be anything more than chief policeman. And, of late years, the belief in the efficacy of doing nothing, thus formulated, has acquired considerable popularity for several reasons. In the first place, men's speculative convictions have become less and less real; their tolerance is large

[1] An English translation has been published under the title of "Essay on the Sphere and Duties of Government."

because their belief is small; they know that the State had better leave things alone unless it has a clear knowledge about them; and, with reason, they suspect that the knowledge of the governing power may stand no higher than the very low watermark of their own.

In the second place, men have become largely absorbed in the mere accumulation of wealth; and as this is a matter in which the plainest and strongest form of self-interest is intensely concerned, science (in the shape of Political Economy) has readily demonstrated that self-interest may be safely left to find the best way of attaining its ends. Rapidity and certainty of intercourse between different countries, the enormous development of the powers of machinery, and general peace (however interrupted by brief periods of warfare), have changed the face of commerce as completely as modern artillery has changed that of war. The merchant found himself as much burdened by ancient protective measures as the soldier by his armour—and negative legislation has been of as much use to the one as the stripping off of breast-plates, greaves, and buff-coat to the other. But because the soldier is better without his armour it does not exactly follow that it is desirable that our defenders should strip themselves stark naked; and it is not more apparent why *laissez-faire*—great and beneficial as it may be in all that relates to the accumulation of wealth—should be the one great commandment which the State is to obey in all other matters; and especially in those in which the justification of *laissez-faire*, namely, the keen insight given by the strong stimulus of direct personal interest, in matters clearly understood, is entirely absent.

Thirdly, to the indifference generated by the absence of fixed beliefs, and to the confidence in the efficacy of *laissez faire*, apparently justified by experience of the

value of that principle when applied to the pursuit of wealth, there must be added that nobler and better reason for a profound distrust of legislative interference, which animates Von Humboldt and shines forth in the pages of Mr. Mill's famous Essay on Liberty—I mean the just fear lest the end should be sacrificed to the means; lest freedom and variety should be drilled and disciplined out of human life in order that the great mill of the State should grind smoothly.

One of the profoundest of living English philosophers, who is at the same time the most thoroughgoing and consistent of the champions of astynomocracy, has devoted a very able and ingenious essay[1] to the drawing out of a comparison between the process by which men have advanced from the savage state to the highest civilization, and that by which an animal passes from the condition of an almost shapeless and structureless germ, to that in which it exhibits a highly complicated structure and a corresponding diversity of powers. Mr. Spencer says with great justice—

"That they gradually increase in mass; that they become, little by little, more complex; that, at the same time, their parts grow more mutually dependent; and that they continue to live and grow as wholes, while successive generations of their units appear and disappear,—are broad peculiarities which bodies politic display, in common with all living bodies, and in which they and living bodies differ from everything else."

In a very striking passage of this essay Mr. Spencer shows with what singular closeness a parallel between the development of a nervous system, which is the governing power of the body in the series of animal organisms, and that of government, in the series of social organisms, can be drawn:—

"Strange as the assertion will be thought," says Mr. Spencer, "our Houses of Parliament discharge in the social economy functions that

[1] "The Social Organism:" Essays. Second Series.

are, in sundry respects, comparable to those discharged by the cerebral masses in a vertebrate animal. . . . . The cerebrum co-ordinates the countless heterogeneous considerations which affect the present and future welfare of the individual as a whole; and the Legislature co-ordinates the countless heterogeneous considerations which affect the immediate and remote welfare of the whole community. We may describe the office of the brain as that of *averaging* the interests of life, physical, intellectual, moral, social; and a good brain is one in which the desires answering to their respective interests are so balanced, that the conduct they jointly dictate sacrifice none of them. Similarly we may describe the office of Parliament as that of *averaging* the interests of the various classes in a community; and a good Parliament is one in which the parties answering to these respective interests are so balanced, that their united legislation concedes to each class as much as consists with the claims of the rest."

All this appears to be very just. But if the resemblances between the body physiological and the body politic are any indication, not only of what the latter is, and how it has become what it is, but of what it ought to be, and what it is tending to become, I cannot but think that the real force of the analogy is totally opposed to the negative view of State function.

Suppose that, in accordance with this view, each muscle were to maintain that the nervous system had no right to interfere with its contraction, except to prevent it from hindering the contraction of another muscle; or each gland, that it had a right to secrete, so long as its secretion interfered with no other; suppose every separate cell left free to follow its own "interests," and *laissez-faire* lord of all, what would become of the body physiological?

The fact is that the sovereign power of the body thinks for the physiological organism, acts for it, and rules the individual components with a rod of iron. Even the blood-corpuscles can't hold a public meeting without being accused of "congestion"—and the brain, like other despots whom we have known, calls out at

once for the use of sharp steel against them. As in Hobbes's "Leviathan," the representative of the sovereign authority in the living organism, though he derives all his powers from the mass which he rules, is above the law. The questioning of his authority involves death, or that partial death which we call paralysis. Hence, if the analogy of the body politic with the body physiological counts for anything, it seems to me to be in favour of a much larger amount of governmental interference than exists at present, or than I, for one, at all desire to see. But, tempting as the opportunity is, I am not disposed to build up any argument in favour of my own case upon this analogy, curious, interesting, and in many respects close, as it is, for it takes no cognizance of certain profound and essential differences between the physiological and the political bodies.

Much as the notion of a "social contract" has been ridiculed, it nevertheless seems to be clear enough, that all social organization whatever depends upon what is substantially a contract, whether expressed or implied, between the members of the society. No society ever was, or ever can be, really held together by force. It may seem a paradox to say that a slaveholder does not make his slaves work by force, but by agreement. And yet it is true. There is a contract between the two which, if it were written out, would run in these terms:—"I undertake to feed, clothe, house, and not to kill, flog, or otherwise maltreat you, Quashie, if you perform a certain amount of work." Quashie, seeing no better terms to be had, accepts the bargain, and goes to work accordingly. A highwayman who garottes me, and then clears out my pockets, robs me by force in the strict sense of the words; but if he puts a pistol to my head and demands my money or my life, and I, prefer-

ring the latter, hand over my purse, we have virtually made a contract, and I perform one of the terms of that contract. If, nevertheless, the highwayman subsequently shoots me, everybody will see that, in addition to the crimes of murder and theft, he has been guilty of a breach of contract.

A despotic Government, therefore, though often a mere combination of slaveholding and highway robbery, nevertheless implies a contract between governor and governed, with voluntary submission on the part of the latter; and *à fortiori*, all other forms of government are in like case.

Now a contract between any two men implies a restriction of the freedom of each in certain particulars. The highwayman gives up his freedom to shoot me, on condition of my giving up my freedom to do as I like with my money: I give up my freedom to kill Quashie, on condition of Quashie's giving up his freedom to be idle. And the essence and foundation of every social organization, whether simple or complex, is the fact that each member of the society voluntarily renounces his freedom in certain directions, in return for the advantages which he expects from association with the other members of that society. Nor are constitutions, laws, or manners, in ultimate analysis, anything but so many expressed or implied contracts between the members of a society to do this, or abstain from that.

It appears to me that this feature constitutes the difference between the social and the physiological organism. Among the higher physiological organisms, there is none which is developed by the conjunction of a number of primitively independent existences into a complex whole. The process of social organization appears to be comparable, not so much to the process of organic development, as to the synthesis of the chemist, by which independent elements are gradually built up into complex

aggregations—in which each element retains an independent individuality, though held in subordination to the whole. The atoms of carbon and hydrogen, oxygen, nitrogen, which enter into a complex molecule, do not lose the powers originally inherent in them, when they unite to form that molecule, the properties of which express those forces of the whole aggregation which are not neutralized and balanced by one another. Each atom has given up something, in order that the atomic society, or molecule, may subsist. And as soon as any one or more of the atoms thus associated resumes the freedom which it has renounced, and follows some external attraction, the molecule is broken up, and all the peculiar properties which depended upon its constitution vanish.

Every society, great or small, resembles such a complex molecule, in which the atoms are represented by men, possessed of all those multifarious attractions and repulsions which are manifested in their desires and volitions, the unlimited power of satisfying which, we call freedom. The social molecule exists in virtue of the renunciation of more or less of this freedom by every individual. It is decomposed, when the attraction of desire leads to the resumption of that freedom, the suppression of which is essential to the existence of the social molecule. And the great problem of that social chemistry we call politics, is to discover what desires of mankind may be gratified, and what must be suppressed, if the highly complex compound, society, is to avoid decomposition. That the gratification of some of men's desires shall be renounced is essential to order; that the satisfaction of others shall be permitted is no less essential to progress; and the business of the sovereign authority—which is, or ought to be, simply a delegation of the people appointed to act for its good—appears to

me to be, not only to enforce the renunciation of the anti-social desires, but, wherever it may be necessary, to promote the satisfaction of those which are conducive to progress.

The great metaphysician, Immanuel Kant, who is at his greatest when he discusses questions which are not metaphysical, wrote, nearly a century ago, a wonderfully instructive essay entitled "A Conception of Universal History in relation to Universal Citizenship,"[1] from which I will borrow a few pregnant sentences :—

"The means of which Nature has availed herself, in order to bring about the development of all the capacities of man, is the antagonism of those capacities to social organization, so far as the latter does in the long run necessitate their definite correlation. By antagonism, I here mean the unsocial sociability of mankind—that is, the combination in them of an impulse to enter into society, with a thorough spirit of opposition which constantly threatens to break up this society. The ground of this lies in human nature. Man has an inclination to enter into society, because in that state he feels that he becomes more a man, or, in other words, that his natural faculties develop. But he has also a great tendency to isolate himself, because he is, at the same time, aware of the unsocial peculiarity of desiring to have everything his own way ; and thus, being conscious of an inclination to oppose others, he is naturally led to expect opposition from them.

"Now it is this opposition which awakens all the dormant powers of men, stimulates them to overcome their inclination to be idle, and, spurred by the love of honour, or power, or wealth, to make themselves a place among their fellows, whom they can neither do with, nor do without.

"Thus they make the first steps from brutishness towards culture, of which the social value of man is the measure. Thus all talents become gradually developed, taste is formed, and by continual enlightenment the foundations of a way of thinking are laid, which gradually changes the mere rude capacity of moral perception into

[1] "Idee zu einer allgemeinen Geschichte in weltburgerlichen Absicht," 1784. This paper has been translated by De Quincey, and attention has been recently drawn to its "signal merits" by the Editor of the *Fortnightly Review* in his Essay on Condorcet. (*Fortnightly Review*, No. xxxviii. N.S. pp. 136, 137.)

determinate practical principles; and thus society, which is originated by a sort of pathological compulsion, becomes metamorphosed into a moral unity." (*Loc. cit.* p. 147.)

"All the culture and art which adorn humanity, the most refined social order, are produced by that unsociability which is compelled by its own existence to discipline itself, and so by enforced art to bring the seeds implanted by nature into full flower." (*Loc. cit.* p. 148.)

In these passages, as in others of this remarkable tract, Kant anticipates the application of the "struggle for existence" to politics, and indicates the manner in which the evolution of society has resulted from the constant attempt of individuals to strain its bonds. If individuality has no play, society does not advance; if individuality breaks out of all bounds, society perishes.

But when men living in society once become aware that their welfare depends upon two opposing tendencies of equal importance—the one restraining, the other encouraging, individual freedom—the question "What are the functions of Government?" is translated into another—namely, What ought we men, in our corporate capacity, to do, not only in the way of restraining that free individuality which is inconsistent with the existence of society, but in encouraging that free individuality which is essential to the evolution of the social organization? The formula which truly defines the function of Government must contain the solution of both the problems involved, and not merely of one of them.

Locke has furnished us with such a formula, in the noblest, and at the same time briefest, statement of the purpose of Government known to me:—

"THE END OF GOVERNMENT IS THE GOOD OF MANKIND."[1]

But the good of mankind is not a something which is absolute and fixed for all men, whatever their capacities

[1] "Of Civil Government," § 229.

or state of civilization. Doubtless it is possible to imagine a true "Civitas Dei," in which every man's moral faculty shall be such as leads him to control all those desires which run counter to the good of mankind, and to cherish only those which conduce to the welfare of society; and in which every man's native intellect shall be sufficiently strong, and his culture sufficiently extensive, to enable him to know what he ought to do and to seek after. And, in that blessed State, police will be as much a superfluity as every other kind of government.

But the eye of man has not beheld that State, and is not likely to behold it for some time to come. What we do see, in fact, is that States are made up of a considerable number of the ignorant and foolish, a small proportion of genuine knaves, and a sprinkling of capable and honest men, by whose efforts the former are kept in a reasonable state of guidance, and the latter of repression. And, such being the case, I do not see how any limit whatever can be laid down as to the extent to which, under some circumstances, the action of Government may be rightfully carried.

Was our own Government wrong in suppressing Thuggee in India? If not, would it be wrong in putting down any enthusiast who attempted to set up the worship of Astarte in the Haymarket? Has the State no right to put a stop to gross and open violations of common decency? And if the State has, as I believe it has, a perfect right to do all these things, are we not bound to admit, with Locke, that it may have a right to interfere with "Popery" and "Atheism," if it be really true that the practical consequences of such beliefs can be proved to be injurious to civil society? The question where to draw the line between those things with which the State ought, and those with which it ought not, to

interfere, then, is one which must be left to be decided separately for each individual case. The difficulty which meets the statesman is the same as that which meets us all in individual life, in which our abstract rights are generally clear enough, though it is frequently extremely hard to say at what point it is wise to cease our attempts to enforce them.

The notion that the social body should be organized in such a manner as to advance the welfare of its members, is as old as political thought; and the schemes of Plato, More, Robert Owen, St. Simon, Comte, and the modern socialists, bear witness that, in every age, men whose capacity is of no mean order, and whose desire to benefit their fellows has rarely been excelled, have been strongly, nay, enthusiastically, convinced that Government may attain its end—the good of the people—by some more effectual process than the very simple and easy one of putting its hands in its pockets, and letting them alone.

It may be, that all the schemes of social organization which have hitherto been propounded are impracticable follies. But if this be so, the fact proves, not that the idea which underlies them is worthless, but only that the science of politics is in a very rudimentary and imperfect state. Politics, as a science, is not older than astronomy; but though the subject-matter of the latter is vastly less complex than that of the former, the theory of the moon's motions is not quite settled yet.

Perhaps it may help us a little way towards getting clearer notions of what the State may and what it may not do, if, assuming the truth of Locke's maxim that "the end of Government is the good of mankind," we consider a little what the good of mankind is.

I take it that the good of mankind means the attainment, by every man, of all the happiness which he can

enjoy without diminishing the happiness of his fellow-men.[1]

If we inquire what kinds of happiness come under this definition, we find those derived from the sense of security or peace; from wealth, or commodity, obtained by commerce; from Art—whether it be architecture, sculpture, painting, music, or literature; from knowledge, or science; and, finally, from sympathy or friendship. No man is injured, but the contrary, by peace. No man is any the worse off because another acquires wealth by trade, or by the exercise of a profession; on the contrary, he cannot have acquired his wealth, except by benefiting others to the full extent of what they considered to be its value; and his wealth is no more than fairy gold if he does not go on benefiting others in the same way. A thousand men may enjoy the pleasure derived from a picture, a symphony, or a poem, without lessening the happiness of the most devoted connoisseur. The investigation of nature is an infinite pasture-ground, where all may graze, and where the more bite, the longer the grass grows, the sweeter is its flavour, and the more it nourishes. If I love a friend, it is no damage to me, but rather a pleasure, if all the world also love him and think of him as highly as I do.

It appears to be universally agreed, for the reasons already mentioned, that it is unnecessary and undesirable for the State to attempt to promote the acquisition of wealth by any direct interference with commerce. But there is no such agreement as to the further question

[1] "Hic est itaque finis ad quem tendo, talem scilicet Naturam acquirere, et ut multi mecum eam acquirant, conari hoc est de mea felicitate etiam operam dare, ut alii multi idem atque ego intelligant, ut eorum intellectus et cupiditas prorsus cum meo intellectu et cupiditate conveniant: atque hoc fiat, necesse est tantum de Natura intelligere, quantum sufficit ad talem naturam acquirendam; deinde formare talem societatem qualis est desideranda, ut quam plurimi quam facillime et secure eo perveniant."—B. Spinoza, *De Intellectûs Emendatione Tractatus.*

whether the State may not promote the acquisition of wealth by indirect means. For example, may the State make a road, or build a harbour, when it is quite clear that by so doing it will open up a productive district, and thereby add enormously to the total wealth of the community? And if so, may the State, acting for the general good, take charge of the means of communication between its members, or of the postal and telegraph services? I have not yet met with any valid argument against the propriety of the State doing what our Government does in this matter; except the assumption, which remains to be proved, that Government will manage these things worse than private enterprise would do. Nor is there any agreement upon the still more important question whether the State ought, or ought not, to regulate the distribution of wealth. If it ought not, then all legislation which regulates inheritance—the statute of Mortmain, and the like—is wrong in principle; and, when a rich man dies, we ought to return to the state of nature, and have a scramble for his property. If, on the other hand, the authority of the State is legitimately employed in regulating these matters, then it is an open question, to be decided entirely by evidence as to what tends to the highest good of the people, whether we keep our present laws, or whether we modify them. At present the State protects men in the possession and enjoyment of their property, and defines what that property is. The justification for its so doing is that its action promotes the good of the people. If it can be clearly proved that the abolition of property would tend still more to promote the good of the people, the State will have the same justification for abolishing property that it now has for maintaining it.

Again, I suppose it is universally agreed that it would be useless and absurd for the State to attempt to pro-

mote friendship and sympathy between man and man directly. But I see no reason why, if it be otherwise expedient, the State may not do something towards that end indirectly. For example, I can conceive the existence of an Established Church which should be a blessing to the community. A Church in which, week by week, services should be devoted, not to the iteration of abstract propositions in theology, but to the setting before men's minds of an ideal of true, just, and pure living; a place in which those who are weary of the burden of daily cares, should find a moment's rest in the contemplation of the higher life which is possible for all, though attained by so few; a place in which the man of strife and of business should have time to think how small, after all, are the rewards he covets compared with peace and charity. Depend upon it, if such a Church existed, no one would seek to disestablish it.

Whatever the State may not do, however, it is universally agreed that it may take charge of the maintenance of internal and external peace. Even the strongest advocate of administrative nihilism admits that Government may prevent aggression of one man on another. But this implies the maintenance of an army and navy, as much as of a body of police; it implies a diplomatic as well as a detective force; and it implies, further, that the State, as a corporate whole, shall have distinct and definite views as to its wants, powers, and obligations.

For independent States stand in the same relation to one another as men in a state of nature, or unlimited freedom. Each endeavours to get all it can, until the inconvenience of the state of war suggests either the formation of those express contracts we call treaties, or mutual consent to those implied contracts which are expressed by international law. The moral rights of a

State rest upon the same basis as those of an individual. If any number of States agree to observe a common set of international laws, they have, in fact, set up a sovereign authority or supra-national government, the end of which, like that of all governments, is the good of mankind; and the possession of as much freedom by each State, as is consistent with the attainment of that end. But there is this difference: that the government thus set up over nations is ideal, and has no concrete representative of the sovereign power; whence the only way of settling any dispute finally is to fight it out. Thus the supra-national society is continually in danger of returning to the state of nature, in which contracts are void; and the possibility of this contingency justifies a government in restricting the liberty of its subjects in many ways that would otherwise be unjustifiable.

Finally, with respect to the advancement of science and art. I have never yet had the good fortune to hear any valid reason alleged why that corporation of individuals we call the State may not do what voluntary effort fails in doing, either from want of intelligence or lack of will. And here it cannot be alleged that the action of the State is always hurtful. On the contrary, in every country in Europe, universities, public libraries, picture galleries, museums, and laboratories, have been established by the State, and have done infinite service to the intellectual and moral progress and the refinement of mankind.

A few days ago I received from one of the most eminent members of the Institut of France a pamphlet entitled "Pourquoi la France n'a pas trouvé d'hommes supérieurs au moment du péril." The writer, M. Pasteur, has no doubt that the cause of the astounding collapse of his countrymen is to be sought in the miserable neglect of the higher branches of culture, which has been one of

the many disgraces of the Second Empire, if not of its predecessors.

"Au point où nous sommes arrivés de ce qu'on appelle la *civilisation moderne*, la culture des sciences dans leur expression la plus élevée est peut-être plus nécessaire encore à l'état moral d'une nation qu'à sa prospérité matérielle.

"Les grandes découvertes, les méditations de la pensée dans les arts, dans les sciences et dans les lettres, en un mot les travaux désintéressés de l'esprit dans tous les genres, les centres d'enseignement propres à les faire connaître, introduisent dans le corps social tout entier l'esprit philosophique ou scientifique, cet esprit de discernement qui soumet tout à une raison sévère, condamne l'ignorance, dissipe les préjugés et les erreurs. Ils élèvent le niveau intellectuel, le sentiment moral; par eux, l'idée divine elle-même se répand et s'exalte. . . . Si, au moment du péril suprême, la France n'a pas trouvé des hommes supérieurs pour mettre en œuvre ses ressources et le courage de ses enfants, il faut l'attribuer, j'en ai la conviction, à ce que la France s'est désintéressée, depuis un demi-siècle, des grands travaux de la pensée, particulièrement dans les sciences exactes."

Individually, I have no love for academies on the continental model, and still less for the system of decorating men of distinction in science, letters, or art, with orders and titles, or enriching them with sinecures. What men of science want is only a fair day's wages for more than a fair day's work; and most of us, I suspect, would be well content if, for our days and nights of unremitting toil, we could secure the pay which a first-class Treasury clerk earns without any obviously trying strain upon his faculties. The sole order of nobility which, in my judgment, becomes a philosopher, is that rank which he holds in the estimation of his fellow-workers, who are the only competent judges in such matters. Newton and Cuvier lowered themselves when the one accepted an idle knighthood, and the other became a baron of the empire. The great men who went to their graves as Michael Faraday and George Grote seem to me to have understood the dignity of know-

ledge better when they declined all such meretricious trappings.

But it is one thing for the State to appeal to the vanity and ambition which are to be found in philosophical as in other breasts, and another to offer men who desire to do the hardest of work for the most modest of tangible rewards, the means of making themselves useful to their age and generation. And this is just what the State does when it founds a public library or museum, or provides the means of scientific research by such grants of money as that administered by the Royal Society.

It is one thing, again, for the State to take all the higher education of the nation into its own hands; it is another to stimulate and to aid, while they are yet young and weak, local efforts to the same end. The Midland Institute, Owens College in Manchester, the newly instituted Science College in Newcastle, are all noble products of local energy and munificence. But the good they are doing is not local—the commonwealth, to its uttermost limits, shares in the benefits they confer; and I am at a loss to understand upon what principle of equity the State, which admits the principle of payment on results, refuses to give a fair equivalent for these benefits; or on what principle of justice the State, which admits the obligation of sharing the duty of primary education with a locality, denies the existence of that obligation when the higher education is in question.

To sum up: If the positive advancement of the peace, wealth, and the intellectual and moral development of its members, are objects which the Government, as the representative of the corporate authority of society, may justly strive after, in fulfilment of its end—the good of mankind; then it is clear that the Government may

undertake to educate the people. For education promotes peace by teaching men the realities of life and the obligations which are involved in the very existence of society; it promotes intellectual development, not only by training the individual intellect, but by sifting out from the masses of ordinary or inferior capacities, those who are competent to increase the general welfare by occupying higher positions; and, lastly, it promotes morality and refinement, by teaching men to discipline themselves, and by leading them to see that the highest, as it is the only permanent, content is to be attained, not by grovelling in the rank and steaming valleys of sense, but by continual striving towards those high peaks, where, resting in eternal calm, reason discerns the undefined but bright ideal of the highest Good—"a cloud by day, a pillar of fire by night."

II.

THE SCHOOL BOARDS: WHAT THEY CAN DO, AND WHAT THEY MAY DO.

An electioneering manifesto would be out of place in the pages of this Review; but any suspicion that may arise in the mind of the reader that the following pages partake of that nature, will be dispelled, if he reflect that they cannot be published [1] until after the day on which the ratepayers of the metropolis will have decided which candidates for seats upon the Metropolitan School Board they will take, and which they will leave.

As one of those candidates, I may be permitted to say, that I feel much in the frame of mind of the Irish bricklayer's labourer, who bet another that he could not carry him to the top of the ladder in his hod. The challenged hodman won his wager, but as the stakes were handed over, the challenger wistfully remarked, "I'd great hopes of falling at the third round from the top." And, in view of the work and the worry which awaits the members of the School Boards, I must confess to an occasional ungrateful hope that the friends who are

[1] Notwithstanding Mr. Huxley's intentions, the Editor took upon himself, in what seemed to him to be the public interest, to send an extract from this article to the newspapers—before the day of the election of the School Board. —Editor of the *Contemporary Review*.

toiling upwards with me in their hod, may, when they reach "the third round from the top," let me fall back into peace and quietness.

But whether fortune befriend me in this rough method, or not, I should like to submit to those of whom I am a potential, but of whom I may not be an actual, colleague, and to others who may be interested in this most important problem—how to get the Education Act to work efficiently—some considerations as to what are the duties of the members of the School Boards, and what are the limits of their power.

I suppose no one will be disposed to dispute the proposition, that the prime duty of every member of such a Board is to endeavour to administer the Act honestly; or in accordance, not only with its letter, but with its spirit. And if so, it would seem that the first step towards this very desirable end is, to obtain a clear notion of what that letter signifies, and what that spirit implies; or, in other words, what the clauses of the Act are intended to enjoin and to forbid. So that it is really not admissible, except for factious and abusive purposes, to assume that any one who endeavours to get at this clear meaning is desirous only of raising quibbles and making difficulties.

Reading the Act with this desire to understand it, I find that its provisions may be classified, as might naturally be expected, under two heads: the one set relating to the subject-matter of education; the other to the establishment, maintenance, and administration of the schools in which that education is to be conducted.

Now it is a most important circumstance, that all the sections of the Act, except four, belong to the latter division; that is, they refer to mere matters of administration. The four sections in question are the seventh, the fourteenth, the sixteenth, and the ninety-seventh.

Of these, the seventh, the fourteenth, and the ninety-seventh deal with the subject-matter of education, while the sixteenth defines the nature of the relations which are to exist between the "Education Department" (an euphemism for the future Minister of Education) and the School Boards. It is the sixteenth clause which is the most important, and, in some respects, the most remarkable of all. It runs thus :—

"If the School Board do, or permit, any act in contravention of, or fail to comply with, the regulations, according to which a school provided by them is required by this Act to be conducted, the Education Department may declare the School Board to be, and such Board shall accordingly be deemed to be, a Board in default, and the Education Department may proceed accordingly; and every act, or omission, of any member of the School Board, or manager appointed by them, or any person under the control of the Board, shall be deemed to be permitted by the Board, unless the contrary be proved.

"If any dispute arises as to whether the School Board have done, or permitted, any act in contravention of, or have failed to comply with, the said regulations, *the matter shall be referred to the Education Department, whose decision thereon shall be final.*"

It will be observed that this clause gives the Minister of Education absolute power over the doings of the School Boards. He is not only the administrator of the Act, but he is its interpreter. I had imagined that on the occurrence of a dispute, not as regards a question of pure administration, but as to the meaning of a clause of the Act, a case might be taken and referred to a court of justice. But I am led to believe that the Legislature has, in the present instance, deliberately taken this power out of the hands of the judges and lodged it in those of the Minister of Education, who, in accordance with our method of making Ministers, will necessarily be a political partisan, and who may be a strong theological sectary into the bargain. And I am informed by members of Parliament who watched the progress of the

Act, that the responsibility for this unusual state of things rests, not with the Government, but with the Legislature, which exhibited a singular disposition to accumulate power in the hands of the future Minister of Education, and to evade the more troublesome difficulties of the education question by leaving them to be settled between that Minister and the School Boards.

I express no opinion whether it is, or is not, desirable that such powers of controlling all the School Boards in the country should be possessed by a person who may be, like Mr. Forster, eminently likely to use these powers justly and wisely, but who also may be quite the reverse. I merely wish to draw attention to the fact that such powers are given to the Minister, whether he be fit or unfit. The extent of these powers becomes apparent when the other sections of the Act referred to are considered. The fourth clause of the seventh section says :—

"The school shall be conducted in accordance with the conditions required to be fulfilled by an elementary school in order to obtain an annual Parliamentary grant."

What these conditions are appears from the following clauses of the ninety-seventh section :—

"The conditions required to be fulfilled by an elementary school in order to obtain an annual Parliamentary grant shall be those contained in the minutes of the Education Department in force for the time being. . . . Provided that no such minute of the Education Department, not in force at the time of the passing of this Act, shall be deemed to be in force until it has lain for not less than one month on the table of both Houses of Parliament."

Let us consider how this will work in practice. A school established by a School Board may receive support from three sources—from the rates, the school fees, and the Parliamentary grant. The latter may be as great as the two former taken together ; and as it may be assumed,

without much risk of error, that a constant pressure will be exerted by the ratepayers on the members who represent them, to get as much out of the Government, and as little out of the rates, as possible, the School Boards will have a very strong motive for shaping the education they give, as nearly as may be, on the model which the Education Minister offers for their imitation, and for the copying of which he is prepared to pay.

The Revised Code did not compel any schoolmaster to leave off teaching anything; but, by the very simple process of refusing to pay for many kinds of teaching, it has practically put an end to them. Mr. Forster is said to be engaged in revising the Revised Code; a successor of his may re-revise it—and there will be no sort of check upon these revisions and counter-revisions, except the possibility of a Parliamentary debate, when the revised, or added, minutes are laid upon the table. What chance is there that any such debate will take place on a matter of detail relating to elementary education—a subject with which members of the Legislature, having been, for the most part, sent to our public schools thirty years ago, have not the least practical acquaintance, and for which they care nothing, unless it derives a political value from its connection with sectarian politics?

I cannot but think, then, that the School Boards will have the appearance, but not the reality, of freedom of action, in regard to the subject-matter of what is commonly called "secular" education.

As respects what is commonly called "religious" education, the power of the Minister of Education is even more despotic. An interest, almost amounting to pathos, attaches itself, in my mind, to the frantic exertions which are at present going on in almost every school division, to elect certain candidates whose names have never before been heard of in connection with

education, and who are either sectarian partisans, or nothing. In my own particular division, a body organized *ad hoc* is moving heaven and earth to get the seven seats filled by seven gentlemen, four of whom are good Churchmen, and three no less good Dissenters. But why should this seven times heated fiery furnace of theological zeal be so desirous to shed its genial warmth over the London School Board? Can it be that these zealous sectaries mean to evade the solemn pledge given in the Act?

"No religious catechism or religious formulary which is distinctive of any particular denomination shall be taught in the school."

I confess I should have thought it my duty to reject any such suggestion, as dishonouring to a number of worthy persons, if it had not been for a leading article and some correspondence which appeared in the *Guardian* of November 9th, 1870.

The *Guardian* is, as everybody knows, one of the best of the "religious" newspapers; and, personally, I have every reason to speak highly of the fairness, and indeed kindness, with which the editor is good enough to deal with a writer who must, in many ways, be so objectionable to him as myself. I quote the following passages from a leading article on a letter of mine, therefore, with all respect, and with a genuine conviction that the course of conduct advocated by the writer must appear to him in a very different light from that under which I see it:—

"The first of these points is the interpretation which Professor Huxley puts on the 'Cowper-Temple clause.' It is, in fact, that which we foretold some time ago as likely to be forced upon it by those who think with him. The clause itself was one of those compromises which it is very difficult to define or to maintain logically. On the one side was the simple freedom to School Boards to establish what schools they pleased, which Mr. Forster originally gave, but against

which the Nonconformists lifted up their voices, because they conceived it likely to give too much power to the Church. On the other side there was the proposition to make the schools secular—intelligible enough, but in the consideration of public opinion simply impossible—and there was the vague impracticable idea, which Mr. Gladstone thoroughly tore to pieces, of enacting that the teaching of all schoolmasters in the new schools should be strictly 'undenominational.' The Cowper-Temple clause was, we repeat, proposed simply to tide over the difficulty. It was to satisfy the Nonconformists and the 'unsectarian,' as distinct from the secular party of the League, by forbidding all distinctive 'catechisms and formularies,' which might have the effect of openly assigning the schools to this or that religious body. It refused, at the same time, to attempt the impossible task of defining what was undenominational; and its au[illegible] even contended, if we understood him correctly, that it would in no way, even indirectly, interfere with the substantial teaching of any master in any school. This assertion we always believed to be untenable; we could not see how, in the face of this clause, a distinctly denominational tone could be honestly given to schools nominally general. But beyond this mere suggestion of an attempt at a general tone of comprehensiveness in religious teaching it was not intended to go, and only because such was its limitation was it accepted by the Government and by the House.

"But now we are told that it is to be construed as doing precisely that which it refused to do. A 'formulary,' it seems, is a collection of formulas, and formulas are simply propositions of whatever kind touching religious faith. All such propositions, if they cannot be accepted by all Christian denominations, are to be proscribed; and it is added significantly that the Jews also are a denomination, and so that any teaching distinctively Christian is perhaps to be excluded, lest it should interfere with their freedom and rights Are we then to fall back on the simple reading of the letter of the Bible? No! this, it is granted, would be an 'unworthy pretence.' The teacher is to give 'grammatical, geographical, or historical explanations;' but he is to keep clear of 'theology proper,' because, as Professor Huxley takes great pains to prove, there is no theological teaching which is not opposed by some sect or other, from Roman Catholicism on the one hand to Unitarianism on the other. It was not, perhaps, hard to see that this difficulty would be started; and to those who, like Professor Huxley, look at it theoretically, without much practical experience of schools, it may appear serious or unanswerable. But there is very little in it practically; when it is faced determinately and handled firmly, it will soon shrink into its true dimensions. The class who are least frightened at it are the school-teachers, simply because they know most about it. It is quite clear that the school-managers must

be cautioned against allowing their schools to be made places of proselytism: but when this is done, the case is simple enough. Leave the masters under this general understanding to teach freely; if there is ground of complaint, let it be made, but leave the *onus probandi* on the objectors. For extreme peculiarities of belief or unbelief there is the Conscience Clause; as to the mass of parents, they will be more anxious to have religion taught than afraid of its assuming this or that particular shade. They will trust the school-managers and teachers till they have reason to distrust them, and experience has shown that they may trust them safely enough. Any attempt to throw the burden of making the teaching undenominational upon the managers must be sternly resisted: it is simply evading the intentions of the Act in an elaborate attempt to carry them out. We thank Professor Huxley for the warning. To be forewarned is to be forearmed."

A good deal of light seems to me to be thrown on the practical significance of the opinions expressed in the foregoing extract by the following interesting letter, which appeared in the same paper:—

"SIR,—I venture to send to you the substance of a correspondence with the Education Department upon the question of the lawfulness of religious teaching in rate schools under section 14 (2) of the Act. I asked whether the words 'which is distinctive,' &c., taken grammatically as limiting the prohibition of any religious formulary, might be construed as allowing (subject, however, to the other provisions of the Act) any religious formulary common to any two denominations anywhere in England to be taught in such schools; and if practically the limit could not be so extended, but would have to be fixed according to the special circumstances of each district, then what degree of general acceptance in a district would exempt such a formulary from the prohibition? The answer to this was as follows:—'It was understood, when clause 14 of the Education Act was discussed in the House of Commons, that, according to a well-known rule of interpreting Acts of Parliament, "denomination" must be held to include "denominations." When any dispute is referred to the Education Department under the last paragraph of section 16, it will be dealt with according to the circumstances of the case.'

"Upon my asking further if I might hence infer that the lawfulness of teaching any religious formulary in a rate school would thus depend *exclusively* on local circumstances, and would accordingly be so decided by the Education Department in case of dispute, I was informed in

explanation that 'their lordships'' letter was intended to convey to me that no general rule, beyond that stated in the first paragraph of their letter, could at present be laid down by them; and that their decision in each particular case must depend on the special circumstances accompanying it.

"I think it would appear from this that it may yet be in many cases both lawful and expedient to teach religious formularies in rate schools. "H. I.

"STEYNING, *November* 5, 1870."

Of course I do not mean to suggest that the editor of the *Guardian* is bound by the opinions of his correspondent; but I cannot help thinking that I do not misrepresent him, when I say that he also thinks "that it may yet be, in many cases, both lawful and expedient to teach religious formularies in rate schools under these circumstances."

It is not uncharitable, therefore, to assume that, the express words of the Act of Parliament notwithstanding, all the sectaries who are toiling so hard for seats in the London School Board have the lively hope of the gentleman from Steyning, that it may be "both lawful and expedient to teach religious formularies in rate schools;" and that they mean to do their utmost to bring this happy consummation about.[1]

Now the pathetic emotion to which I have referred, as accompanying my contemplations of the violent struggles of so many excellent persons, is caused by the

[1] A passage in an article on the "Working of the Education Act," in the *Saturday Review* for Nov. 19, 1870, completely justifies this anticipation of the line of action which the sectaries mean to take. After commending the Liverpool compromise, the writer goes on to say:—

"If this plan is fairly adopted in Liverpool, the fourteenth clause of the Act will in effect be restored to its original form, and the majority of the ratepayers in each district be permitted to decide to what denomination the school shall belong."

In a previous paragraph the writer speaks of a possible "mistrust" of one another by the members of the Board, and seems to anticipate "accusations of dishonesty." If any of the members of the Board adopt his views, I think it highly probable that he may turn out to be a true prophet.

circumstance that, so far as I can judge, their labour is in vain.

Supposing that the London School Board contains, as it probably will do, a majority of sectaries; and that they carry over the heads of a minority, a resolution that certain theological formulas, about which they all happen to agree,—say, for example, the doctrine of the Trinity,—shall be taught in the schools. Do they fondly imagine that the minority will not at once dispute their interpretation of the Act, and appeal to the Education Department to settle that dispute? And if so, do they suppose that any Minister of Education, who wants to keep his place, will tighten boundaries which the Legislature has left loose; and will give a "final decision" which shall be offensive to every Unitarian and to every Jew in the House of Commons, besides creating a precedent which will afterwards be used to the injury of every Nonconformist? The editor of the *Guardian* tells his friends sternly to resist every attempt to throw the burden of making the teaching undenominational on the managers, and thanks me for the warning I have given him. I return the thanks, with interest, for *his* warning, as to the course the party he represents intends to pursue, and for enabling me thus to draw public attention to a perfectly constitutional and effectual mode of checkmating them.

And, in truth, it is wonderful to note the surprising entanglement into which our able editor gets himself in the struggle between his native honesty and judgment and the necessities of his party. "We could not see," says he, "in the face of this clause how a distinct denominational tone could be honestly given to schools nominally general." There speaks the honest and clear-headed man. "Any attempt to throw the burden of making the teaching undenominational must be sternly

resisted." There speaks the advocate holding a brief for his party. "Verily," as Trinculo says, "the monster hath two mouths:" the one, the forward mouth, tells us very justly that the teaching cannot "honestly" be "distinctly denominational;" but the other, the backward mouth, asserts that it must by no manner of means be "undenominational." Putting the two utterances together, I can only interpret them to mean that the teaching is to be "indistinctly denominational." If the editor of the *Guardian* had not shown signs of anger at my use of the term "theological fog," I should have been tempted to suppose it must have been what he had in his mind, under the name of "indistinct denominationalism." But this reading being plainly inadmissible, I can only imagine that he inculcates the teaching of formulas common to a number of denominations.

But the Education Department has already told the gentleman from Steyning that any such proceeding will be illegal. "According to a well-known rule of interpreting Acts of Parliament, 'denomination' would be held to include 'denominations.'" In other words, we must read the Act thus:—

"No religious catechism or religious formulary which is distinctive of any particular *denominations* shall be taught."

Thus we are really very much indebted to the editor of the *Guardian* and his correspondent. The one has shown us that the sectaries mean to try to get as much denominational teaching as they can agree upon among themselves, forced into the elementary schools; while the other has obtained a formal declaration from the Education Department that any such attempt will contravene the Act of Parliament, and that, therefore, the unsectarian, law-abiding members of the School Boards may safely reckon upon bringing down upon

their opponents the heavy hand of the Minister of Education.[1]

So much for the powers of the School Boards. Limited as they seem to be, it by no means follows that such Boards, if they are composed of intelligent and practical men, really more in earnest about education than about sectarian squabbles, may not exert a very great amount of influence. And, from many circumstances, this is especially likely to be the case with the London School Board, which, if it conducts itself wisely, may become a true educational parliament, as subordinate in authority to the Minister of Education, theoretically, as the Legislature is to the Crown, and yet, like the Legislature, possessed of great practical authority. And I suppose that no Minister of Education would be other than glad to have the aid of the deliberations of such a body, or fail to pay careful attention to its recommendations.

What, then, ought to be the nature and scope of the education which a School Board should endeavour to give to every child under its influence, and for which it should try to obtain the aid of the Parliamentary grants? In my judgment it should include at least the following kinds of instruction and of discipline :—

1. Physical training and drill, as part of the regular business of the school.

It is impossible to insist too much on the importance of this part of education for the children of the poor of great towns. All the conditions of their lives are unfavourable to their physical well-being. They are badly

[1] Since this paragraph was written, Mr. Forster, in speaking at the Birkbeck Institution, has removed all doubt as to what his "final decision" will be in the case of such disputes being referred to him :—"I have the fullest confidence that in the reading and explaining of the Bible, what the children will be taught will be the great truths of Christian life and conduct, which all of us desire they should know, and that no effort will be made to cram into their poor little minds, theological dogmas which their tender age prevents them from understanding."

lodged, badly housed, badly fed, and live from one year's end to another in bad air, without chance of a change. They have no play-grounds; they amuse themselves with marbles and chuck-farthing, instead of cricket or hare-and-hounds; and if it were not for the wonderful instinct which leads all poor children of tender years to run under the feet of cab-horses whenever they can, I know not how they would learn to use their limbs with agility.

Now there is no real difficulty about teaching drill and the simpler kinds of gymnastics. It is done admirably well, for example, in the North Surrey Union schools; and a year or two ago, when I had an opportunity of inspecting these schools, I was greatly struck with the effect of such training upon the poor little waifs and strays of humanity, mostly picked out of the gutter, who are being made into cleanly, healthy, and useful members of society in that excellent institution.

Whatever doubts people may entertain about the efficacy of natural selection, there can be none about artificial selection; and the breeder who should attempt to make, or keep up, a fine stock of pigs, or sheep, under the conditions to which the children of the poor are exposed, would be the laughing-stock even of the bucolic mind. Parliament has already done something in this direction, by declining to be an accomplice in the asphyxiation of school children. It refuses to make any grant to a school in which the cubical contents of the school-room are inadequate to allow of proper respiration. I should like to see it make another step in the same direction, and either refuse to give a grant to a school in which physical training is not a part of the programme, or, at any rate, offer to pay upon such training. If something of the kind is not done, the English physique, which has been, and is still, on the whole, a

grand one, will become as extinct as the dodo, in the great towns.

And then the moral and intellectual effect of drill, as an introduction to, and aid of, all other sorts of training, must not be overlooked. If you want to break in a colt, surely the first thing to do is to catch him and get him quietly to face his trainer; to know his voice and bear his hand; to learn that colts have something else to do with their heels than to kick them up whenever they feel so inclined; and to discover that the dreadful human figure has no desire to devour, or even to beat him, but that, in case of attention and obedience, he may hope for patting and even a sieve of oats.

But, your "street Arabs," and other neglected poor children, are rather worse and wilder than colts; for the reason that the horse-colt has only his animal instincts in him, and his mother, the mare, has been always tender over him, and never came home drunk and kicked him in her life; while the man-colt is inspired by that very real devil, perverted manhood, and *his* mother may have done all that and more. So, on the whole, it may probably be even more expedient to begin your attempt to get at the higher nature of the child, than at that of the colt, from the physical side.

2. Next in order to physical training I put the instruction of children, and especially of girls, in the elements of household work and of domestic economy; in the first place for their own sakes, and in the second for that of their future employers.

Everyone who knows anything of the life of the English poor is aware of the misery and waste caused by their want of knowledge of domestic economy, and by their lack of habits of frugality and method. I suppose it is no exaggeration to say that a poor Frenchwoman would make the money which the wife of a poor

Englishman spends in food go twice as far, and at the same time turn out twice as palatable a dinner. Why Englishmen, who are so notoriously fond of good living, should be so helplessly incompetent in the art of cookery, is one of the great mysteries of nature; but from the varied abominations of the railway refreshment-rooms to the monotonous dinners of the poor, English feeding is either wasteful or nasty, or both.

And as to domestic service, the groans of the housewives of England ascend to heaven! In five cases out of six, the girl who takes a "place" has to be trained by her mistress in the first rudiments of decency and order; and it is a mercy if she does not turn up her nose at anything like the mention of an honest and proper economy. Thousands of young girls are said to starve, or worse, yearly in London; and at the same time thousands of mistresses of households are ready to pay high wages for a decent housemaid, or cook, or a fair workwoman; and can by no means get what they want.

Surely, if the elementary schools are worth anything, they may put an end to a state of things which is demoralizing the poor, while it is wasting the lives of those better off in small worries and annoyances.

3. But the boys and girls for whose education the School Boards have to provide, have not merely to discharge domestic duties, but each of them is a member of a social and political organization of great complexity, and has, in future life, to fit himself into that organization, or be crushed by it. To this end it is surely needful, not only that they should be made acquainted with the elementary laws of conduct, but that their affections should be trained, so as to love with all their hearts that conduct which tends to the attainment of the highest good for themselves and their fellow-men,

and to hate with all their hearts that opposite course of action which is fraught with evil.

So far as the laws of conduct are determined by the intellect, I apprehend that they belong to science, and to that part of science which is called morality. But the engagement of the affections in favour of that particular kind of conduct which we call good, seems to me to be something quite beyond mere science. And I cannot but think that it, together with the awe and reverence, which have no kinship with base fear, but arise whenever one tries to pierce below the surface of things, whether they be material or spiritual, constitutes all that has any unchangeable reality in religion.

And just as I think it would be a mistake to confound the science, morality, with the affection, religion; so do I conceive it to be a most lamentable and mischievous error, that the science, theology, is so confounded in the minds of many—indeed, I might say, of the majority of men.

I do not express any opinion as to whether theology is a true science, or whether it does not come under the apostolic definition of "science falsely so called;" though I may be permitted to express the belief that if the Apostle to whom that much misapplied phrase is due could make the acquaintance of much of modern theology, he would not hesitate a moment in declaring that it is exactly what he meant the words to denote.

But it is at any rate conceivable, that the nature of the Deity, and His relations to the universe, and more especially to mankind, are capable of being ascertained, either inductively or deductively, or by both processes. And, if they have been ascertained, then a body of science has been formed which is very properly called theology.

Further, there can be no doubt that affection for the Being thus defined and described by theologic science

would be properly termed religion; but it would not be the whole of religion. The affection for the ethical ideal defined by moral science would claim equal if not superior rights. For suppose theology established the existence of an evil deity—and some theologies, even Christian ones, have come very near this,—is the religious affection to be transferred from the ethical ideal to any such omnipotent demon? I trow not. Better a thousand times that the human race should perish under his thunderbolts than it should say, "Evil, be thou my good."

There is nothing new, that I know of, in this statement of the relations of religion with the science of morality on the one hand and that of theology on the other. But I believe it to be altogether true, and very needful, at this time, to be clearly and emphatically recognized as such, by those who have to deal with the education question.

We are divided into two parties—the advocates of so-called "religious" teaching on the one hand, and those of so-called "secular" teaching on the other. And both parties seem to me to be not only hopelessly wrong, but in such a position that if either succeeded completely, it would discover, before many years were over, that it had made a great mistake and done serious evil to the cause of education.

For, leaving aside the more far-seeing minority on each side, what the "religious" party is crying for is mere theology, under the name of religion; while the "secularists" have unwisely and wrongfully admitted the assumption of their opponents, and demand the abolition of all "religious" teaching, when they only want to be free of theology—Burning your ship to get rid of the cockroaches!

But my belief is, that no human being, and no society

composed of human beings, ever did, or ever will, come to much, unless their conduct was governed and guided by the love of some ethical ideal. Undoubtedly, your gutter child may be converted by mere intellectual drill into "the subtlest of all the beasts of the field;" but we know what has become of the original of that description, and there is no need to increase the number of those who imitate him successfully without being aided by the rates. And if I were compelled to choose for one of my own children, between a school in which real religious instruction is given, and one without it, I should prefer the former, even though the child might have to take a good deal of theology with it. Nine-tenths of a dose of bark is mere half-rotten wood; but one swallows it for the sake of the particles of quinine, the beneficial effect of which may be weakened, but is not destroyed, by the wooden dilution, unless in a few cases of exceptionally tender stomachs.

Hence, when the great mass of the English people declare that they want to have the children in the elementary schools taught the Bible, and when it is plain from the terms of the Act, the debates in and out of Parliament, and especially the emphatic declarations of the Vice-President of the Council, that it was intended that such Bible-reading should be permitted, unless good cause for prohibiting it could be shown, I do not see what reason there is for opposing that wish. Certainly, I, individually, could with no shadow of consistency oppose the teaching of the children of other people to do that which my own children are taught to do. And, even if the reading the Bible were not, as I think it is, consonant with political reason and justice, and with a desire to act in the spirit of the education measure, I am disposed to think it might still be well to read that book in the elementary schools.

I have always been strongly in favour of secular education, in the sense of education without theology; but I must confess I have been no less seriously perplexed to know by what practical measures the religious feeling, which is the essential basis of conduct, was to be kept up, in the present utterly chaotic state of opinion on these matters, without the use of the Bible. The Pagan moralists lack life and colour, and even the noble Stoic, Marcus Antoninus, is too high and refined for an ordinary child. Take the Bible as a whole; make the severest deductions which fair criticism can dictate for shortcomings and positive errors; eliminate, as a sensible lay-teacher would do, if left to himself, all that it is not desirable for children to occupy themselves with; and there still remains in this old literature a vast residuum of moral beauty and grandeur. And then consider the great historical fact that, for three centuries, this book has been woven into the life of all that is best and noblest in English history; that it has become the national epic of Britain, and is as familiar to noble and simple, from John-o'-Groat's House to Land's End, as Dante and Tasso once were to the Italians; that it is written in the noblest and purest English, and abounds in exquisite beauties of mere literary form; and, finally, that it forbids the veriest hind who never left his village to be ignorant of the existence of other countries and other civilizations, and of a great past, stretching back to the furthest limits of the oldest nations in the world. By the study of what other book could children be so much humanized and made to feel that each figure in that vast historical procession fills, like themselves, but a momentary space in the interval between two eternities; and earns the blessings or the curses of all time, according to its effort to do good and hate evil, even as they also are earning their payment for their work?

On the whole, then, I am in favour of reading the Bible, with such grammatical, geographical, and historical explanations by a lay-teacher as may be needful, with rigid exclusion of any further theological teaching than that contained in the Bible itself. And in stating what this is, the teacher would do well not to go beyond the precise words of the Bible; for if he does, he will, in the first place, undertake a task beyond his strength, seeing that all the Jewish and Christian sects have been at work upon that subject for more than two thousand years, and have not yet arrived, and are not in the least likely to arrive, at an agreement; and, in the second place, he will certainly begin to teach something distinctively denominational, and thereby come into violent collision with the Act of Parliament.

4. The intellectual training to be given in the elementary schools must of course, in the first place, consist in learning to use the means of acquiring knowledge, or reading, writing, and arithmetic; and it will be a great matter to teach reading so completely that the act shall have become easy and pleasant. If reading remains "hard," that accomplishment will not be much resorted to for instruction, and still less for amusement—which last is one of its most valuable uses to hard-worked people.

But along with a due proficiency in the use of the means of learning, a certain amount of knowledge, of intellectual discipline, and of artistic training should be conveyed in the elementary schools; and in this direction—for reasons which I am afraid to repeat, having urged them so often—I can conceive no subject-matter of education so appropriate and so important as the rudiments of physical science, with drawing, modelling, and singing. Not only would such teaching afford the best possible preparation for the technical schools about

which so much is now said, but the organization for carrying it into effect already exists. The Science and Art Department, the operations of which have already attained considerable magnitude, not only offers to examine and pay the results of such examination in elementary science and art, but it provides what is still more important, viz. a means of giving children of high natural ability, who are just as abundant among the poor as among the rich, a helping hand. A good old proverb tells us that "One should not take a razor to cut a block:" the razor is soon spoiled, and the block is not so well cut as it would be with a hatchet. But it is worse economy to prevent a possible Watt from being anything but a stoker, or to give a possible Faraday no chance of doing anything but to bind books. Indeed, the loss in such cases of mistaken vocation has no measure; it is absolutely infinite and irreparable. And among the arguments in favour of the interference of the State in education, none seems to be stronger than this—that it is the interest of every one that ability should be neither wasted, nor misapplied, by any one; and, therefore, that every one's representative, the State, is necessarily fulfilling the wishes of its constituents when it is helping the capacities to reach their proper places.

It may be said that the scheme of education here sketched is too large to be effected in the time during which the children will remain at school; and, secondly, that even if this objection did not exist, it would cost too much.

I attach no importance whatever to the first objection until the experiment has been fairly tried. Considering how much catechism, lists of the kings of Israel, geography of Palestine, and the like, children are made to swallow now, I cannot believe there will be any

difficulty in inducing them to go through the physical training, which is more than half play; or the instruction in household work, or in those duties to one another and to themselves, which have a daily and hourly practical interest. That children take kindly to elementary science and art no one can doubt who has tried the experiment properly. And if Bible-reading is not accompanied by constraint and solemnity, as if it were a sacramental operation, I do not believe there is anything in which children take more pleasure. At least I know that some of the pleasantest recollections of my childhood are connected with the voluntary study of an ancient Bible which belonged to my grandmother. There were splendid pictures in it, to be sure; but I recollect little or nothing about them save a portrait of the high priest in his vestments. What come vividly back on my mind are remembrances of my delight in the histories of Joseph and of David; and of my keen appreciation of the chivalrous kindness of Abraham in his dealings with Lot. Like a sudden flash there returns back upon me, my utter scorn of the pettifogging meanness of Jacob, and my sympathetic grief over the heartbreaking lamentation of the cheated Esau, "Hast thou not a blessing for me also, O my father?" And I see, as in a cloud, pictures of the grand phantasmagoria of the Book of Revelation.

I enumerate, as they issue, the childish impressions which come crowding out of the pigeon-holes in my brain, in which they have lain almost undisturbed for forty years. I prize them as an evidence that a child of five or six years old, left to his own devices, may be deeply interested in the Bible, and draw sound moral sustenance from it. And I rejoice that I was left to deal with the Bible alone; for if I had had some theologica. "explainer" at my side, he might have tried, as such

do, to lessen my indignation against Jacob, and thereby have warped my moral sense for ever; while the great apocalyptic spectacle of the ultimate triumph of right and justice might have been turned to the base purposes of a pious lampooner of the Papacy.

And as to the second objection—costliness—the reply is, first, that the rate and the Parliamentary grant together ought to be enough, considering that science and art teaching is already provided for; and, secondly, that if they are not, it may be well for the educational parliament to consider what has become of those endowments which were originally intended to be devoted, more or less largely, to the education of the poor.

When the monasteries were spoiled, some of their endowments were applied to the foundation of cathedrals; and in all such cases it was ordered that a certain portion of the endowment should be applied to the purposes of education. How much is so applied? Is that which may be so applied given to help the poor, who cannot pay for education, or does it virtually subsidize the comparatively rich, who can? How are Christ's Hospital and Alleyn's foundation securing their right purposes, or how far are they perverted into contrivances for affording relief to the classes who can afford to pay for education? How —— But this paper is already too long, and, if I begin, I may find it hard to stop asking questions of this kind, which after all are worthy only of the lowest of Radicals.

III.

ON MEDICAL EDUCATION.

(AN ADDRESS TO THE STUDENTS OF THE FACULTY OF MEDICINE IN UNIVERSITY COLLEGE, LONDON, MAY 18, 1870, ON THE OCCASION OF THE DISTRIBUTION OF PRIZES FOR THE SESSION.)

It has given me sincere pleasure to be here to-day, at the desire of your highly respected President and the Council of the College. In looking back upon my own past, I am sorry to say that I have found that it is a quarter of a century since I took part in those hopes and in those fears by which you have all recently been agitated, and which now are at an end. But, although so long a time has elapsed since I was moved by the same feelings, I beg leave to assure you that my sympathy with both victors and vanquished remains fresh—so fresh, indeed, that I could almost try to persuade myself that, after all, it cannot be so very long ago. My business during the last hour, however, has been to show that sympathy with one side only, and I assure you I have done my best to play my part heartily, and to rejoice in the success of those who have succeeded. Still, I should like to remind you at the end of it all, that success on an occasion of this kind, valuable and important as it is, is in reality only putting the foot

upon one rung of the ladder which leads upwards ; and that the rung of a ladder was never meant to rest upon, but only to hold a man's foot long enough to enable him to put the other somewhat higher. I trust that you will all regard these successes as simply reminders that your next business is, having enjoyed the success of the day, no longer to look at that success, but to look forward to the next difficulty that is to be conquered. And now, having had so much to say to the successful candidates, you must forgive me if I add that a sort of under-current of sympathy has been going on in my mind all the time for those who have not been successful, for those valiant knights who have been overthrown in your tourney, and have not made their appearance in public. I trust that, in accordance with old custom, they, wounded and bleeding, have been carried off to their tents, to be carefully tended by the fairest of maidens ; and in these days, when the chances are that every one of such maidens will be a qualified practitioner, I have no doubt that all the splinters will have been carefully extracted, and that they are now physically healed. But there may remain some little fragment of moral or intellectual discouragement, and therefore I will take the liberty to remark that your chairman to-day, if he occupied his proper place, would be among them. Your chairman, in virtue of his position, and for the brief hour that he occupies that position, is a person of importance ; and it may be some consolation to those who have failed if I say, that the quarter of a century which I have been speaking of, takes me back to the time when I was up at the University of London, a candidate for honours in anatomy and physiology, and when I was exceedingly well beaten by my excellent friend Dr. Ransom, of Nottingham. There is a person here who recollects that circumstance very well. I refer to your venerated

teacher and mine, Dr. Sharpey. He was at that time one of the examiners in anatomy and physiology, and you may be quite sure that, as he was one of the examiners, there remained not the smallest doubt in my mind of the propriety of his judgment, and I accepted my defeat with the most comfortable assurance that I had thoroughly well earned it. But, gentlemen, the competitor having been a worthy one, and the examination a fair one, I cannot say that I found in that circumstance anything very discouraging. I said to myself, "Never mind; what's the next thing to be done?" And I found that policy of "never minding" and going on to the next thing to be done, to be the most important of all policies in the conduct of practical life. It does not matter how many tumbles you have in this life, so long as you do not get dirty when you tumble; it is only the people who have to stop to be washed and made clean, who must necessarily lose the race. And I can assure you that there is the greatest practical benefit in making a few failures early in life. You learn that which is of inestimable importance—that there are a great many people in the world who are just as clever as you are. You learn to put your trust, by and by, in an economy and frugality of the exercise of your powers, both moral and intellectual; and you very soon find out, if you have not found it out before, that patience and tenacity of purpose are worth more than twice their weight of cleverness. In fact, if I were to go on discoursing on this subject, I should become almost eloquent in praise of non-success; but, lest so doing should seem, in any way, to wither well-earned laurels, I will turn from that topic, and ask you to accompany me in some considerations touching another subject which has a very profound interest for me, and which I think ought to have an equally profound interest for you.

I presume that the great majority of those whom I address propose to devote themselves to the profession of medicine; and I do not doubt, from the evidences of ability which have been given to-day, that I have before me a number of men who will rise to eminence in that profession, and who will exert a great and deserved influence upon its future. That in which I am interested, and about which I wish to speak, is the subject of medical education, and I venture to speak about it for the purpose, if I can, of influencing you, who may have the power of influencing the medical education of the future. You may ask, by what authority do I venture, being a person not concerned in the practice of medicine, to meddle with that subject? I can only tell you it is a fact, of which a number of you I dare say are aware by experience (and I trust the experience has no painful associations), that I have been for a considerable number of years (twelve or thirteen years to the best of my recollection) one of the examiners in the University of London. You are further aware that the men who come up to the University of London are the picked men of the medical schools of London, and therefore such observations as I may have to make upon the state of knowledge of these gentlemen, if they be justified, in regard to any faults I may have to find, cannot be held to indicate defects in the capacity, or in the power of application of those gentlemen, but must be laid, more or less, to the account of the prevalent system of medical education. I will tell you what has struck me—but in speaking in this frank way, as one always does about the defects of one's friends, I must beg you to disabuse your minds of the notion that I am alluding to any particular school, or to any particular college, or to any particular person; and to believe that if I am silent when I should be glad to speak with high praise, it is because that

praise would come too close to this locality. What has struck me, then, in this long experience of the men best instructed in physiology from the medical schools of London, is (with the many and brilliant exceptions to which I have referred), taking it as a whole, and broadly, the singular unreality of their knowledge of physiology. Now, I use that word "unreality" advisedly: I do not say "scanty;" on the contrary, there is plenty of it—a great deal too much of it—but it is the quality, the nature of the knowledge, which I quarrel with. I know I used to have—I don't know whether I have now, but I had once upon a time—a bad reputation among students for setting up a very high standard of acquirement, and I dare say you may think that the standard of this old examiner, who happily is now very nearly an extinct examiner, has been pitched too high. Nothing of the kind, I assure you. The defects I have noticed, and the faults I have to find, arise entirely from the circumstance that my standard is pitched too low. This is no paradox, gentlemen, but quite simply the fact. The knowledge I have looked for was a real, precise, thorough, and practical knowledge of fundamentals; whereas that which the best of the candidates, in a large proportion of cases, have had to give me was a large, extensive, and inaccurate knowledge of superstructure; and that is what I mean by saying that my demands went too low, and not too high. What I have had to complain of is, that a large proportion of the gentlemen who come up for physiology to the University of London do not know it as they know their anatomy, and have not been taught it as they have been taught their anatomy. Now, I should not wonder at all if I heard a great many "No, noes" here; but I am not talking about University College; as I have told you before, I am talking about the average education of medical

schools. What I have found, and found so much reason to lament, is, that while anatomy has been taught as a science ought to be taught, as a matter of autopsy, and observation, and strict discipline; in a very large number of cases, physiology has been taught as if it were a mere matter of books and of hearsay. I declare to you, gentlemen, that I have often expected to be told, when I have been asked a question about the circulation of the blood, that Professor Breitkopf is of opinion that it circulates, but that the whole thing is an open question. I assure you that I am hardly exaggerating the state of mind on matters of fundamental importance which I have found over and over again to obtain among gentlemen coming up to that picked examination of the University of London. Now, I do not think that is a desirable state of things. I cannot understand why physiology should not be taught—in fact, you have here abundant evidence that it can be taught—with the same definiteness and the same precision as anatomy is taught. And you may depend upon this, that the only physiology which is to be of any good whatever in medical practice, or in its application to the study of medicine, is that physiology which a man knows of his own knowledge; just as the only anatomy which would be of any good to the surgeon is the anatomy which he knows of his own knowledge. Another peculiarity I have found in the physiology which has been current, and that is, that in the minds of a great many gentlemen it has been supplanted by histology. They have learnt a great deal of histology, and they have fancied that histology and physiology are the same things. I have asked for some knowledge of the physics and the mechanics and the chemistry of the human body, and I have been met by talk about cells. I declare to you I believe it will take me two years, at least, of absolute rest from the business

of an examiner to hear the word "cell," "germinal matter," or "carmine," without a sort of inward shudder.

Well, now, gentlemen, I am sure my colleagues in this examination will bear me out in saying that I have not been exaggerating the evils and defects which are current —have been current—in a large quantity of the physiological teaching, the results of which come before examiners. And it becomes a very interesting question to know how all this comes about, and in what way it can be remedied. How it comes about will be perfectly obvious to any one who has considered the growth of medicine. I suppose that medicine and surgery first began by some savage, more intelligent than the rest, discovering that a certain herb was good for a certain pain, and that a certain pull, somehow or other, set a dislocated joint right. I suppose all things had their humble beginnings, and medicine and surgery were in the same condition. People who wear watches know nothing about watchmaking. A watch goes wrong and it stops; you see the owner giving it a shake, or, if he is very bold, he opens the case, and gives the balance-wheel a turn. Gentlemen, that is empirical practice, and you know what are the results upon the watch. I should think you can divine what are the results of analogous operations upon the human body. And because men of sense very soon found that such were the effects of meddling with very complicated machinery they did not understand, I suppose the first thing, as being the easiest, was to study the nature of the works of the human watch, and the next thing was to study the way the parts worked together, and the way the watch worked. Thus, by degrees, we have had growing up our body of anatomists, or knowers of the construction of the human watch, and our physiologists, who know how the

machine works. And just as any sensible man, who has a valuable watch, does not meddle with it himself, but goes to some one who has studied watchmaking, and understands what the effect of doing this or that may be; so, I suppose, the man who, having charge of that valuable machine, his own body, wants to have it kept in good order, comes to a professor of the medical art for the purpose of having it set right, believing that, by deduction from the facts of structure and from the facts of function, the physician will divine what may be the matter with his bodily watch at that particular time, and what may be the best means of setting it right. If that may be taken as a just representation of the relation of the theoretical branches of medicine—what we may call the institutes of medicine, to use an old term—to the practical branches, I think it will be obvious to you that they are of prime and fundamental importance. Whatever tends to affect the teaching of them injuriously must tend to destroy and to disorganize the whole fabric of the medical art. I think every sensible man has seen this long ago; but the difficulties in the way of attaining good teaching in the different branches of the theory, or institutes, of medicine are very serious. It is a comparatively easy matter—pray mark that I use the word "comparatively"—it is a comparatively easy matter to learn anatomy and to teach it; it is a very difficult matter to learn physiology and to teach it. It is a very difficult matter to know and to teach those branches of physics and those branches of chemistry which bear directly upon physiology; and hence it is that, as a matter of fact, the teaching of physiology, and the teaching of the physics and the chemistry which bear upon it, must necessarily be in a state of relative imperfection; and there is nothing to be grumbled at in the fact that this relative imperfection

exists. But is the relative imperfection which exists only such as is necessary, or is it made worse by our practical arrangements? I believe—and if I did not so believe I should not have troubled you with these observations—I believe it is made infinitely worse by our practical arrangements, or rather, I ought to say, our very unpractical arrangements. Some very wise man long ago affirmed that every question, in the long run, was a question of finance; and there is a good deal to be said for that view. Most assuredly the question of medical teaching is, in a very large and broad sense, a question of finance. What I mean is this: that in London the arrangements of the medical schools, and the number of them, are such as to render it almost impossible that men who confine themselves to the teaching of the theoretical branches of the profession should be able to make their bread by that operation; and, you know, if a man cannot make his bread, he cannot teach—at least his teaching comes to a speedy end. That is a matter of physiology. Anatomy is fairly well taught, because it lies in the direction of practice, and a man is all the better surgeon for being a good anatomist. It does not absolutely interfere with the pursuits of a practical surgeon if he should hold a Chair of Anatomy—though I do not for one moment say that he would not be a better teacher if he did not devote himself to practice. (Applause.) Yes, I know exactly what that cheer means, but I am keeping as carefully as possible from any sort of allusion to Professor Ellis. But the fact is, that even human anatomy has now grown to be so large a matter, that it takes the whole devotion of a man's life to put the great mass of knowledge upon that subject into such a shape that it can be teachable to the mind of the ordinary student. What the student wants in a professor is a man who shall stand between him and the

infinite diversity and variety of human knowledge, and who shall gather all that together, and extract from it that which is capable of being assimilated by the mind. That function is a vast and an important one, and unless, in such subjects as anatomy, a man is wholly free from other cares, it is almost impossible that he can perform it thoroughly and well. But if it be hardly possible for a man to pursue anatomy without actually breaking with his profession, how is it possible for him to pursue physiology?

I get every year those very elaborate reports of Henle and Meissner—volumes of, I suppose, 400 pages altogether—and they consist merely of abstracts of the memoirs and works which have been written on Anatomy and Physiology — only abstracts of them! How is a man to keep up his acquaintance with all that is doing in the physiological world—in a world advancing with enormous strides every day and every hour—if he has to be distracted with the cares of practice? You know very well it must be impracticable to do so. Our men of ability join our medical schools with an eye to the future. They take the Chairs of Anatomy or of Physiology; and by and by they leave those Chairs for the more profitable pursuits into which they have drifted by professional success, and so they become clothed, and physiology is bare. The result is, that in those schools in which physiology is thus left to the benevolence, so to speak, of those who have no time to look to it, the effect of such teaching comes out obviously, and is made manifest in what I spoke of just now—the unreality, the bookishness of the knowledge of the taught. And if this is the case in physiology, still more must it be the case in those branches of physics which are the foundation of physiology; although it may be less the case in chemistry, because for an able chemist a certain

honourable and independent career lies in the direction of his work, and he is able, like the anatomist, to look upon what he may teach to the student as not absolutely taking him away from his bread-winning pursuits.

But it is of no use to grumble about this state of things unless one is prepared to indicate some sort of practical remedy. And I believe—and I venture to make the statement because I am wholly independent of all sorts of medical schools, and may, therefore, say what I believe without being supposed to be affected by any personal interest—but I say I believe that the remedy for this state of things, for that imperfection of our theoretical knowledge which keeps down the ability of England at the present time in medical matters, is a mere affair of mechanical arrangement; that so long as you have a dozen medical schools scattered about in different parts of the metropolis, and dividing the students among them, so long, in all the smaller schools at any rate, it is impossible that any other state of things than that which I have been depicting should obtain. Professors must live; to live they must occupy themselves with practice, and if they occupy themselves with practice, the pursuit of the abstract branches of science must go to the wall. All this is a plain and obvious matter of common-sense reasoning. I believe you will never alter this state of things until, either by consent or by *force majeure*—and I should be very sorry to see the latter applied—but until there is some new arrangement, and until all the theoretical branches of the profession, the institutes of medicine, are taught in London in not more than one or two, or at the outside three, central institutions, no good will be effected. If that large body of men, the medical students of London, were obliged in the first place to get a knowledge of the theoretical branches of their

profession in two or three central schools, there would be abundant means for maintaining able professors—not, indeed, for enriching them, as they would be able to enrich themselves by practice—but for enabling them to make that choice which such men are so willing to make; namely, the choice between wealth and a modest competency, when that modest competency is to be combined with a scientific career, and the means of advancing knowledge. I do not believe that all the talking about, and tinkering of, medical education will do the slightest good until the fact is clearly recognized, that men must be thoroughly grounded in the theoretical branches of their profession, and that to this end the teaching of those theoretical branches must be confined to two or three centres.

Now let me add one other word, and that is, that if I were a despot, I would cut down these branches to a very considerable extent. The next thing to be done beyond that which I mentioned just now, is to go back to primary education. The great step towards a thorough medical education is to insist upon the teaching of the elements of the physical sciences in all schools, so that medical students shall not go up to the medical colleges utterly ignorant of that with which they have to deal; to insist on the elements of chemistry, the elements of botany, and the elements of physics being taught in our ordinary and common schools, so that there shall be some preparation for the discipline of medical colleges. And, if this reform were once effected, you might confine the "Institutes of Medicine" to physics as applied to physiology—to chemistry as applied to physiology—to physiology itself, and to anatomy. Afterwards, the student, thoroughly grounded in these matters, might go to any hospital he pleased for the purpose of studying the practical branches of his profession. The practical

teaching might be made as local as you like; and you might use to advantage the opportunities afforded by all these local institutions for acquiring a knowledge of the practice of the profession. But you may say: "This is abolishing a great deal; you are getting rid of botany and zoology to begin with." I have not a doubt that they ought to be got rid of, as branches of special medical education; they ought to be put back to an earlier stage, and made branches of general education. Let me say, by way of self-denying ordinance, for which you will, I am sure, give me credit, that I believe that comparative anatomy ought to be absolutely abolished. I say so, not without a certain fear of the Vice-Chancellor of the University of London who sits upon my left. But I do not think the charter gives him very much power over me; moreover, I shall soon come to an end of my examinership, and therefore I am not afraid, but shall go on to say what I was going to say, and that is, that in my belief it is a downright cruelty—I have no other word for it—to require from gentlemen who are engaged in medical studies, the pretence—for it is nothing else, and can be nothing else, than a pretence—of a knowledge of comparative anatomy as part of their medical curriculum. Make it part of their Arts teaching if you like, make it part of their general education if you like, make it part of their qualification for the scientific degree by all means—that is its proper place; but to require that gentlemen whose whole faculties should be bent upon the acquirement of a real knowledge of human physiology should worry themselves with getting up hearsay about the alternation of generations in the Salpæ is really monstrous. I cannot characterize it in any other way. And having sacrificed my own pursuit, I am sure I may sacrifice other people's; and I make this remark with all the more willingness because I discovered, on

reading the name of your Professors just now, that the Professor of Materia Medica is not present. I must confess, if I had my way I should abolish Materia Medica[1] altogether. I recollect, when I was first under examination at the University of London, Dr. Pereira was the examiner, and you know that "Pereira's Materia Medica" was a book *de omnibus rebus*. I recollect my struggles with that book late at night and early in the morning (I worked very hard in those days), and I do believe that I got that book into my head somehow or other, but then I will undertake to say that I forgot it all a week afterwards. Not one trace of a knowledge of drugs has remained in my memory from that time to this; and really, as a matter of common sense, I cannot understand the arguments for obliging a medical man to know all about drugs and where they come from. Why not make him belong to the Iron and Steel Institute, and learn something about cutlery, because he uses knives?

But do not suppose that, after all these deductions, there would not be ample room for your activity. Let us count up what we have left. I suppose all the time for medical education that can be hoped for is, at the outside, about four years. Well, what have you to master in those four years upon my supposition? Physics applied to physiology; chemistry applied to physiology; physiology; anatomy; surgery; medicine (including therapeutics); obstetrics; hygiene; and medical jurisprudence—nine subjects for four years! And when you consider what those subjects are, and that the acquisition of anything beyond the rudiments of any one of them may tax the energies of a lifetime, I think that even those energies which you young gentlemen have been displaying for the last hour or two might

[1] It will, I hope, be understood that I do not include Therapeutics under this head.

be taxed to keep you thoroughly up to what is wanted for your medical career.

I entertain a very strong conviction that any one who adds to medical education one iota or tittle beyond what is absolutely necessary, is guilty of a very grave offence. Gentlemen, it will depend upon the knowledge that you happen to possess,—upon your means of applying it within your own field of action,—whether the bills of mortality of your district are increased or diminished; and that, gentlemen, is a very serious consideration indeed. And, under those circumstances, the subjects with which you have to deal being so difficult, their extent so enormous, and the time at your disposal so limited, I could not feel my conscience easy if I did not, on such an occasion as this, raise a protest against employing your energies upon the acquisition of any knowledge which may not be absolutely needed in your future career.

IV.

YEAST.

It has been known, from time immemorial, that the sweet liquids which may be obtained by expressing the juices of the fruits and stems of various plants, or by steeping malted barley in hot water, or by mixing honey with water—are liable to undergo a series of very singular changes, if freely exposed to the air and left to themselves, in warm weather. However clear and pellucid the liquid may have been when first prepared, however carefully it may have been freed, by straining and filtration, from even the finest visible impurities, it will not remain clear. After a time it will become cloudy and turbid; little bubbles will be seen rising to the surface, and their abundance will increase until the liquid hisses as if it were simmering on the fire. By degrees, some of the solid particles which produce the turbidity of the liquid collect at its surface into a scum, which is blown up by the emerging air-bubbles into a thick, foamy froth. Another moiety sinks to the bottom, and accumulates as a muddy sediment, or "lees."

When this action has continued, with more or less violence, for a certain time, it gradually moderates. The evolution of bubbles slackens, and finally comes to an

end; scum and lees alike settle at the bottom, and the fluid is once more clear and transparent. But it has acquired properties of which no trace existed in the original liquid. Instead of being a mere sweet fluid, mainly composed of sugar and water, the sugar has more or less completely disappeared, and it has acquired that peculiar smell and taste which we call "spirituous." Instead of being devoid of any obvious effect upon the animal economy, it has become possessed of a very wonderful influence on the nervous system; so that in small doses it exhilarates, while in larger it stupefies, and may even destroy life.

Moreover, if the original fluid is put into a still, and heated for a while, the first and last product of its distillation is simple water; while, when the altered fluid is subjected to the same process, the matter which is first condensed in the receiver is found to be a clear, volatile substance, which is lighter than water, has a pungent taste and smell, possesses the intoxicating powers of the fluid in an eminent degree, and takes fire the moment it is brought in contact with a flame. The alchemists called this volatile liquid, which they obtained from wine, "spirits of wine," just as they called hydrochloric acid "spirits of salt," and as we, to this day, call refined turpentine "spirits of turpentine." As the "spiritus," or breath, of a man was thought to be the most refined and subtle part of him, the intelligent essence of man was also conceived as a sort of breath, or spirit; and, by analogy, the most refined essence of anything was called its "spirit." And thus it has come about that we use the same word for the soul of man and for a glass of gin.

At the present day, however, we even more commonly use another name for this peculiar liquid—namely, "alcohol," and its origin is not less singular. The Dutch

physician, Van Helmont, lived in the latter part of the sixteenth and the beginning of the seventeenth century —in the transition period between alchemy and chemistry —and was rather more alchemist than chemist. Appended to his "Opera Omnia," published in 1707, there is a very needful "Clavis ad obscuriorum sensum referandum," in which the following passage occurs :—

"ALCOHOL.—Chymicis est liquor aut pulvis summè subtilisatus, vocabulo Orientalibus quoque, cum primis Habessinis, familiari, quibus *cohol* speciatim pulverem impalpabilem ex antimonio pro oculis tingendis denotat. . . Hodie autem, ob analogiam, quivis pulvis tenerior, ut pulvis oculorum cancri summè subtilisatus *alcohol* audit, haud aliter ac spiritus rectificatissimi *alcolisati* dicuntur."

Similarly, Robert Boyle speaks of a fine powder as "alcohol;" and, so late as the middle of the last century, the English lexicographer, Nathan Bailey, defines "alcohol" as "the pure substance of anything separated from the more gross, a very fine and impalpable powder, or a very pure, well-rectified spirit." But, by the time of the publication of Lavoisier's "Traité Élémentaire de Chimie," in 1789, the term "alcohol," "alkohol," or "alkool" (for it is spelt in all three ways), which Van Helmont had applied primarily to a fine powder, and only secondarily to spirits of wine, had lost its primary meaning altogether; and, from the end of the last century until now, it has, I believe, been used exclusively as the denotation of spirits of wine, and bodies chemically allied to that substance.

The process which gives rise to alcohol in a saccharine fluid is known to us as "fermentation;" a term based upon the apparent boiling up or "effervescence" of the fermenting liquid, and of Latin origin.

Our Teutonic cousins call the same process "gähren," "gäsen," "göschen," and "gischen;" but, oddly enough, we do not seem to have retained their verb or their

substantive denoting the action itself, though we do use names identical with, or plainly derived from, theirs for the scum and lees. These are called, in Low German, "gäscht" and "gischt;" in Anglo-Saxon, "gest," "gist," and "yst," whence our "yeast." Again, in Low German and in Anglo-Saxon, there is another name for yeast, having the form "barm," or "beorm;" and, in the Midland Counties, "barm" is the name by which yeast is still best known. In High German, there is a third name for yeast, "hefe," which is not represented in English, so far as I know.

All these words are said by philologers to be derived from roots expressive of the intestine motion of a fermenting substance. Thus "hefe" is derived from "heben," to raise; "barm" from "beren" or "bären," to bear up; "yeast," "yst," and "gist," have all to do with seething and foam, with "yeasty waves," and "gusty" breezes.

The same reference to the swelling up of the fermenting substance is seen in the Gallo-Latin terms "levure" and "leaven."

It is highly creditable to the ingenuity of our ancestors that the peculiar property of fermented liquids, in virtue of which they "make glad the heart of man," seems to have been known in the remotest periods of which we have any record. All savages take to alcoholic fluids as if they were to the manner born. Our Vedic forefathers intoxicated themselves with the juice of the "soma;" Noah, by a not unnatural reaction against a superfluity of water, appears to have taken the earliest practicable opportunity of qualifying that which he was obliged to drink; and the ghosts of the ancient Egyptians were solaced by pictures of banquets in which the wine-cup passes round, graven on the walls of their tombs. A knowledge of the process of fermentation, therefore,

was in all probability possessed by the prehistoric populations of the globe; and it must have become a matter of great interest even to primæval wine-bibbers to study the methods by which fermented liquids could be surely manufactured. No doubt, therefore, it was soon discovered that the most certain, as well as the most expeditious, way of making a sweet juice ferment was to add to it a little of the scum, or lees, of another fermenting juice. And it can hardly be questioned that this singular excitation of fermentation in one fluid, by a sort of infection, or inoculation, of a little ferment taken from some other fluid, together with the strange swelling, foaming, and hissing of the fermented substance, must have always attracted attention from the more thoughtful. Nevertheless, the commencement of the scientific analysis of the phenomena dates from a period not earlier than the first half of the seventeenth century.

At this time, Van Helmont made a first step, by pointing out that the peculiar hissing and bubbling of a fermented liquid is due, not to the evolution of common air (which he, as the inventor of the term "gas," calls "gas ventosum"), but to that of a peculiar kind of air such as is occasionally met with in caves, mines, and wells, and which he calls "gas sylvestre."

But a century elapsed before the nature of this "gas sylvestre," or, as it was afterwards called, "fixed air," was clearly determined, and it was found to be identical with that deadly "choke-damp" by which the lives of those who descend into old wells, or mines, or brewers' vats, are sometimes suddenly ended; and with the poisonous aëriform fluid which is produced by the combustion of charcoal, and now goes by the name of carbonic acid gas.

During the same time it gradually became clear that

the presence of sugar was essential to the production of alcohol and the evolution of carbonic acid gas, which are the two great and conspicuous products of fermentation. And finally, in 1787, the Italian chemist, Fabroni, made the capital discovery that the yeast ferment, the presence of which is necessary to fermentation, is what he termed a "vegeto-animal" substance—or is a body which gives off ammoniacal salts when it is burned, and is, in other ways, similar to the gluten of plants and the albumen and casein of animals.

These discoveries prepared the way for the illustrious Frenchman, Lavoisier, who first approached the problem of fermentation with a complete conception of the nature of the work to be done. The words in which he expresses this conception, in the treatise on elementary chemistry to which reference has already been made, mark the year 1789 as the commencement of a revolution of not less moment in the world of science than that which simultaneously burst over the political world, and soon engulfed Lavoisier himself in one of its mad eddies.

"We may lay it down as an incontestable axiom that, in all the operations of art and nature, nothing is created; an equal quantity of matter exists both before and after the experiment: the quality and quantity of the elements remain precisely the same, and nothing takes place beyond changes and modifications in the combinations of these elements. Upon this principle, the whole art of performing chemical experiments depends; we must always suppose an exact equality between the elements of the body examined and those of the products of its analysis.

"Hence, since from must of grapes we procure alcohol and carbonic acid, I have an undoubted right to suppose that must consists of carbonic acid and alcohol. From these premisses we have two modes of ascertaining what passes during vinous fermentation: either by determining the nature of, and the elements which compose, the fermentable substances; or by accurately examining the products resulting from fermentation; and it is evident that the knowledge of either of these must lead to accurate conclusions concerning the nature and com-

position of the other. From these considerations it became necessary accurately to determine the constituent elements of the fermentable substances; and for this purpose I did not make use of the compound juices of fruits, the rigorous analysis of which is perhaps impossible, but made choice of sugar, which is easily analysed, and the nature of which I have already explained. This substance is a true vegetable oxyd, with two bases, composed of hydrogen and carbon, brought to the state of an oxyd by means of a certain proportion of oxygen; and these three elements are combined in such a way that a very slight force is sufficient to destroy the equilibrium of their connection."

After giving the details of his analysis of sugar and of the products of fermentation, Lavoisier continues:—

"The effect of the vinous fermentation upon sugar is thus reduced to the mere separation of its elements into two portions; one part is oxygenated at the expense of the other, so as to form carbonic acid; while the other part, being disoxygenated in favour of the latter, is converted into the combustible substance called alkohol; therefore, if it were possible to re-unite alkohol and carbonic acid together, we ought to form sugar."[1]

Thus Lavoisier thought he had demonstrated that the carbonic acid and the alcohol which are produced by the process of fermentation, are equal in weight to the sugar which disappears; but the application of the more refined methods of modern chemistry to the investigation of the products of fermentation by Pasteur, in 1860, proved that this is not exactly true, and that there is a deficit of from 5 to 7 per cent. of the sugar which is not covered by the alcohol and carbonic acid evolved. The greater part of this deficit is accounted for by the discovery of two substances, glycerine and succinic acid, of the existence of which Lavoisier was unaware, in the fermented liquid. But about $1\frac{1}{2}$ per cent. still remains to be made good. According to Pasteur, it has been appropriated by the yeast, but the fact that such appropriation takes place cannot be said to be actually proved.

[1] "Elements of Chemistry." By M. Lavoisier. Translated by Robert Kerr. Second Edition, 1793 (pp. 186—196).

However this may be, there can be no doubt that the constituent elements of fully 98 per cent. of the sugar which has vanished during fermentation have simply undergone rearrangement; like the soldiers of a brigade, who at the word of command divide themselves into the independent regiments to which they belong. The brigade is sugar, the regiments are carbonic acid, succinic acid, alcohol, and glycerine.

From the time of Fabroni, onwards, it has been admitted that the agent by which this surprising rearrangement of the particles of the sugar is effected is the yeast. But the first thoroughly conclusive evidence of the necessity of yeast for the fermentation of sugar was furnished by Appert, whose method of preserving perishable articles of food excited so much attention in France at the beginning of this century. Gay-Lussac, in his "Mémoire sur la Fermentation,"[1] alludes to Appert's method of preserving beer-wort unfermented for an indefinite time, by simply boiling the wort and closing the vessel in which the boiling fluid is contained, in such a way as thoroughly to exclude air; and he shows that, if a little yeast be introduced into such wort, after it has cooled, the wort at once begins to ferment, even though every precaution be taken to exclude air. And this statement has since received full confirmation from Pasteur.

On the other hand, Schwann, Schroeder and Dusch, and Pasteur, have amply proved that air may be allowed to have free access to beer-wort, without exciting fermentation, if only efficient precautions are taken to prevent the entry of particles of yeast along with the air.

Thus, the truth that the fermentation of a simple solution of sugar in water depends upon the presence of

[1] "Annales de Chimie," 1810.

yeast, rests upon an unassailable foundation; and the inquiry into the exact nature of the substance which possesses such a wonderful chemical influence becomes profoundly interesting.

The first step towards the solution of this problem was made two centuries ago by the patient and painstaking Dutch naturalist, Leeuwenhoek, who in the year 1680 wrote thus:—

"Sæpissime examinavi fermentum cerevisiæ, semperque hoc ex globulis per materiam pellucidam fluitantibus, quam cerevisiam esse censui, constare observavi: vidi etiam evidentissime, unumquemque hujus fermenti globulum denuo ex sex distinctis globullis constare, accurate eidem quantitate et formæ, cui globulis sanguinis nostri, respondentibus.

"Verum talis mihi de horum origine et formatione conceptus formabam; globulis nempe ex quibus farina Tritici, Hordei, Avenæ, Fagotritici, se constat aquæ calore dissolvi et aquæ commisceri; hac, vero aqua, quam cerevisiam vocare licet, refrigescente, multos ex minimis particulis in cerevisia coadunari, et hoc pacto efficere particulam sive globulum, quæ sexta pars est globuli fæcis, et iterum sex ex hisce globulis conjungi."[1]

Thus Leeuwenhoek discovered that yeast consists of globules floating in a fluid; but he thought that they were merely the starchy particles of the grain from which the wort was made, re-arranged. He discovered the fact that yeast had a definite structure, but not the meaning of the fact. A century and a half elapsed, and the investigation of yeast was recommenced almost simultaneously by Cagniard de la Tour in France, and by Schwann and Kützing in Germany. The French observer was the first to publish his results; and the subject received at his hands and at those of his colleague, the botanist Turpin, full and satisfactory investigation.

The main conclusions at which they arrived are these. The globular, or oval, corpuscles which float so thickly in

[1] Leeuwenhoek, "Arcana Naturæ Detecta." Ed. Nov., 1731.

the yeast as to make it muddy, though the largest are not more than one two-thousandth of an inch in diameter, and the smallest may measure less than one seven-thousandth of an inch, are living organisms. They multiply with great rapidity, by giving off minute buds, which soon attain the size of their parent, and then either become detached or remain united, forming the compound globules of which Leeuwenhoek speaks, though the constancy of their arrangement in sixes existed only in the worthy Dutchman's imagination.

It was very soon made out that these yeast organisms, to which Turpin gave the name of *Torula cerevisiæ*, were more nearly allied to the lower Fungi than to anything else. Indeed Turpin, and subsequently Berkeley and Hoffmann, believed that they had traced the development of the *Torula* into the well-known and very common mould —the *Penicillium glaucum*. Other observers have not succeeded in verifying these statements; and my own observations lead me to believe, that while the connection between *Torula* and the moulds is a very close one, it is of a different nature from that which has been supposed. I have never been able to trace the development of *Torula* into a true mould; but it is quite easy to prove that species of true mould, such as *Penicillium*, when sown in an appropriate nidus, such as a solution of tartrate of ammonia and yeast-ash, in water, with or without sugar, give rise to *Torulæ*, similar in all respects to *T. cerevisiæ*, except that they are, on the average, smaller. Moreover, Bail has observed the development of a *Torula* larger than *T. cerevisiæ*, from a *Mucor*, a mould allied to *Penicillium*.

It follows, therefore, that the *Torulæ*, or organisms of yeast, are veritable plants; and conclusive experiments have proved that the power which causes the rearrangement of the molecules of the sugar is intimately connected

with the life and growth of the plant. In fact, whatever arrests the vital activity of the plant also prevents it from exciting fermentation.

Such being the facts with regard to the nature of yeast, and the changes which it effects in sugar, how are they to be accounted for? Before modern chemistry had come into existence, Stahl, stumbling, with the stride of genius, upon the conception which lies at the bottom of all modern views of the process, put forward the notion that the ferment, being in a state of internal motion, communicated that motion to the sugar, and thus caused its resolution into new stubstances. And Lavoisier, as we have seen, adopts substiantially the same view. But Fabroni, full of the then novel conception of acids and bases and double decompositions, propounded the hypothesis that sugar is an oxide with two bases, and the ferment a carbonate with two bases; that the carbon of the ferment unites with the oxygen of the sugar, and gives rise to carbonic acid; while the sugar, uniting with the nitrogen of the ferment, produces a new substance analogous to opium. This is decomposed by distillation, and gives rise to alcohol. Next, in 1803, Thénard propounded a hypothesis which partakes somewhat of the nature of both Stahl's and Fabroni's views. "I do not believe with Lavoisier," he says, "that all the carbonic acid formed proceeds from the sugar. How, in that case, could we conceive the action of the ferment on it? I think that the first portions of the acid are due to a combination of the carbon of the ferment with the oxygen of the sugar, and that it is by carrying off a portion of oxygen from the last that the ferment causes the fermentation to commence—the equilibrium between the principles of the sugar being disturbed, they combine afresh to form carbonic acid and alcohol."

The three views here before us may be familiarly

exemplified by supposing the sugar to be a card-house. According to Stahl, the ferment is somebody who knocks the table, and shakes the card-house down; according to Fabroni, the ferment takes out some cards, but puts others in their places; according to Thénard, the ferment simply takes a card out of the bottom story, the result of which is that all the others fall.

As chemistry advanced, facts came to light which put a new face upon Stahl's hypothesis, and gave it a safer foundation than it previously possessed. The general nature of these phenomena may be thus stated:—A body, A, without giving to, or taking from, another body, B, any material particles, causes B to decompose into other substances, C, D, E, the sum of the weights of which is equal to the weight of B, which decomposes.

Thus, bitter almonds contain two substances, amygdalin and synaptase, which can be extracted, in a separate state, from the bitter almonds. The amygdalin thus obtained, if dissolved in water, undergoes no change; but if a little synaptase be added to the solution, the amygdalin splits up into bitter almond oil, prussic acid, and a kind of sugar.

A short time after Cagniard de la Tour discovered the yeast plant, Liebig, struck with the similarity between this and other such processes and the fermentation of sugar, put forward the hypothesis that yeast contains a substance which acts upon sugar, as synaptase acts upon amygdalin. And as the synaptase is certainly neither organized nor alive, but a mere chemical substance, Liebig treated Cagniard de la Tour's discovery with no small contempt, and, from that time to the present, has steadily repudiated the notion that the decomposition of the sugar is, in any sense, the result of the vital activity of the *Torula*. But, though the notion that the *Torula* is a creature which eats sugar and excretes carbonic acid

and alcohol, which is not unjustly ridiculed in the most surprising paper that ever made its appearance in a grave scientific journal,[1] may be untenable, the fact that the *Torulæ* are alive, and that yeast does not excite fermentation unless it contains living *Torulæ*, stands fast. Moreover, of late years, the essential participation of living organisms in fermentation other than the alcoholic, has been clearly made out by Pasteur and other chemists.

However, it may be asked, is there any necessary opposition between the so-called "vital" and the strictly physico-chemical views of fermentation? It is quite possible that the living *Torula* may excite fermentation in sugar, because it constantly produces, as an essential part of its vital manifestations, some substance which acts upon the sugar, just as the synaptase acts upon the amygdalin. Or it may be, that, without the formation of any such special substance, the physical condition of the living tissue of the yeast plant is sufficient to effect that small disturbance of the equilibrium of the particles of the sugar, which Lavoisier thought sufficient to effect its decomposition.

Platinum in a very fine state of division—known as platinum black, or *noir de platine*—has the very singular property of causing alcohol to change into acetic acid with great rapidity. The vinegar plant, which is closely

[1] "Das enträthselte Geheimniss der geistigen Gahrung (Vorläufige briefliche Mittheilung)" is the title of an anonymous contribution to Wohler and Liebig's "Annalen der Pharmacie" for 1839, in which a somewhat Rabelaisian imaginary description of the organization of the "yeast animals" and of the manner in which their functions are performed, is given with a circumstantiality worthy of the author of Gulliver's Travels. As a specimen of the writer's humour, his account of what happens when fermentation comes to an end may suffice. "Sobald nämlich die Thiere keinen Zucker mehr vorfinden, so fressen sie sich gegenseitig selbst auf, was durch eine eigene Manipulation geschieht; alles wird verdaut bis auf die Eier, welche unverändert durch den Darmkanal hineingehen · man hat zuletzt wieder gahrungsfahige Hefe, nämlich den Saamen der Thiere, der übrig bleibt."

allied to the yeast plant, has a similar effect upon dilute alcohol, causing it to absorb the oxygen of the air, and become converted into vinegar; and Liebig's eminent opponent, Pasteur, who has done so much for the theory and the practice of vinegar-making, himself suggests that in this case—

"La cause du phénomène physique qui accompagne la vie de la plante réside dans un état physique propre, analogue à celui du noir de platine. Mais il est essentiel de remarquer que cet état physique de la plante est étroitement lié avec la vie de cette plante."[1]

Now, if the vinegar plant gives rise to the oxidation of alcohol, on account of its merely physical constitution, it is at any rate possible that the physical constitution of the yeast plant may exert a decomposing influence on sugar.

But, without presuming to discuss a question which leads us into the very arcana of chemistry, the present state of speculation upon the *modus operandi* of the yeast plant in producing fermentation is represented, on the one hand, by the Stahlian doctrine, supported by Liebig, according to which the atoms of the sugar are shaken into new combinations, either directly by the *Torulæ*, or indirectly, by some substance formed by them; and, on the other hand, by the Thénardian doctrine, supported by Pasteur, according to which the yeast plant assimilates part of the sugar, and, in so doing, disturbs the rest, and determines its resolution into the products of fermentation. Perhaps the two views are not so much opposed as they seem at first sight to be.

But the interest which attaches to the influence of the yeast plants upon the medium in which they live and grow does not arise solely from its bearing upon the theory of fermentation. So long ago as 1838, Turpin compared the *Torulæ* to the ultimate elements of the

[1] "Etudes sur les Mycodermes," Comptes-Rendus, liv., 1862.

tissues of animals and plants—"Les organes élémentaires de leurs tissus, comparables aux petits végétaux des levures ordinaires, sont aussi les décompositeurs des substances qui les environnent."

Almost at the same time, and, probably, equally guided by his study of yeast, Schwann was engaged in those remarkable investigations into the form and development of the ultimate structural elements of the tissues of animals, which led him to recognize their fundamental identity with the ultimate structural elements of vegetable organisms.

The yeast plant is a mere sac, or "cell," containing a semi-fluid matter, and Schwann's microscopic analysis resolved all living organisms, in the long run, into an aggregation of such sacs or cells, variously modified; and tended to show, that all, whatever their ultimate complication, begin their existence in the condition of such simple cells.

In his famous "Mikroskopische Untersuchungen" Schwann speaks of *Torula* as a "cell;" and, in a remarkable note to the passage in which he refers to the yeast plant, Schwann says:—

"I have been unable to avoid mentioning fermentation, because it is the most fully and exactly known operation of cells, and represents, in the simplest fashion, the process which is repeated by every cell of the living body."

In other words, Schwann conceives that every cell of the living body exerts an influence on the matter which surrounds and permeates it, analogous to that which a *Torula* exerts on the saccharine solution by which it is bathed. A wonderfully suggestive thought, opening up views of the nature of the chemical processes of the living body, which have hardly yet received all the development of which they are capable.

Kant defined the special peculiarity of the living body

to be that the parts exist for the sake of the whole and the whole for the sake of the parts. But when Turpin and Schwann resolved the living body into an aggregation of quasi-independent cells, each, like a *Torula*, leading its own life and having its own laws of growth and development, the aggregation being dominated and kept working towards a definite end only by a certain harmony among these units, or by the superaddition of a controlling apparatus, such as a nervous system, this conception ceased to be tenable. The cell lives for its own sake, as well as for the sake of the whole organism: and the cells, which float in the blood, live at its expense, and profoundly modify it, are almost as much independent organisms as the *Torulæ* which float in beer-wort.

Schwann burdened his enunciation of the "cell theory" with two false suppositions; the one, that the structures he called "nucleus" and "cell-wall" are essential to a cell; the other, that cells are usually formed independently of other cells; but, in 1839, it was a vast and clear gain to arrive at the conception, that the vital functions of all the higher animals and plants are the resultant of the forces inherent in the innumerable minute cells of which they are composed, and that each of them is, itself, an equivalent of one of the lowest and simplest of independent living beings—the *Torula*.

From purely morphological investigations, Turpin and Schwann, as we have seen, arrived at the notion of the fundamental unity of structure of living beings. And, before long, the researches of chemists gradually led up to the conception of the fundamental unity of their composition.

So far back as 1803, Thénard pointed out, in most distinct terms, the important fact that yeast contains a

nitrogenous "animal" substance; and that such a substance is contained in all ferments. Before him, Fabroni and Fourcroy speak of the "vegeto-animal" matter of yeast. In 1844 Mulder endeavoured to demonstrate that a peculiar substance, which he called "protein," was essentially characteristic of living matter.

In 1846, Payen writes:—

"Enfin, une loi sans exception me semble apparaître dans les faits nombreux que j'ai observés et conduire à envisager sous un nouveau jour la vie végétale; si je ne m abuse, tout ce que dans les tissus végétaux la vue directe où amplifiée nous permet de discerner sous la forme de cellules et de vaisseaux, ne représente autre chose que les enveloppes protectrices, les réservoirs et les conduits, à l'aide desquels les corps animés qui les secrètent et les façonnent, se logent, puisent et charrient leurs aliments, déposent et isolent les matières excrétées."

And again:—

"Afin de compléter aujourd'hui l'énoncé du fait général, je rappellerai que les corps, doué des fonctions accomplies dans les tissus des plantes, sont formés des éléments qui constituent, en proportion peu variable, les organismes animaux; qu'ainsi l'on est conduit à reconnaître une immense unité de composition élémentaire dans tous les corps vivants de la nature."[1]

In the year (1846) in which these remarkable passages were published, the eminent German botanist, Von Mohl, invented the word "protoplasm," as a name for one portion of those nitrogenous contents of the cells of living plants, the close chemical resemblance of which to the essential constituents of living animals is so strongly indicated by Payen. And through the twenty-five years that have passed, since the matter of life was first called protoplasm, a host of investigators, among whom Cohn, Max Schulze, and Kühne must be named as leaders, have accumulated evidence, morphological, physiological, and

[1] "Mém. sur les Développements des Végétaux," &c.—"Mém. Présentées," ix. 1846.

chemical, in favour of that "immense unité de composition élémentaire dans tous les corps vivants de la nature," into which Payen had, so early, a clear insight.

As far back as 1850, Cohn wrote, apparently without any knowledge of what Payen had said before him :—

"The protoplasm of the botanist, and the contractile substance and sarcode of the zoologist, must be, if not identical, yet in a high degree analogous substances. Hence, from this point of view, the difference between animals and plants consists in this ; that, in the latter, the contractile substance, as a primordial utricle, is enclosed within an inert cellulose membrane, which permits it only to exhibit an internal motion, expressed by the phenomena of rotation and circulation, while, in the former, it is not so enclosed. The protoplasm in the form of the primordial utricle is, as it were, the animal element in the plant, but which is imprisoned, and only becomes free in the animal ; or, to strip off the metaphor which obscures simple thought, the energy of organic vitality which is manifested in movement is especially exhibited by a nitrogenous contractile substance, which in plants is limited and fettered by an inert membrane, in animals not so."[1]

In 1868, thinking that an untechnical statement of the views current among the leaders of biological science might be interesting to the general public, I gave a lecture embodying them in Edinburgh. Those who have not made the mistake of attempting to approach biology, either by the high *à priori* road of mere philosophical speculation, or by the mere low *à posteriori* lane offered by the tube of a microscope, but have taken the trouble to become acquainted with well-ascertained facts and with their history, will not need to be told that in what I had to say "as regards protoplasm" in my lecture "On the Physical Basis of Life," there was nothing new ; and, as I hope, nothing that the present state of knowledge does not justify us in believing to be true. Under these circumstances, my surprise may be imagined, when I found, that the mere statement of

[1] Cohn, "Ueber Protococcus pluvialis," in the "Nova Acta" for 1850.

facts and of views, long familiar to me as part of the common scientific property of continental workers, raised a sort of storm in this country, not only by exciting the wrath of unscientific persons whose pet prejudices they seemed to touch, but by giving rise to quite superfluous explosions on the part of some who should have been better informed.

Dr. Stirling, for example, made my essay the subject of a special critical lecture,[1] which I have read with much interest, though, I confess, the meaning of much of it remains as dark to me as does the "Secret of Hegel" after Dr. Stirling's elaborate revelation of it. Dr. Stirling's method of dealing with the subject is peculiar. "Protoplasm" is a question of history, so far as it is a name; of fact, so far as it is a thing. Dr. Stirling has not taken the trouble to refer to the original authorities for his history, which is consequently a travesty; and still less has he concerned himself with looking at the facts, but contents himself with taking them also at second-hand. A most amusing example of this fashion of dealing with scientific statements is furnished by Dr. Stirling's remarks upon my account of the protoplasm of the nettle hair. That account was drawn up from careful and often-repeated observation of the facts. Dr. Stirling thinks he is offering a valid criticism, when he says that my valued friend Professor Stricker gives a somewhat different statement about protoplasm. But why in the world did not this distinguished Hegelian look at a nettle hair for himself, before venturing to speak about the matter at all? Why trouble himself about what either Stricker or I say, when any tyro can see the facts for himself, if he is provided with those not rare articles, a nettle and a microscope? But I suppose this would have been "*Aufklärung*"—a recur-

[1] Subsequently published under the title of "As regards Protoplasm."

rence to the base common-sense philosophy of the eighteenth century, which liked to see before it believed, and to understand before it criticised. Dr. Stirling winds up his paper with the following paragraph :—

"In short, the whole position of Mr. Huxley, (1) that all organisms consist alike of the same life-matter, (2) which life-matter is, for its part, due only to chemistry, must be pronounced untenable—nor less untenable (3) the materialism he would found on it."

The paragraph contains three distinct assertions concerning my views, and just the same number of utter misrepresentations of them. That which I have numbered (1) turns on the ambiguity of the word "same," for a discussion of which I would refer Dr. Stirling to a great hero of "*Aufklärung*," Archbishop Whately; statement number (2) is, in my judgment, absurd, and certainly I have never said anything resembling it; while, as to number (3), one great object of my essay was to show that what is called "materialism" has no sound philosophical basis!

As we have seen, the study of yeast has led investigators face to face with problems of immense interest in pure chemistry, and in animal and vegetable morphology. Its physiology is not less rich in subjects for inquiry. Take, for example, the singular fact that yeast will increase indefinitely when grown in the dark, in water containing only tartrate of ammonia, a small percentage of mineral salts, and sugar. Out of these materials the *Torulæ* will manufacture nitrogenous protoplasm, cellulose, and fatty matters, in any quantity, although they are wholly deprived of those rays of the sun, the influence of which is essential to the growth of ordinary plants. There has been a great deal of speculation lately, as to how the living organisms buried beneath two or three thousand fathoms of water, and therefore in all probability almost deprived of light, live.

If any of them possess the same powers as yeast (and the same capacity for living without light is exhibited by some other fungi) there would seem to be no difficulty about the matter.

Of the pathological bearings of the study of yeast, and other such organisms, I have spoken elsewhere. It is certain that, in some animals, devastating epidemics are caused by fungi of low order—similar to those of which *Torula* is a sort of offshoot. It is certain that such diseases are propagated by contagion and infection, in just the same way as ordinary contagious and infectious diseases are propagated. Of course, it does not follow from this, that all contagious and infectious diseases are caused by organisms of as definite and independent a character as the *Torula;* but, I think, it does follow that it is prudent and wise to satisfy oneself in each particular case, that the "germ theory" cannot and will not explain the facts, before having recourse to hypotheses which have no equal support from analogy.

V.

ON THE FORMATION OF COAL.

THE lumps of coal in a coal-scuttle very often have a roughly cubical form. If one of them be picked out and examined with a little care, it will be found that its six sides are not exactly alike. Two opposite sides are comparatively smooth and shining, while the other four are much rougher, and are marked by lines which run parallel with the smooth sides. The coal readily splits along these lines, and the split surfaces thus formed are parallel with the smooth faces. In other words, there is a sort of rough and incomplete stratification in the lump of coal, as if it were a book, the leaves of which had stuck together very closely.

Sometimes the faces along which the coal splits are not smooth, but exhibit a thin layer of dull, charred-looking substance, which is known as "mineral charcoal."

Occasionally one of the faces of a lump of coal will present impressions, which are obviously those of the stem, or leaves, of a plant; but though hard mineral masses of pyrites, and even fine mud, may occur here and there, neither sand nor pebbles are met with.

When the coal burns, the chief ultimate products of its combustion are carbonic acid, water, and ammoniacal

products, which escape up the chimney; and a greater or less amount of residual earthy salts, which take the form of ash. These products are, to a great extent, such as would result from the burning of so much wood.

These properties of coal may be made out without any very refined appliances, but the microscope reveals something more. Black and opaque as ordinary coal is, slices of it become transparent if they are cemented in Canada balsam, and rubbed down very thin, in the ordinary way of making thin sections of non-transparent bodies. But as the thin slices, made in this way, are very apt to crack and break into fragments, it is better to employ marine glue as the cementing material. By the use of this substance, slices of considerable size and of extreme thinness and transparency may be obtained.[1]

Now let us suppose two such slices to be prepared from our lump of coal—one parallel with the bedding, the other perpendicular to it; and let us call the one the horizontal, and the other the vertical, section. The horizontal section will present more or less rounded yellow patches and streaks, scattered irregularly through the dark brown, or blackish, ground substance; while the vertical section will exhibit more elongated bars and granules of the same yellow materials, disposed in lines which correspond, roughly, with the general direction of the bedding of the coal.

This is the microscopic structure of an ordinary piece of coal. But if a great series of coals, from different localities and seams, or even from different parts of the same seam, be examined, this structure will be found to vary in two directions. In the anthracitic, or stone-coals, which burn like coke, the yellow matter diminishes, and the ground substance becomes more predominant,

[1] My assistant in the Museum of Practical Geology, Mr. Newton, invented this excellent method of obtaining thin slices of coal.

and blacker, and more opaque, until it becomes impossible to grind a section thin enough to be translucent; while, on the other hand, in such as the "Better-Bed" coal of the neighbourhood of Bradford, which burns with much flame, the coal is of a far lighter colour, and transparent sections are very easily obtained. In the browner parts of this coal, sharp eyes will readily detect multitudes of curious little coin-shaped bodies, of a yellowish brown colour, embedded in the dark brown ground substance. On the average, these little brown bodies may have a diameter of about one-twentieth of an inch. They lie with their flat surfaces nearly parallel with the two smooth faces of the block in which they are contained; and, on one side of each, there may be discerned a figure, consisting of three straight linear marks, which radiate from the centre of the disk, but do not quite reach its circumference. In the horizontal section these disks are often converted into more or less complete rings; while in the vertical sections they appear like thick hoops, the sides of which have been pressed together. The disks are, therefore, flattened bags; and favourable sections show that the three-rayed marking is the expression of three clefts, which penetrate one wall of the bag.

The sides of the bags are sometimes closely approximated; but, when the bags are less flattened, their cavities are, usually, filled with numerous, irregularly rounded, hollow bodies, having the same kind of wall as the large ones, but not more than one seven-hundredth of an inch in diameter.

In favourable specimens, again, almost the whole ground substance appears to be made up of similar bodies—more or less carbonized or blackened—and, in these, there can be no doubt that, with the exception of patches of mineral charcoal, here and there, the whole

mass of the coal is made up of an accumulation of the larger and of the smaller sacs.

But, in one and the same slice, every transition can be observed from this structure to that which has been described as characteristic of ordinary coal. The latter appears to rise out of the former, by the breaking-up and increasing carbonization of the larger and the smaller sacs. And, in the anthracitic coals, this process appears to have gone to such a length, as to destroy the original structure altogether, and to replace it by a completely carbonized substance.

Thus coal may be said, speaking broadly, to be composed of two constituents: firstly, mineral charcoal; and, secondly, coal proper. The nature of the mineral charcoal has long since been determined. Its structure shows it to consist of the remains of the stems and leaves of plants, reduced to little more than their carbon. Again, some of the coal is made up of the crushed and flattened bark, or outer coat, of the stems of plants, the inner wood of which has completely decayed away. But what I may term the "saccular matter" of the coal, which, either in its primary or in its degraded form, constitutes by far the greater part of all the bituminous coals I have examined, is certainly not mineral charcoal; nor is its structure that of any stem or leaf. Hence its real nature is, at first, by no means apparent, and has been the subject of much discussion.

The first person who threw any light upon the problem, as far as I have been able to discover, was the well-known geologist, Professor Morris. It is now thirty-four years since he carefully described and figured the coin-shaped bodies, or larger sacs, as I have called them, in a note appended to the famous paper "On the Coalbrookdale Coal-Field," published at that time, by the present President of the Geological Society, Mr. Prest-

wich. With much sagacity, Professor Morris divined the real nature of these bodies, and boldly affirmed them to be the spore-cases of a plant allied to the living club-mosses.

But discovery sometimes makes a long halt; and it is only a few years since Mr. Carruthers determined the plant (or rather one of the plants) which produces these spore-cases, by finding the discoidal sacs still adherent to the leaves of the fossilized cone which produced them. He gave the name of *Flemingites gracilis* to the plant of which the cones form a part. The branches and stem of this plant are not yet certainly known, but there is no sort of doubt that it was closely allied to the *Lepidodendron*, the remains of which abound in the coal formation. The *Lepidodendra* were shrubs and trees which put one more in mind of an *Araucaria* than of any other familiar plant; and the ends of the fruiting branches were terminated by cones, or catkins, somewhat like the bodies so named in a fir, or a willow. These conical fruits, however, did not produce seeds; but the leaves of which they were composed bore upon their surfaces sacs full of spores or sporangia, such as those one sees on the under surface of a bracken leaf. Now, it is these sporangia of the Lepidodendroid plant *Flemingites* which were identified by Mr. Carruthers with the free sporangia described by Professor Morris, which are the same as the large sacs of which I have spoken. And, more than this, there is no doubt that the small sacs are the spores, which were originally contained in the sporangia.

The living club-mosses are, for the most part, insignificant and creeping herbs, which, superficially, very closely resemble true mosses, and none of them reach more than two or three feet in height. But, in their essential structure, they very closely resemble the earliest

Lepidodendroid trees of the coal: their stems and leaves are similar; so are their cones; and no less like are the sporangia and spores; while even in their size, the spores of the *Lepidodendron* and those of the existing *Lycopodium*, or club-moss, very closely approach one another.

Thus, the singular conclusion is forced upon us, that the greater and the smaller sacs of the "Better-Bed" and other coals, in which the primitive structure is well preserved, are simply the sporangia and spores of certain plants, many of which were closely allied to the existing club-mosses. And if, as I believe, it can be demonstrated that ordinary coal is nothing but "saccular" coal which has undergone a certain amount of that alteration which, if continued, would convert it into anthracite; then, the conclusion is obvious, that the great mass of the coal we burn is the result of the accumulation of the spores and spore-cases of plants, other parts of which have furnished the carbonized stems and the mineral charcoal, or have left their impressions on the surfaces of the layer.

Of the multitudinous speculations which, at various times, have been entertained respecting the origin and mode of formation of coal, several appear to be negatived, and put out of court, by the structural facts the significance of which I have endeavoured to explain. These facts, for example, do not permit us to suppose that coal is an accumulation of peaty matter, as some have held.

Again, the late Professor Quekett was one of the first observers who gave a correct description of what I have termed the "saccular" structure of coal; and, rightly perceiving that this structure was something quite different from that of any known plant, he imagined that it proceeded from some extinct vegetable organism which was peculiarly abundant amongst the coal-forming plants.

But this explanation is at once shown to be untenable when the smaller and the larger sacs are proved to be spores or sporangia.

Some, once more, have imagined that coal was of submarine origin; and though the notion is amply and easily refuted by other considerations, it may be worth while to remark, that it is impossible to comprehend how a mass of light and resinous spores should have reached the bottom of the sea, or should have stopped in that position if they had got there.

At the same time, it is proper to remark that I do not presume to suggest that all coal must needs have the same structure; or that there may not be coals in which the proportions of wood and spores, or spore-cases, are very different from those which I have examined. All I repeat is, that none of the coals which have come under my notice have enabled me to observe such a difference. But, according to Principal Dawson, who has so sedulously examined the fossil remains of plants in North America, it is otherwise with the vast accumulations of coal in that country.

"The true coal," says Dr. Dawson, "consists principally of the flattened bark of Sigillarioid and other trees, intermixed with leaves of Ferns and *Cordaites*, and other herbaceous *débris*, and with fragments of decayed wood, constituting 'mineral charcoal,' all these materials having manifestly alike grown and accumulated where we find them."[1]

When I had the pleasure of seeing Principal Dawson in London last summer, I showed him my sections of coal, and begged him to re-examine some of the American coals on his return to Canada, with an eye to the presence of spores and sporangia, such as I was able to show him in our English and Scotch coals. He has been good enough to do so; and in a letter dated September 26th, 1870, he informs me that—

[1] "Acadian Geology," 2nd edition, p. 138.

"Indications of spore-cases are rare, except in certain coarse shaly coals and portions of coals, and in the roofs of the seams. The most marked case I have yet met with is the shaly coal referred to as containing *Sporangites* in my paper on the conditions of accumulation of coal (*Journal of the Geological Society*, vol. xxii. pp. 115, 139, and 165). The purer coals certainly consist principally of cubical tissues with some true woody matter, and the spore-cases, &c., are chiefly in the coarse and shaly layers. This is my old doctrine in my two papers in the *Journal of the Geological Society*, and I see nothing to modify it. Your observations, however, make it probable that the frequent *clear spots* in the cannels are spore-cases."

Dr. Dawson's results are the more remarkable, as the numerous specimens of British coal, from various localities, which I have examined, tell one tale as to the predominance of the spore and sporangium element in their composition; and as it is exactly in the finest and purest coals, such as the "Better-Bed" coal of Lowmoor, that the spores and sporangia obviously constitute almost the entire mass of the deposit.

Coal, such as that which has been described, is always found in sheets, or "seams," varying from a fraction of an inch to many feet in thickness, enclosed in the substance of the earth at very various depths, between beds of rock of different kinds. As a rule, every seam of coal rests upon a thicker, or thinner, bed of clay, which is known as "under-clay." These alternations of beds of coal, clay, and rock may be repeated many times, and are known as the "coal-measures;" and in some regions, as in South Wales and in Nova Scotia, the coal-measures attain a thickness of twelve or fourteen thousand feet, and enclose eighty or a hundred seams of coal, each with its under-clay, and separated from those above and below by beds of sandstone and shale.

The position of the beds which constitute the coal-measures is infinitely diverse. Sometimes they are tilted up vertically, sometimes they are horizontal, sometimes curved into great basins; sometimes they come to the

surface, sometimes they are covered up by thousands of feet of rock. But, whatever their present position, there is abundant and conclusive evidence that every under-clay was once a surface soil. Not only do carbonized root-fibres frequently abound in these under-clays; but the stools of trees, the trunks of which are broken off and confounded with the bed of coal, have been repeatedly found passing into radiating roots, still embedded in the under-clay. On many parts of the coast of England, what are commonly known as "submarine forests" are to be seen at low water. They consist, for the most part, of short stools of oak, beech, and fir trees, still fixed by their long roots in the bed of blue clay in which they originally grew. If one of these submarine forest beds should be gradually depressed and covered up by new deposits, it would present just the same characters as an under-clay of the coal, if the *Sigillaria* and *Lepidodendron* of the ancient world were substituted for the oak, or the beech, of our own times.

In a tropical forest, at the present day, the trunks of fallen trees, and the stools of such trees as may have been broken by the violence of storms, remain entire for but a short time. Contrary to what might be expected, the dense wood of the tree decays, and suffers from the ravages of insects, more swiftly than the bark. And the traveller, setting his foot on a prostrate trunk, finds that it is a mere shell, which breaks under his weight, and lands his foot amidst the insects, or the reptiles, which have sought food or refuge within.

The trees of the coal forests present parallel conditions. When the fallen trunks which have entered into the composition of the bed of coal are identifiable, they are mere double shells of bark, flattened together in consequence of the destruction of the woody core; and Sir Charles Lyell and Principal Dawson discovered, in the

hollow stools of coal trees of Nova Scotia, the remains of snails, millipedes, and salamander-like creatures, embedded in a deposit of a different character from that which surrounded the exterior of the trees. Thus, in endeavouring to comprehend the formation of a seam of coal, we must try to picture to ourselves a thick forest, formed for the most part of trees like gigantic club-mosses, mares'-tails, and tree ferns, with here and there some that had more resemblance to our existing yews and fir-trees. We must suppose that, as the seasons rolled by, the plants grew and developed their spores and seeds; that they shed these in enormous quantities, which accumulated on the ground beneath; and that, every now and then, they added a dead frond or leaf; or, at longer intervals, a rotten branch, or a dead trunk, to the mass.

A certain proportion of the spores and seeds no doubt fulfilled their obvious function, and, carried by the wind to unoccupied regions, extended the limits of the forest; many might be washed away by rain into streams, and be lost; but a large portion must have remained, to accumulate like beech-mast, or acorns, beneath the trees of a modern forest.

But, in this case, it may be asked, why does not our English coal consist of stems and leaves to a much greater extent than it does? What is the reason of the predominance of the spores and spore-cases in it?

A ready answer to this question is afforded by the study of a living full-grown club-moss. Shake it upon a piece of paper, and it emits a cloud of fine dust, which falls over the paper, and is the well-known Lycopodium powder. Now this powder used to be, and I believe still is, employed for two objects, which seem at first sight to have no particular connection with one another. It is, or was, employed in making lightning, and in making pills. The coats of the spores contain so much

resinous matter, that a pinch of Lycopodium powder, thrown through the flame of a candle, burns with an instantaneous flash, which has long done duty for lightning on the stage. And the same character makes it a capital coating for pills; for the resinous powder prevents the drug from being wetted by the saliva, and thus bars the nauseous flavour from the sensitive papillæ of the tongue.

But this resinous matter, which lies in the walls of the spores and sporangia, is a substance not easily altered by air and water, and hence tends to preserve these bodies, just as the bituminized cerecloth preserves an Egyptian mummy; while, on the other hand, the merely woody stem and leaves tend to rot, as fast as the wood of the mummy's coffin has rotted. Thus the mixed heap of spores, leaves, and stems in the coal-forest would be persistently searched by the long-continued action of air and rain; the leaves and stems would gradually be reduced to little but their carbon, or, in other words, to the condition of mineral charcoal in which we find them; while the spores and sporangia remained as a comparatively unaltered and compact residuum.

There is, indeed, tolerably clear evidence that the coal must, under some circumstances, have been converted into a substance hard enough to be rolled into pebbles, while it yet lay at the surface of the earth; for in some seams of coal, the courses of rivulets, which must have been living water, while the stratum in which their remains are found was still at the surface, have been observed to contain rolled pebbles of the very coal through which the stream has cut its way.

The structural facts are such as to leave no alternative but to adopt the view of the origin of such coal as I have described, which has just been stated; but, happily, the process is not without analogy at the present day. I

possess a specimen of what is called "white coal" from Australia. It is an inflammable material, burning with a bright flame, and having much the consistence and appearance of oat-cake, which, I am informed, covers a considerable area. It consists, almost entirely, of a compacted mass of spores and spore-cases. But the fine particles of blown sand which are scattered through it, show that it must have accumulated, subaërially, upon the surface of a soil covered by a forest of cryptogamous plants, probably tree-ferns.

As regards this important point of the subaërial region of coal, I am glad to find myself in entire accordance with Principal Dawson, who bases his conclusions upon other, but no less forcible, considerations. In a passage, which is the continuation of that already cited, he writes:—

"(3) The microscopical structure and chemical composition of the beds of cannel coal and earthy bitumen, and of the more highly bituminous and carbonaceous shale, show them to have been of the nature of the fine vegetable mud which accumulates in the ponds and shallow lakes of modern swamps. When such fine vegetable sediment is mixed, as is often the case, with clay, it becomes similar to the bituminous limestone and calcareo-bituminous shales of the coal-measures. (4) A few of the under-clays, which support beds of coal, are of the nature of the vegetable mud above referred to; but the greater part are argillo-arenaceous in composition, with little vegetable matter, and bleached by the drainage from them of water containing the products of vegetable decay. They are, in short, loamy or clay soils, and must have been sufficiently above water to admit of drainage. The absence of sulphurets, and the occurrence of carbonate of iron in connection with them, prove that, when they existed as soils, rain-water, and not sea-water, percolated them. (5) The coal and the fossil forests present many evidences of subaërial conditions. Most of the erect and prostrate trees had become hollow shells of bark before they were finally embedded, and their wood had broken into cubical pieces of mineral charcoal. Land-snails and galley-worms (*Xylobius*) crept into them, and they became dens, or traps, for reptiles. Large quantities of mineral charcoal occur on the surface of all the large beds of coal. None of these appearances could have been produced by subaqueous

action. (6) Though the roots of the *Sigillaria* bear more resemblance to the rhizomes of certain aquatic plants; yet, structurally, they are absolutely identical with the roots of Cycads, which the stems also resemble. Further, the *Sigillariæ* grew on the same soils which supported Conifers, *Lepidodendra*, *Cordaites*, and Ferns—plants which could not have grown in water. Again, with the exception perhaps of some *Pinnulariæ* and *Asterophyllites*, there is a remarkable absence from the coal measures of any form of properly aquatic vegetation. (7) The occurrence of marine, or brackish-water animals, in the roofs of coal-beds, or even in the coal itself, affords no evidence of subaqueous accumulation, since the same thing occurs in the case of modern submarine forests. For these and other reasons, some of which are more fully stated in the papers already referred to, while I admit that the areas of coal accumulation were frequently submerged, I must maintain that the true coal is a subaërial accumulation by vegetable growth on soils, wet and swampy it is true, but not submerged."

I am almost disposed to doubt whether it is necessary to make the concession of "wet and swampy;" otherwise, there is nothing that I know of to be said against this excellent conspectus of the reasons for believing in the subaërial origin of coal.

But the coal accumulated upon the area covered by one of the great forests of the carboniferous epoch would, in course of time, have been wasted away by the small, but constant, wear and tear of rain and streams, had the land which supported it remained at the same level, or been gradually raised to a greater elevation. And, no doubt, as much coal as now exists has been destroyed, after its formation, in this way. What are now known as coal districts owe their importance to the fact that they were areas of slow depression, during a greater or less portion of the carboniferous epoch; and that, in virtue of this circumstance, Mother Earth was enabled to cover up her vegetable treasures, and preserve them from destruction.

Wherever a coal-field now exists, there must formerly have been free access for a great river, or for a shallow

sea, bearing sediment in the shape of sand and mud. When the coal-forest area became slowly depressed, the waters must have spread over it, and have deposited their burden upon the surface of the bed of coal, in the form of layers, which are now converted into shale, or sandstone. Then followed a period of rest, in which the superincumbent shallow waters became completely filled up, and finally replaced, by fine mud, which settled down into a new under-clay, and furnished the soil for a fresh forest growth. This flourished, and heaped up its spores and wood into coal, until the stage of slow depression recommenced. And, in some localities, as I have mentioned, the process was repeated until the first of the alternating beds had sunk to near three miles below its original level at the surface of the earth.

In reflecting on the statement, thus briefly made, of the main facts connected with the origin of the coal formed during the carboniferous epoch, two or three considerations suggest themselves.

In the first place, the great phantom of geological time rises before the student of this, as of all other, fragments of the history of our earth—springing irrepressibly out of the facts, like the Djin from the jar which the fisherman so incautiously opened; and like the Djin again, being vaporous, shifting, and indefinable, but unmistakably gigantic. However modest the bases of one's calculation may be, the minimum of time assignable to the coal period remains something stupendous.

Principal Dawson is the last person likely to be guilty of exaggeration in this matter, and it will be well to consider what he has to say about it:—

"The rate of accumulation of coal was very slow. The climate of the period, in the northern temperate zone, was of such a character that the true conifers show rings of growth, not larger, nor much less distinct, than those of many of their modern congeners. The *Sigil-*

lariæ and *Calamites* were not, as often supposed, composed wholly, or even principally, of lax and soft tissues, or necessarily short-lived. The former had, it is true, a very thick inner bark; but their dense woody axis, their thick and nearly imperishable outer bark, and their scanty and rigid foliage, would indicate no very rapid growth or decay. In the case of the *Sigillariæ*, the variations in the leaf-scars in different parts of the trunk, the intercalation of new ridges at the surface representing that of new woody wedges in the axis, the transverse marks left by the stages of upward growth, all indicate that several years must have been required for the growth of stems of moderate size. The enormous roots of these trees, and the condition of the coal-swamps, must have exempted them from the danger of being overthrown by violence. They probably fell in successive generations from natural decay; and making every allowance for other materials, we may safely assert that every foot of thickness of pure bituminous coal implies the quiet growth and fall of at least fifty generations of *Sigillariæ*, and therefore an undisturbed condition of forest growth enduring through many centuries. Further, there is evidence that an immense amount of loose parenchymatous tissue, and even of wood, perished by decay, and we do not know to what extent even the most durable tissues may have disappeared in this way; so that, in many coal-seams, we may have only a very small part of the vegetable matter produced."

Undoubtedly the force of these reflections is not diminished when the bituminous coal, as in Britain, consists of accumulated spores and spore-cases, rather than of stems. But, suppose we adopt Principal Dawson's assumption, that one foot of coal represents fifty generations of coal plants; and, further, make the moderate supposition that each generation of coal plants took ten years to come to maturity—then, each foot-thickness of coal represents five hundred years. The superimposed beds of coal in one coal-field may amount to a thickness of fifty or sixty feet, and therefore the coal alone, in that field, represents $500 \times 50 = 25{,}000$ years. But the actual coal is but an insignificant portion of the total deposit, which, as has been seen, may amount to between two and three miles of vertical thickness. Suppose it be 12,000 feet—which is 240 times the thickness of the

actual coal—is there any reason why we should believe it may not have taken 240 times as long to form? I know of none. But, in this case, the time which the coal-field represents would be $25{,}000 \times 240 = 6{,}000{,}000$ years. As affording a definite chronology, of course such calculations as these are of no value; but they have much use in fixing one's attention upon a possible minimum. A man may be puzzled if he is asked how long Rome took a-building; but he is proverbially safe if he affirms it not to have been built in a day; and our geological calculations are all, at present, pretty much on that footing.

A second consideration which the study of the coal brings prominently before the mind of anyone who is familiar with palæontology is, that the coal Flora, viewed in relation to the enormous period of time which it lasted, and to the still vaster period which has elapsed since it flourished, underwent little change while it endured, and in its peculiar characters, differs strangely little from that which at present exists.

The same species of plants are to be met with throughout the whole thickness of a coal-field, and the youngest are not sensibly different from the oldest. But more than this. Notwithstanding that the carboniferous period is separated from us by more than the whole time represented by the secondary and tertiary formations, the great types of vegetation were as distinct then as now. The structure of the modern club-moss furnishes a complete explanation of the fossil remains of the *Lepidodendra*, and the fronds of some of the ancient ferns are hard to distinguish from existing ones. At the same time, it must be remembered, that there is nowhere in the world, at present, any *forest* which bears more than a rough analogy with a coal-forest. The types may remain, but the details of their form, their relative proportions,

their associates, are all altered. And the tree-fern forest of Tasmania, or New Zealand, gives one only a faint and remote image of the vegetation of the ancient world.

Once more, an invariably-recurring lesson of geological history, at whatever point its study is taken up: the lesson of the almost infinite slowness of the modification of living forms. The lines of the pedigrees of living things break off almost before they begin to converge.

Finally, yet another curious consideration. Let us suppose that one of the stupid, salamander-like Labyrinthodonts, which pottered, with much belly and little leg, like Falstaff in his old age, among the coal-forests, could have had thinking power enough in his small brain to reflect upon the showers of spores which kept on falling through years and centuries, while perhaps not one in ten million fulfilled its apparent purpose, and reproduced the organism which gave it birth: surely he might have been excused for moralizing upon the thoughtless and wanton extravagance which Nature displayed in her operations.

But we have the advantage over our shovel-headed predecessor—or possibly ancestor—and can perceive that a certain vein of thrift runs through this apparent prodigality. Nature is never in a hurry, and seems to have had always before her eyes the adage, "Keep a thing long enough, and you will find a use for it." She has kept her beds of coal many millions of years without being able to find much use for them; she has sent them down beneath the sea, and the sea-beasts could make nothing of them; she has raised them up into dry land, and laid the black veins bare, and still, for ages and ages, there was no living thing on the face of the earth that could see any sort of value in them; and it was only the other day, so to speak, that she turned a new creature

out of her workshop, who by degrees acquired sufficient wits to make a fire, and then to discover that the black rock would burn.

I suppose that nineteen hundred years ago, when Julius Cæsar was good enough to deal with Britain as we have dealt with New Zealand, the primæval Briton, blue with cold and woad, may have known that the strange black stone, of which he found lumps here and there in his wanderings, would burn, and so help to warm his body and cook his food. Saxon, Dane, and Norman swarmed into the land. The English people grew into a powerful nation, and Nature still waited for a full return of the capital she had invested in the ancient club-mosses. The eighteenth century arrived, and with it James Watt. The brain of that man was the spore out of which was developed the steam-engine, and all the prodigious trees and branches of modern industry which have grown out of this. But coal is as much an essential condition of this growth and development as carbonic acid is for that of a club-moss. Wanting coal, we could not have smelted the iron needed to make our engines, nor have worked our engines when we had got them. But take away the engines, and the great towns of Yorkshire and Lancashire vanish like a dream. Manufactures give place to agriculture and pasture, and not ten men can live where now ten thousand are amply supported.

Thus, all this abundant wealth of money and of vivid life is Nature's interest upon her investment in club-mosses, and the like, so long ago. But what becomes of the coal which is burnt in yielding this interest? Heat comes out of it, light comes out of it, and if we could gather together all that goes up the chimney; and all that remains in the grate of a thoroughly-burnt coal-fire, we should find ourselves in possession of a quantity of

carbonic acid, water, ammonia, and mineral matters, exactly equal in weight to the coal. But these are the very matters with which Nature supplied the club-mosses which made the coal. She is paid back principal and interest at the same time; and she straightway invests the carbonic acid, the water, and the ammonia in new forms of life, feeding with them the plants that now live. Thrifty Nature! Surely no prodigal, but most notable of housekeepers!

VI.

ON CORAL AND CORAL REEFS.

THE marine productions which are commonly known by the names of "Corals" and "Corallines," were thought by the ancients to be sea-weeds, which had the singular property of becoming hard and solid, when they were fished up from their native depths and came into contact with the air.

> "Sic et curalium, quo primum contigit auras
> Tempore durescit : mollis fuit herba sub undis,"

says Ovid (Metam. xv.); and it was not until the seventeenth century that Boccone was emboldened, by personal experience of the facts, to declare that the holders of this belief were no better than "idiots," who had been misled by the softness of the outer coat of the living red coral to imagine that it was soft all through.

Messer Boccone's strong epithet is probably undeserved, as the notion he controverts, in all likelihood, arose merely from the misinterpretation of the strictly true statement which any coral fisherman would make to a curious inquirer; namely, that the outside coat of the red coral is quite soft when it is taken out of the sea. At any rate, he did good service by eliminating this much error from the current notions about coral. But

the belief that corals are plants remained, not only in the popular, but in the scientific mind; and it received what appeared to be a striking confirmation from the researches of Marsigli in 1706. For this naturalist, having the opportunity of observing freshly-taken red coral, saw that its branches were beset with what looked like delicate and beautiful flowers, each having eight petals. It was true that these "flowers" could protrude and retract themselves, but their motions were hardly more extensive, or more varied, than those of the leaves of the sensitive plant; and therefore they could not be held to militate against the conclusion so strongly suggested by their form and their grouping upon the branches of a tree-like structure.

Twenty years later, a pupil of Marsigli, the young Marseilles physician, Peyssonel, conceived the desire to study these singular sea-plants, and was sent by the French Government on a mission to the Mediterranean for that purpose. The pupil undertook the investigation full of confidence in the ideas of his master, but being able to see and think for himself, he soon discovered that those ideas by no means altogether corresponded with reality. In an essay entitled "Traité du Corail," which was communicated to the French Academy of Science, but which has never been published, Peyssonel writes:—

"Je fis fleurir le corail dans des vases pleins d'eau de mer, et j'observai que ce que nous croyons être la fleur de cette prétendue plante n'était au vrai, qu'un insecte semblable à une petite Ortie ou Poulpe. J'avais le plaisir de voir remuer les pattes, ou pieds, de cette Ortie, et ayant mis le vase plein d'eau où le corail était à une douce chaleur auprès du feu, tous les petites insectes s'épanouirent. . . . L'Ortie sortie étend les pieds, et forme ce que M. de Marsigli et moi avions pris pour les pétales de la fleur. Le calice de cette prétendue fleur est le corps même de l'animal avancé et sorti hors de la cellule."[1]

[1] This extract from Peysonnel's manuscript is given by M. Lacaze Duthiers in his valuable "Histoire Naturelle du Corail" (1866).

The comparison of the flowers of the coral to a "petite ortie" or "little nettle" is perfectly just, but needs explanation. "Ortie de mer," or "sea-nettle," is, in fact, the French appellation for our "sea-anemone," a creature with which everybody, since the great aquarium mania, must have become familiar, even to the limits of boredom. In 1710, the great naturalist, Réaumur, had written a memoir for the express purpose of demonstrating that these "orties" are animals; and with this important paper Peyssonel must necessarily have been familiar. Therefore, when he declared the "flowers" of the red coral to be little "orties," it was the same thing as saying that they were animals of the same general nature as sea-anemones. But to Peyssonel's contemporaries this was an extremely startling announcement. It was hard to imagine the existence of such a thing as an association of animals into a structure with stem and branches altogether like a plant, and fixed to the soil as a plant is fixed; and the naturalists of that day preferred not to imagine it. Even Réaumur could not bring himself to accept the notion, and France being blessed with Academicians, whose great function (as the late Bishop Wilson and an eminent modern writer have so well shown) is to cause sweetness and light to prevail, and to prevent such unmannerly fellows as Peyssonel from blurting out unedifying truths, they suppressed him; and, as aforesaid, his great work remained in manuscript, and may at this day be consulted by the curious in that state, in the "Bibliothèque du Muséum d'Histoire Naturelle." Peyssonel, who evidently was a person of savage and untameable disposition, so far from appreciating the kindness of the Academicians in giving him time to reflect upon the unreasonableness, not to say rudeness, of making public statements in opposition to the views of some of the most distinguished of their body, seems bitterly to

have resented the treatment he met with. For he sent all further communications to the Royal Society of London, which never had, and it is to be hoped never will have, anything of an academic constitution; and finally took himself off to Guadaloupe, and became lost to science altogether.

Fifteen or sixteen years after the date of Peyssonel's suppressed paper, the Abbé Trembley published his wonderful researches upon the fresh-water *Hydra*. Bernard de Jussieu and Guettard followed them up by like inquiries upon the marine sea-anemones and corallines; Reaumur, convinced against his will of the entire justice of Peyssonel's views, adopted them, and made him a half-and-half apology in the preface to the next published volume of the "Mémoires pour servir à l'Histoire des Insectes;" and, from this time forth, Peyssonel's doctrine that corals are the work of animal organisms has been part of the body of established scientific truth.

Peyssonel, in the extract from his memoir already cited, compares the flower-like animal of the coral to a "poulpe," which is the French form of the name "polypus,"—"the many-footed,"—which the ancient naturalists gave to the soft-bodied cuttle-fishes, which, like the coral animal, have eight arms, or tentacles, disposed around a central mouth. Réaumur, admitting the analogy indicated by Peyssonel, gave the name of *polypes*, not only to the sea-anemone, the coral animal, and the fresh-water *Hydra*, but to what are now known as the *Polyzoa*, and he termed the skeleton which they fabricate a "*polypier*" or "polypidom."

The progress of discovery, since Réaumur's time, has made us very completely acquainted with the structure and habits of all these polypes. We know that, among the sea-anemones and coral-forming animals, each polype has a mouth leading to a stomach, which is open at its

inner end, and thus communicates freely with the general cavity of the body; that the tentacles placed round the mouth are hollow, and that they perform the part of arms in seizing and capturing prey. It is known that many of these creatures are capable of being multiplied by artificial division, the divided halves growing, after a time, into complete and separate animals; and that many are able to perform a very similar process naturally, in such a manner that one polype may, by repeated incomplete divisions, give rise to a sort of sheet, or turf, formed by innumerable connected, and yet independent, descendants. Or, what is still more common, a polype may throw out buds, which are converted into polypes, or branches bearing polypes, until a tree-like mass, sometimes of very considerable size, is formed.

This is what happens in the case of the red coral of commerce. A minute polype, fixed to the rocky bottom of the deep sea, grows up into a branched trunk. The end of every branch and twig is terminated by a polype; and all the polypes are connected together by a fleshy substance, traversed by innumerable canals which place each polype in communication with every other, and carry nourishment to the substance of the supporting stem. It is a sort of natural co-operative store, every polype helping the whole, at the same time as it helps itself. The interior of the stem, like that of the branches, is solidified by the deposition of carbonate of lime in its tissue, somewhat in the same fashion as our own bones are formed of animal matter impregnated with lime salts; and it is this dense skeleton (usually turned deep red by a peculiar colouring matter) cleared of the soft animal investment, as the heart-wood of a tree might be stripped of its bark, which is the red coral.

In the case of the red coral, the hard skeleton belongs to the interior of the stem and branches only; but, in

the commoner white corals, each polype has a complete skeleton of its own. These polypes ate sometimes solitary, in which case the whole skeleton is represented by a single cup, with partitions radiating from its centre to its circumference. When the polypes formed by budding or division remain associated, the polypidom is sometimes made up of nothing but an aggregation of these cups, while at other times the cups are at once separated and held together, by an intermediate substance, which represents the branches of the red coral. The red coral polype again is a comparatively rare animal, inhabiting a limited area, the skeleton of which has but a very insignificant mass; while the white corals are very common, occur in almost all seas, and form skeletons which are sometimes extremely massive.

With a very few exceptions, both the red and the white coral polypes are, in their adult state, firmly adherent to the sea-bottom; nor do their buds naturally become detached and locomotive. But, in addition to budding and division, these creatures possess the more ordinary methods of multiplication; and, at particular seasons, they give rise to numerous eggs of minute size. Within these eggs the young are formed, and they leave the egg in a condition which has no sort of resemblance to the perfect animal. It is, in fact, a minute oval body, many hundred times smaller than the full-grown creature, and it swims about with great activity by the help of multitudes of little hair-like filaments, called cilia, with which its body is covered. These cilia all lash the water in one direction, and so drive the little body along as if it were propelled by thousands of extremely minute paddles. After enjoying its freedom for a longer or shorter time, and being carried either by the force of its own cilia, or by currents which bear it along, the embryo coral settles down to the bottom, loses its cilia, and

becomes fixed to the rock, gradually assuming the polype form and growing up to the size of its parent. As the infant polypes of the coral may retain this free and active condition for many hours, or even days, and as a tidal or other current in the sea may easily flow at the speed of two or even more miles in an hour, it is clear that the embryo must often be transported to very considerable distances from the parent. And it is easily understood how a single polype, which may give rise to hundreds, or perhaps thousands, of embryos, may, by this process of partly active and partly passive migration, cover an immense surface with its offspring. The masses of coral which may be formed by the assemblages of polypes which spring by budding, or by dividing, from a single polype, occasionally attain very considerable dimensions. Such skeletons are sometimes great plates, many feet long and several feet in thickness; or they may form huge half globes, like the brainstone corals, or may reach the magnitude of stout shrubs, or even small trees. There is reason to believe that such masses as these take a long time to form, and hence that the age a polype tree, or polype turf, may attain, may be considerable. But, sooner or later, the coral polypes, like all other things, die; the soft flesh decays, while the skeleton is left as a stony mass at the bottom of the sea, where it retains its integrity for a longer or a shorter time, according as its position affords it more or less protection from the wear and tear of the waves.

The polypes which give rise to the white coral are found, as has been said, in the seas of all parts of the world; but in the temperate and cold oceans they are scattered and comparatively small in size, so that the skeletons of those which die do not accumulate in any considerable quantity. But it is otherwise in the greater part of the ocean which lies in the warmer parts of the

world, comprised within a distance of about 1,800 miles on each side of the equator. Within the zone thus bounded, by far the greater part of the ocean is inhabited by coral polypes, which not only form very strong and large skeletons, but associate together into great masses, like the thickets and the meadow turf, or, better still, the accumulations of peat, to which plants give rise on the dry land. These masses of stony matter, heaped up beneath the waters of the ocean, become as dangerous to mariners as so much ordinary rock, and to these, as to common rock ridges, the seaman gives the name of "reefs."

Such coral reefs cover many thousand square miles in the Pacific and in the Indian Oceans. There is one reef, or rather great series of reefs, called the Barrier Reef, which stretches, almost continuously, for more than 1,100 miles off the east coast of Australia. Multitudes of the island in the Pacific are either reefs themselves, or are surrounded by reefs. The Red Sea is in many parts almost a maze of such reefs; and they abound no less in the West Indies, along the coast of Florida, and even as far north as the Bahama Islands. But it is a very remarkable circumstance that, within the area of what we may call the "coral zone," there are no coral reefs upon the west coast of America, nor upon the west coast of Africa; and it is a general fact that the reefs are interrupted, or absent, opposite the mouths of great rivers. The causes of this apparent caprice in the distribution of coral reefs are not far to seek. The polypes which fabricate them require for their vigorous growth a temperature which must not fall below 68° Fahrenheit all the year round, and this temperature is only to be found within the distance on each side of the equator which has been mentioned, or thereabouts. But even within the coral zone this degree of warmth is not every-

where to be had. On the west coast of America, and on the corresponding coast of Africa, currents of cold water from the icy regions which surround the South Pole set northward, and it appears to be due to their cooling influence that the sea in these regions is free from the reef builders. Again, the coral polypes cannot live in water which is rendered brackish by floods from the land, or which is perturbed by mud from the same source, and hence it is that they cease to exist opposite the mouths of rivers, which damage them in both these ways.

Such is the general distribution of the reef-building corals, but there are some very interesting and singular circumstances to be observed in the conformation of the reefs, when we consider them individually. The reefs, in fact, are of three different kinds; some of them stretch out from the shore, almost like a prolongation of the beach, covered only by shallow water, and in the case of an island, surrounding it like a fringe of no considerable breadth. These are termed "fringing reefs." Others are separated by a channel which may attain a width of many miles, and a depth of twenty or thirty fathoms or more, from the nearest land; and when this land is an island, the reef surrounds it like a low wall, and the sea between the reef and the land is, as it were, a moat inside this wall. Such reefs as these are called "encircling" when they surround an island; and "barrier" reefs, when they stretch parallel with the coast of a continent. In both these cases there is ordinary dry land inside the reef, and separated from it only by a narrower or a wider, a shallower or a deeper, space of sea, which is called a "lagoon," or "inner passage." But there is a third kind of reef, of very common occurrence in the Pacific and Indian Oceans, which goes by the name of an "Atoll." This is, to all intents and purposes, an encircling reef, without anything to encircle; or, in

other words, without an island in the middle of its lagoon. The atoll has exactly the appearance of a vast, irregularly oval, or circular, breakwater, enclosing smooth water in its midst. The depth of the water in the lagoon rarely exceeds twenty or thirty fathoms, but, outside the reef, it deepens with great rapidity to 200 or 300 fathoms. The depth immediately outside the barrier, or encircling, reefs, may also be very considerable; but, at the outer edge of a fringing reef, it does not amount usually to more than twenty or twenty-five fathoms; in other words, from 120 to 150 feet.

Thus, if the water of the ocean could be suddenly drained away, we should see the atolls rising from the sea-bed like vast truncated cones, and resembling so many volcanic craters, except that their sides would be steeper than those of an ordinary volcano. In the case of the encircling reefs, the cone, with the enclosed island, would look like Vesuvius with Monte Nuovo within the old crater of Somma; while, finally, the island with a fringing reef would have the appearance of an ordinary hill, or mountain, girded by a vast parapet, within which would lie a shallow moat. And the dry bed of the Pacific might afford grounds for an inhabitant of the moon to speculate upon the extraordinary subterranean activity to which these vast and numerous "craters" bore witness!

When the structure of a fringing reef is investigated, the bottom of the lagoon is found to be covered with fine whitish mud, which results from the breaking up of the dead corals. Upon this muddy floor there lie, here and there, growing corals, or occasionally great blocks of dead coral, which have been torn by storms from the outer edge of the reef, and washed into the lagoon. Shell-fish and worms of various kinds abound; and fish, some of which prey upon the coral, sport in the deeper pools.

But the corals which are to be seen growing in the shallow waters of the lagoon are of a different kind from those which abound on the outer edge of the reef, and of which the reef is built up. Close to the seaward edge of the reef, over which, even in calm weather, a surf almost always breaks, the coral rock is encrusted with a thick coat of a singular vegetable organism, which contains a great deal of lime—the so-called *Nullipora*. Beyond this, in the part of the edge of the reef which is always covered by the breaking waves, the living, true, reef-polypes make their appearance; and, in different forms, coat the steep seaward face of the reef to a depth of 100 or even 150 feet. Beyond this depth the sounding-lead rests, not upon the wall-like face of the reef, but on the ordinary shelving sea-bottom. And the distance to which a fringing reef extends from the land corresponds with that at which the sea has a depth of twenty or five-and-twenty fathoms.

If, as we have supposed, the sea could be suddenly withdrawn from around an island provided with a fringing reef, such as the Mauritius, the reef would present the aspect of a terrace, its seaward face, 100 feet or more high, blooming with the animal flowers of the coral, while its surface would be hollowed out into a shallow and irregular moat-like excavation.

The coral mud, which occupies the bottom of the lagoon, and with which all the interstices of the coral skeletons which accumulate to form the reef are filled up, does not proceed from the washing action of the waves alone; innumerable fishes, and other creatures which prey upon the coral, add a very important contribution of finely-triturated calcareous matter; and the corals and mud becoming incorporated together, gradually harden and give rise to a sort of limestone rock, which may vary a good deal in texture. Sometimes it remains friable

and chalky, but, more often, the infiltration of water, charged with carbonic acid, dissolves some of the calcareous matter, and deposits it elsewhere in the interstices of the nascent rock, thus glueing and cementing the particles together into a hard mass; or it may even dissolve the carbonate of lime more extensively, and re-deposit it in a crystalline form. On the beach of the lagoon, where the coral sand is washed into layers by the action of the waves, its grains become thus fused together into strata of a limestone, so hard that they ring when struck with a hammer, and inclined at a gentle angle, corresponding with that of the surface of the beach. The hard parts of the many animals which live upon the reef become imbedded in this coral limestone, so that a block may be full of shells of bivalves and univalves, or of sea-urchins; and even sometimes encloses the eggs of turtles in a state of petrifaction. The active and vigorous growth of the reef goes on only at the seaward margins, where the polypes are exposed to the wash of the surf, and are thereby provided with an abundant supply of air and of food. The interior portion of the reef may be regarded as almost wholly an accumulation of dead skeletons. Where a river comes down from the land there is a break in the reef, for the reasons which have been already mentioned.

The origin and mode of formation of a fringing reef, such as that just described, are plain enough. The embryos of the coral polypes have fixed themselves upon the submerged shore of the island, as far out as they could live, namely, to a depth of twenty or twenty-five fathoms. One generation has succeeded another, building itself up upon the dead skeletons of its predecessor. The mass has been consolidated by the infiltration of coral mud, and hardened by partial solution and redeposition, until a great rampart of coral rock 100 or 150 feet high on its sea-

ward face has been formed all round the island, with only such gaps as result from the outflow of rivers, in the place of sally-ports.

The structure of the rocky accumulation in the encircling reefs and in the atolls is essentially the same as in the fringing reef. But, in addition to the differences of depth inside and out, they present some other peculiarities. These reefs, and especially the atolls, are usually interrupted at one part of their circumference, and this part is always situated on the leeward side of the reef, or that which is the more sheltered side. Now, as all these reefs are situated within the region in which the trade-winds prevail, it follows that, on the north side of the equator, where the trade-wind is a north-easterly wind, the opening of the reef is on the south-west side: while in the southern hemisphere, where the trade-winds blow from the south-east, the opening lies to the north-west. The curious practical result follows from this structure, that the lagoons of these reefs really form admirable harbours, if a ship can only get inside them. But the main difference between the encircling reefs and the atolls, on the one hand, and the fringing reefs on the other, lies in the fact of the much greater depth of water on the seaward faces of the former. As a consequence of this fact, the whole of this face is not, as it is in the case of the fringing reef, covered with living coral polypes. For, as we have seen, these polypes cannot live at a greater depth than about twenty-five fathoms; and actual observation has shown that while, down to this depth, the sounding-lead will bring up branches of live coral from the outer wall of such a reef, at a greater depth it fetches to the surface nothing but dead coral and coral sand. We must, therefore, picture to ourselves an atoll, or an encircling reef, as fringed for 100 feet, or more, from its

summit, with coral polypes busily engaged in fabricating coral; while, below this comparatively narrow belt, its surface is a bare and smooth expanse of coral sand, supported upon and within a core of coral limestone. Thus, if the bed of the Pacific were suddenly laid bare, as was just now supposed, the appearance of the reef-mountains would be exactly the reverse of that presented by many high mountains on land. For these are white with snow at the top, while their bases are clothed with an abundant and gaudily-coloured vegetation. But the coral cones would look grey and barren below, while their summits would be gay with a richly-coloured parterre of flower-like coral polypes.

The practical difficulties of sounding upon, and of bringing up portions of, the seaward face of an atoll or of an encircling reef, are so great, in consequence of the constant and dangerous swell which sets towards it, that no exact information concerning the depth to which the reefs are composed of coral has yet been obtained. There is no reason to doubt, however, that the reef-cone has the same structure from its summit to its base, and that its sea-wall is throughout mainly composed of dead coral.

And now arises a serious difficulty. If the coral polypes cannot live at a greater depth than 100 or 150 feet, how can they have built up the base of the reef-cone, which may be 2,000 feet, or more, below the surface of the sea?

In order to get over this objection, it was at one time supposed that the reef-building polypes had settled upon the summits of a chain of submarine mountains. But what is there in physical geography to justify the assumption of the existence of a chain of mountains stretching for 1,000 miles or more, and so nearly of the same height, that none should rise above the level of the sea, nor fall 150 feet below that level?

How again, on this hypothesis, are atolls to be accounted for, unless, as some have done, we take refuge in the wild supposition that every atoll corresponds with the crater of a submarine volcano? And what explanation does it afford of the fact that, in some parts of the ocean, only atolls and encircling reefs occur, while others present none but fringing reefs?

These and other puzzling facts remained insoluble until the publication, in the year 1840, of Mr. Darwin's famous work on coral reefs; in which a key was given to all the difficult yroblems connected with the subject, and every difficulty was shown to be capable of solution by deductive reasoning from a happy combination of certain well-established geological and biological truths. Mr. Darwin, in fact, showed, that so long as the level of the sea remains unaltered in any area in which coral reefs are being formed, or if the level of the sea relatively to that of the land is falling, the only reefs which can be formed are fringing reefs. While if, on the contrary, the level of the sea is rising relatively to that of the land, at a rate not faster than that at which the upward growth of the coral can keep pace with it, the reef will gradually pass from the condition of a fringing, into that of an encircling or barrier reef. And, finally, that if the relative level of the sea rise so much that the encircled land is completely submerged, the reef must necessarily pass into the condition of an atoll.

For, suppose the relative level of the sea to remain stationary, after a fringing reef has reached that distance from the land at which the depth of water amounts to 150 feet. Then the reef cannot extend seaward by the migration of coral germs, because these coral germs would find the bottom of the sea to be too deep for them to live in. And the only manner in which the reef could extend outwards, would be by the gradual

accumulation, at the foot of its seaward face, of a talus of coral fragments torn off by the violence of the waves, which talus might, in course of time, become high enough to bring its upper surface within the limits of coral growth, and in that manner provide a sort of factitious sea-bottom upon which the coral embryos might perch. If, on the other hand, the level of the sea were slowly and gradually lowered, it is clear that the parts of its bottom originally beyond the limit of coral growth, would gradually be brought within the required distance of the surface, and thus the reef might be indefinitely extended. But this process would give rise neither to an encircling reef nor to an atoll, but to a broad belt of upheaved coral rock, increasing the dimensions of the dry land, and continuous seawards with the fresh fringing reef.

Suppose, however, that the sea-level rose instead of falling, at the same slow and gradual rate at which we know it to be rising in some parts of the world—not more, in fact, than a few inches, or, at most, a foot or two, in a hundred years. Then, while the reef would be unable to extend itself seaward, the sea-bottom outside it being gradually more and more removed from the depth at which the life of the coral polypes is possible, it would be able to grow upwards as fast as the sea rose. But the growth would take place almost exclusively around the circumference of the reef, this being the only region in which the coral polypes would find the conditions favourable for their existence. The bottom of the lagoon would be raised, in the main, only by the coral *débris* and coral mud, formed in the manner already described; consequently, the margins of the reef would rise faster than the bottom, or, in other words, the lagoon would constantly become deeper. And, at the same time, it would gradually increase in breadth;

as the rising sea, covering more and more of the land, would occupy a wider space between the edge of the reef and what remained of the land. Thus the rising sea would eventually convert a large island with a fringing reef, into a small island surrounded by an encircling reef. And it will be obvious that when the rising of the sea has gone so far as completely to cover the highest points of the island, the reef will have passed into the condition of an atoll.

But how is it possible that the relative level of the land and sea should be altered to this extent? Clearly, only in one of two ways: either the sea must have risen over those areas which are now covered by atolls and encircling reefs; or, the land upon which the sea rests must have been depressed to a corresponding extent.

If the sea has risen, its rise must have taken place over the whole world simultaneously, and it must have risen to the same height over all parts of the coral zone. Grounds have been shown for the belief that the general level of the sea may have been different at different times; it has been suggested, for example, that the accumulation of ice about the poles during one of the cold periods of the earth's history, necessarily implies a diminution in the volume of the sea proportioned to the amount of its water thus permanently locked up in the Arctic and Antarctic ice-cellars; while, in the warm periods, the greater or less disappearance of the polar ice-cap implies a corresponding addition of water to the ocean. And no doubt this reasoning must be admitted to be sound in principle; though it is very hard to say what practical effect the additions and subtractions thus made have had on the level of the ocean; inasmuch as such additions and subtractions might be either intensified or nullified, by contemporaneous changes in the level of the land. And no one has yet shown that any

such great melting of polar ice, and consequent raising of the level of the water of the ocean, has taken place since the existing atolls began to be formed.

In the absence of any evidence that the sea has ever risen to the extent required to give rise to the encircling reefs and the atolls, Mr. Darwin adopted the opposite hypothesis, viz. that the land has undergone extensive and slow depression in those localities in which these structures exist.

It seems, at first, a startling paradox, to suppose that the land is less fixed than the sea; but that such is the case is the uniform testimony of geology. Beds of sandstone or limestone, thousands of feet thick, and all full of marine remains, occur in various parts of the earth's surface, and prove, beyond a doubt, that when these beds were formed, that portion of the sea-bottom which they then occupied underwent a slow and gradual depression to a distance which cannot have been less than the thickness of those beds, and may have been very much greater. In supposing, therefore, that the great areas of the Pacific and of the Indian Ocean, over which atolls and encircling reefs are found scattered, have undergone a depression of some hundreds, or, it may be, thousands of feet, Mr. Darwin made a supposition which had nothing forced or improbable, but was entirely in accordance with what we know to have taken place over similarly extensive areas, in other periods of the world's history. But Mr. Darwin subjected his hypothesis to an ingenious indirect test. If his view be correct, it is clear that neither atolls, nor encircling reefs, should be found in those portions of the ocean in which we have reason to believe, on independent grounds, that the sea-bottom has long been either stationary, or slowly rising. Now it is known that, as a general rule, the level of the land is either stationary,

or is undergoing a slow upheaval, in the neighbourhood of active volcanoes; and, therefore, neither atolls nor encircling reefs ought to be found in regions in which volcanoes are numerous and active. And this turns out to be the case. Appended to Mr. Darwin's great work on coral reefs, there is a map on which atolls and encircling reefs are indicated by one colour, fringing reefs by another, and active volcanoes by a third. And it is at once obvious that the lines of active volcanoes lie around the margins of the areas occupied by the atolls and the encircling reefs. It is exactly as if the upheaving volcanic agencies had lifted up the edges of these great areas, while their centres had undergone a corresponding depression. An atoll area may, in short, be pictured as a kind of basin, the margins of which have been pushed up by the subterranean forces, to which the craters of the volcanoes have, at intervals given vent.

Thus we must imagine the area of the Pacific now covered by the Polynesian Archipelago, as having been, at some former time, occupied by large islands, or, may be, by a great continent, with the ordinarily diversified surface of plain, and hill, and mountain chain. The shores of this great land were doubtless fringed by coral reefs; and, as it slowly underwent depression, the hilly regions, converted into islands, became, at first, surrounded by fringing reefs, and then, as depression went on, these became converted into encircling reefs, and these, finally, into atolls, until a maze of reefs and coral-girdled islets took the place of the original land masses.

Thus the atolls and the encircling reefs furnish us with clear, though indirect, evidence of changes in the physical geography of large parts of the earth's surface; and even, as my lamented friend, the late Professor

Jukes, has suggested, give us indications of the manner in which some of the most puzzling facts connected with the distribution of animals have been brought about. For example, Australia and New Guinea are separated by Torres Straits, a broad belt of sea 100 or 120 miles wide. Nevertheless, there is in many respects a curious resemblance between the land animals which inhabit New Guinea and the land animals which inhabit Australia. But, at the same time, the marine shell-fish which are found in the shallow waters of the shores of New Guinea, are quite different from those which are met with upon the coasts of Australia. Now, the eastern end of Torres Straits is full of atolls, which, in fact, form the northern termination of the Great Barrier Reef which skirts the eastern coast of Australia. It follows, therefore, that the eastern end of Torres Straits is an area of depression, and it is very possible, and on many grounds highly probable, that, in former times, Australia and New Guinea were directly connected together, and that Torres Straits did not exist. If this were the case, the existence of cassowaries and of marsupial quadrupeds, both in New Guinea and in Australia, becomes intelligible; while the difference between the littoral molluscs of the north and the south shores of Torres Straits is readily explained by the great probability that, when the depression in question took place, and what was, at first, an arm of the sea became converted into a strait separating Australia from New Guinea, the northern shore of this new sea became tenanted with marine animals from the north, while the southern shore was peopled by immigrants from the already existing marine Australian fauna.

Inasmuch as the growth of the reef depends upon that of successive generations of coral polypes, and as each generation takes a certain time to grow to its

full size, and can only separate its calcareous skeleton from the water in which it lives at a certain rate, it is clear that the reefs are records not only of changes in physical geography, but of the lapse of time. It is by no means easy, however, to estimate the exact value of reef-chronology, and the attempts which have been made to determine the rate at which a reef grows vertically, have yielded anything but precise results. A cautious writer, Mr. Dana, whose extensive study of corals and coral reefs makes him an eminently competent judge, states his conclusion in the following terms :—

"The rate of growth of the common branching madrepore is not over one and a half inches a year. As the branches are open, this would not be equivalent to more than half an inch in height of solid coral for the whole surface covered by the madrepore ; and, as they are also porous, to not over three-eighths of an inch of solid limestone. But a coral plantation has large bare patches without corals, and the coral sands are widely distributed by currents, part of them to depths over one hundred feet where there are no living corals ; not more than one-sixth of the surface of a reef region is, in fact, covered with growing species. This reduces the three-eighths to *one-sixteenth*. Shells and other organic relics may contribute one-fourth as much as corals. At the outside, the average upward increase of the whole reef-ground per year would not exceed *one-eighth* of an inch.

"Now some reefs are at least two thousand feet thick, which at one-eighth of an inch a year, corresponds to one hundred and ninety-two thousand years."[1]

Halve, or quarter, this estimate if you will, in order to be certain of erring upon the right side, and still there remains a prodigious period during which the ancestors of the existing coral polypes have been undisturbedly at work ; and during which, therefore, the climatal conditions over the coral area must have been much what they are now.

And all this lapse of time has occurred within the most recent period of the history of the earth. The

[1] Dana, "Manual of Geology," p. 591.

remains of reefs formed by coral polypes of different kinds from those which exist now, enter largely into the composition of the limestones of the Jurassic period; and still more widely different coral polypes have contributed their quota to the vast thickness of the carboniferous and Devonian strata. Then as regards the latter group of rocks in America, the high authority already quoted tells us:—

"The Upper Helderberg period is eminently the coral reef period of the palæozoic ages. Many of the rocks abound in coral, and are as truly coral reefs as the modern reefs of the Pacific. The corals are sometimes standing on the rocks in the position they had when growing: others are lying in fragments, as they were broken and heaped by the waves; and others were reduced to a compact limestone by the finer trituration before consolidation into rock. This compact variety is the most common kind among the coral reef rocks of the present seas; and it often contains but few distinct fossils, although formed in water that abounded in life. At the fall of the Ohio, near Louisville, there is a magnificent display of the old reef. Hemispherical *Favosites*, five or six feet in diameter, lie there nearly as perfect as when they were covered by their flower-like polypes; and besides these, there are various branching corals, and a profusion of *Cyathophyllia*, or cup-corals." [1]

Thus, in all the great periods of the earth's history of which we know anything, a part of the then living matter has had the form of polypes, competent to separate from the water of the sea the carbonate of lime necessary for their own skeletons. Grain by grain, and particle by particle, they have built up vast masses of rock, the thickness of which is measured by hundreds of feet, and their area by thousands of square miles. The slow oscillations of the crust of the earth, producing great changes in the distribution of land and water, have often obliged the living matter of the coral-builders to shift the locality of its operations; and, by variation and adaptation to these modifications of condition, its forms

[1] Dana, "Manual of Geology," p. 272.

have as often changed. The work it has done in the past is, for the most part, swept away, but fragments remain; and, if there were no other evidence, suffice to prove the general constancy of the operations of Nature in this world, through periods of almost inconceivable duration.

VII.

ON THE METHODS AND RESULTS OF ETHNOLOGY.

ETHNOLOGY is the science which determines the distinctive characters of the persistent modifications of mankind; which ascertains the distribution of those modifications in present and past times, and seeks to discover the causes, or conditions of existence, both of the modifications and of their distribution. I say "persistent" modifications, because, unless incidentally, ethnology has nothing to do with chance and transitory peculiarities of human structure. And I speak of "persistent modifications" or "stocks" rather than of "varieties," or "races," or "species," because each of these last well-known terms implies, on the part of its employer, a preconceived opinion touching one of those problems, the solution of which is the ultimate object of the science; and in regard to which, therefore, ethnologists are especially bound to keep their minds open and their judgments freely balanced.

Ethnology, as thus defined, is a branch of ANTHROPOLOGY, the great science which unravels the complexities of human structure; traces out the relations of man to other animals; studies all that is especially human in the mode in which man's complex functions are performed;

and searches after the conditions which have determined his presence in the world. And anthropology is a section of ZOOLOGY, which again is the animal half of BIOLOGY —the science of life and living things.

Such is the position of ethnology, such are the objects of the ethnologist. The paths or methods, by following which he may hope to reach his goal, are diverse. He may work at man from the point of view of the pure zoologist, and investigate the anatomical and physiological peculiarities of Negroes, Australians, or Mongolians, just as he would inquire into those of pointers, terriers, and turnspits,—"persistent modifications" of man's almost universal companion. Or he may seek aid from researches into the most human manifestation of humanity—Language; and assuming that what is true of speech is true of the speaker—a hypothesis as questionable in science as it is in ordinary life—he may apply to mankind themselves the conclusions drawn from a searching analysis of their words and grammatical forms.

Or, the ethnologist may turn to the study of the practical life of men; and relying upon the inherent conservatism and small inventiveness of untutored mankind, he may hope to discover in manners and customs, or in weapons, dwellings, and other handiwork, a clue to the origin of the resemblances and differences of nations. Or, he may resort to that kind of evidence which is yielded by History proper, and consists of the beliefs of men concerning past events, embodied in traditional, or in written, testimony. Or, when that thread breaks, Archæology, which is the interpretation of the unrecorded remains of man's works, belonging to the epoch since the world has reached its present condition, may still guide him. And, when even the dim light of archæology fades, there yet remains Palæontology, which, in these latter years, has brought to daylight once more the

exuvia of ancient populations, whose world was not our world, who have been buried in river beds immemorially dry, or carried by the rush of waters into caves, inaccessible to inundation since the dawn of tradition.

Along each, or all, of these paths the ethnologist may press towards his goal; but they are not equally straight, or sure, or easy to tread. The way of palæontology has but just been laid open to us. Archæological and historical investigations are of great value for all those peoples whose ancient state has differed widely from their present condition, and who have the good or evil fortune to possess a history. But on taking a broad survey of the world, it is astonishing how few nations present either condition. Respecting five-sixths of the persistent modifications of mankind, history and archæology are absolutely silent. For half the rest, they might as well be silent for anything that is to be made of their testimony. And, finally, when the question arises as to what was the condition of mankind more than a paltry two or three thousand years ago, history and archæology are, for the most part, mere dumb dogs. What light does either of these branches of knowledge throw on the past of the man of the New World, if we except the Central Americans and the Peruvians; on that of the Africans, save those of the valley of the Nile and a fringe of the Mediterranean; on that of all the Polynesian, Australian, and central Asiatic peoples, the former of whom probably, and the last certainly, were, at the dawn of history, substantially what they are now? While thankfully accepting what history has to give him, therefore, the ethnologist must not look for too much from her.

Is more to be expected from inquiries into the customs and handicrafts of men? It is to be feared not. In reasoning from identity of custom to identity of stock the difficulty always obtrudes itself, that the minds of

men being everywhere similar, differing in quality and quantity but not in kind of faculty, like circumstances must tend to produce like contrivances ; at any rate, so long as the need to be met and conquered is of a very simple kind. That two nations use calabashes or shells for drinking-vessels, or that they employ spears, or clubs, or swords and axes of stone and metal as weapons and implements, cannot be regarded as evidence that these two nations had a common origin, or even that intercommunication ever took place between them ; seeing that the convenience of using calabashes or shells for such purposes, and the advantage of poking an enemy with a sharp stick, or hitting him with a heavy one, must be early forced by nature upon the mind of even the stupidest savage. And when he had found out the use of a stick, he would need no prompting to discover the value of a chipped or wetted stone, or an angular piece of native metal, for the same object. On the other hand, it may be doubted whether the chances are not greatly against independent peoples arriving at the manufacture of a boomerang, or of a bow ; which last, if one comes to think of it, is a rather complicated apparatus ; and the tracing of the distribution of inventions as complex as these, and of such strange customs as betel-chewing and tobacco-smoking, may afford valuable ethnological hints.

Since the time of Leibnitz, and guided by such men as Humboldt, Abel Remusat, and Klaproth, Philology has taken far higher ground. Thus Prichard affirms that "the history of nations, termed Ethnology, must be mainly founded on the relations of their languages."

An eminent living philologer, August Schleicher, in a recent essay, puts forward the claims of his science still more forcibly :—

"If, however, language is the human κατ' ἐξοχήν, the suggestion arises whether it should not form the basis of any scientific systematic

arrangement of mankind; whether the foundation of the natural classification of the genus Homo has not been discovered in it.

"How little constant are cranial peculiarities and other so-called race characters! Language, on the other hand, is always a perfectly constant diagnostic. A German may occasionally compete in hair and prognathism with a negro, but a negro language will never be his mother tongue. Of how little importance for mankind the so-called race characters are, is shown by the fact that speakers of languages belonging to one and the same linguistic family may exhibit the peculiarities of various races. Thus the settled Osmanli Turk exhibits Caucasian characters, while other so-called Tartaric Turks exemplify the Mongol type. On the other hand, the Magyar and the Basque do not depart in any essential physical peculiarity from the Indo-Germans, whilst the Magyar, Basque, and Indo-Germanic tongues are widely different. Apart from their inconstancy, again, the so-called race characters can hardly yield a scientifically natural system. Languages, on the other hand, readily fall into a natural arrangement, like that of which other vital products are susceptible, especially when viewed from their morphological side. . . . The externally visible structure of the cerebral and facial skeletons, and of the body generally, is less important than that no less material but infinitely more delicate corporeal structure, the function of which is speech. I conceive, therefore, that the natural classification of languages is also the natural classification of mankind. With language, moreover, all the higher manifestations of man's vital activity are closely interwoven, so that these receive due recognition in and by that of speech."[1]

Without the least desire to depreciate the value of philology as an adjuvant to ethnology, I must venture to doubt, with Rudolphi, Desmoulins, Crawfurd, and others, its title to the leading position claimed for it by the writers whom I have just quoted. On the contrary, it seems to me obvious that, though, in the absence of any evidence to the contrary, unity of languages may afford a certain presumption in favour of the unity of stock of the peoples speaking those languages, it cannot be held to prove that unity of stock, unless philologers are prepared to demonstrate, that no nation can lose its language and

[1] August Schleicher. Ueber die Bedeutung der Sprache für die Naturgeschichte des Menschen, pp. 16—18. Weimar, 1858.

acquire that of a distinct nation, without a change of blood corresponding with the change of language. Desmoulins long ago put this argument exceedingly well:—

"Let us imagine the recurrence of one of those slow, or sudden, political revolutions, or say of those secular changes which among different people and at different epochs have annihilated historical monuments and even extinguished tradition. In that case, the evidence, now so clear, that the negroes of Hayti were slaves imported by a French colony, who, by the very effect of the subordination involved in slavery, lost their own diverse languages and adopted that of their masters, would vanish. And metaphysical philosophers, observing the identity of Haytian French with that spoken on the shores of the Seine and the Loire, would argue that the men of St. Domingo with woolly heads, black and oily skins, small calves, and slightly bent knees, are of the same race, descended from the same parental stock, as the Frenchmen with silky brown, chestnut, or fair hair, and white skins. For they would say, their languages are more similar than French is to German or Spanish."[1]

It must not be imagined that the case put by Desmoulins is a merely hypothetical one. Events precisely similar to the transport of a body of Africans to the West India Islands, indeed, cannot have happened among uncivilized races, but similar results have followed the importation of bodies of conquerors among an enslaved people over and over again. There is hardly a country in Europe in which two or more nations speaking widely different tongues have not become intermixed; and there is hardly a language of Europe of which we have any right to think that its structure affords a just indication of the amount of that intermixture.

As Dr. Latham has well said:—

"It is certain that the language of England is of Anglo-Saxon origin, and that the remains of the original Keltic are unimportant. It is by no means so certain that the blood of Englishmen is equally Germanic. A vast amount of Kelticism, not found in our tongue, very probably exists in our pedigrees. The ethnology of France is still more complicated. Many writers make the Parisian a Roman on the

[1] Desmoulins, "Histoire Naturelle des Races Humaines," p. 345. 1826.

strength of his language; whilst others make him a Kelt on the strength of certain moral characteristics, combined with the previous Kelticism of the original Gauls. Spanish and Portuguese, as languages, are derivations from the Latin; Spain and Portugal, as countries, are Iberic, Latin, Gothic, and Arab, in different proportions. Italian is modern Latin all the world over; yet surely there must be much Keltic blood in Lombardy, and much Etruscan intermixture in Tuscany.

"In the ninth century every man between the Elbe and the Niemen spoke some Slavonic dialect; they now nearly all speak German. Surely the blood is less exclusively Gothic than the speech."[1]

In other words, what philologer, if he had nothing but the vocabulary and grammar of the French and English languages to guide him, would dream of the real causes of the unlikeness of a Norman to a Provençal, of an Orcadian to a Cornishman? How readily might he be led to suppose that the different climatal conditions to which these speakers of one tongue have so long been exposed, have caused their physical differences; and how little would he suspect that these are due (as we happen to know they are) to wide differences of blood.

Few take duly into account the evidence which exists as to the ease with which unlettered savages gain or lose a language. Captain Erskine, in his interesting "Journal of a Cruise among the Islands of the Western Pacific," especially remarks upon the "avidity with which the inhabitants of the polyglot islands of Melanesia, from New Caledonia to the Solomon Islands, adopt the improvements of a more perfect language than their own, which different causes and accidental communication still continue to bring to them;" and he adds that "among the Melanesian islands scarcely one was found by us which did not possess, in some cases still imperfectly, the decimal system of numeration in addition to their own, in which they reckon only to five."

[1] Latham, "Man and his Migrations," p. 171.

Yet how much philological reasoning in favour of the affinity or diversity of two distinct peoples has been based on the mere comparison of numerals!

But the most instructive example of the fallacy which may attach to merely philological reasonings, is that afforded by the Feejeans, who are, physically, so intimately connected with the adjacent Negritos of New Caledonia, &c., that no one can doubt to what stock they belong, and who yet, in the form and substance of their language, are Polynesian. The case is as remarkable as if the Canary Islands should have been found to be inhabited by negroes speaking Arabic, or some other clearly Semitic dialect, as their mother tongue. As it happens, the physical peculiarities of the Feejeans are so striking, and the conditions under which they live are so similar to those of the Polynesians, that no one has ventured to suggest that they are merely modified Polynesians—a suggestion which could otherwise certainly have been made. But if languages may be thus transferred from one stock to another, without any corresponding intermixture of blood, what ethnological value has philology?—what security does unity of language afford us that the speakers of that language may not have sprung from two, or three, or a dozen, distinct sources?

Thus we come, at last, to the purely zoological method, from which it is not unnatural to expect more than from any other, seeing that, after all, the problems of ethnology are simply those which are presented to the zoologist by every widely distributed animal he studies. The father of modern zoology seems to have had no doubt upon this point. At the twenty-eighth page of the standard twelfth edition of the "Systema Naturæ," in fact, we find:—

I. PRIMATES.

Dentes primores incisores: superiores IV. paralleli, mammæ pectorales II.

1. HOMO. Nosce te ipsum.
Sapiens. 1. H. diurnus : *varians cultura, loco.*
Ferus. Tetrapus, mutus, hirsutus.

. . . . . .

Americanus α. Rufus, cholericus, rectus—*Pilis* nigris, rectis, crassis—*Naribus* patulis—*Facie* ephelitica : *Mento* subimberbi.
Pertinax, contentus, liber. *Pingit* se lineis dædaleis rubris.
Regitur Consuetudine.

Europæus β. Albus sanguineus torosus. *Pilis* flavescentibus, prolixis. *Oculis* cœruleis.
Levis, argutus, inventor. *Tegitur* Vestimentis arctis. *Regitur* Ritibus.

Asiaticus γ. Luridus, melancholicus, rigidus. *Pilis* nigricantibus. *Oculis* fuscis. *Severus*, fastuosus, avarus. *Tegitur* Indumentis laxis.
Regitur Opinionibus.

Afer δ. Niger, phlegmaticus, laxus. *Pilis* atris, contortuplicatis. *Cute* holosericea. *Naso* simo. *Labiis* tumidis. *Feminis* sinus pudoris.
Mammæ lactantes prolixæ.
Vafer, segnis, negligens. *Ungit* se pingui. *Regitur* Arbitrio.

Monstrosus ε. Solo (a) et arte (b c) variat. :
a. *Alpini* parvi, agiles, timidi.
Patagonici magni, segnes.
b. *Monorchides* ut minus fertiles : Hottentotti.
Junceæ puellæ, abdomine attenuato : Europeæ.
c. *Macrocephali* capiti conico : Chinenses.
Plagiocephali capite antice compresso : Canadenses.

Turn a few pages further on in the same volume, and there appears, with a fine impartiality in the distribution of capitals and sub-divisional headings :—

III. FERÆ.

Dentes primores superiores sex, acutiusculi. Canini solitarii.

. . . . . .

12. CANIS.	*Dentes primores* superiores VI.: laterales longiores distantes: intermedii lobati. Inferiores VI.: laterales lobati.
	Laniarii solitarii, incurvati.
	Molares VI. s. VII. (pluresve quam in reliquis).
familiaris 1.	C. cauda (sinistrorsum) recurvata. . . . .
domesticus α.	auriculis erectis, cauda subtus lanata.
sagax β.	auriculis pendulis, digito spurio ad tibias posticas.
grajus γ.	magnitudine lupi, trunco curvato, rostro attenuato, &c. &c.

Linnæus' definition of what he considers to be mere varieties of the species Man are, it will be observed, as completely free from any allusion to linguistic peculiarities as those brief and pregnant sentences in which he sketches the characters of the varieties of the species Dog. "Pilis nigris, naribus patulis" may be set against "auriculis erectis, cauda subtus lanata;" while the remarks on the morals and manners of the human subject seem as if they were thrown in merely by way of makeweight.

Buffon, Blumenbach (the founder of ethnology as a special science), Rudolphi, Bory de St. Vincent, Desmoulins, Cuvier, Retzius, indeed I may say all the naturalists proper, have dealt with man from a no less completely zoological point of view; while, as might have been expected, those who have been least naturalists, and most linguists, have most neglected the zoological method, the neglect culminating in those who have been altogether devoid of acquaintance with anatomy.

Prichard's proposition, that language is more persistent than physical characters, is one which has never been

proved, and indeed admits of no proof, seeing that the records of language do not extend so far as those of physical characters. But, until the superior tenacity of linguistic over physical peculiarities is shown, and until the abundant evidence which exists, that the language of a people may change without corresponding physical change in that people, is shown to be valueless, it is plain that the zoological court of appeal is the highest for the ethnologist, and that no evidence can be set against that derived from physical characters.

What, then, will a new survey of mankind from the Linnean point of view teach us?

The great antipodal block of land we call Australia has, speaking roughly, the form of a vast quadrangle, 2,000 miles on the side, and extends from the hottest tropical, to the middle of the temperate, zone. Setting aside the foreign colonists introduced within the last century, it is inhabited by people no less remarkable for the uniformity, than for the singularity, of their physical characters and social state. For the most part of fair stature, erect and well built, except for an unusual slenderness of the lower limbs, the AUSTRALIANS have dark, usually chocolate-coloured skins; fine dark wavy hair; dark eyes, overhung by beetle brows; coarse, projecting jaws; broad and dilated, but not especially flattened, noses; and lips which, though prominent, are eminently flexible.

The skulls of these people are always long and narrow, with a smaller development of the frontal sinuses than usually corresponds with such largely developed brow ridges. An Australian skull of a round form, or one the transverse diameter of which exceeds eight-tenths of its length, has never been seen. These people, in a word, are eminently "dolichocephalic," or long-headed; but,

with this one limitation, their crania present considerable variations, some being comparatively high and arched, while others are more remarkably depressed than almost any other human skulls.

The female pelvis differs comparatively little from the European; but in the pelves of male Australians which I have examined, the antero-posterior and transverse diameters approach equality more nearly than is the case in Europeans.

No Australian tribe has ever been known to cultivate the ground, to use metals, pottery, or any kind of textile fabric. They rarely construct huts. Their means of navigation are limited to rafts or canoes, made of sheets of bark. Clothing, except skin cloaks for protection from cold, is a superfluity with which they dispense; and though they have some singular weapons, almost peculiar to themselves, they are wholly unacquainted with bows and arrows.

It is but a step, as it were, across Bass's Straits to Tasmania. Neither climate nor the characteristic forms of vegetable or animal life change largely on the south side of the Straits, but the early voyagers found Man singularly different from him on the north side. The skin of the Tasmanian was dark, though he lived between parallels of latitude corresponding with those of middle Europe in our own hemisphere; his jaws projected, his head was long and narrow; his civilization was about on a footing with that of the Australian, if not lower, for I cannot discover that the Tasmanian understood the use of the throwing-stick. But he differed from the Australian in his woolly, negro-like hair, whence the name of NEGRITO, which has been applied to him and his congeners.

Such Negritos—differing more or less from the Tasmanian, but agreeing with him in dark skin and woolly

hair—occupy New Caledonia, the New Hebrides, the Louisiade Archipelago; and stretching to the Papuan Islands, and for a doubtful extent beyond them to the north and west, form a sort of belt, or zone, of Negrito population, interposed between the Australians on the west and the inhabitants of the great majority of the Pacific islands on the east.

The cranial characters of the Negritos vary considerably more than those of their skin and hair, the most notable circumstance being the strong Australian aspect which distinguishes many Negrito skulls, while others tend rather towards forms common in the Polynesian islands.

In civilization, New Caledonia exhibits an advance upon Tasmania, and, farther north, there is a still greater improvement. But the bows and arrows, the perched houses, the outrigger canoes, the habits of betel-chewing and of kawa-drinking, which abound more or less among the northern Negritos, are probably to be regarded not as the products of an indigenous civilization, but merely as indications of the extent to which foreign influences have modified the primitive social state of these people.

From Tasmania or New Caledonia, to New Zealand or Tongataboo, is again but a brief voyage; but it brings about a still more notable change in the aspect of the indigenous population than that effected by the passage of Bass's Straits. Instead of being chocolate-coloured people, the Maories and Tongans are light brown; instead of woolly, they have straight, or wavy, black hair. And if from New Zealand, we travel some 5,000 miles east to Easter Island; and from Easter Island, for as great a distance north-west, to the Sandwich Islands; and thence 7,000 miles, westward and southward, to Sumatra; and even across the Indian Ocean, into the interior of Madagascar, we shall everywhere meet with people whose hair

is straight or wavy, and whose skins exhibit various shades of brown. These are the Polynesians, Micronesians, Indonesians, whom Latham has grouped together under the common title of AMPHINESIANS.

The cranial characters of these people, as of the Negritos, are less constant than those of their skin and hair. The Maori has a long skull; the Sandwich Islander a broad skull. Some, like these, have strong brow ridges; others, like the Dayaks and many Polynesians, have hardly any nasal indentation.

It is only in the westernmost parts of their area that the Amphinesian nations know anything about bows and arrows as weapons, or are acquainted with the use of metals or with pottery. Everywhere they cultivate the ground, construct houses, and skilfully build and manage outrigger, or double, canoes; while, almost everywhere, they use some kind of fabric for clothing.

Between Easter Island, or the Sandwich Islands, and any part of the American coast is a much wider interval than that between Tasmania and New Zealand, but the ethnological interval between the American and the Polynesian is less than that between either of the previously named stocks.

The typical AMERICAN has straight black hair and dark eyes, his skin exhibiting various shades of reddish or yellowish brown, sometimes inclining to olive. The face is broad and scantily bearded; the skull wide and high. Such people extend from Patagonia to Mexico, and much farther north along the west coast. In the main a race of hunters, they had nevertheless, at the time of the discovery of the Americas, attained a remarkable degree of civilization in some localities. They had domesticated ruminants, and not only practised agriculture, but had learned the value of irrigation. They manufactured textile fabrics, were masters of the potter's

art, and knew how to erect massive buildings of stone. They understood the working of the precious, though not of the useful, metals; and had even attained to a rude kind of hieroglyphic, or picture, writing.

The Americans not only employ the bow and arrow, but, like some Amphinesians, the blow-pipe, as offensive weapons: but I am not aware that the outrigger canoe has ever been observed among them.

I have reason to suspect that some of the Fuegian tribes differ cranially from the typical Americans; and the Northern and Eastern American tribes have longer skulls than their Southern compatriots. But the ESQUIMAUX, who roam on the desolate and ice-bound coasts of Arctic America, certainly present us with a new stock. The Esquimaux (among whom the Greenlanders are included), in fact, though they share the straight black hair of the proper Americans, are a duller complexioned, shorter, and more squat people, and they have still more prominent cheek-bones. But the circumstance which most completely separates them from the typical Americans, is the form of their skulls, which instead of being broad, high, and truncated behind, are eminently long, usually low, and prolonged backwards.

These Hyperborean people clothe themselves in skins, know nothing of pottery, and hardly anything of metals. Dependent for existence upon the produce of the chase, the seal and the whale are to them what the cocoa-nut tree and the plantain are to the savages of more genial climates. Not only are those animals meat and raiment, but they are canoes, sledges, weapons, tools, windows, and fire; while they support the dog, who is the indispensable ally and beast of burden of the Esquimaux.

It is admitted that the Tchuktchi, on the eastern side of Behring's Straits, are, in all essential respects,

Esquimaux; and I do not know that there is any satisfactory evidence to show that the Tunguses and Samoiedes do not essentially share the physical characters of the same people. Southward, there are indications of Esquimaux characters among the Japanese, and it is possible that their influence may be traced yet further.

However this may be, Eastern Asia, from Mantchouria to Siam, Thibet, and Northern Hindostan, is continuously inhabited by men, usually of short stature, with skins varying in colour from yellow to olive; with broad cheek-bones and faces that, owing to the insignificance of the nose, are exceedingly flat; and with small, obliquely-set, black eyes and straight black hair, which sometimes attains a very great length upon the scalp, but is always scanty upon the face and body. The skull is never much elongated, and is, generally, remarkably broad and rounded, with hardly any nasal depression, and but slight, if any, projection of the jaws.

Many of these people, for whom the old name of MONGOLIANS may be retained, are nomades; others, as the Chinese, have attained a remarkable and apparently indigenous civilization, only surpassed by that of Europe.

At the north-western extremity of Europe the Lapps repeat the characters of the Eastern Asiatics. Between these extreme points, the Mongolian stock is not continuous, but is represented by a chain of more or less isolated tribes, who pass under the name of Calmucks and Tartars, and form Mongolian islands, as it were, in the midst of an ocean of other people.

The waves of this ocean are the nations for whom, in order to avoid the endless confusion produced by our present half-physical, half-philological classification, I shall use a new name—XANTHOCHROI—indicating that they are "yellow" haired and "pale" in complexion.

The Chinese historians of the Han dynasty, writing in the third century before our era, describe, with much minuteness, certain numerous and powerful barbarians with "yellow hair, green eyes, and prominent noses," who, the black-haired, skew-eyed, and flat-nosed annalists remark in passing, are "just like the apes from whom they are descended." These people held, in force, the upper waters of the Yenisei, and thence under various names stretched southward to Thibet and Kashgar. Fair-haired and blue-eyed northern enemies were no less known to the ancient Hindoos, to the Persians, and to the Egyptians, on the south of the great central Asiatic area; while the testimony of all European antiquity is to the effect that, before and since the period in question, there lay beyond the Danube, the Rhine, and the Seine, a vast and dangerous yellow or red haired, fair-skinned, blue-eyed population. Whether the disturbers of the marches of the Roman Empire were called Gauls or Germans, Goths, Alans, or Scythians, one thing seems certain, that until the invasion of the Huns, they were tall, fair, blue-eyed men.

If any one should think fit to assume that in the year 100 B.C., there was one continuous Xanthochroic population from the Rhine to the Yenisei, and from the Ural mountains to the Hindoo Koosh, I know not that any evidence exists by which that position could be upset, while the existing state of things is rather in its favour than otherwise. For the Scandinavians, wholly, the Germans to a great extent, the Slavonian and the Finnish tribes, some of the inhabitants of Greece, many Turks, some Kirghis, and some Mantchous, the Ossetes in the Caucasus, the Siahposh, the Rohillas, are at the present day fair, yellow or red haired, and blue-eyed; and the interpolation of tribes of Mongolian hair and complexion, as far west as the Caspian Steppes and the

Crimea, might justly be accounted for by those subsequent westward irruptions of the Mongolian stock, of which history furnishes abundant testimony.

The furthermost limit of the Xanthochroi north-westward is Iceland and the British Isles; south-westward, they are traceable at intervals through the Berber country, and end in the Canary Islands.

The cranial characters of the Xanthochroi are not, at present, strictly definable. The Scandinavians are certainly long-headed; but many Germans, the Swiss so far as they are Germanized, the Slavonians, the Fins, and the Turks, are short-headed. What were the cranial characters of the ancient "U-suns" and "Ting-lings" of the valley of the Yenisei is unknown.

West of the area occupied by the chief mass of the Xanthochroi, and north of the Sahara, is a broad belt of land, shaped like a ≻. Between the forks of the Y lies the Mediterranean; the stem of it is Arabia. The stem is bathed by the Indian Ocean, the western ends of the forks by the Atlantic. The people inhabiting the area thus roughly sketched have, like the Xanthochroi, prominent noses, pale skins and wavy hair, with abundant beards; but, unlike them, the hair is black or dark, and the eyes usually so. They may thence be called the MELANOCHROI. Such people are found in the British Islands, in Western and Southern Gaul, in Spain, in Italy south of the Po, in parts of Greece, in Syria and Arabia, stretching as far northward and eastward as the Caucasus and Persia. They are the chief inhabitants of Africa north of the Sahara, and, like the Xanthochroi, they end in the Canary Islands. They are known as Kelts, Iberians, Etruscans, Romans, Pelasgians, Berbers, Semites. The majority of them are long-headed, and of smaller stature than the Xanthochroi.

It is needless to remark upon the civilization of these

two great stocks. With them has originated everything that is highest in science, in art, in law, in politics, and in mechanical inventions. In their hands, at the present moment, lies the order of the social world, and to them its progress is committed.

South of the Atlas, and of the Great Desert, Middle Africa exhibits a new type of humanity in the NEGRO, with his dark skin, woolly hair, projecting jaws, and thick lips. As a rule, the skull of the Negro is remarkably long; it rarely approaches the broad type, and never exhibits the roundness of the Mongolian. A cultivator of the ground, and dwelling in villages; a maker of pottery, and a worker in the useful as well as the ornamental metals; employing the bow and arrow as well as the spear, the typical negro stands high in point of civilization above the Australian.

Resembling the Negroes in cranial characters, the BUSHMEN of South Africa differ from them in their yellowish brown skins, their tufted hair, their remarkably small stature, and their tendency to fatty and other integumentary outgrowths; nor is the wonderful click with which their speech is interspersed to be overlooked in enumerating the physical characteristics of this strange people.

The so-called "Drawidian" populations of Southern Hindostan lead us back, physically as well as geographically, towards the Australians; while the diminutive MINCOPIES of the Andaman Islands lie midway between the Negro and Negrito races, and, as Mr. Busk has pointed out, occasionally present the rare combination of Brachycephaly, or short-headedness, with woolly hair.

In the preceding progress along the outskirts of the habitable world, eleven readily distinguishable stocks, or persistent modifications, of mankind, have been recog-

nized. I have purposely omitted such people as the Abyssinians and the Hindoos, who there is every reason to believe result from the intermixture of distinct stocks. Perhaps I ought, for like reasons, to have ignored the Mincopies. But I do not pretend that my enumeration is complete or, in any sense, perfect. It is enough for my purpose if it be admitted (and I think it cannot be denied) that those which I have mentioned exist, are well marked, and occupy the greater part of the habitable globe.

In attempting to classify these persistent modifications after the manner of naturalists, the first circumstance that attracts one's attention is the broad contrast between the people with straight and wavy hair, and those with crisp, woolly, or tufted hair. Bory de St. Vincent, noting this fundamental distinction, divided mankind accordingly into the two primary groups of *Leiotrichi* and *Ulotrichi*,—terms which are open to criticism, but which I adopt in the accompanying table, because they have been used. It is better for science to accept a faulty name which has the merit of existence, than to burthen it with a faultless newly invented one.

Under each of these divisions are two columns, one for the Brachycephali, or short heads, and one for the Dolichocephali,[1] or long heads. Again, each column is subdivided transversely into four compartments, one for the "leucous," people with fair complexions and yellow or red hair; one for the "leucomelanous," with dark hair and pale skins; one for the "xanthomelanous," with black hair and yellow, brown, or olive skins; and one for the "melanous," with black hair and dark brown or blackish skins.

1 Skulls, the transverse diameter of which is more than eight-tenths the long diameter, are short; those which have the transverse diameter less than eight-tenths the longitudinal, are long.

Leiotrichi.		Ulotrichi.	
Dolichocephali.	Brachycephali.	Dolichocephali.	Brachycephali.
Leucous.			
. . . . Xanthochroi			
Leucomelanous.			
. . . . Melanochroi			
Xanthomelanous.			
Esquimaux.	Mongolians.	*Bushmen.*	
Amphinesians.			
Americans.			
Melanous.			
Australians.		Negroes.	*Mincopies* (?)
		Negritos.	

*** *The names of the stocks known only since the fifteenth century are put into italics. If the "Skrälings" of the Norse discoverers of America were Esquimaux, Europeans became acquainted with the latter six or seven centuries earlier.*

It is curious to observe that almost all the woolly-headed people are also long-headed; while among the straight-haired nations broad heads preponderate, and only two stocks, the Esquimaux and the Australians, are exclusively long-headed.

One of the acutest and most original of ethnologists, Desmoulins, originated the idea, which has subsequently been fully developed by Agassiz, that the distribution of the persistent modifications of man is governed by the same laws as that of other animals, and that both fall into the same great distributional provinces. Thus, Australia; America, south of Mexico; the Arctic regions; Europe, Syria, Arabia, and North Africa, taken together, are each regions eminently characterized by the nature of their animal and vegetable populations, and each, as we have seen, has its peculiar and characteristic form of man. But it may be doubted whether the parallel thus drawn will hold good strictly, and in all cases. The Tasmanian Fauna and Flora are essentially Australian, and the like is true to a less extent of many, if not of

all, the Papuan islands; but the Negritos who inhabit these islands are strikingly different from the Australians. Again, the differences between the Mongolians and the Xanthochroi are out of all proportion greater than those between the Faunæ and Floræ of Central and Eastern Asia. But whatever the difficulties in the way of the detailed application of this comparison of the distribution of men with that of animals, it is well worthy of being borne in mind, and carried as far as it will go.

Apart from all speculation, a very curious fact regarding the distribution of the persistent modifications of mankind becomes apparent on inspecting an Ethnological chart, projected in such a manner that the Pacific Ocean occupies its centre. Such a chart exhibits an Australian area occupied by dark smooth-haired people, separated by an incomplete inner zone of dark woolly-haired Negritos and Negroes, from an outer zone of comparatively pale and smooth-haired men, occupying the Americas, and nearly all Asia and North Africa.

Such is a brief sketch of the characters and distribution of the persistent modifications, or stocks, of mankind at the present day. If we seek for direct evidence of how long this state of things has lasted, we shall find little enough, and that little far from satisfactory. Of the eleven different stocks enumerated, seven have been known to us for less than 400 years; and of these seven not one possessed a fragment of written history at the time it came into contact with European civilization. The other four—the Negroes, Mongolians, Xanthochroi, and Melanochroi—have always existed in some of the localities in which they are now found, nor do the negroes ever seem to have voluntarily travelled beyond the limits of their present area. But ancient history is in a great

measure the record of the mutual encroachments of the other three stocks.

On the whole, however, it is wonderful how little change has been effected by these mutual invasions and intermixtures. As at the present time, so at the dawn of history, the Melanochroi fringed the Atlantic and the Mediterranean; the Xanthochroi occupied most of Central and Eastern Europe, and much of Western and Central Asia; while Mongolians held the extreme east of the Old World. So far as history teaches us, the populations of Europe, Asia, and Africa were, twenty centuries ago, just what they are now, in their broad features and general distribution.

The evidence yielded by Archæology is not very definite, but, so far as it goes, it is to much the same effect. The mound builders of Central America seem to have had the characteristic short and broad head of the modern inhabitants of that continent. The tumuli and tombs of Ancient Scandinavia, of pre-Roman Britain, of Gaul, of Switzerland, reveal two types of skull—a broad and a long—of which, in Scandinavia, the broad seems to have belonged to the older stock, while the reverse was probably the case in Britain, and certainly in Switzerland. It has been assumed that the broad-skulled people of ancient Scandinavia were Lapps; but there is no proof of the fact, and they may have been, like the broad-skulled Swiss and Germans, Xanthochroi. One of the greatest of ethnological difficulties is to know where the modern Swedes, Norsemen, and Saxons got their long heads, as all their neighbours, Fins, Lapps, Slavonians, and South Germans, are broad-headed. Again, who were the small-handed, long-headed people of the "bronze epoch," and what has become of the infusion of their blood among the Xanthochroi?

At present Palæontology yields no safe data to the

ethnologist. We know absolutely nothing of the ethnological characters of the men of Abbeville and Hoxne; but must be content with the demonstration, in itself of immense value, that Man existed in Western Europe when its physical condition was widely different from what it is now, and when animals existed, which, though they belong to what is, properly speaking, the present order of things, have long been extinct. Beyond the limits of a fraction of Europe, Palæontology tells us nothing of man or of his works.

To sum up our knowledge of the ethnological past of man: so far as the light is bright, it shows him substantially as he is now; and, when it grows dim, it permits us to see no sign that he was other than he is now.

It is a general belief that men of different stocks differ as much physiologically as they do morphologically; but it is very hard to prove, in any particular case, how much of a supposed national characteristic is due to inherent physiological peculiarities, and how much to the influence of circumstances. There is much evidence to show, however, that some stocks enjoy a partial or complete immunity from diseases which destroy, or decimate, others. Thus there seems good ground for the belief that Negroes are remarkably exempt from yellow fever; and that, among Europeans, the melanochrous people are less obnoxious to its ravages than the xanthochrous. But many writers, not content with physiological differences of this kind, undertake to prove the existence of others of far greater moment; and, indeed, to show that certain stocks of mankind exhibit, more or less distinctly, the physiological characters of true species. Unions between these stocks, and still more between the half-breeds arising from their mixture, are affirmed to be either infertile, or less fertile than those

which take place between males and females of either stock under the same circumstances. Some go so far as to assert that no mixed breeds of mankind can maintain themselves without the assistance of one or other of the parent stocks, and that, consequently, they must inevitably be obliterated in the long run.

Here, again, it is exceedingly difficult to obtain trustworthy evidence, and to free the effects of the pure physiological experiment from adventitious influences. The only trial which, by a strange chance, was kept clear of all such influences—the only instance in which two distinct stocks of mankind were crossed, and their progeny intermarried without any admixture from without—is the famous case of the Pitcairn Islanders, who were the progeny of Bligh's English sailors by Tahitian women. The results of this experiment, as everybody knows, are dead against those who maintain the doctrine of human hybridity, seeing that the Pitcairn Islanders, even though they necessarily contracted consanguineous marriages, throve and multiplied exceedingly.

But those who are disposed to believe in this doctrine should study the evidence brought forward in its support by M. Broca, its latest and ablest advocate, and compare this evidence with that which the botanists, as represented by a Gaertner, or by a Darwin, think it indispensable to obtain before they will admit the infertility of crosses between two allied kinds of plants. They will then, I think, be satisfied that the doctrine in question rests upon a very unsafe foundation; that the facts adduced in its support are capable of many other interpretations; and, indeed, that from the very nature of the case, demonstrative evidence one way or the other is almost unattainable. *A priori*, I should be disposed to expect a certain amount of infertility between some of the extreme modifications of mankind; and still more

between the offsprings of their intermixture. *A posteriori*, I cannot discover any satisfactory proof that such infertility exists.

From the facts of ethnology I now turn to the theories and speculations of ethnologists, which have been devised to explain these facts, and to furnish satisfactory answers to the inquiry—what conditions have determined the existence of the persistent modifications of mankind, and have caused their distribution to be what it is?

These speculations may be grouped under three heads: firstly, the Monogenist hypotheses; secondly, those of the Polygenists; and thirdly, that which would result from a simple application of Darwinian principles to mankind.

According to the Monogenists, all mankind have sprung from a single pair, whose multitudinous progeny spread themselves over the world, such as it now is, and became modified into the forms we meet with in the various regions of the earth, by the effect of the climatal and other conditions to which they were subjected.

The advocates of this hypothesis are divisible into several schools. There are those who represent the most numerous, respectable, and would-be orthodox of the public, and are what may be called "Adamites," pure and simple. They believe that Adam was made out of earth somewhere in Asia, about six thousand years ago; that Eve was modelled from one of his ribs; and that the progeny of these two having been reduced to the eight persons who were landed on the summit of Mount Ararat after an universal deluge, all the nations of the earth have proceeded from these last, have migrated to their present localities, and have become converted into Negroes, Australians, Mongolians, &c., within that time. Five-sixths of the public are taught this Adamitic Monogenism, as if it were an established truth, and believe it.

I do not; and I am not acquainted with any man of science, or duly instructed person, who does.

A second school of monogenists, not worthy of much attention, attempts to hold a place midway between the Adamites and a third division, who take up a purely scientific position, and require to be dealt with accordingly. This third division, in fact, numbers in its ranks Linnæus, Buffon, Blumenbach, Cuvier, Prichard, and many distinguished living ethnologists.

These "Rational Monogenists," or, at any rate, the more modern among them, hold, firstly, that the present condition of the earth has existed for untold ages; secondly, that, at a remote period, beyond the ken of Archbishop Usher, man was created, somewhere between the Caucasus and the Hindoo Koosh; thirdly, that he might have migrated thence to all parts of the inhabited world, seeing that none of them are unattainable from some other inhabited part, by men provided with only such means of transport as savages are known to possess and must have invented; fourthly, that the operation of the existing diversities of climate and other conditions upon people so migrating, is sufficient to account for all the diversities of mankind.

Of the truth of the first of these propositions no competent judge now entertains any doubt. The second is more open to discussion, for in these latter days many question the special creation of man: and even if his special creation be granted, there is not a shadow of a reason why he should have been created in Asia rather than anywhere else. Of all the odd myths that have arisen in the scientific world, the "Caucasian mystery," invented quite innocently by Blumenbach, is the oddest. A Georgian woman's skull was the handsomest in his collection. Hence it became his model exemplar of human skulls, from which all others might be regarded as

deviations; and out of this, by some strange intellectual hocus-pocus, grew up the notion that the Caucasian man is the prototypic "Adamic" man, and his country the primitive centre of our kind. Perhaps the most curious thing of all is, that the said Georgian skull, after all, is not a skull of average form, but distinctly belongs to the brachycephalic group.

With the third proposition I am quite disposed to agree, though it must be recollected that it is one thing to allow that a given migration is possible, and another to admit there is good reason to believe it has really taken place.

But I can find no sufficient ground for accepting the fourth proposition; and I doubt if it would ever have obtained its general currency except for the circumstance that fair Europeans are very readily tanned and embrowned by the sun. But I am not aware that there is a particle of proof that the cutaneous change thus effected can become hereditary, any more than that the enlarged livers, which plague our countrymen in India, can be transmitted;—while there is very strong evidence to the contrary. Not only, in fact, are there such cases as those of the English families in Barbadoes, who have remained for six generations unaltered in complexion, but which are open to the objection that they may have received infusions of fresh European blood; but there is the broad fact, that not a single indigenous Negro exists either in the great alluvial plains of tropical South America, or in the exposed islands of the Polynesian Archipelago, or among the populations of equatorial Borneo or Sumatra. No satisfactory explanation of these obvious difficulties has been offered by the advocates of the direct influence of conditions. And as for the more important modifications observed in the structure of the brain, and in the form of the skull, no one

has ever pretended to show in what way they can be effected directly by climate.

It is here, in fact, that the strength of the Polygenists, or those who maintain that men primitively arose, not from one, but from many stocks, lies. Show us, they say to the Monogenists, a single case in which the characters of a human stock have been essentially modified without its being demonstrable, or, at least, highly probable, that there has been intermixture of blood with some foreign stock. Bring forward any instance in which a part of the world, formerly inhabited by one stock, is now the dwelling-place of another, and we will prove the change to be the result of migration, or of intermixture, and not of modification of character by climatic influences. Finally, prove to us that the evidence in favour of the specific distinctness of many animals, admitted to be distinct species by all zoologists, is a whit better than that upon which we maintain the specific distinctness of men.

If presenting unanswerable objections to your adversary were the same thing as proving your own case, the Polygenists would be in a fair way towards victory; but, unfortunately, as I have already observed, they have as yet completely failed to adduce satisfactory positive proof of the specific diversity of mankind. Like the Monogenists, the Polygenists are of several sects; some imagine that their assumed species of mankind were created where we find them—the African in Africa, and the Australian in Australia, along with the other animals of their distributional province; others conceive that each species of man has resulted from the modification of some antecedent species of ape—the American from the broad-nosed Simians of the New World, the African from the Troglodytic stock, the Mongolian from the Orangs.

The first hypothesis is hardly likely to win much

favour. The whole tendency of modern science is to thrust the origination of things further and further into the background; and the chief philosophical objection to Adam being, not his oneness, but the hypothesis of his special creation; the multiplication of that objection tenfold is, whatever it may look, an increase, instead of a diminution, of the difficulties of the case. And, as to the second alternative, it may safely be affirmed that, even if the differences between men are specific, they are so small, that the assumption of more than one primitive stock for all is altogether superfluous. Surely no one can now be found to assert that any two stocks of mankind differ as much as a chimpanzee and an orang do; still less that they are as unlike as either of these is to any New World Simian!

Lastly, the granting of the Polygenist premises does not, in the slightest degree, necessitate the Polygenist conclusion. Admit that Negroes and Australians, Negritos and Mongols are distinct species, or distinct genera, if you will, and you may yet, with perfect consistency, be the strictest of Monogenists, and even believe in Adam and Eve as the primæval parents of all mankind.

It is to Mr. Darwin we owe this discovery: it is he who, coming forward in the guise of an eclectic philosopher, presents his doctrine as the key to ethnology, and as reconciling and combining all that is good in the Monogenistic and Polygenistic schools.

It is true that Mr. Darwin has not, in so many words, applied his views to ethnology; but even he who "runs and reads" the "Origin of Species" can hardly fail to do so; and, furthermore, Mr. Wallace and M. Pouchet have recently treated of ethnological questions from this point of view. Let me, in conclusion, add my own contribution to the same store.

I assume Man to have arisen in the manner which I

have discussed elsewhere, and probably, though by no means necessarily, in one locality. Whether he arose singly, or a number of examples appeared contemporaneously, is also an open question for the believer in the production of species by the gradual modification of pre-existing ones. At what epoch of the world's history this took place, again, we have no evidence whatever. It may have been in the older tertiary, or earlier, but what is most important to remember is, that the discoveries of late years have proved that man inhabited Western Europe, at any rate, before the occurrence of those great physical changes which have given Europe its present aspect. And as the same evidence shows that man was the contemporary of animals which are now extinct, it is not too much to assume that his existence dates back at least as far as that of our present Fauna and Flora, or before the epoch of the drift.

But if this be true, it is somewhat startling to reflect upon the prodigious changes which have taken place in the physical geography of this planet since man has been an occupant of it.

During that period the greater part of the British islands, of Central Europe, of Northern Asia, have been submerged beneath the sea and raised up again. So has the great desert of Sahara, which occupies the major part of Northern Africa. The Caspian and the Aral seas have been one, and their united waters have probably communicated with both the Arctic and the Mediterranean oceans. The greater part of North America has been under water, and has emerged. It is highly probable that a large part of the Malayan Archipelago has sunk, and its primitive continuity with Asia has been destroyed. Over the great Polynesian area subsidence has taken place to the extent of many thousands of feet—subsidence of so vast a character, in fact, that

if a continent like Asia had once occupied the area of the Pacific, the peaks of its mountains would now show not more numerous than the islands of the Polynesian Archipelago.

What lands may have been thickly populated for untold ages, and subsequently have disappeared and left no sign above the waters, it is of course impossible for us to say; but unless we are to make the wholly unjustifiable assumption that no dry land rose elsewhere when our present dry land sank, there must be half-a-dozen Atlantises beneath the waves of the various oceans of the world. But if the regions which have undergone these slow and gradual, but immense alterations, were wholly or in part inhabited before the changes I have indicated began—and it is more probable that they were, than that they were not—what a wonderfully efficient "Emigration Board" must have been at work all over the world long before canoes, or even rafts, were invented; and before men were impelled to wander by any desire nobler or stronger than hunger. And as these rude and primitive families were thrust, in the course of long series of generations, from land to land, impelled by encroachments of sea or of marsh, or by severity of summer heat or winter cold, to change their positions, what opportunities must have been offered for the play of natural selection, in preserving one family variation and destroying another!

Suppose, for example, that some families of a horde which had reached a land charged with the seeds of yellow fever, varied in the direction of woolliness of hair and darkness of skin. Then, if it be true that these physical characters are accompanied by comparative or absolute exemptions from that scourge, the inevitable tendency would be to the preservation and multiplication of the darker and woollier families, and

the elimination of the whiter and smoother-haired. In fact, by the operation of causes precisely similar to those which, in the famous instance cited by Mr. Darwin, have given rise to a race of black pigs in the forests of Louisiana, a negro stock would eventually people the region.

Again, how often, by such physical changes, must a stock have been isolated from all others for innumerable generations, and have found ample time for the hereditary hardening of its special peculiarities into the enduring characters of a persistent modification.

Nor, if it be true that the physiological difference of species may be produced by variation and natural selection, as Mr. Darwin supposes, would it be at all astonishing if, in some of these separated stocks, the process of differentiation should have gone so far as to give rise to the phenomena of hybridity. In the face of the overwhelming evidence in favour of the unity of the origin of mankind afforded by anatomical considerations, satisfactory proof of the existence of any degree of sterility in the unions of members of two of the "persistent modifications" of mankind, might well be appealed to by Mr. Darwin as crucial evidence of the truth of his views regarding the origin of species in general.

VIII.

ON SOME FIXED POINTS IN BRITISH ETHNOLOGY.

In view of the many discussions to which the complicated problems offered by the ethnology of the British Islands have given rise, it may be useful to attempt to pick out, from amidst the confused masses of assertion and of inference, those propositions which appear to rest upon a secure foundation, and to state the evidence by which they are supported. Such is the purpose of the present paper.

Some of these well-based propositions relate to the physical characters of the people of Britain and their neighbours; while others concern the languages which they spoke. I shall deal, in the first place, with the physical questions.

I. *Eighteen hundred years ago the population of Britain comprised people of two types of complexion —the one fair, and the other dark. The dark people resembled the Aquitani and the Iberians; the fair people were like the Belgic Gauls.*

The chief direct evidence of the truth of this proposition is the well-known passage of Tacitus:—

"Ceterum Britanniam qui mortales initio coluerint, indigenæ an advecti, ut inter barbaros, parum compertum. Habitus corporum

varii: atque ex eo argumenta: nam rutilæ Caledoniam habituntium comæ, magni artus Germanicam originem asseverant. Silurum colorati vultus et torti plerumque crines, et posita contra Hispaniam, Iberos veteres trajecisse, easque sedes occupasse, fidem faciunt. Proximi Gallis et similes sunt; seu durante originis vi, seu procurrentibus in diversa terris, positio cœli corporibus habitum dedit. In universum tamen æstimanti, Gallos vicinum solum occupasse, credibile est; eorum sacra deprehendas, superstitionum persuasione; sermo haud multum diversus." [1]

This passage, it will be observed, contains statements as to facts, and certain conclusions deduced from these facts. The matters of fact asserted are: firstly, that the inhabitants of Britain exhibit much diversity in their physical characters; secondly, that the Caledonians are red-haired and large-limbed, like the Germans; thirdly, that the Silures have curly hair and dark complexions, like the people of Spain; fourthly, that the British people nearest Gaul resemble the "Galli."

Tacitus, therefore, states positively what the Caledonians and Silures were like; but the interpretation of what he says about the other Britons must depend upon what we learn from other sources as to the characters of these "Galli." Here the testimony of "divus Julius" comes in with great force and appropriateness. Cæsar writes:—

"Britanniæ pars interior ab iis incolitur, quos natos in insula ipsi memoria proditum dicunt: marituma pars ab iis, qui prædæ ac belli inferendi causa ex Belgio transierant; qui omnes fere iis nominibus civitatum appellantur quibus orti ex civitatibus eo pervenerunt, et bello inlato ibi permanserunt atque agros colere cœperunt." [2]

From these passages it is obvious that in the opinion of Cæsar and Tacitus, the southern Britons resembled the northern Gauls, and especially the Belgæ; and the evidence of Strabo is decisive as to the characters in which the two people resembled one another: "The men

[1] Taciti Agricola, c. 11. [2] De Bello Gallico, v. 12.

[of Britain] are taller than the Kelts, with hair less yellow; they are slighter in their persons."[1]

The evidence adduced appears to leave no reasonable ground for doubting that, at the time of the Roman conquest, Britain contained people of two types, the one dark and the other fair complexioned, and that there was a certain difference between the latter in the north and in the south of Britain: the northern folk being, in the judgment of Tacitus, or, more properly, according to the information he had received from Agricola and others, more similar to the Germans than the latter. As to the distribution of these stocks, all that is clear is, that the dark people were predominant in certain parts of the west of the southern half of Britain, while the fair stock appears to have furnished the chief elements of the population elsewhere.

No ancient writer troubled himself with measuring skulls, and therefore there is no direct evidence as to the cranial characters of the fair and the dark stocks. The indirect evidence is not very satisfactory. The tumuli of Britain of pre-Roman date have yielded two extremely different forms of skull, the one broad and the other long; and the same variety has been observed in the skulls of the ancient Gauls.[2] The suggestion is obvious that the one form of skull may have been associated with the fair, and the other with the dark, complexion. But any conclusion of this kind is at once checked by the reflection that the extremes of long and short-headedness are to be met with among the fair inhabitants of Germany and of Scandinavia at the present day—the south-western Germans and the Swiss being markedly broad-headed,

[1] "The Geography of Strabo." Translated by Hamilton and Falconer: v. 5.

[2] See Dr. Thurnam "On the Two principal Forms of Ancient British and Gaulish Skulls."

while the Scandinavians are as predominantly long-headed.

What the natives of Ireland were like at the time of the Roman conquest of Britain, and for centuries afterwards, we have no certain knowledge; but the earliest trustworthy records prove the existence, side by side with one another, of a fair and a dark stock, in Ireland as in Britain. The long form of skull is predominant among the ancient, as among modern, Irish.

II. *The people termed Gauls, and those called Germans, by the Romans, did not differ in any important physical character.*

The terms in which the ancient writers describe both Gauls and Germans are identical. They are always tall people, with massive limbs, fair skins, fierce blue eyes, and hair the colour of which ranges from red to yellow. Zeuss, the great authority on these matters, affirms broadly that no distinction in bodily feature is to be found between the Gauls, the Germans, and the Wends, so far as their characters are recorded by the old historians; and he proves his case by citations from a cloud of witnesses.

An attempt has been made to show that the colour of the hair of the Gauls must have differed very much from that which obtained among the Germans, on the strength of the story told by Suetonius (Caligula, 4), that Caligula tried to pass off Gauls for Germans by picking out the tallest, and making them "rutilare et summittere comam."

The Baron de Belloguet remarks upon this passage :—

"It was in the very north of Gaul, and near the sea, that Caligula got up this military comedy. And the fact proves that the Belgæ were already sensibly different from their ancestors, whom Strabo had found almost identical with their *brothers* on the other side of the Rhine."

But the fact recorded by Suetonius, if fact it be, proves nothing; for the Germans themselves were in the habit of reddening their hair. Ammianus Marcellinus[1] tells how, in the year 367 A.D., the Roman commander, Jovinus, surprised a body of Alemanni near the town now called Charpeigne, in the valley of the Moselle; and how the Roman soldiers, as, concealed by the thick wood, they stole upon their unsuspecting enemies, saw that some were bathing and others "comas rutilantes ex more." More than two centuries earlier Pliny gives indirect evidence to the same effect when he says of soap:—

"Galliarum hoc inventum rutilandis capillis . . . apud Germanos majore in usu viris quam fœminis."[2]

Here we have a writer who flourished only a short time after the date of the Caligula story, telling us that the Gauls invented soap for the purpose of doing that which, according to Suetonius, Caligula forced them to do. And, further, the combined and independent testimony of Pliny and Ammianus assures us that the Germans were as much in the habit of reddening their hair as the Gauls. As to De Belloguet's supposition that, even in Caligula's time, the Gauls had become darker than their ancestors were, it is directly contradicted by Ammianus Marcellinus, who knew the Gauls well. "Celsioris staturæ et candidi pœne Galli sunt omnes, et rutili, luminumque torvitate terribiles," is his description; and it would fit the Gauls who sacked Rome.

III. *In none of the invasions of Britain which have taken place since the Roman dominion, has any other type of man been introduced than one or other of the two which existed during that dominion.*

The North Germans, who effected what is commonly called the Saxon conquest of Britain, were, most

[1] Res Gestæ, xxvii. [2] Historia Naturalis, xxviii. 51.

assuredly, a fair, yellow, or red-haired, blue-eyed, long-skulled people. So were the Danes and the Norsemen who followed them; though it is very possible that the active slave trade which went on, and the intercourse with Ireland, may have introduced a certain admixture of the dark stock into both Denmark and Norway. The Norman conquest brought in new ethnological elements, the precise value of which cannot be estimated with exactness; but as to their quality, there can be no question, inasmuch as even the wide area from which William drew his followers could yield him nothing but the fair and the dark types of men, already present in Britain. But whether the Norman settlers, on the whole, strengthened the fair or the dark element, is a problem, the elements of the solution of which are not attainable.

I am unable to discover any grounds for believing that a Lapp element has ever entered into the population of these islands. So far as the physical evidence goes, it is perfectly consistent with the hypothesis that the only constituent stocks of that population, now, or at any other period about which we have evidence, are the dark whites, whom I have proposed to call "*Melanochroi*," and the fair whites, or "*Xanthochroi*."

IV. *The Xanthochroi and the Melanochroi of Britain are, speaking broadly, distributed, at present, as they were in the time of Tacitus; and their representatives on the continent of Europe have the same general distribution as at the earliest period of which we have any record.*

At the present day, and notwithstanding the extensive intermixture effected by the movements consequent on civilization and on political changes, there is a predominance of dark men in the west, and of fair men in the east and north, of Britain. At the present day, as from the earliest times, the predominant constituents of the

riverain population of the North Sea and the eastern half of the British Channel, are fair men. The fair stock continues in force through Central Europe, until it is lost in Central Asia. Offshoots of this stock extend into Spain, Italy, and Northern India, and by way of Syria and North Africa, to the Canary Islands. They were known in very early times to the Chinese, and in still earlier to the ancient Egyptians, as frontier tribes. The Thracians were notorious for their fair hair and blue eyes many centuries before our era.

On the other hand, the dark stock predominates in Southern and Western France, in Spain, along the Ligurian shore, and in Western and Southern Italy; in Greece, Asia, Syria, and North Africa; in Arabia, Persia, Affghanistan, and Hindostan, shading gradually, through all stages of darkening, into the type of the modern Egyptian, or of the wild Hill-man of the Dekkan. Nor is there any record of the existence of a different population in all these countries.

The extreme north of Europe, and the northern part of Western Asia, are at present occupied by a Mongoloid stock, and, in the absence of evidence to the contrary, may be assumed to have been so peopled from a very remote epoch. But, as I have said, I can find no evidence that this stock ever took part in peopling Britain. Of the three great stocks of mankind which extend from the western coast of the great Eurasiatic continent to its southern and eastern shores, the Mongoloids occupy a vast triangle, the base of which is the whole of Eastern Asia, while its apex lies in Lapland. The Melanochroi, on the other hand, may be represented as a broad band stretching from Ireland to Hindostan; while the Xanthochroic area lies between the two, thins out, so to speak, at either end, and mingles, at its margins, with both its neighbours.

Such is a brief and summary statement of what I believe to be the chief facts relating to the physical ethnology of the people of Britain. The conclusions which I draw from these and other facts are—(1) That the Melanochroi and the Xanthochroi are two separate races in the biological sense of the word race; (2) That they have had the same general distribution as at present from the earliest times of which any record exists on the continent of Europe; (3) That the population of the British Islands is derived from them, and from them only.

The people of Europe, however, owe their national names, not to their physical characteristics, but to their languages, or to their political relations; which, it is plain, need not have the slightest relation to these characteristics.

Thus, it is quite certain that, in Cæsar's time, Gaul was divided politically into three nationalities — the Belgæ, the Celtæ, and the Aquitani; and that the last were very widely different, both in language and in physical characteristics, from the two former. The Belgæ and the Celtæ, on the other hand, differed comparatively little either in physique or in language. On the former point there is the distinct testimony of Strabo; as to the latter, St. Jerome states that the "Galatians had almost the same language as the Treviri." Now, the Galatians were emigrant Volcæ Tectosages, and therefore Celtæ; while the Treviri were Belgæ.

At the present day, the physical characters of the people of Belgic Gaul remain distinct from those of the people of Aquitaine, notwithstanding the immense changes which have taken place since Cæsar's time; but Belgæ, Celtæ, and Aquitani (all but a mere fraction of the last two, represented by the Basques and the Britons) are fused into one nationality, "le peuple

Français." But they have adopted the language of one set of invaders, and the name of another; their original names and languages having almost disappeared. Suppose that the French language remained as the sole evidence of the existence of the population of Gaul, would the keenest philologer arrive at any other conclusion than that this population was essentially and fundamentally a "Latin" race, which had had some communication with Celts and Teutons? Would he so much as suspect the former existence of the Aquitani?

Community of language testifies to close contact between the people who speak the language, but to nothing else; philology has absolutely nothing to do with ethnology, except so far as it suggests the existence or the absence of such contact. The contrary assumption, that language is a test of race, has introduced the utmost confusion into ethnological speculation, and has nowhere worked greater scientific and practical mischief than in the ethnology of the British Islands.

What is known, for certain, about the languages spoken in these islands and their affinities may, I believe, be summed up as follows:—

I. *At the time of the Roman conquest, one language, the Celtic, under two principal dialectical divisions, the Cymric and the Gaelic, was spoken throughout the British Islands. Cymric was spoken in Britain, Gaelic in Ireland.*

If a language allied to Basque had in earlier times been spoken in the British Islands, there is no evidence that any Euskarian-speaking people remained at the time of the Roman conquest. The dark and the fair population of Britain alike spoke Celtic tongues, and therefore the name "Celt" is as applicable to the one as to the other.

What was spoken in Ireland can only be surmised by

reasoning from the knowledge of later times; but there seems to be no doubt that it was Gaelic; and that the Gaelic dialect was introduced into the Western Highlands by Irish invaders.

II. *The Belgæ and the Celtæ, with the offshoots of the latter in Asia Minor, spoke dialects of the Cymric division of Celtic.*

The evidence of this proposition lies in the statement of St. Jerome before cited; in the similarity of the names of places in Belgic Gaul and in Britain; and in the direct comparison of sundry ancient Gaulish and Belgic words which have been preserved, with the existing Cymric dialects, for which I must refer to the learned work of Brandes.

Formerly, as at the present day, the Cymric dialects of Celtic were spoken by both the fair and the dark stocks.

III. *There is no record of Gaelic being spoken anywhere save in Ireland, Scotland, and the Isle of Man.*

This appears to be the final result of the long discussions which have taken place on this much-debated question. As is the case with the Cymric dialects, Gaelic is now spoken by both dark and fair stocks.

IV. *When the Teutonic languages first became known, they were spoken only by Xanthochroi, that is to say, by the Germans, the Scandinavians, and Goths. And they were imported by Xanthochroi into Gaul and into Britain.*

In Gaul the imported Teutonic dialect has been completely overpowered by the more or less modified Latin, which it found already in possession; and what Teutonic blood there may be in modern Frenchmen is not adequately represented in their language. In Britain, on the contrary, the Teutonic dialects have overpowered the pre-existing forms of speech, and the people are vastly less "Teutonic" than their language. Whatever

may have been the extent to which the Celtic-speaking population of the eastern half of Britain was trodden out and supplanted by the Teutonic-speaking Saxons and Danes, it is quite certain that no considerable displacement of the Celtic-speaking people occurred in Cornwall, Wales, or the Highlands of Scotland; and that nothing approaching to the extinction of that people took place in Devonshire, Somerset, or the western moiety of Britain generally. Nevertheless, the fundamentally Teutonic English language is now spoken throughout Britain, except by an insignificant fraction of the population in Wales and the Western Highlands. But it is obvious that this fact affords not the slightest justification for the common practice of speaking of the present inhabitants of Britain as an "Anglo-Saxon" people. It is, in fact, just as absurd as the habit of talking of the French people as a "Latin" race, because they speak a language which is, in the main, derived from Latin. And the absurdity becomes the more patent when those who have no hesitation in calling a Devonshire man, or a Cornish man, an "Anglo-Saxon," would think it ridiculous to call a Tipperary man by the same title, though he and his forefathers may have spoken English for as long a time as the Cornish man.

Ireland, at the earliest period of which we have any knowledge, contained, like Britain, a dark and a fair stock, which, there is every reason to believe, were identical with the dark and the fair stocks of Britain. When the Irish first became known they spoke a Gaelic dialect, and though, for many centuries, Scandinavians made continual incursions upon, and settlements among them, the Teutonic languages made no more way among the Irish than they did among the French. How much Scandinavian blood was introduced there is no evidence to show. But after the conquest of Ireland by Henry II.,

the English people, consisting in part of the descendants of Cymric speakers, and in part of the descendants of Teutonic speakers, made good their footing in the eastern half of the island, as the Saxons and Danes made good theirs in England; and did their best to complete the parallel by attempting the extirpation of the Gaelic-speaking Irish. And they succeeded to a considerable extent; a large part of Eastern Ireland is now peopled by men who are substantially English by descent, and the English language has spread over the land far beyond the limits of English blood.

Ethnologically, the Irish people were originally, like the people of Britain, a mixture of Melanochroi and Xanthochroi. They resembled the Britons in speaking a Celtic tongue; but it was a Gaelic and not a Cymric form of the Celtic language. Ireland was untouched by the Roman conquest, nor do the Saxons seem to have had any influence upon her destinies, but the Danes and Norsemen poured in a contingent of Teutonism, which has been largely supplemented by English and Scotch efforts.

What, then, is the value of the ethnological difference between the Englishman of the western half of England and the Irishman of the eastern half of Ireland? For what reason does the one deserve the name of a "Celt," and not the other? And further, if we turn to the inhabitants of the western half of Ireland, why should the term "Celts" be applied to them more than to the inhabitants of Cornwall? And if the name is applicable to the one as justly as to the other, why should not intelligence, perseverance, thrift, industry, sobriety, respect for law, be admitted to be Celtic virtues? And why should we not seek for the cause of their absence in something else than the idle pretext of "Celtic blood?"

I have been unable to meet with any answers to these questions.

V. *The Celtic and the Teutonic dialects are members of the same great Aryan family of languages; but there is evidence to show that a non-Aryan language was at one time spoken over a large extent of the area occupied by Melanochroi in Europe.*

The non-Aryan language here referred to is the Euskarian, now spoken only by the Basques, but which seems in earlier times to have been the language of the Aquitanians and Spaniards, and may possibly have extended much further to the East. Whether it has any connection with the Ligurian and Oscan dialects are questions upon which, of course, I do not presume to offer any opinion. But it is important to remark that it is a language the area of which has gradually diminished without any corresponding extirpation of the people who primitively spoke it; so that the people of Spain and of Aquitaine at the present day must be largely "Euskarian" by descent in just the same sense as the Cornish men are "Celtic" by descent.

Such seem to me to be the main facts respecting the ethnology of the British islands and of Western Europe, which may be said to be fairly established. The hypothesis by which I think (with De Belloguet and Thurnam) the facts may best be explained is this: In very remote times Western Europe and the British islands were inhabited by the dark stock, or the Melanochroi, alone, and these Melanochroi spoke dialects allied to the Euskarian. The Xanthochroi, spreading over the great Eurasiatic plains westward, and speaking Aryan dialects, gradually invaded the territories of the Melanochroi. The Xanthochroi, who thus came into contact with the Western Melanochroi, spoke a Celtic language; and that Celtic language, whether Cymric or Gaelic, spread over

the Melanochroi far beyond the limits of intermixture of blood, supplanting Euskarian, just as English and French have supplanted Celtic. Even as early as Cæsar's time, I suppose that the Euskarian was everywhere, except in Spain and in Aquitaine, replaced by Celtic, and thus the Celtic speakers were no longer of one ethnological stock, but of two. Both in Western Europe and in England a third wave of language—in the one case Latin, in the other Teutonic—has spread over the same area. In Western Europe, it has left a fragment of the primary Euskarian in one corner of the country, and a fragment of the secondary Celtic in another. In the British islands, only outlying pools of the secondary linguistic wave remain in Wales, the Highlands, Ireland, and the Isle of Man. If this hypothesis is a sound one, it follows that the name of Celtic is not properly applicable to the Melanochroic or dark stock of Europe. They are merely, so to speak, secondary Celts. The primary and aboriginal Celtic-speaking people are Xanthochroi—the typical Gauls of the ancient writers, and the close allies by blood, customs, and language, of the Germans.

IX.

PALÆONTOLOGY AND THE DOCTRINE OF EVOLUTION.

(THE ANNIVERSARY ADDRESS TO THE GEOLOGICAL SOCIETY, FOR 1870.)

It is now eight years since, in the absence of the late Mr. Leonard Horner, who then presided over us, it fell to my lot, as one of the Secretaries of this Society, to draw up the customary Annual Address. I availed myself of the opportunity to endeavour to "take stock" of that portion of the science of biology which is commonly called "palæontology," as it then existed; and, discussing one after another the doctrines held by palæontologists, I put before you the results of my attempts to sift the well-established from the hypothetical or the doubtful. Permit me briefly to recall to your minds what those results were:—

1. The living population of all parts of the earth's surface which have yet been examined has undergone a succession of changes which, upon the whole, have been of a slow and gradual character.

2. When the fossil remains which are the evidences of these successive changes, as they have occurred in any two more or less distant parts of the surface of the earth, are compared, they exhibit a certain broad and general

parallelism. In other words, certain forms of life in one locality occur in the same general order of succession as, or are *homotaxial* with, similar forms in the other locality.

3. Homotaxis is not to be held identical with synchronism without independent evidence. It is possible that similar, or even identical, faunæ and floræ in two different localities may be of extremely different ages, if the term "age" is used in its proper chronological sense. I stated that "geographical provinces, or zones, may have been as distinctly marked in the Palæozoic epoch as at present; and those seemingly sudden appearances of new genera and species, which we ascribe to new creation, may be simple results of migration."

4. The opinion that the oldest known fossils are the earliest forms of life has no solid foundation.

5. If we confine ourselves to positively ascertained facts, the total amount of change in the forms of animal and vegetable life, since the existence of such forms is recorded, is small. When compared with the lapse of time since the first appearance of these forms, the amount of change is wonderfully small. Moreover, in each great group of the animal and vegetable kingdoms, there are certain forms which I termed PERSISTENT TYPES, which have remained, with but very little apparent change, from their first appearance to the present time.

6. In answer to the question "What, then, does an impartial survey of the positively ascertained truths of palæontology testify in relation to the common doctrines of progressive modification, which suppose that modification to have taken place by a necessary progress from more to less embryonic forms, from more to less generalized types, within the limits of the period represented by the fossiliferous rocks?" I reply, "It negatives these doctrines; for it either shows us no evidence of such

modification, or demonstrates such modification as has occurred to have been very slight; and, as to the nature of that modification, it yields no evidence whatsoever that the earlier members of any long-continued group were more generalized in structure than the later ones."

I think that I cannot employ my last opportunity of addressing you, officially, more properly—I may say more dutifully—than in revising these old judgments with such help as further knowledge and reflection, and an extreme desire to get at the truth, may afford me.

1. With respect to the first proposition, I may remark that whatever may be the case among the physical geologists, catastrophic palæontologists are practically extinct. It is now no part of recognized geological doctrine that the species of one formation all died out and were replaced by a brand-new set in the next formation. On the contrary, it is generally, if not universally, agreed that the succession of life has been the result of a slow and gradual replacement of species by species; and that all appearances of abruptness of change are due to breaks in the series of deposits, or other changes in physical conditions. The continuity of living forms has been unbroken from the earliest times to the present day.

2, 3. The use of the word "homotaxis" instead of "synchronism" has not, so far as I know, found much favour in the eyes of geologists. I hope, therefore, that it is a love for scientific caution, and not mere personal affection for a bantling of my own, which leads me still to think that the change of phrase is of importance, and that the sooner it is made, the sooner shall we get rid of a number of pitfalls which beset the reasoner upon the facts and theories of geology.

One of the latest pieces of foreign intelligence which has reached us is the information that the Austrian geologists have, at last, succumbed to the weighty

evidence which M. Barrande has accumulated, and have admitted the doctrine of colonies. But the admission of the doctrine of colonies implies the further admission that even identity of organic remains is no proof of the synchronism of the deposits which contain them.

4. The discussions touching the *Eozoon*, which commenced in 1864, have abundantly justified the fourth proposition. In 1862, the oldest record of life was in the Cambrian rocks; but if the *Eozoon* be, as Principal Dawson and Dr. Carpenter have shown so much reason for believing, the remains of a living being, the discovery of its true nature carried life back to a period which, as Sir William Logan has observed, is as remote from that during which the Cambrian rocks were deposited, as the Cambrian epoch itself is from the tertiaries. In other words, the ascertained duration of life upon the globe was nearly doubled at a stroke.

5. The significance of persistent types, and of the small amount of change which has taken place even in those forms which can be shown to have been modified, becomes greater and greater in my eyes, the longer I occupy myself with the biology of the past.

Consider how long a time has elapsed since the Miocene epoch. Yet, at that time, there is reason to believe that every important group in every order of the *Mammalia* was represented. Even the comparatively scanty Eocene fauna yields examples of the orders *Cheiroptera*, *Insectivora*, *Rodentia*, and *Perissodactyla*; of *Artiodactyla* under both the Ruminant and the Porcine modifications; of *Carnivora*, *Cetacea*, and *Marsupialia*.

Or, if we go back to the older half of the Mesozoic epoch, how truly surprising it is to find every order of the *Reptilia*, except the *Ophidia*, represented; while some groups, such as the *Ornithoscelida* and the *Ptero-*

sauria, more specialized than any which now exist, abounded.

There is one division of the *Amphibia* which offers especially important evidence upon this point, inasmuch as it bridges over the gap between the Mesozoic and the Palæozoic formations (often supposed to be of such prodigious magnitude), extending, as it does, from the bottom of the Carboniferous series to the top of the Trias, if not into the Lias. I refer to the Labyrinthodonts. As the address of 1862 was passing through the press, I was able to mention, in a note, the discovery of a large Labyrinthodont, with well-ossified vertebræ, in the Edinburgh coal-field. Since that time eight or ten distinct genera of Labyrinthodonts have been discovered in the Carboniferous rocks of England, Scotland, and Ireland, not to mention the American forms described by Principal Dawson and Professor Cope. So that, at the present time, the Labyrinthodont Fauna of the Carboniferous rocks is more extensive and diversified than that of the Trias, while its chief types, so far as osteology enables us to judge, are quite as highly organized. Thus it is certain that a comparatively highly organized vertebrate type, such as that of the Labyrinthodonts, is capable of persisting, with no considerable change, through the period represented by the vast deposits which constitute the Carboniferous, the Permian, and the Triassic formations.

The very remarkable results which have been brought to light by the sounding and dredging operations, which have been carried on with such remarkable success by the expeditions sent out by our own, the American, and the Swedish Governments, under the supervision of able naturalists, have a bearing in the same direction. These investigations have demonstrated the existence, at great depths in the ocean, of living animals in some cases identical with, in others very similar to, those which are

found fossilized in the white chalk. The *Globigerinæ*, Cyatholiths, Coccospheres, Discoliths in the one are absolutely identical with those in the other; there are identical, or closely analogous, species of Sponges, Echinoderms, and Brachiopods. Off the coast of Portugal, there now lives a species of *Beryx*, which, doubtless, leaves its bones and scales here and there in the Atlantic ooze, as its predecessor left its spoils in the mud of the sea of the Cretaceous epoch.

Many years ago[1] I ventured to speak of the Atlantic mud as "modern chalk," and I know of no fact inconsistent with the view which Professor Wyville Thomson has advocated, that the modern chalk is not only the lineal descendant of the ancient chalk, but that it remains, so to speak, in the possession of the ancestral estate; and that from the Cretaceous period (if not much earlier) to the present day, the deep sea has covered a large part of what is now the area of the Atlantic. But if *Globigerina*, and *Terebratula caput-serpentis* and *Beryx*, not to mention other forms of animals and of plants, thus bridge over the interval between the present and the Mesozoic periods, is it possible that the majority of other living things underwent a "sea-change into something new and strange" all at once?

6. Thus far I have endeavoured to expand and to enforce by fresh arguments, but not to modify in any important respect, the ideas submitted to you on a former occasion. But when I come to the propositions touching progressive modification, it appears to me, with the help of the new light which has broken from various quarters, that there is much ground for softening the somewhat Brutus-like severity with which, in 1862, I dealt with a doctrine, for the truth of which I should

[1] See an article in the *Saturday Review*, for 1858, on "Chalk, Ancient and Modern."

have been glad enough to be able to find a good foundation. So far, indeed, as the *Invertebrata* and the lower *Vertebrata* are concerned, the facts and the conclusions which are to be drawn from them appear to me to remain what they were. For anything that, as yet, appears to the contrary, the earliest known Marsupials may have been as highly organized as their living congeners; the Permian lizards show no signs of inferiority to those of the present day; the Labyrinthodonts cannot be placed below the living Salamander and Triton; the Devonian Ganoids are closely related to *Polypterus* and to *Lepidosiren*.

But when we turn to the higher *Vertebrata*, the results of recent investigations, however we may sift and criticise them, seem to me to leave a clear balance in favour of the doctrine of the evolution of living forms one from another. Nevertheless, in discussing this question, it is very necessary to discriminate carefully between the different kinds of evidence from fossil remains which are brought forward in favour of evolution.

Every fossil which takes an intermediate place between forms of life already known, may be said, so far as it is intermediate, to be evidence in favour of evolution, inasmuch as it shows a possible road by which evolution may have taken place. But the mere discovery of such a form does not, in itself, prove that evolution took place by and through it, nor does it constitute more than presumptive evidence in favour of evolution in general. Suppose A, B, C to be three forms, while B is intermediate in structure between A and C. Then the doctrine of evolution offers four possible alternatives. A may have become C by way of B; or C may have become A by way of B; or A and C may be independent modifications of B; or A, B, and C may be independent modifications of some unknown D. Take the case of the Pigs,

the *Anoplotheridæ*, and the Ruminants. The *Anoplotheridæ* are intermediate between the first and the last; but this does not tell us whether the Ruminants have come from the Pigs, or the Pigs from Ruminants, or both from *Anoplotheridæ*, or whether Pigs, Ruminants, and *Anoplotheridæ* alike may not have diverged from some common stock.

But if it can be shown that A, B, and C exhibit successive stages in the degree of modification, or specialization, of the same type; and if, further, it can be proved that they occur in successively newer deposits, A being in the oldest and C in the newest, then the intermediate character of B has quite another importance, and I should accept it, without hesitation, as a link in the genealogy of C. I should consider the burden of proof to be thrown upon anyone who denied C to have been derived from A by way of B, or in some closely analogous fashion; for it is always probable that one may not hit upon the exact line of filiation, and, in dealing with fossils, may mistake uncles and nephews for fathers and sons.

I think it necessary to distinguish between the former and the latter classes of intermediate forms, as *intercalary types* and *linear types*. When I apply the former term, I merely mean to say that as a matter of fact, the form B, so named, is intermediate between the others, in the sense in which the *Anoplotherium* is intermediate between the Pigs and the Ruminants—without either affirming, or denying, any direct genetic relation between the three forms involved. When I apply the latter term, on the other hand, I mean to express the opinion that the forms A, B, and C constitute a line of descent, and that B is thus part of the lineage of C.

From the time when Cuvier's wonderful researches upon the extinct Mammals of the Paris gypsum first made intercalary types known, and caused them to be recognized

as such, the number of such forms has steadily increased among the higher *Mammalia*. Not only do we now know numerous intercalary forms of *Ungulata*, but M. Gaudry's great monograph upon the fossils of Pikermi (which strikes me as one of the most perfect pieces of palæontological work I have seen for a long time) shows us, among the *Primates*, *Mesopithecus* as an intercalary form between the *Semnopitheci* and the *Macaci;* and among the *Carnivora*, *Hyænictis* and *Ictitherium* as intercalary, or, perhaps, linear types between the *Viverridæ* and the *Hyænidæ*.

Hardly any order of the higher *Mammalia* stands so apparently separate and isolated from the rest as that of the *Cetacea;* though a careful consideration of the structure of the pinnipede *Carnivora*, or Seals, shows, in them, many an approximation towards the still more completely marine mammals. The extinct *Zeuglodon*, however, presents us with an intercalary form between the type of the Seals and that of the Whales. The skull of this great Eocene sea-monster, in fact, shows by the narrow and prolonged interorbital region; the extensive union of the parietal bones in a sagittal suture; the well-developed nasal bones; the distinct and large incisors implanted in premaxillary bones, which take a full share in bounding the fore part of the gape; the two-fanged molar teeth with triangular and serrated crowns, not exceeding five on each side in each jaw; and the existence of a deciduous dentition—its close relation with the Seals. While, on the other hand, the produced rostral form of the snout, the long symphysis, and the low coronary process of the mandible are approximations to the cetacean form of those parts.

The scapula resembles that of the cetacean *Hyperoödon*, but the supra-spinous fossa is larger and more seal-like; as is the humerus, which differs from that of the *Cetacea*

in presenting true articular surfaces for the free jointing of the bones of the fore-arm. In the apparently complete absence of hinder limbs, and in the characters of the vertebral column, the *Zeuglodon* lies on the cetacean side of the boundary line; so that, upon the whole, the Zeuglodonts, transitional as they are, are conveniently retained in the cetacean order. And the publication, in 1864, of M. Van Beneden's memoir on the Miocene and Pliocene *Squalodon*, furnished much better means than anatomists previously possessed of fitting in another link of the chain which connects the existing *Cetacea* with *Zeuglodon*. The teeth are much more numerous, although the molars exhibit the zeuglodont double fang; the nasal bones are very short, and the upper surface of the rostrum presents the groove, filled up during life by the prolongation of the ethmoidal cartilage, which is so characteristic of the majority of the *Cetacea*.

It appears to me that, just as among the existing *Carnivora*, the walruses and the eared seals are intercalary forms between the fissipede Carnivora and the ordinary seals, so the Zeuglodonts are intercalary between the *Carnivora*, as a whole, and the *Cetacea*. Whether the Zeuglodonts are also linear types in their relation to these two groups cannot be ascertained, until we have more definite knowledge than we possess at present, respecting the relations in time of the *Carnivora* and *Cetacea*.

Thus far we have been concerned with the intercalary types which occupy the intervals between Families or Orders of the same class; but the investigations which have been carried on by Professor Gegenbaur, Professor Cope, and myself into the structure and relations of the extinct reptilian forms of the *Ornithoscelida* (or *Dinosauria* and *Compsognatha*) have brought to light the existence of intercalary forms between what have hitherto

been always regarded as very distinct classes of the vertebrate sub-kingdom, namely *Reptilia* and *Aves*. Whatever inferences may, or may not, be drawn from the fact, it is now an established truth that, in many of these *Ornithoscelida*, the hind limbs and the pelvis are much more similar to those of Birds than they are to those of Reptiles, and that these Bird-reptiles, or Reptile-birds, were more or less completely bipedal.

When I addressed you in 1862, I should have been bold indeed had I suggested that palæontology would before long show us the possibility of a direct transition from the type of the lizard to that of the ostrich. At the present moment we have, in the *Ornithoscelida*, the intercalary type, which proves that transition to be something more than a possibility; but it is very doubtful whether any of the genera of *Ornithoscelida* with which we are at present acquainted are the actual linear types by which the transition from the lizard to the bird was effected. These, very probably, are still hidden from us in the older formations.

Let us now endeavour to find some cases of true linear types, or forms which are intermediate between others because they stand in a direct genetic relation to them. It is no easy matter to find clear and unmistakable evidence of filiation among fossil animals; for, in order that such evidence should be quite satisfactory, it is necessary that we should be acquainted with all the most important features of the organization of the animals which are supposed to be thus related, and not merely with the fragments upon which the genera and species of the palæontologist are so often based. M. Gaudry has arranged the species of *Hyænidæ*, *Proboscidea*, *Rhinocerotidæ*, and *Equidæ* in their order of filiation from their earliest appearance in the Miocene epoch to the present time, and Professor Rütimeyer has

drawn up similar schemes for the Oxen and other *Ungulata*—with what, I am disposed to think, is a fair and probable approximation to the order of nature. But, as no one is better aware than these two learned, acute, and philosophical biologists, all such arrangements must be regarded as provisional, except in those cases in which, by a fortunate accident, large series of remains are obtainable from a thick and wide-spread series of deposits. It is easy to accumulate probabilities—hard to make out some particular case in such a way that it will stand rigorous criticism.

After much search, however, I think that such a case is to be made out in favour of the pedigree of the Horses.

The genus *Equus* is represented as far back as the latter part of the Miocene epoch; but in deposits belonging to the middle of that epoch its place is taken by two other genera, *Hipparion* and *Anchitherium*;[1] and, in the lowest Miocene and upper Eocene, only the last genus occurs. A species of *Anchitherium* was referred by Cuvier to the *Palæotheria* under the name of *P. aurelianense*. The grinding-teeth are in fact very similar in shape and in pattern, and in the absence of any thick layer of cement, to those of some species of *Palæotherium*, especially Cuvier's *Palæotherium minus*, which has been formed into a separate genus, *Plagiolophus*, by Pomel. But in the fact that there are only six full-sized grinders in the lower jaw, the first premolar being very small; that the anterior grinders are as large

[1] Hermann von Meyer gave the name of *Anchitherium* to *A. Ezquerræ*; and in his paper on the subject he takes great pains to distinguish the latter as the type of a new genus, from Cuvier's *Palæotherium d'Orléans*. But it is precisely the *Palæotherium d'Orléans* which is the type of Christol's genus *Hipparitherium*; and thus, though *Hipparitherium* is of later date than *Anchitherium*, it seemed to me to have a sort of equitable right to recognition when this address was written. On the whole, however, it seems most convenient to adopt *Anchitherium*.

as, or rather larger than, the posterior ones; that the second premolar has an anterior prolongation; and that the posterior molar of the lower jaw has, as Cuvier pointed out, a posterior lobe of much smaller size and different form, the dentition of *Anchitherium* departs from the type of the *Palæotherium*, and approaches that of the Horse.

Again, the skeleton of *Anchitherium* is extremely equine. M. Christol goes so far as to say that the description of the bones of the horse, or the ass, current in veterinary works, would fit those of *Anchitherium*. And, in a general way, this may be true enough; but there are some most important differences, which, indeed, are justly indicated by the same careful observer. Thus the ulna is complete throughout, and its shaft is not a mere rudiment, fused into one bone with the radius. There are three toes, one large in the middle and one small on each side. The femur is quite like that of a horse, and has the characteristic fossa above the external condyle. In the British Museum there is a most instructive specimen of the leg-bones, showing that the fibula was represented by the external malleolus and by a flat tongue of bone, which extends up from it on the outer side of the tibia, and is closely ankylosed with the latter bone.[1] The hind toes are three, like those of the fore leg; and the middle metatarsal bone is much less compressed from side to side than that of the horse.

In the *Hipparion* the teeth nearly resemble those of the Horses, though the crowns of the grinders are not so long; like those of the Horses, they are abundantly coated with cement. The shaft of the ulna is reduced

[1] I am indebted to M. Gervais for a specimen which indicates that the fibula was complete, at any rate, in some cases; and for a very interesting ramus of a mandible, which shows that, as in the *Palæotheria*, the hindermost milk-molar of the lower jaw was devoid of the posterior lobe which exists in the hindermost true molar.

to a mere style ankylosed throughout nearly its whole length with the radius, and appearing to be little more than a ridge on the surface of the latter bone until it is carefully examined. The front toes are still three, but the outer ones are more slender than in *Anchitherium*, and their hoofs smaller in proportion to that of the middle toe; they are, in fact, reduced to mere dew-claws, and do not touch the ground. In the leg, the distal end of the fibula is so completely united with the tibia that it appears to be a mere process of the latter bone, as in the Horses.

In *Equus*, finally, the crowns of the grinding-teeth become longer, and their patterns are slightly modified; the middle of the shaft of the ulna usually vanishes, and its proximal and distal ends ankylose with the radius. The phalanges of the two outer toes in each foot disappear, their metacarpal and metatarsal bones being left as the "splints."

The *Hipparion* has large depressions on the face in front of the orbits, like those for the "larmiers" of many ruminants; but traces of these are to be seen in some of the fossil horses from the Sewalik Hills; and, as Leidy's recent researches show, they are preserved in *Anchitherium*.

When we consider these facts, and the further circumstance that the Hipparions, the remains of which have been collected in immense numbers, were subject, as M. Gaudry and others have pointed out, to a great range of variation, it appears to me impossible to resist the conclusion that the types of the *Anchitherium*, of the *Hipparion*, and of the ancient Horses constitute the lineage of the modern Horses, the *Hipparion* being the intermediate stage between the other two, and answering to B in my former illustration.

The process by which the *Anchitherium* has been con-

verted into *Equus* is one of specialization, or of more and more complete deviation from what might be called the average form of an ungulate mammal. In the Horses, the reduction of some parts of the limbs, together with the special modification of those which are left, is carried to a greater extent than in any other hoofed mammals. The reduction is less and the specialization is less in the *Hipparion*, and still less in the *Anchitherium;* but yet, as compared with other mammals, the reduction and specialization of parts in the *Anchitherium* remain great.

Is it not probable then, that, just as in the Miocene epoch, we find an ancestral equine form less modified than *Equus*, so, if we go back to the Eocene epoch, we shall find some quadruped related to the *Anchitherium*, as *Hipparion* is related to *Equus*, and consequently departing less from the average form?

I think that this desideratum is very nearly, if not quite, supplied by *Plagiolophus*, remains of which occur abundantly in some parts of the Upper and Middle Eocene formations. The patterns of the grinding-teeth of *Plagiolophus* are similar to those of *Anchitherium*, and their crowns are as thinly covered with cement; but the grinders diminish in size forwards, and the last lower molar has a large hind lobe, convex outwards and concave inwards, as in *Palæotherium*. The ulna is complete and much larger than in any of the *Equidæ*, while it is more slender than in most of the true *Palæotheria;* it is fixedly united, but not ankylosed, with the radius. There are three toes in the fore limb, the outer ones being slender, but less attenuated than in the *Equidæ*. The femur is more like that of the *Palæotheria* than that of the horse, and has only a small depression above its outer condyle in the place of the great fossa which is so obvious in the *Equidæ*. The fibula is distinct, but very

slender, and its distal end is ankylosed with the tibia. There are three toes on the hind foot having similar proportions to those on the fore foot. The principal metacarpal and metatarsal bones are flatter than they are in any of the *Equidæ;* and the metacarpal bones are longer than the metatarsals, as in the *Palæotheria.*

In its general form, *Plagiolophus* resembles a very small and slender horse,[1] and is totally unlike the reluctant, pig-like creature depicted in Cuvier's restoration of his *Palæotherium minus* in the "Ossemens Fossiles."

It would be hazardous to say that *Plagiolophus* is the exact radical form of the Equine quadrupeds; but I do not think there can be any reasonable doubt that the latter animals have resulted from the modification of some quadruped similar to *Plagiolophus.*

We have thus arrived at the Middle Eocene formation, and yet have traced back the Horses only to a three-toed stock; but these three-toed forms, no less than the Equine quadrupeds themselves, present rudiments of the two other toes which appertain to what I have termed the "average" quadruped. If the expectation raised by the splints of the Horses that, in some ancestor of the Horses, these splints would be found to be complete digits, has been verified, we are furnished with very strong reasons for looking for a no less complete verification of the expectation that the three-toed *Plagiolophus*-like "avus" of the horse must have had a five-toed "atavus" at some earlier period.

No such five-toed "atavus," however, has yet made its appearance among the few middle and older Eocene *Mammalia* which are known.

[1] Such, at least, is the conclusion suggested by the proportions of the skeleton figured by Cuvier and De Blainville; but perhaps something between a Horse and an Agouti would be nearest the mark.

Another series of closely affiliated forms, though the evidence they afford is perhaps less complete than that of the Equine series, is presented to us by the *Dichobune* of the Eocene epoch, the *Cainotherium* of the Miocene, and the *Tragulidæ*, or so-called "Musk-deer," of the present day.

The *Tragulidæ* have no incisors in the upper jaw, and only six grinding-teeth on each side of each jaw; while the canine is moved up to the outer incisor, and there is a diastema, in the lower jaw. There are four complete toes on the hind foot, but the middle metatarsals usually become, sooner or later, ankylosed into a cannon bone. The navicular and the cuboid unite, and the distal end of the fibula is ankylosed with the tibia.

In *Cainotherium* and *Dichobune* the upper incisors are fully developed. There are seven grinders; the teeth form a continuous series without a diastema. The metatarsals, the navicular and cuboid, and the distal end of the fibula, remain free. In the *Cainotherium*, also, the second metacarpal is developed, but is much shorter than the third, while the fifth is absent or rudimentary. In this respect it resembles *Anoplotherium secundarium*. This circumstance, and the peculiar pattern of the upper molars in *Cainotherium*, lead me to hesitate in considering it as the actual ancestor of the modern *Tragulidæ*. If *Dichobune* has a four-toed fore foot (though I am inclined to suspect that it resembles *Cainotherium*), it will be a better representative of the oldest forms of the Traguline series; but *Dichobune* occurs in the Middle Eocene, and is, in fact, the oldest known artiodactyle mammal. Where, then, must we look for its five-toed ancestor?

If we follow down other lines of recent and tertiary *Ungulata*, the same question presents itself. The Pigs are traceable back through the Miocene epoch to the Upper Eocene, where they appear in the two well-marked

forms of *Hyopotamus* and *Chœropotamus*; but *Hyopotamus* appears to have had only two toes.

Again, all the great groups of the Ruminants, the *Bovidæ*, *Antilopidæ*, *Camelopardalidæ*, and *Cervidæ*, are represented in the Miocene epoch, and so are the Camels. The Upper Eocene *Anoplotherium*, which is intercalary between the Pigs and the *Tragulidæ*, has only two or, at most, three toes. Among the scanty mammals of the Lower Eocene formation we have the perissodactyle *Ungulata* represented by *Coryphodon*, *Hyracotherium*, and *Pliolophus*. Suppose for a moment, for the sake of following out the argument, that *Pliolophus* represents the primary stock of the Perissodactyles, and *Dichobune* that of the Artiodactyles (though I am far from saying that such is the case), then we find, in the earliest fauna of the Eocene epoch to which our investigations carry us, the two divisions of the *Ungulata* completely differentiated, and no trace of any common stock of both, or of five-toed predecessors to either. With the case of the Horses before us, justifying a belief in the production of new animal forms by modification of old ones, I see no escape from the necessity of seeking for these ancestors of the *Ungulata* beyond the limits of the Tertiary formations.

I could as soon admit special creation, at once, as suppose that the Perissodactyles and Artiodactyles had no five-toed ancestors. And when we consider how large a portion of the Tertiary period elapsed before *Anchitherium* was converted into *Equus*, it is difficult to escape the conclusion that a large proportion of time anterior to the Tertiary period must have been expended in converting the common stock of the *Ungulata* into Perissodactyles and Artiodactyles.

The same moral is inculcated by the study of every other order of Tertiary monodelphous *Mammalia*. Each

of these orders is represented in the Miocene epoch: the Eocene formation, as I have already said, contains *Cheiroptera, Insectivora, Rodentia, Ungulata, Carnivora,* and *Cetacea.* But the *Cheiroptera* are extreme modifications of the *Insectivora,* just as the *Cetacea* are extreme modifications of the Carnivorous type; and therefore it is to my mind incredible that monodelphous *Insectivora* and *Carnivora* should not have been abundantly developed, along with *Ungulata,* in the Mesozoic epoch. But if this be the case, how much further back must we go to find the common stock of the monodelphous *Mammalia?* As to the *Didelphia,* if we may trust the evidence which seems to be afforded by their very scanty remains, a Hypsiprymnoid form existed at the epoch of the Trias, contemporaneously with a Carnivorous form. At the epoch of the Trias, therefore, the *Marsupialia* must have already existed long enough to have become differentiated into carnivorous and herbivorous forms. But the *Monotremata* are lower forms than the *Didelphia,* which last are intercalary between the *Ornithodelphia* and the *Monodelphia.* To what point of the Palæozoic epoch, then, must we, upon any rational estimate, relegate the origin of the *Monotremata?*

The investigation of the occurrence of the classes and of the orders of the *Sauropsida* in time points in exactly the same direction. If, as there is great reason to believe, true Birds existed in the Triassic epoch, the ornithoscelidous forms by which Reptiles passed into Birds must have preceded them. In fact there is, even at present, considerable ground for suspecting the existence of *Dinosauria* in the Permian formations; but, in that case, lizards must be of still earlier date. And if the very small differences which are observable between the *Crocodilia* of the older Mesozoic formations and those of the present day furnish any sort of approxi-

mation towards an estimate of the average rate of change among the *Sauropsida*, it is almost appalling to reflect how far back in Palæozoic times we must go, before we can hope to arrive at that common stock from which the *Crocodilia*, *Lacertilia*, *Ornithoscelida*, and *Plesiosauria*, which had attained so great a development in the Triassic epoch, must have been derived.

The *Amphibia* and *Pisces* tell the same story. There is not a single class of vertebrated animals which, when it first appears, is represented by analogues of the lowest known members of the same class. Therefore, if there is any truth in the doctrine of evolution, every class must be vastly older than the first record of its appearance upon the surface of the globe. But if considerations of this kind compel us to place the origin of vertebrated animals at a period sufficiently distant from the Upper Silurian, in which the first Elasmobranchs and Ganoids occur, to allow of the evolution of such fishes as these from a Vertebrate as simple as the *Amphioxus*, I can only repeat that it is appalling to speculate upon the extent to which that origin must have preceded the epoch of the first recorded appearance of vertebrate life.

Such is the further commentary which I have to offer upon the statement of the chief results of palæontology which I formerly ventured to lay before you.

But the growth of knowledge in the interval makes me conscious of an omission of considerable moment in that statement, inasmuch as it contains no reference to the bearings of palæontology upon the theory of the distribution of life; nor takes note of the remarkable manner in which the facts of distribution, in present and past times, accord with the doctrine of evolution, especially in regard to land animals.

That connection between palæontology and geology

and the present distribution of terrestrial animals, which so strikingly impressed Mr. Darwin, thirty years ago, as to lead him to speak of a "law of succession of types," and of the wonderful relationship on the same continent between the dead and the living, has recently received much elucidation from the researches of Gaudry, of Rütimeyer, of Leidy, and of Alphonse Milne-Edwards, taken in connection with the earlier labours of our lamented colleague Falconer; and it has been instructively discussed in the thoughtful and ingenious work of Mr. Andrew Murray "On the Geographical Distribution of Mammals." [1]

I propose to lay before you, as briefly as I can, the ideas to which a long consideration of the subject has given rise in my mind.

If the doctrine of evolution is sound, one of its immediate consequences clearly is, that the present distribution of life upon the globe is the product of two factors, the one being the distribution which obtained in the immediately preceding epoch, and the other the character and the extent of the changes which have taken place in physical geography between the one epoch and the other; or, to put the matter in another way, the Fauna and Flora of any given area, in any given epoch, can consist only of such forms of life as are directly descended from those which constituted the Fauna and Flora of the same area in the immediately preceding epoch, unless the physical geography (under which I include climatal conditions) of the area has been so altered as to give rise to immigration of living forms from some other area.

[1] The paper "On the Form and Distribution of the Land-tracts during the Secondary and Tertiary Periods respectively; and on the Effect upon Animal Life which great Changes in Geographical Configuration have probably produced," by Mr. Searles V. Wood, jun., which was published in the *Philosophical Magazine*, in 1862, was unknown to me when this Address was written. It is well worthy of the most careful study.

The evolutionist, therefore, is bound to grapple with the following problem whenever it is clearly put before him:—Here are the Faunæ of the same area during successive epochs. Show good cause for believing either that these Faunæ have been derived from one another by gradual modification, or that the Faunæ have reached the area in question by migration from some area in which they have undergone their development.

I propose to attempt to deal with this problem, so far as it is exemplified by the distribution of the terrestrial *Vertebrata*, and I shall endeavour to show you that it is capable of solution in a sense entirely favourable to the doctrine of evolution.

I have elsewhere[1] stated at length the reasons which lead me to recognize four primary distributional provinces for the terrestrial *Vertebrata* in the present world, namely,—first, the *Novozelanian*, or New-Zealand province; secondly, the *Australian* province, including Australia, Tasmania, and the Negrito Islands; thirdly, *Austro-Columbia*, or South America *plus* North America as far as Mexico; and fourthly, the rest of the world, or *Arctogæa*, in which province America north of Mexico constitutes one sub-province, Africa south of the Sahara a second, Hindostan a third, and the remainder of the Old World a fourth.

Now the truth which Mr. Darwin perceived and promulgated as "the law of the succession of types" is, that, in all these provinces, the animals found in Pliocene or later deposits are closely affined to those which now inhabit the same provinces; and that, conversely, the forms characteristic of other provinces are absent. North and South America, perhaps, present one or

[1] "On the Classification and Distribution of the Alectoromorphæ;" Proceedings of the Zoological Society, 1868.

two exceptions to the last rule, but they are readily susceptible of explanation. Thus, in Australia, the later Tertiary mammals are marsupials (possibly with exception of the Dog and a Rodent or two, as at present). In Austro-Columbia the later Tertiary fauna exhibits numerous and varied forms of Platyrrhine Apes, Rodents, Cats, Dogs, Stags, *Edentata*, and Opossums; but, as at present, no Catarrhine Apes, no Lemurs, no *Insectivora*, Oxen, Antelopes, Rhinoceroses, nor *Didelphia* other than Opossums. And in the wide-spread Arctogæal province, the Pliocene and later mammals belong to the same groups as those which now exist in the province. The law of succession of types, therefore, holds good for the present epoch as compared with its predecessor. Does it equally well apply to the Pliocene fauna when we compare it with that of the Miocene epoch? By great good fortune, an extensive mammalian fauna of the latter epoch has now become known, in four very distant portions of the Arctogæal province which do not differ greatly in latitude. Thus Falconer and Cautley have made known the fauna of the sub-Himalayas and the Perim Islands; Gaudry that of Attica; many observers that of Central Europe and France; and Leidy that of Nebraska, on the eastern flank of the Rocky Mountains. The results are very striking. The total Miocene fauna comprises many genera and species of Catarrhine Apes, of Bats, of *Insectivora;* of Arctogæal types of *Rodentia;* of *Proboscidea;* of equine, rhinocerotic, and tapirine quadrupeds; of cameline, bovine, antilopine, cervine, and traguline Ruminants; of Pigs and Hippopotamuses; of *Viverridæ* and *Hyænidæ* among other *Carnivora;* with *Edentata* allied to the Arctogæal *Orycteropus* and *Manis*, and not to the Austro-Columbian Edentates. The only type present in the Miocene, but absent in the existing, fauna of

Eastern Arctogæa, is that of the *Didelphidæ*, which, however, remains in North America.

But it is very remarkable that while the Miocene fauna of the Arctogæal province, as a whole, is of the same character as the existing fauna of the same province, as a whole, the component elements of the fauna were differently associated. In the Miocene epoch, North America possessed Elephants, Horses, Rhinoceroses, and a great number and variety of Ruminants and Pigs, which are absent in the present indigenous fauna; Europe had its Apes, Elephants, Rhinoceroses, Tapirs, Musk-deer, Giraffes, Hyænas, great Cats, Edentates, and Opossum-like Marsupials, which have equally vanished from its present fauna; and in Northern India, the African types of Hippopotamuses, Giraffes, and Elephants were mixed up with what are now the Asiatic types of the latter, and with Camels, and Semnopithecine and Pithecine Apes of no less distinctly Asiatic forms.

In fact the Miocene mammalian fauna of Europe and the Himalayan regions contains, associated together, the types which are at present separately located in the South-African and Indian sub-provinces of Arctogæa. Now there is every reason to believe, on other grounds, that both Hindostan, south of the Ganges, and Africa, south of the Sahara, were separated by a wide sea from Europe and North Asia during the Middle and Upper Eocene epochs. Hence it becomes highly probable that the well-known similarities, and no less remarkable differences, between the present Faunæ of India and South Africa have arisen in some such fashion as the following. Some time during the Miocene epoch, possibly when the Himalayan chain was elevated, the bottom of the nummulitic sea was upheaved and converted into dry land, in the direction of a line extending from Abyssinia to

the mouth of the Ganges. By this means, the Dekhan on the one hand, and South Africa on the other, became connected with the Miocene dry land and with one another. The Miocene mammals spread gradually over this intermediate dry land; and if the condition of its eastern and western ends offered as wide contrasts as the valleys of the Ganges and Arabia do now, many forms which made their way into Africa must have been different from those which reached the Dekhan, while others might pass into both these sub-provinces.

That there was a continuity of dry land between Europe and North America during the Miocene epoch, appears to me to be a necessary consequence of the fact that many genera of terrestrial mammals, such as *Castor*, *Hystrix*, *Elephas*, *Mastodon*, *Equus*, *Hipparion*, *Anchitherium*, *Rhinoceros*, *Cervus*, *Amphicyon*, *Hyænarctos*, and *Machairodus*, are common to the Miocene formations of the two areas, and have as yet been found (except perhaps *Anchitherium*) in no deposit of earlier age. Whether this connection took place by the east, or by the west, or by both sides of the Old World, there is at present no certain evidence, and the question is immaterial to the present argument; but, as there are good grounds for the belief that the Australian province and the Indian and South-African sub-provinces were separated by sea from the rest of Arctogæa before the Miocene epoch, so it has been rendered no less probable, by the investigations of Mr. Carrick Moore and Professor Duncan, that Austro-Columbia was separated by sea from North America during a large part of the Miocene epoch.

It is unfortunate that we have no knowledge of the Miocene mammalian fauna of the Australian and Austro-Columbian provinces; but, seeing that not a trace of a Platyrrhine Ape, of a Procyonine Carnivore, of a charac-

teristically South-American Rodent, of a Sloth, an Armadillo, or an Ant-eater has yet been found in Miocene deposits of Arctogæa, I cannot doubt that they already existed in the Miocene Austro-Columbian province.

Nor is it less probable that the characteristic types of Australian Mammalia were already developed in that region in Miocene times.

But Austro-Columbia presents difficulties from which Australia is free; *Camelidæ* and *Tapiridæ* are now indigenous in South America as they are in Arctogæa; and, among the Pliocene Austro-Columbian mammals, the Austro-Columbian genera *Equus*, *Mastodon*, and *Machairodus* are numbered. Are these Postmiocene immigrants, or Præmiocene natives?

Still more perplexing are the strange and interesting forms *Toxodon*, *Macrauchenia*, *Typotherium*, and a new Anoplotherioid mammal (*Homalodotherium*) which Dr. Cunningham sent over to me some time ago from Patagonia. I confess I am strongly inclined to surmise that these last, at any rate, are remnants of the population of Austro-Columbia before the Miocene epoch, and were not derived from Arctogæa by way of the north and east.

The fact that this immense fauna of Miocene Arctogæa is now fully and richly represented only in India and in South Africa, while it is shrunk and depauperized in North Asia, Europe, and North America, becomes at once intelligible, if we suppose that India and South Africa had but a scanty mammalian population before the Miocene immigration, while the conditions were highly favourable to the new comers. It is to be supposed that these new regions offered themselves to the Miocene Ungulates, as South America and Australia offered themselves to the cattle, sheep, and horses of modern colonists. But, after these great areas were thus peopled, came the

Glacial epoch, during which the excessive cold, to say nothing of depression and ice-covering, must have almost depopulated all the northern parts of Arctogæa, destroying all the higher mammalian forms, except those which, like the Elephant and Rhinoceros, could adjust their coats to the altered conditions. Even these must have been driven away from the greater part of the area; only those Miocene mammals which had passed into Hindostan and into South Africa would escape decimation by such changes in the physical geography of Arctogæa. And when the northern hemisphere passed into its present condition, these lost tribes of the Miocene Fauna were hemmed by the Himalayas, the Sahara, the Red Sea, and the Arabian deserts, within their present boundaries.

Now, on the hypothesis of evolution, there is no sort of difficulty in admitting that the differences between the Miocene forms of the mammalian Fauna and those which exist at present are the results of gradual modification; and, since such differences in distribution as obtain are readily explained by the changes which have taken place in the physical geography of the world since the Miocene epoch, it is clear that the result of the comparison of the Miocene and present Faunæ is distinctly in favour of evolution. Indeed I may go further. I may say that the hypothesis of evolution explains the facts of Miocene, Pliocene, and Recent distribution, and that no other supposition even pretends to account for them. It is, indeed, a conceivable supposition that every species of Rhinoceros and every species of Hyæna, in the long succession of forms between the Miocene and the present species, was separately constructed out of dust, or out of nothing, by supernatural power; but until I receive distinct evidence of the fact, I refuse to run the risk of insulting any sane man by supposing that he seriously holds such a notion.

Let us now take a step further back in time, and inquire into the relations between the Miocene Fauna and its predecessor of the Upper Eocene formation.

Here it is to be regretted that our materials for forming a judgment are nothing to be compared in point of extent or variety with those which are yielded by the Miocene strata. However, what we do know of this Upper Eocene Fauna of Europe gives sufficient positive information to enable us to draw some tolerably safe inferences. It has yielded representatives of *Insectivora*, of *Cheiroptera*, of *Rodentia*, of *Carnivora*, of artiodactyle and perissodactyle *Ungulata*, and of opossum-like Marsupials. No Australian type of Marsupial has been discovered in the Upper Eocene strata, nor any Edentate mammal. The genera (except perhaps in the case of some of the *Insectivora*, *Cheiroptera*, and *Rodentia*) are different from those of the Miocene epoch, but present a remarkable general similarity to the Miocene and recent genera. In several cases, as I have already shown, it has now been clearly made out that the relation between the Eocene and Miocene forms is such that the Eocene form is the less specialized; while its Miocene ally is more so, and the specialization reaches its maximum in the recent forms of the same type.

So far as the Upper Eocene and the Miocene Mammalian Faunæ are comparable, their relations are such as in no way to oppose the hypothesis that the older are the progenitors of the more recent forms, while, in some cases, they distinctly favour that hypothesis. The period in time and the changes in physical geography represented by the nummulitic deposits are undoubtedly very great, while the remains of Middle Eocene and Older Eocene Mammals are comparatively few. The general facies of the Middle Eocene Fauna, however, is quite that of the Upper. The Older Eocene pre-nummulitic

mammalian Fauna contains Bats, two genera of *Carnivora*, three genera of *Ungulata* (probably all perissodactyle), and a didelphid Marsupial; all these forms, except perhaps the Bat and the Opossum, belong to genera which are not known to occur out of the Lower Eocene formation. The *Coryphodon* appears to have been allied to the Miocene and later Tapirs, while *Pliolophus*, in its skull and dentition, curiously partakes of both artiodactyle and perissodactyle characters; the third trochanter upon its femur, and its three-toed hind foot, however, appear definitely to fix its position in the latter division.

There is nothing, then, in what is known of the older Eocene mammals of the Arctogæal province to forbid the supposition that they stood in an ancestral relation to those of the Calcaire Grossier and the Gypsum of the Paris basin, and that our present fauna, therefore, is directly derived from that which already existed in Arctogæa at the commencement of the Tertiary period. But if we now cross the frontier between the Cainozoic and the Mesozoic faunæ, as they are preserved within the Arctogæal area, we meet with an astounding change, and what appears to be a complete and unmistakable break in the line of biological continuity.

Among the twelve or fourteen species of *Mammalia* which are said to have been found in the Purbecks, not one is a member of the orders *Cheiroptera*, *Rodentia*, *Ungulata*, or *Carnivora*, which are so well represented in the Tertiaries. No *Insectivora* are certainly known, nor any opossum-like Marsupials. Thus there is a vast negative difference between the Cainozoic and the Mesozoic mammalian faunæ of Europe. But there is a still more important positive difference, inasmuch as all these Mammalia appear to be Marsupials belonging to Australian groups, and thus appertaining to a different

distributional province from the Eocene and Miocene marsupials, which are Austro-Columbian. So far as the imperfect materials which exist enable a judgment to be formed, the same law appears to have held good for all the earlier Mesozoic *Mammalia*. Of the Stonesfield slate mammals, one, *Amphitherium*, has a definitely Australian character; one, *Phascolotherium*, may be either Dasyurid or Didelphine; of a third, *Stereognathus*, nothing can at present be said. The two mammals of the Trias, also, appear to belong to Australian groups.

Every one is aware of the many curious points of resemblance between the marine fauna of the European Mesozoic rocks and that which now exists in Australia. But if there was this Australian facies about both the terrestrial and the marine faunæ of Mesozoic Europe, and if there is this unaccountable and immense break between the fauna of Mesozoic and that of Tertiary Europe, is it not a very obvious suggestion that, in the Mesozoic epoch, the Australian province included Europe, and that the Arctogæal province was contained within other limits? The Arctogæal province is at present enormous, while the Australian is relatively small. Why should not these proportions have been different during the Mesozoic epoch?

Thus I am led to think that by far the simplest and most rational mode of accounting for the great change which took place in the living inhabitants of the European area at the end of the Mesozoic epoch, is the supposition that it arose from a vast alteration of the physical geography of the globe; whereby an area long tenanted by Cainozoic forms was brought into such relations with the European area that migration from the one to the other became possible, and took place on a great scale.

This supposition relieves us, at once, from the difficulty in which we were left, some time ago, by the arguments

which I used to demonstrate the necessity of the existence of all the great types of the Eocene epoch in some antecedent period.

It is this Mesozoic continent (which may well have lain in the neighbourhood of what are now the shores of the North Pacific Ocean) which I suppose to have been occupied by the Mesozoic *Monodelphia*; and it is in this region that I conceive they must have gone through the long series of changes by which they were specialized into the forms which we refer to different orders. I think it very probable that what is now South America may have received the characteristic elements of its mammalian fauna during the Mesozoic epoch; and there can be little doubt that the general nature of the change which took place at the end of the Mesozoic epoch in Europe was the upheaval of the eastern and northern regions of the Mesozoic sea-bottom into a westward extension of the Mesozoic continent, over which the mammalian fauna, by which it was already peopled, gradually spread. This invasion of the land was prefaced by a previous invasion of the Cretaceous sea by modern forms of mollusca and fish.

It is easy to imagine how an analogous change might come about in the existing world. There is, at present, a great difference between the fauna of the Polynesian Islands and that of the west coast of America. The animals which are leaving their spoils in the deposits now forming in these localities are widely different. Hence, if a gradual shifting of the deep sea, which at present bars migration between the easternmost of these islands and America, took place to the westward, while the American side of the sea-bottom was gradually upheaved, the palæontologist of the future would find, over the Pacific area, exactly such a change as I am supposing to have occurred in the North-Atlantic area

at the close of the Mesozoic period. An Australian fauna would be found underlying an American fauna, and the transition from the one to the other would be as abrupt as that between the Chalk and lower Tertiaries; and as the drainage-area of the newly formed extension of the American continent gave rise to rivers and lakes, the mammals mired in their mud would differ from those of like deposits on the Australian side, just as the Eocene mammals differ from those of the Purbecks.

How do similar reasonings apply to the other great change of life—that which took place at the end of the Palæozoic period?

In the Triassic epoch, the distribution of the dry land and of terrestrial vertebrate life appears to have been, generally, similar to that which existed in the Mesozoic epoch; so that the Triassic continents and their faunæ seem to be related to the Mesozoic lands and their faunæ, just as those of the Miocene epoch are related to those of the present day. In fact, as I have recently endeavoured to prove to the Society, there was an Arctogæal continent and an Arctogæal province of distribution in Triassic times as there is now; and the *Sauropsida* and *Marsupialia* which constituted that fauna were, I doubt not, the progenitors of the *Sauropsida* and *Marsupialia* of the whole Mesozoic epoch.

Looking at the present terrestrial fauna of Australia, it appears to me to be very probable that it is essentially a remnant of the fauna of the Triassic, or even of an earlier, age;[1] in which case Australia must at that time have been in continuity with the Arctogæal continent.

But now comes the further inquiry, Where was the

[1] Since this Address was read, Mr. Krefft has sent us news of the discovery in Australia of a freshwater fish of strangely Palæozoic aspect, and apparently a Ganoid intermediate between *Dipterus* and *Lepidosiren*.

highly differentiated Sauropsidan fauna of the Trias in Palæozoic times? The supposition that the Dinosaurian, Crocodilian, Dicynodontian, and Plesiosaurian types were suddenly created at the end of the Permian epoch may be dismissed, without further consideration, as a monstrous and unwarranted assumption. The supposition that all these types were rapidly differentiated out of *Lacertilia*, in the time represented by the passage from the Palæozoic to the Mesozoic formation, appears to me to be hardly more credible, to say nothing of the indications of the existence of Dinosaurian forms in the Permian rocks which have already been obtained.

For my part, I entertain no sort of doubt that the Reptiles, Birds, and Mammals of the Trias are the direct descendants of Reptiles, Birds, and Mammals which existed in the latter part of the Palæozoic epoch, but not in any area of the present dry land which has yet been explored by the geologist.

This may seem a bold assumption, but it will not appear unwarrantable to those who reflect upon the very small extent of the earth's surface which has hitherto exhibited the remains of the great Mammalian fauna of the Eocene times. In this respect, the Permian land Vertebrate fauna appears to me to be related to the Triassic much as the Eocene is to the Miocene. Terrestrial reptiles have been found in Permian rocks only in three localities; in some spots of France, and recently of England, and over a more extensive area in Germany. Who can suppose that the few fossils yet found in these regions give any sufficient representation of the Permian fauna?

It may be said that the Carboniferous formations demonstrate the existence of a vast extent of dry land in the present dry-land area, and that the supposed terrestrial Palæozoic Vertebrate Fauna ought to

have left its remains in the Coal-measures, especially a there is now reason to believe that much of the coal wa formed by the accumulation of spores and sporangia o dry land. But if we consider the matter more closely I think that this apparent objection loses its force. It clear that, during the Carboniferous epoch, the vast are of land which is now covered by Coal-measures mus have been undergoing a gradual depression. The dr land thus depressed must, therefore, have existed, a such, before the Carboniferous epoch—in other word in Devonian times—and its terrestrial population ma never have been other than such as existed during th Devonian, or some previous epoch, although much high forms may have been developed elsewhere.

Again, let me say that I am making no gratuitou assumption of inconceivable changes. It is clear tha the enormous area of Polynesia is, on the whole, an are over which depression has taken place to an immens extent; consequently a great continent, or assemblag of subcontinental masses of land, must have existed a some former time, and that at a recent period, geological speaking, in the area of the Pacific. But if that con tinent had contained Mammals, some of them must hav remained to tell the tale; and as it is well known tha these islands have no indigenous *Mammalia*, it is sa to assume that none existed. Thus, midway betwee Australia and South America, each of which possesses a abundant and diversified mammalian fauna, a mass land, which may have been as large as both put togethe must have existed without a mammalian inhabitan Suppose that the shores of this great land were fringe as those of tropical Australia are now, with belts mangroves, which would extend landwards on the on side, and be buried beneath littoral deposits on the oth side, as depression went on; and great beds of mangro

lignite might accumulate over the sinking land. Let upheaval of the whole now take place, in such a manner as to bring the emerging land into continuity with the South-American or Australian continent, and, in course of time, it would be peopled by an extension of the fauna of one of these two regions—just as I imagine the European Permian dry land to have been peopled.

I see nothing whatever against the supposition that distributional provinces of terrestrial life existed in the Devonian epoch, inasmuch as M. Barrande has proved that they existed much earlier. I am aware of no reason for doubting that, as regards the grades of terrestrial life contained in them, one of these may have been related to another as New Zealand is to Australia, or as Australia is to India, at the present day. Analogy seems to me to be rather in favour of, than against, the supposition that while only Ganoid fishes inhabited the fresh waters of our Devonian land, *Amphibia* and *Reptilia*, or even higher forms, may have existed, though we have not yet found them. The earliest Carboniferous *Amphibia* now known, such as *Anthracosaurus*, are so highly specialized that I can by no means conceive that they have been developed out of piscine forms in the interval between the Devonian and the Carboniferous periods, considerable as that is. And I take refuge in one of two alternatives: either they existed in our own area during the Devonian epoch and we have simply not yet found them; or they formed part of the population of some other distributional province of that day, and only entered our area by migration at the end of the Devonian epoch. Whether *Reptilia* and *Mammalia* existed along with them is to me, at present, a perfectly open question, which is just as likely to receive an affirmative as a negative answer from future inquirers.

Let me now gather together the threads of my argu-

mentation into the form of a connected hypothetical view of the manner in which the distribution of living and extinct animals has been brought about.

I conceive that distinct provinces of the distribution of terrestrial life have existed since the earliest period at which that life is recorded, and possibly much earlier; and I suppose, with Mr. Darwin, that the progress of modification of terrestrial forms is more rapid in areas of elevation than in areas of depression. I take it to be certain that Labyrinthodont *Amphibia* existed in the distributional province which included the dry land depressed during the Carboniferous epoch; and I conceive that, in some other distributional provinces of that day, which remained in the condition of stationary or of increasing dry land, the various types of the terrestrial *Sauropsida* and of the *Mammalia* were gradually developing.

The Permian epoch marks the commencement of a new movement of upheaval in our area, which attained its maximum in the Triassic epoch, when dry land existed in North America, Europe, Asia, and Africa, as it does now. Into this great new continental area the Mammals, Birds, and Reptiles developed during the Palæozoic epoch spread, and formed the great Triassic Arctogæal province. But, at the end of the Triassic period, the movement of depression recommenced in our area, though it was doubtless balanced by elevation elsewhere; modification and development, checked in the one province, went on in that "elsewhere;" and the chief forms of Mammals, Birds, and Reptiles, as we know them, were evolved and peopled the Mesozoic continent. I conceive Australia to have become separated from the continent as early as the end of the Triassic epoch, or not much later. The Mesozoic continent must, I conceive, have lain to the east, about the shores of the North Pacific and Indian Oceans;

and I am inclined to believe that it continued along the eastern side of the Pacific area to what is now the province of Austro-Columbia, the characteristic fauna of which is probably a remnant of the population of the latter part of this period.

Towards the latter part of the Mesozoic period the movement of upheaval around the shores of the Atlantic once more recommenced, and was very probably accompanied by a depression around those of the Pacific. The Vertebrate fauna elaborated in the Mesozoic continent moved westward and took possession of the new lands, which gradually increased in extent up to, and in some directions after, the Miocene epoch.

It is in favour of this hypothesis, I think, that it is consistent with the persistence of a general uniformity in the positions of the great masses of land and water. From the Devonian period, or earlier, to the present day, the four great oceans, Atlantic, Pacific, Arctic, and Antarctic, may have occupied their present positions, and only their coasts and channels of communication have undergone an incessant alteration. And, finally, the hypothesis I have put before you requires no supposition that the rate of change in organic life has been either greater or less in ancient times than it is now; nor any assumption, either physical or biological, which has not its justification in analogous phenomena of existing nature.

I have now only to discharge the last duty of my office, which is to thank you, not only for the patient attention with which you have listened to me so long to-day, but also for the uniform kindness with which, for the past two years, you have rendered my endeavours to perform the important, and often laborious, functions of your President a pleasure instead of a burden.

X.

BIOGENESIS AND ABIOGENESIS.

(THE PRESIDENTIAL ADDRESS TO THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE FOR 1870.)

IT has long been the custom for the newly installed President of the British Association for the Advancement of Science to take advantage of the elevation of the position in which the suffrages of his colleagues had, for the time, placed him, and, casting his eyes around the horizon of the scientific world, to report to them what could be seen from his watch-tower; in what directions the multitudinous divisions of the noble army of the improvers of natural knowledge were marching; what important strongholds of the great enemy of us all, ignorance, had been recently captured; and, also, with due impartiality, to mark where the advanced posts of science had been driven in, or a long-continued siege had made no progress.

I propose to endeavour to follow this ancient precedent, in a manner suited to the limitations of my knowledge and of my capacity. I shall not presume to attempt a panoramic survey of the world of science, nor even to give a sketch of what is doing in the one great province of biology, with some portions of which my ordinary occupations render me familiar. But I shall endeavour to put before you the history of the rise and progress of

a single biological doctrine; and I shall try to give some notion of the fruits, both intellectual and practical, which we owe, directly or indirectly, to the working out, by seven generations of patient and laborious investigators, of the thought which arose, more than two centuries ago, in the mind of a sagacious and observant Italian naturalist.

It is a matter of every-day experience that it is difficult to prevent many articles of food from becoming covered with mould; that fruit, sound enough to all appearance, often contains grubs at the core; that meat, left to itself in the air, is apt to putrefy and swarm with maggots. Even ordinary water, if allowed to stand in an open vessel, sooner or later becomes turbid and full of living matter.

The philosophers of antiquity, interrogated as to the cause of these phenomena, were provided with a ready and a plausible answer. It did not enter their minds even to doubt that these low forms of life were generated in the matters in which they made their appearance. Lucretius, who had drunk deeper of the scientific spirit than any poet of ancient or modern times except Goethe, intends to speak as a philosopher, rather than as a poet, when he writes that "with good reason the earth has gotten the name of mother, since all things are produced out of the earth. And many living creatures, even now, spring out of the earth, taking form by the rains and the heat of the sun."[1] The axiom of ancient science, "that

[1] It is thus that Mr. Munro renders

> "Linquitur, ut merito maternum nomen adepta
> Terra sit, e terra quoniam sunt cuncta creata.
> Multaque nunc etiam exsistant animalia terris
> Imbribus et calido solis concreta vapore."
>
> *De Rerum Natura*, lib. v. 793—796.

But would not the meaning of the last line be better rendered "Developed in rain-water and in the warm vapours raised by the sun"?

the corruption of one thing is the birth of another," had its popular embodiment in the notion that a seed dies before the young plant springs from it; a belief so wide-spread and so fixed, that Saint Paul appeals to it in one of the most splendid outbursts of his fervid eloquence:—

"Thou fool, that which thou sowest is not quickened, except it die."[1]

The proposition that life may, and does, proceed from that which has no life, then, was held alike by the philosophers, the poets, and the people, of the most enlightened nations, eighteen hundred years ago; and it remained the accepted doctrine of learned and unlearned Europe, through the Middle Ages, down even to the seventeenth century.

It is commonly counted among the many merits of our great countryman, Harvey, that he was the first to declare the opposition of fact to venerable authority in this, as in other matters; but I can discover no justification for this wide-spread notion. After careful search through the "Exercitationes de Generatione," the most that appears clear to me is, that Harvey believed all animals and plants to spring from what he terms a "*primordium vegetale*," a phrase which may nowadays be rendered "a vegetative germ;" and this, he says, is "*oviforme*," or "egg-like;" not, he is careful to add, that it necessarily has the shape of an egg, but because it has the constitution and nature of one. That this "*primordium oviforme*" must needs, in all cases, proceed from a living parent is nowhere expressly maintained by Harvey, though such an opinion may be thought to be implied in one or two passages; while, on the other hand, he does, more than once, use language which is consistent only with a full belief in spontaneous or equivocal generation.[2] In fact,

[1] 1 Corinthians xv. 36.

[2] See the following passage in Exercitatio I.:—"Item *sponte nascentia*

the main concern of Harvey's wonderful little treatise is not with generation, in the physiological sense, at all, but with development; and his great object is the establishment of the doctrine of epigenesis.

The first distinct enunciation of the hypothesis that all living matter has sprung from pre-existing living matter, came from a contemporary, though a junior, of Harvey, a native of that country, fertile in men great in all departments of human activity, which was to intellectual Europe, in the sixteenth and seventeenth centuries, what Germany is in the nineteenth. It was in Italy, and from Italian teachers, that Harvey received the most important part of his scientific education. And it was a student trained in the same schools, Francesco Redi—a man of the widest knowledge and most versatile abilities, distinguished alike as scholar, poet, physician, and naturalist—who, just two hundred and two years ago, published his "Esperienze intorno alla Generazione degl' Insetti," and gave to the world the idea, the growth of which it is my purpose to trace. Redi's book went through five editions in twenty years; and the extreme simplicity of his experiments, and the clearness of his arguments, gained for his views, and for their consequences, almost universal acceptance.

Redi did not trouble himself much with speculative considerations, but attacked particular cases of what was supposed to be "spontaneous generation" experimentally. Here are dead animals, or pieces of meat, says he; I

dicuntur; non quod ex *putredine* oriunda sint, sed quod casu, naturæ sponte, et æquivocâ (ut aiunt) generatione, a parentibus sui dissimilibus proveniant." Again, in "De Uteri Membranis:"—"In cunctorum viventium generatione (sicut diximus) hoc solenne est, ut ortum ducunt a *primordio* aliquo, quod tum materiam tum efficiendi potestatem in se habet: sitque adeo id, ex quo et a quo quicquid nascitur, ortum suum ducat. Tale primordium in animalibus (*sive ab aliis generantibus proveniant, sive sponte, aut ex putredine nascentur*) est humor in tunicâ aliquâ aut putami ne conclusus." Compare also what Redi has to say respecting Harvey's opinions, "Esperienze," p. 11.

expose them to the air in hot weather, and in a few days they swarm with maggots. You tell me that these are generated in the dead flesh; but if I put similar bodies, while quite fresh, into a jar, and tie some fine gauze over the top of the jar, not a maggot makes its appearance, while the dead substances, nevertheless, putrefy just in the same way as before. It is obvious, therefore, that the maggots are not generated by the corruption of the meat; and that the cause of their formation must be a something which is kept away by gauze. But gauze will not keep away aëriform bodies, or fluids. This something must, therefore, exist in the form of solid particles too big to get through the gauze. Nor is one long left in doubt what these solid particles are; for the blow-flies, attracted by the odour of the meat, swarm round the vessel, and, urged by a powerful but in this case misleading instinct, lay eggs out of which maggots are immediately hatched upon the gauze. The conclusion, therefore, is unavoidable; the maggots are not generated by the meat, but the eggs which give rise to them are brought through the air by the flies.

These experiments seem almost childishly simple, and one wonders how it was that no one ever thought of them before. Simple as they are, however, they are worthy of the most careful study, for every piece of experimental work since done, in regard to this subject, has been shaped upon the model furnished by the Italian philosopher. As the results of his experiments were the same, however varied the nature of the materials he used, it is not wonderful that there arose in Redi's mind a presumption, that, in all such cases of the seeming production of life from dead matter, the real explanation was the introduction of living germs from without into that dead matter.[1] And thus the hypothesis that living

[1] "Pure contentandomi sempre in questa ed in ciascuna altro cosa, da ciascuno più savio, là dove io difettuosamente parlassi, esser corretto; non tacero,

matter always arises by the agency of pre-existing living matter, took definite shape; and had, henceforward, a right to be considered and a claim to be refuted, in each particular case, before the production of living matter in any other way could be admitted by careful reasoners. It will be necessary for me to refer to this hypothesis so frequently, that, to save circumlocution, I shall call it the hypothesis of *Biogenesis;* and I shall term the contrary doctrine—that living matter may be produced by not living matter—the hypothesis of *Abiogenesis.*

In the seventeenth century, as I have said, the latter was the dominant view, sanctioned alike by antiquity and by authority; and it is interesting to observe that Redi did not escape the customary tax upon a discoverer of having to defend himself against the charge of impugning the authority of the Scriptures;[1] for his

che per molte osservazioni molti volti da me fatte, mi sento inclinato a credere che la terra, da quelle prime piante, e da quei primi animali in poi, che ella nei primi giorni del mondo produsse per comandamento del sovrano ed omnipotente Fattore, non abbia mai più prodotto da se medesima nè erba nè albero, nè animale alcuno perfetto o imperfetto che ei se fosse; e che tutto quello, che ne' tempi trapassati è nato e che ora nascere in lei, o da lei veggiamo, venga tutto dalla semenza reale e vera delle piante, e degli animali stessi, i quali col mezzo del proprio seme la loro spezie conservano. E se bene tutto giorno scorghiamo da' cadaveri degli animali, e da tutte quante le maniere dell' erbe, e de' fiori, e dei frutti imputriditi, e corrotti nascere vermi infiniti—

'Nonne vides quæcunque mora, fluidoque calore
Corpora tabescunt in parva animalia verti'—

Io mi sento, dico, inclinato a credere che tutti quei vermi si generino dal seme paterno; e che le carni, e l' erbe, e l' altre cose tutte putrefatte, o putrefattibili non facciano altra parte, nè abbiano altro ufizio nella generazione degl' insetti, se non d'apprestare un luogo o un nido proporzionato, in cui dagli animali nel tempo della figliatura sieno portati, e partoriti i vermi, o l' uova o l' altre semenze dei vermi, i quali tosto che nati sono, trovano in esso nido un sufficiente alimento abilissimo per nutricarsi: e se in quello non son portate dalle madri queste suddette semenze, niente mai, e replicatamente niente, vi s' ingegneri e nasca."—REDI, *Esperienze,* pp. 14—16.

[1] "Molti, e molti altri ancora vi potrei annoverare, se non fossi chiamato a rispondere alle rampogne di alcuni, che bruscamente mi rammentano ciò, che si legge nel capitolo quattordicesimo del sacrosanto Libro de' giudici. . . . . "—REDI, *loc cit.* p. 45.

adversaries declared that the generation of bees from the carcase of a dead lion is affirmed, in the Book of Judges, to have been the origin of the famous riddle with which Samson perplexed the Philistines :—

> "Out of the eater came forth meat,
> And out of the strong came forth sweetness."

Against all odds, however, Redi, strong with the strength of demonstrable fact, did splendid battle for Biogenesis ; but it is remarkable that he held the doctrine in a sense which, if he had lived in these times, would have infallibly caused him to be classed among the defenders of "spontaneous generation." "Omne vivum ex vivo," "no life without antecedent life," aphoristically sums up Redi's doctrine ; but he went no further. It is most remarkable evidence of the philosophic caution and impartiality of his mind, that although he had speculatively anticipated the manner in which grubs really are deposited in fruits and in the galls of plants, he deliberately admits that the evidence is insufficient to bear him out ; and he therefore prefers the supposition that they are generated by a modification in the living substance of the plants themselves. Indeed, he regards these vegetable growths as organs, by means of which the plant gives rise to an animal, and looks upon this production of specific animals as the final cause of the galls and of at any rate some fruits. And he proposes to explain the occurrence of parasites within the animal body in the same way.[1]

[1] The passage ("Esperienze," p. 129) is worth quoting in full :—

"Se dovessi palesarvi il mio sentimento crederei che i frutti, i legumi, gli alberi e le foglie, in due maniere inverminassero. Una, perchè venendo i bachi per di fuora, e cercando l'alimento, col rodere ci aprono la strada. ed arrivano alla più interna midolla de' frutti e de' legni. L'altra maniera si è, che io per me stimerei, che non fosse gran fatto disdicevole il credere, che quell' anima o quella virtù, la quale genera i fiori ed i frutti nelle piante viventi, sia quella stessa che generi ancora i bachi di esse piante. E chi sà forse, che molti frutti

It is of great importance to apprehend Redi's position rightly; for the lines of thought he laid down for us are those upon which naturalists have been working ever since. Clearly, he held *Biogenesis* as against *Abiogenesis*; and I shall immediately proceed, in the first place, to inquire how far subsequent investigation has borne him out in so doing.

But Redi also thought that there were two modes of Biogenesis. By the one method, which is that of common and ordinary occurrence, the living parent gives rise to offspring which passes through the same cycle of changes as itself—like gives rise to like; and this has been termed *Homogenesis*. By the other mode, the living parent was supposed to give rise to offspring

degli alberi non sieno prodotti, non per un fine primario e principale, ma bensì per un uffizio secondario e servile, destinato alla generazione di que' vermi, servendo a loro in vece di matrice, in cui dimorino un prefisso e determinato tempo; il quale arrivato escan fuora a godere il sole.

"Io m' immagino, che questo mio pensiero non vi parrà totalmente un paradosso; mentre farete riflessione a quelle tante sorte di galle, di gallozzole, di coccole, di ricci, di calici, di cornetti e di lappole, che son produtte dalle quercel dalle farnie, da' cerri, da' sugheri, da' lecci e da altri simili alberi da ghianda; imperciocchè in quelle gallozzole, e particolarmente nelle più grosse, che si chiamano coronati, ne' ricci capelluti, che ciuffoli da' nostri contadini son detti; nei ricci legnosi del cerro, ne' ricci stellati della quercia, nelle galluzze della foglia del leccio si vede evidentissimamente, che la prima e principale intenzione della natura è formare dentro di quelle un animale volante; vedendosi nel centro della gallozzola un uovo, che col crescere e col maturarsi di essa gallozzola va crescendo e maturando anch' egli, e cresce altresì a suo tempo quel verme, che nell' uovo si racchiude; il qual verme, quando la gallozzola è finita di maturare e che è venuto il termine destinato al suo nascimento, diventa, di verme che era, una mosca. . . . . Io vi confesso ingenuamente, che prima d'aver fatte queste mie esperienze intorno alla generazione degl' insetti mi dava a credere, o per dir meglio sospettava, che forse la gallozzola nascesse, perchè arrivando la mosca nel tempo della primavera, e facendo una piccolissima fessura ne' rami più teneri della quercia, in quella fessura nascondesse uno de suoi semi, il quale fosse cagione che sbocciasse fuora la gallozzola; e che mai non si vedessero galle o gallozzole o ricci o cornetti o calici o coccole, se non in que' rami, ne' quali le mosche avessero depositate le loro semenze; e mi dava ad intendere, che le gallozzole fossero una malattia cagionata nelle querce dalle punture delle mosche, in quella giusa stessa che dalle punture d'altri animaletti simiglievoli veggiamo crescere de' tumori ne' corpi degli animali."

which passed through a totally different series of states from those exhibited by the parent, and did not return into the cycle of the parent; this is what ought to be called *Heterogenesis*, the offspring being altogether, and permanently, unlike the parent. The term Heterogenesis, however, has unfortunately been used in a different sense, and M. Milne-Edwards has therefore substituted for it *Xenogenesis*, which means the generation of something foreign. After discussing Redi's hypothesis of universal Biogenesis, then, I shall go on to ask how far the growth of science justifies his other hypothesis of Xenogenesis.

The progress of the hypothesis of Biogenesis was triumphant and unchecked for nearly a century. The application of the microscope to anatomy in the hands of Grew, Leeuwenhoek, Swammerdam, Lyonnet, Vallisnieri, Réaumur, and other illustrious investigators of nature of that day, displayed such a complexity of organization in the lowest and minutest forms, and everywhere revealed such a prodigality of provision for their multiplication by germs of one sort or another, that the hypothesis of Abiogenesis began to appear not only untrue, but absurd; and, in the middle of the eighteenth century, when Needham and Buffon took up the question, it was almost universally discredited.[1]

But the skill of the microscope-makers of the eighteenth century soon reached its limit. A microscope magnifying 400 diameters was a *chef d'œuvre* of the opticians of that

[1] Needham, writing in 1750, says:—

"Les naturalistes modernes s'accordent unanimement à établir, comme une vérité certaine, que toute plante vient de sa sémence spécifique, tout animal d'un œuf ou de quelque chose d'analogue préexistant dans la plante, ou dans l'animal de même espèce qui l'a produit."—*Nouvelles Observations*, p. 169.

"Les naturalistes ont généralement cru que les animaux microscopiques étaient engendrés par des œufs transportés dans l'air, ou déposés dans des eaux dormantes par des insectes volans."—*Ibid.* p. 176.

day; and, at the same time, by no means trustworthy. But a magnifying power of 400 diameters, even when definition reaches the exquisite perfection of our modern achromatic lenses, hardly suffices for the mere discernment of the smallest forms of life. A speck, only $\frac{1}{25}$th of an inch in diameter, has, at ten inches from the eye, the same apparent size as an object $\frac{1}{10000}$th of an inch in diameter, when magnified 400 times; but forms of living matter abound, the diameter of which is not more than $\frac{1}{40000}$th of an inch. A filtered infusion of hay, allowed to stand for two days, will swarm with living things among which, any which reaches the diameter of a human red blood-corpuscle, or about $\frac{1}{3200}$th of an inch, is a giant. It is only by bearing these facts in mind, that we can deal fairly with the remarkable statements and speculations put forward by Buffon and Needham in the middle of the eighteenth century.

When a portion of any animal or vegetable body is infused in water, it gradually softens and disintegrates; and, as it does so, the water is found to swarm with minute active creatures, the so-called Infusorial Animalcules, none of which can be seen, except by the aid of the microscope; while a large proportion belong to the category of smallest things of which I have spoken, and which must have looked like mere dots and lines under the ordinary microscopes of the eighteenth century.

Led by various theoretical considerations which I cannot now discuss, but which looked promising enough in the lights of their time, Buffon and Needham doubted the applicability of Redi's hypothesis to the infusorial animalcules, and Needham very properly endeavoured to put the question to an experimental test. He said to himself, If these infusorial animalcules come from germs, their germs must exist either in the substance infused, or

in the water with which the infusion is made, or in the superjacent air. Now the vitality of all germs is destroyed by heat. Therefore, if I boil the infusion, cork it up carefully, cementing the cork over with mastic, and then heat the whole vessel by heaping hot ashes over it, I must needs kill whatever germs are present. Consequently, if Redi's hypothesis hold good, when the infusion is taken away and allowed to cool, no animalcules ought to be developed in it; whereas, if the animalcules are not dependent on pre-existing germs, but are generated from the infused substance, they ought, by and by, to make their appearance. Needham found that, under the circumstances in which he made his experiments, animalcules always did arise in the infusions, when a sufficient time had elapsed to allow for their development.

In much of his work Needham was associated with Buffon, and the results of their experiments fitted in admirably with the great French naturalist's hypothesis of "organic molecules," according to which, life is the indefeasible property of certain indestructible molecules of matter, which exist in all living things, and have inherent activities by which they are distinguished from not living matter. Each individual living organism is formed by their temporary combination. They stand to it in the relation of the particles of water to a cascade, or a whirlpool; or to a mould, into which the water is poured. The form of the organism is thus determined by the reaction between external conditions and the inherent activities of the organic molecules of which it is composed; and, as the stoppage of a whirlpool destroys nothing but a form, and leaves the molecules of the water, with all their inherent activities intact, so what we call the death and putrefaction of an animal, or of a plant, is merely the breaking up of the form, or manner of asso-

ciation, of its constituent organic molecules, which are then set free as infusorial animalcules.

It will be perceived that this doctrine is by no means identical with *Abiogenesis*, with which it is often confounded. On this hypothesis, a piece of beef, or a handful of hay, is dead only in a limited sense. The beef is dead ox, and the hay is dead grass; but the "organic molecules" of the beef or the hay are not dead, but are ready to manifest their vitality as soon as the bovine or herbaceous shrouds in which they are imprisoned are rent by the macerating action of water. The hypothesis therefore must be classified under Xenogenesis, rather than under Abiogenesis. Such as it was, I think it will appear, to those who will be just enough to remember that it was propounded before the birth of modern chemistry, and of the modern optical arts, to be a most ingenious and suggestive speculation.

But the great tragedy of Science—the slaying of a beautiful hypothesis by an ugly fact—which is so constantly being enacted under the eyes of philosophers, was played, almost immediately, for the benefit of Buffon and Needham.

Once more, an Italian, the Abbé Spallanzani, a worthy successor and representative of Redi in his acuteness, his ingenuity, and his learning, subjected the experiments and the conclusions of Needham to a searching criticism. It might be true that Needham's experiments yielded results such as he had described, but did they bear out his arguments? Was it not possible, in the first place, he had not completely excluded the air by his corks and mastic? And was it not possible, in the second place, that he had not sufficiently heated his infusions and the superjacent air? Spallanzani joined issue with the English naturalist on both these pleas, and he showed that if, in the first place, the glass vessels in which the

infusions were contained were hermetically sealed by fusing their necks, and if, in the second place, they were exposed to the temperature of boiling water for three-quarters of an hour,[1] no animalcules ever made their appearance within them. It must be admitted that the experiments and arguments of Spallanzani furnish a complete and a crushing reply to those of Needham. But we all too often forget that it is one thing to refute a proposition, and another to prove the truth of a doctrine which, implicitly or explicitly, contradicts that proposition; and the advance of science soon showed that though Needham might be quite wrong, it did not follow that Spallanzani was quite right.

Modern Chemistry, the birth of the latter half of the eighteenth century, grew apace, and soon found herself face to face with the great problems which biology had vainly tried to attack without her help. The discovery of oxygen led to the laying of the foundations of a scientific theory of respiration, and to an examination of the marvellous interactions of organic substances with oxygen. The presence of free oxygen appeared to be one of the conditions of the existence of life, and of those singular changes in organic matters which are known as fermentation and putrefaction. The question of the generation of the infusory animalcules thus passed into a new phase. For what might not have happened to the organic matter of the infusions, or to the oxygen of the air, in Spallanzani's experiments? What security was there that the development of life which ought to have taken place had not been checked or prevented by these changes?

The battle had to be fought again. It was needful to repeat the experiments under conditions which would make sure that neither the oxygen of the air, nor the

[1] See Spallanzani, "Opere," vi pp. 42 and 51.

composition of the organic matter, was altered in such a manner as to interfere with the existence of life.

Schulze and Schwann took up the question from this point of view in 1836 and 1837. The passage of air through red-hot glass tubes, or through strong sulphuric acid, does not alter the proportion of its oxygen, while it must needs arrest, or destroy, any organic matter which may be contained in the air. These experimenters, therefore, contrived arrangements by which the only air which should come into contact with a boiled infusion should be such as had either passed through red-hot tubes or through strong sulphuric acid. The result which they obtained was that an infusion so treated developed no living things, while, if the same infusion was afterwards exposed to the air, such things appeared rapidly and abundantly. The accuracy of these experiments has been alternately denied and affirmed. Supposing them to be accepted, however, all that they really proved was that the treatment to which the air was subjected destroyed *something* that was essential to the development of life in the infusion. This "something" might be gaseous, fluid, or solid; that it consisted of germs remained only an hypothesis of greater or less probability.

Contemporaneously with these investigations a remarkable discovery was made by Cagniard de la Tour. He found that common yeast is composed of a vast accumulation of minute plants. The fermentation of must, or of wort, in the fabrication of wine and of beer, is always accompanied by the rapid growth and multiplication of these *Torulæ*. Thus, fermentation, in so far as it was accompanied by the development of microscopical organisms in enormous numbers, became assimilated to the decomposition of an infusion of ordinary animal or vegetable matter; and it was an obvious suggestion that the organisms were, in some way or other, the causes

both of fermentation and of putrefaction. The chemists, with Berzelius and Liebig at their head, at first laughed this idea to scorn; but in 1843, a man then very young, who has since performed the unexampled feat of attaining to high eminence alike in Mathematics, Physics, and Physiology—I speak of the illustrious Helmholtz—reduced the matter to the test of experiment by a method alike elegant and conclusive. Helmholtz separated a putrefying or a fermenting liquid from one which was simply putrescible or fermentable, by a membrane which allowed the fluids to pass through and become intermixed, but stopped the passage of solids. The result was, that while the putrescible or the fermentable liquids became impregnated with the results of the putrescence or fermentation which was going on on the other side of the membrane, they neither putrefied (in the ordinary way) nor fermented; nor were any of the organisms which abounded in the fermenting or putrefying liquid generated in them. Therefore the cause of the development of these organisms must lie in something which cannot pass through membranes; and as Helmholtz's investigations were long antecedent to Graham's researches upon colloids, his natural conclusion was that the agent thus intercepted must be a solid material. In point of fact, Helmholtz's experiments narrowed the issue to this: that which excites fermentation and putrefaction, and at the same time gives rise to living forms in a fermentable or putrescible fluid, is not a gas and is not a diffusible fluid; therefore it is either a colloid, or it is matter divided into very minute solid particles.

The researches of Schroeder and Dusch in 1854, and of Schroeder alone, in 1859, cleared up this point by experiments which are simply refinements upon those of Redi. A lump of cotton-wool is, physically speaking, a pile of many thicknesses of a very fine gauze, the fineness

of the meshes of which depends upon the closeness of the compression of the wool. Now, Schroeder and Dusch found, that, in the case of all the putrefiable materials which they used (except milk and yolk of egg), an infusion boiled, and then allowed to come into contact with no air but such as had been filtered through cotton-wool, neither putrefied, nor fermented, nor developed living forms. It is hard to imagine what the fine sieve formed by the cotton-wool could have stopped except minute solid particles. Still the evidence was incomplete until it had been positively shown, first, that ordinary air does contain such particles; and, secondly, that filtration through cotton-wool arrests these particles and allows only physically pure air to pass. This demonstration has been furnished within the last year by the remarkable experiments of Professor Tyndall. It has been a common objection of Abiogenists that, if the doctrine of Biogeny is true, the air must be thick with germs; and they regard this as the height of absurdity. But Nature occasionally is exceedingly unreasonable, and Professor Tyndall has proved that this particular absurdity may nevertheless be a reality. He has demonstrated that ordinary air is no better than a sort of stir-about of excessively minute solid particles; that these particles are almost wholly destructible by heat; and that they are strained off, and the air rendered optically pure, by its being passed through cotton-wool.

But it remains yet in the order of logic, though not of history, to show that among these solid destructible particles there really do exist germs capable of giving rise to the development of living forms in suitable menstrua. This piece of work was done by M. Pasteur in those beautiful researches which will ever render his name famous; and which, in spite of all attacks upon them, appear to me now, as they did seven years

ago,[1] to be models of accurate experimentation and logical reasoning. He strained air through cotton-wool, and found, as Schroeder and Dusch had done, that it contained nothing competent to give rise to the development of life in fluids highly fitted for that purpose. But the important further links in the chain of evidence added by Pasteur are three. In the first place he subjected to microscopic examination the cotton-wool which had served as strainer, and found that sundry bodies clearly recognizable as germs, were among the solid particles strained off. Secondly, he proved that these germs were competent to give rise to living forms by simply sowing them in a solution fitted for their development. And, thirdly, he showed that the incapacity of air strained through cotton-wool to give rise to life, was not due to any occult change effected in the constituents of the air by the wool, by proving that the cotton-wool might be dispensed with altogether, and perfectly free access left between the exterior air and that in the experimental flask. If the neck of the flask is drawn out into a tube and bent downwards; and if, after the contained fluid has been carefully boiled, the tube is heated sufficiently to destroy any germs which may be present in the air which enters as the fluid cools, the apparatus may be left to itself for any time and no life will appear in the fluid. The reason is plain. Although there is free communication between the atmosphere laden with germs and the germless air in the flask, contact between the two takes place only in the tube; and as the germs cannot fall upwards, and there are no currents, they never reach the interior of the flask. But if the tube be broken short off where it proceeds from the flask, and free access be thus given to germs

[1] "Lectures to Working Men on the Causes of the Phenomena of Organic Nature," 1863.

falling vertically out of the air, the fluid, which has remained clear and desert for months, becomes, in a few days, turbid and full of life.

These experiments have been repeated over and over again by independent observers with entire success; and there is one very simple mode of seeing the facts for oneself, which I may as well describe.

Prepare a solution (much used by M. Pasteur, and often called "Pasteur's solution") composed of water with tartrate of ammonia, sugar, and yeast-ash dissolved therein.[1] Divide it into three portions in as many flasks; boil all three for a quarter of an hour; and, while the steam is passing out, stop the neck of one with a large plug of cotton-wool, so that this also may be thoroughly steamed. Now set the flasks aside to cool, and, when their contents are cold, add to one of the open ones a drop of filtered infusion of hay which has stood for twenty-four hours, and is consequently full of the active and excessively minute organisms known as *Bacteria*. In a couple of days of ordinary warm weather the contents of this flask will be milky from the enormous multiplication of *Bacteria*. The other flask, open and exposed to the air, will, sooner or later, become milky with *Bacteria*, and patches of mould may appear in it; while the liquid in the flask, the neck of which is plugged with cotton-wool, will remain clear for an indefinite time. I have sought in vain for any explanation of these facts, except the obvious one, that the air contains germs competent to give rise to *Bacteria*, such as those with which the first solution has been knowingly and purposely inoculated, and to the mould-*Fungi*. And I have not yet been able to meet with any advocate of Abiogenesis who seriously

[1] Infusion of hay treated in the same way yields similar results; but as it contains organic matter, the argument which follows cannot be based upon it.

maintains that the atoms of sugar, tartrate of ammonia, yeast-ash, and water, under no influence but that of free access of air and the ordinary temperature, re-arrange themselves and give rise to the protoplasm of *Bacterium*. But the alternative is to admit that these *Bacteria* arise from germs in the air; and if they are thus propagated, the burden of proof that other like forms are generated in a different manner, must rest with the assertor of that proposition.

To sum up the effect of this long chain of evidence:—

It is demonstrable that a fluid eminently fit for the development of the lowest forms of life, but which contains neither germs, nor any protein compound, gives rise to living things in great abundance if it is exposed to ordinary air; while no such development takes place, if the air with which it is in contact is mechanically freed from the solid particles which ordinarily float in it, and which may be made visible by appropriate means.

It is demonstrable that the great majority of these particles are destructible by heat, and that some of them are germs, or living particles, capable of giving rise to the same forms of life as those which appear when the fluid is exposed to unpurified air.

It is demonstrable that inoculation of the experimental fluid with a drop of liquid known to contain living particles gives rise to the same phenomena as exposure to unpurified air.

And it is further certain that these living particles are so minute that the assumption of their suspension in ordinary air presents not the slightest difficulty. On the contrary, considering their lightness and the wide diffusion of the organisms which produce them, it is impossible to conceive that they should not be suspended in the atmosphere in myriads.

Thus the evidence, direct and indirect, in favour of *Biogenesis* for all known forms of life must, I think, be admitted to be of great weight.

On the other side, the sole assertions worthy of attention are that hermetically sealed fluids, which have been exposed to great and long-continued heat, have sometimes exhibited living forms of low organization when they have been opened.

The first reply that suggests itself is the probability that there must be some error about these experiments, because they are performed on an enormous scale every day with quite contrary results. Meat, fruits, vegetables, the very materials of the most fermentable and putrescible infusions, are preserved to the extent, I suppose I may say, of thousands of tons every year, by a method which is a mere application of Spallanzani's experiment. The matters to be preserved are well boiled in a tin case provided with a small hole, and this hole is soldered up when all the air in the case has been replaced by steam. By this method they may be kept for years without putrefying, fermenting, or getting mouldy. Now this is not because oxygen is excluded, inasmuch as it is now proved that free oxygen is not necessary for either fermentation or putrefaction. It is not because the tins are exhausted of air, for *Vibriones* and *Bacteria* live, as Pasteur has shown, without air or free oxygen. It is not because the boiled meats or vegetables are not putrescible or fermentable, as those who have had the misfortune to be in a ship supplied with unskilfully closed tins well know. What is it, therefore, but the exclusion of germs? I think that Abiogenists are bound to answer this question before they ask us to consider new experiments of precisely the same order.

And in the next place, if the results of the experiments I refer to are really trustworthy, it by no means

follows that Abiogenesis has taken place. The resistance of living matter to heat is known to vary within considerable limits, and to depend, to some extent, upon the chemical and physical qualities of the surrounding medium. But if, in the present state of science, the alternative is offered us,—either germs can stand a greater heat than has been supposed, or the molecules of dead matter, for no valid or intelligible reason that is assigned, are able to re-arrange themselves into living bodies, exactly such as can be demonstrated to be frequently produced in another way,—I cannot understand how choice can be, even for a moment, doubtful.

But though I cannot express this conviction of mine too strongly, I must carefully guard myself against the supposition that I intend to suggest that no such thing as Abiogenesis ever has taken place in the past, or ever will take place in the future. With organic chemistry, molecular physics, and physiology yet in their infancy, and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call "vital" may not, some day, be artificially brought together. All I feel justified in affirming is, that I see no reason for believing that the feat has been performed yet.

And looking back through the prodigious vista of the past, I find no record of the commencement of life, and therefore I am devoid of any means of forming a definite conclusion as to the conditions of its appearance. Belief, in the scientific sense of the word, is a serious matter, and needs strong foundations. To say, therefore, in the admitted absence of evidence, that I have any belief as to the mode in which the existing forms of life have originated, would be using words in a wrong sense. But expectation is permissible where belief is not; and if it

were given me to look beyond the abyss of geologically recorded time to the still more remote period when the earth was passing through physical and chemical conditions, which it can no more see again than a man can recall his infancy, I should expect to be a witness of the evolution of living protoplasm from not living matter. I should expect to see it appear under forms of great simplicity, endowed, like existing fungi, with the power of determining the formation of new protoplasm from such matters as ammonium carbonates, oxalates and tartrates, alkaline and earthy phosphates, and water, without the aid of light. That is the expectation to which analogical reasoning leads me; but I beg you once more to recollect that I have no right to call my opinion anything but an act of philosophical faith.

So much for the history of the progress of Redi's great doctrine of Biogenesis, which appears to me, with the limitations I have expressed, to be victorious along the whole line at the present day.

As regards the second problem offered to us by Redi, whether Xenogenesis obtains, side by side with Homogenesis,—whether, that is, there exist not only the ordinary living things, giving rise to offspring which run through the same cycle as themselves, but also others, producing offspring which are of a totally different character from themselves,—the researches of two centuries have led to a different result. That the grubs found in galls are no product of the plants on which the galls grow, but are the result of the introduction of the eggs of insects into the substance of these plants, was made out by Vallisnieri, Réaumur, and others, before the end of the first half of the eighteenth century. The tapeworms, bladderworms, and flukes continued to be a stronghold of the advocates of Xenogenesis for a much longer period. Indeed, it is only within the

last thirty years that the splendid patience of Von Siebold, Van Beneden, Leuckart, Küchenmeister, and other helminthologists, has succeeded in tracing every such parasite, often through the strangest wanderings and metamorphoses, to an egg derived from a parent, actually or potentially like itself; and the tendency of inquiries elsewhere has all been in the same direction. A plant may throw off bulbs, but these, sooner or later, give rise to seeds or spores, which develop into the original form. A polype may give rise to Medusæ, or a pluteus to an Echinoderm, but the Medusa and the Echinoderm give rise to eggs which produce polypes or plutei, and they are therefore only stages in the cycle of life of the species.

But if we turn to pathology, it offers us some remarkable approximations to true Xenogenesis.

As I have already mentioned, it has been known since the time of Vallisnieri and of Réaumur, that galls in plants, and tumours in cattle, are caused by insects, which lay their eggs in those parts of the animal or vegetable frame of which these morbid structures are outgrowths. Again, it is a matter of familiar experience to everybody that mere pressure on the skin will give rise to a corn. Now the gall, the tumour, and the corn are parts of the living body, which have become, to a certain degree, independent and distinct organisms. Under the influence of certain external conditions, elements of the body, which should have developed in due subordination to its general plan, set up for themselves and apply the nourishment which they receive to their own purposes.

From such innocent productions as corns and warts, there are all gradations to the serious tumours which, by their mere size and the mechanical obstruction they cause, destroy the organism out of which they are developed;

while, finally, in those terrible structures known as cancers, the abnormal growth has acquired powers of reproduction and multiplication, and is only morphologically distinguishable from the parasitic worm, the life of which is neither more nor less closely bound up with that of the infested organism.

If there were a kind of diseased structure, the histological elements of which were capable of maintaining a separate and independent existence out of the body, it seems to me that the shadowy boundary between morbid growth and Xenogenesis would be effaced. And I am inclined to think that the progress of discovery has almost brought us to this point already. I have been favoured by Mr. Simon with an early copy of the last published of the valuable "Reports on the Public Health," which, in his capacity of their medical officer, he annually presents to the Lords of the Privy Council. The appendix to this report contains an introductory essay "On the Intimate Pathology of Contagion," by Dr. Burdon-Sanderson, which is one of the clearest, most comprehensive, and well-reasoned discussions of a great question which has come under my notice for a long time. I refer you to it for details and for the authorities for the statements I am about to make.

You are familiar with what happens in vaccination. A minute cut is made in the skin, and an infinitesimal quantity of vaccine matter is inserted into the wound. Within a certain time a vesicle appears in the place of the wound, and the fluid which distends this vesicle is vaccine matter, in quantity a hundred or a thousandfold that which was originally inserted. Now what has taken place in the course of this operation? Has the vaccine matter, by its irritative property, produced a mere blister, the fluid of which has the same irritative property? Or does the vaccine matter contain living

particles, which have grown and multiplied where they have been planted? The observations of M. Chauveau, extended and confirmed by Dr. Sanderson himself, appear to leave no doubt upon this head. Experiments, similar in principle to those of Helmholtz on fermentation and putrefaction, have proved that the active element in the vaccine lymph is non-diffusible, and consists of minute particles not exceeding $\frac{1}{20000}$ of an inch in diameter, which are made visible in the lymph by the microscope. Similar experiments have proved that two of the most destructive of epizootic diseases, sheep-pox and glanders, are also dependent for their existence and their propagation upon extremely small living solid particles, to which the title of *microzymes* is applied. An animal suffering under either of these terrible diseases is a source of infection and contagion to others, for precisely the same reason as a tub of fermenting beer is capable of propagating its fermentation by "infection," or "contagion," to fresh wort. In both cases it is the solid living particles which are efficient; the liquid in which they float, and at the expense of which they live, being altogether passive.

Now arises the question, are these microzymes the results of *Homogenesis*, or of *Xenogenesis;* are they capable, like the *Torulæ* of yeast, of arising only by the development of pre-existing germs; or may they be, like the constituents of a nut-gall, the results of a modification and individualization of the tissues of the body in which they are found, resulting from the operation of certain conditions? Are they parasites in the zoological sense, or are they merely what Virchow has called "heterologous growths"? It is obvious that this question has the most profound importance, whether we look at it from a practical or from a theoretical point of view. A parasite may be stamped out by

destroying its germs, but a pathological product can only be annihilated by removing the conditions which give rise to it.

It appears to me that this great problem will have to be solved for each zymotic disease separately, for analogy cuts two ways. I have dwelt upon the analogy of pathological modification, which is in favour of the xenogenetic origin of microzymes; but I must now speak of the equally strong analogies in favour of the origin of such pestiferous particles by the ordinary process of the generation of like from like.

It is, at present, a well-established fact that certain diseases, both of plants and of animals, which have all the characters of contagious and infectious epidemics, are caused by minute organisms. The smut of wheat is a well-known instance of such a disease, and it cannot be doubted that the grape-disease and the potato-disease fall under the same category. Among animals, insects are wonderfully liable to the ravages of contagious and infectious diseases caused by microscopic *Fungi*.

In autumn, it is not uncommon to see flies motionless upon a window-pane, with a sort of magic circle, in white, drawn round them. On microscopic examination, the magic circle is found to consist of innumerable spores, which have been thrown off in all directions by a minute fungus called *Empusa muscæ*, the spore-forming filaments of which stand out like a pile of velvet from the body of the fly. These spore-forming filaments are connected with others which fill the interior of the fly's body like so much fine wool, having eaten away and destroyed the creature's viscera. This is the full-grown condition of the *Empusa*. If traced back to its earliest stages, in flies which are still active, and to all appearance healthy, it is found to exist in the form of minute corpuscles which float in the blood of the fly. These

multiply and lengthen into filaments, at the expense of the fly's substance; and when they have at last killed the patient, they grow out of its body and give off spores. Healthy flies shut up with diseased ones catch this mortal disease, and perish like the others. A most competent observer, M. Cohn, who studied the development of the *Empusa* very carefully, was utterly unable to discover in what manner the smallest germs of the *Empusa* got into the fly. The spores could not be made to give rise to such germs by cultivation; nor were such germs discoverable in the air, or in the food of the fly. It looked exceedingly like a case of Abiogenesis, or, at any rate, of Xenogenesis; and it is only quite recently that the real course of events has been made out. It has been ascertained, that when one of the spores falls upon the body of a fly, it begins to germinate, and sends out a process which bores its way through the fly's skin; this, having reached the interior cavities of its body, gives off the minute floating corpuscles which are the earliest stage of the *Empusa*. The disease is "contagious," because a healthy fly coming in contact with a diseased one, from which the spore-bearing filaments protrude, is pretty sure to carry off a spore or two. It is "infectious" because the spores become scattered about all sorts of matter in the neighbourhood of the slain flies.

The silkworm has long been known to be subject to a very fatal and infectious disease called the *Muscardine*. Audouin transmitted it by inoculation. This disease is entirely due to the development of a fungus, *Botrytis Bassiana*, in the body of the caterpillar; and its contagiousness and infectiousness are accounted for in the same way as those of the fly-disease. But, of late years, a still more serious epizootic has appeared among the

silkworms ; and I may mention a few facts which will give you some conception of the gravity of the injury which it has inflicted on France alone.

The production of silk has been for centuries an important branch of industry in Southern France, and in the year 1853 it had attained such a magnitude that the annual produce of the French sericulture was estimated to amount to a tenth of that of the whole world, and represented a money-value of 117,000,000 francs, or nearly five millions sterling. What may be the sum which would represent the money-value of all the industries connected with the working up of the raw silk thus produced, is more than I can pretend to estimate. Suffice it to say, that the city of Lyons is built upon French silk as much as Manchester was upon American cotton before the civil war.

Silkworms are liable to many diseases ; and, even before 1853, a peculiar epizootic, frequently accompanied by the appearance of dark spots upon the skin (whence the name of "Pébrine" which it has received), had been noted for its mortality. But in the years following 1853 this malady broke out with such extreme violence, that, in 1858, the silk-crop was reduced to a third of the amount which it had reached in 1853 ; and, up till within the last year or two, it has never attained half the yield of 1853. This means not only that the great number of people engaged in silk growing are some thirty millions sterling poorer than they might have been ; it means not only that high prices have had to be paid for imported silkworm eggs, and that, after investing his money in them, in paying for mulberry-leaves and for attendance, the cultivator has constantly seen his silkworms perish and himself plunged in ruin ; but it means that the looms of Lyons have lacked employment, and that, for years, enforced idleness and misery have been

the portion of a vast population which, in former days, was industrious and well-to-do.

In 1858 the gravity of the situation caused the French Academy of Sciences to appoint Commissioners, of whom a distinguished naturalist, M. de Quatrefages, was one, to inquire into the nature of this disease, and, if possible, to devise some means of staying the plague. In reading the Report[1] made by M. de Quatrefages in 1859, it is exceedingly interesting to observe that his elaborate study of the Pébrine forced the conviction upon his mind that, in its mode of occurrence and propagation, the disease of the silkworm is, in every respect, comparable to the cholera among mankind. But it differs from the cholera, and so far is a more formidable malady, in being hereditary, and in being, under some circumstances, contagious as well as infectious.

The Italian naturalist, Filippi, discovered in the blood of the silkworms affected by this strange disorder a multitude of cylindrical corpuscles, each about $\frac{1}{6000}$ of an inch long. These have been carefully studied by Lebert, and named by him *Panhistophyton;* for the reason that in subjects in which the disease is strongly developed, the corpuscles swarm in every tissue and organ of the body, and even pass into the undeveloped eggs of the female moth. But are these corpuscles causes, or mere concomitants, of the disease? Some naturalists took one view and some another; and it was not until the French Government, alarmed by the continued ravages of the malady, and the inefficiency of the remedies which had been suggested, despatched M. Pasteur to study it, that the question received its final settlement; at a great sacrifice, not only of the time and peace of mind of that eminent philosopher, but, I regret to have to add, of his health.

[1] "Études sur les Maladies actuelles des Vers à Soie," p. 53.

But the sacrifice has not been in vain. It is now certain that this devastating, cholera-like, Pébrine is the effect of the growth and multiplication of the *Panhistophyton* in the silkworm. It is contagious and infectious, because the corpuscles of the *Panhistophyton* pass away from the bodies of the diseased caterpillars, directly or indirectly, to the alimentary canal of healthy silkworms in their neighbourhood; it is hereditary, because the corpuscles enter into the eggs while they are being formed, and consequently are carried within them when they are laid; and for this reason, also, it presents the very singular peculiarity of being inherited only on the mother's side. There is not a single one of all the apparently capricious and unaccountable phenomena presented by the Pébrine, but has received its explanation from the fact that the disease is the result of the presence of the microscopic organism, *Panhistophyton*.

Such being the facts with respect to the Pébrine, what are the indications as to the method of preventing it? It is obvious that this depends upon the way in which the *Panhistophyton* is generated. If it may be generated by Abiogenesis, or by Xenogenesis, within the silkworm or its moth, the extirpation of the disease must depend upon the prevention of the occurrence of the conditions under which this generation takes place. But if, on the other hand, the *Panhistophyton* is an independent organism, which is no more generated by the silkworm than the mistletoe is generated by the apple-tree or the oak on which it grows, though it may need the silkworm for its development in the same way as the mistletoe needs the tree, then the indications are totally different. The sole thing to be done is to get rid of and keep away the germs of the *Panhistophyton*. As might be imagined, from the course of his previous investigations, M. Pasteur was led to believe that the latter was the right theory; and,

guided by that theory, he has devised a method of extirpating the disease, which has proved to be completely successful wherever it has been properly carried out.

There can be no reason, then, for doubting that, among insects, contagious and infectious diseases, of great malignity, are caused by minute organisms which are produced from pre-existing germs, or by homogenesis; and there is no reason, that I know of, for believing that what happens in insects may not take place in the highest animals. Indeed, there is already strong evidence that some diseases of an extremely malignant and fatal character to which man is subject, are as much the work of minute organisms as is the Pébrine. I refer for this evidence to the very striking facts adduced by Professor Lister in his various well-known publications on the antiseptic method of treatment. It appears to me impossible to rise from the perusal of those publications without a strong conviction that the lamentable mortality which so frequently dogs the footsteps of the most skilful operator, and those deadly consequences of wounds and injuries which seem to haunt the very walls of great hospitals, and are, even now, destroying more men than die of bullet or bayonet, are due to the importation of minute organisms into wounds, and their increase and multiplication; and that the surgeon who saves most lives will be he who best works out the practical consequences of the hypothesis of Redi.

I commenced this Address by asking you to follow me in an attempt to trace the path which has been followed by a scientific idea, in its long and slow progress from the position of a probable hypothesis to that of an established law of nature. Our survey has not taken us into very attractive regions; it has lain, chiefly, in a land flowing with the abominable, and peopled with mere grubs and mouldiness. And it may be imagined with

what smiles and shrugs, practical and serious contemporaries of Redi and of Spallanzani may have commented on the waste of their high abilities in toiling at the solution of problems which, though curious enough in themselves, could be of no conceivable utility to mankind.

Nevertheless, you will have observed that before we had travelled very far upon our road, there appeared, on the right hand and on the left, fields laden with a harvest of golden grain, immediately convertible into those things which the most solidly practical men will admit to have value—viz., money and life.

The direct loss to France caused by the Pébrine in seventeen years cannot be estimated at less than fifty millions sterling; and if we add to this what Redi's idea, in Pasteur's hands, has done for the wine-grower and for the vinegar-maker, and try to capitalize its value, we shall find that it will go a long way towards repairing the money losses caused by the frightful and calamitous war of this autumn. And as to the equivalent of Redi's thought in life, how can we over-estimate the value of that knowledge of the nature of epidemic and epizootic diseases, and consequently of the means of checking, or eradicating, them, the dawn of which has assuredly commenced?

Looking back no further than ten years, it is possible to select three (1863, 1864, and 1869) in which the total number of deaths from scarlet-fever alone amounted to ninety thousand. That is the return of killed, the maimed and disabled being left out of sight. Why, it is to be hoped that the list of killed in the present bloodiest of all wars will not amount to more than this! But the facts which I have placed before you must leave the least sanguine without a doubt that the nature and the causes of this scourge will, one day, be as well

understood as those of the Pébrine are now; and that the long-suffered massacre of our innocents will come to an end.

And thus mankind will have one more admonition that "the people perish for lack of knowledge;" and that the alleviation of the miseries, and the promotion of the welfare, of men must be sought, by those who will not lose their pains, in that diligent, patient, loving study of all the multitudinous aspects of Nature, the results of which constitute exact knowledge, or Science. It is the justification and the glory of this great meeting that it is gathered together for no other object than the advancement of the moiety of science which deals with those phenomena of nature which we call physical. May its endeavours be crowned with a full measure of success!

XI.

MR. DARWIN'S CRITICS.[1]

THE gradual lapse of time has now separated us by more than a decade from the date of the publication of the "Origin of Species"—and whatever may be thought or said about Mr. Darwin's doctrines, or the manner in which he has propounded them, this much is certain, that, in a dozen years, the "Origin of Species" has worked as complete a revolution in biological science as the "Principia" did in astronomy—and it has done so, because, in the words of Helmholtz, it contains "an essentially new creative thought."[2]

And as time has slipped by, a happy change has come over Mr. Darwin's critics. The mixture of ignorance and insolence which, at first, characterized a large proportion of the attacks with which he was assailed, is no longer the sad distinction of anti-Darwinian criticism. Instead of abusive nonsense, which merely discredited its writers, we read essays, which are, at worst, more or less

[1] 1. "Contributions to the Theory of Natural Selection." By A. R. Wallace. 1870.—2. "The Genesis of Species." By St. George Mivart, F.R.S. Second Edition. 1871.—3. "Darwin's Descent of Man." *Quarterly Review*, July 1871.

[2] Helmholtz: "Ueber das Ziel und die Fortschritte der Naturwissenschaft." Eröffnungsrede für die Naturforscherversammlung zu Innsbruck. 1869.

intelligent and appreciative; while, sometimes, like that which appeared in the *North British Review* for 1867, they have a real and permanent value.

The several publications of Mr. Wallace and Mr. Mivart contain discussions of some of Mr. Darwin's views, which are worthy of particular attention, not only on account of the acknowledged scientific competence of these writers, but because they exhibit an attention to those philosophical questions which underlie all physical science, which is as rare as it is needful. And the same may be said of an article in the *Quarterly Review* for July 1871, the comparison of which with an article in the same Review for July 1860, is perhaps the best evidence which can be brought forward of the change which has taken place in public opinion on "Darwinism."

The Quarterly Reviewer admits "the certainty of the action of natural selection" (p. 49); and further allows that there is an *à priori* probability in favour of the evolution of man from some lower animal form, if these lower animal forms themselves have arisen by evolution.

Mr. Wallace and Mr. Mivart go much further than this. They are as stout believers in evolution as Mr. Darwin himself; but Mr. Wallace denies that man can have been evolved from a lower animal by that process of natural selection which he, with Mr. Darwin, holds to have been sufficient for the evolution of all animals below man; while Mr. Mivart, admitting that natural selection has been one of the conditions of the evolution of the animals below man, maintains that natural selection must, even in their case, have been supplemented by "some other cause"—of the nature of which, unfortunately, he does not give us any idea. Thus Mr. Mivart is less of a Darwinian than Mr. Wallace, for he has less faith in the power of natural selection. But he

is more of an evolutionist than Mr. Wallace, because Mr. Wallace thinks it necessary to call in an intelligent agent—a sort of supernatural Sir John Sebright—to produce even the animal frame of man; while Mr. Mivart requires no Divine assistance till he comes to man's soul.

Thus there is a considerable divergence between Mr. Wallace and Mr. Mivart. On the other hand, there are some curious similarities between Mr. Mivart and the Quarterly Reviewer, and these are sometimes so close, that, if Mr. Mivart thought it worth while, I think he might make out a good case of plagiarism against the Reviewer, who studiously abstains from quoting him.

Both the Reviewer and Mr. Mivart reproach Mr. Darwin with being, "like so many other physicists," entangled in a radically false metaphysical system, and with setting at nought the first principles of both philosophy and religion. Both enlarge upon the necessity of a sound philosophical basis, and both, I venture to add, make a conspicuous exhibition of its absence. The Quarterly Reviewer believes that man "differs more from an elephant or a gorilla than do these from the dust of the earth on which they tread," and Mr. Mivart has expressed the opinion that there is more difference between man and an ape than there is between an ape and a piece of granite.[1]

And even when Mr. Mivart (p. 86) trips in a matter of anatomy, and creates a difficulty for Mr. Darwin out of a supposed close similarity between the eyes of fishes and cephalopods, which (as Gegenbaur and others have clearly shown) does not exist, the Quarterly Reviewer adopts the argument without hesitation (p. 66).

There is another important point, however, in which it is hard to say whether Mr. Mivart diverges from the Quarterly Reviewer or not.

[1] See the *Tablet* for March 11, 1871.

The Reviewer declares that Mr. Darwin has, "with needless opposition, set at nought the first principles of both philosophy and religion" (p. 90).

It looks, at first, as if this meant, that Mr. Darwin's views being false, the opposition to "religion" which flows from them must be needless. But I suspect this is not the right view of the meaning of the passage, as Mr. Mivart, from whom the Quarterly Reviewer plainly draws so much inspiration, tells us that "the consequences which have been drawn from evolution, whether exclusively Darwinian or not, to the prejudice of religion, by no means follow from it, and are in fact illegitimate" (p. 5).

I may assume, then, that the Quarterly Reviewer and Mr. Mivart admit that there is no necessary opposition between "evolution, whether exclusively Darwinian or not," and religion. But then, what do they mean by this last much-abused term? On this point the Quarterly Reviewer is silent. Mr. Mivart, on the contrary, is perfectly explicit, and the whole tenor of his remarks leaves no doubt that by "religion" he means theology; and by theology, that particular variety of the great Proteus, which is expounded by the doctors of the Roman Catholic Church, and held by the members of that religious community to be the sole form of absolute truth and of saving faith.

According to Mr. Mivart, the greatest and most orthodox authorities upon matters of Catholic doctrine agree in distinctly asserting "derivative creation" or evolution; "and thus their teachings harmonize with all that modern science can possibly require" (p. 305).

I confess that this bold assertion interested me more than anything else in Mr. Mivart's book. What little knowledge I possessed of Catholic doctrine, and of the influence exerted by Catholic authority in former times,

had not led me to expect that modern science was likely to find a warm welcome within the pale of the greatest and most consistent of theological organizations.

And my astonishment reached its climax when I found Mr. Mivart citing Father Suarez as his chief witness in favour of the scientific freedom enjoyed by Catholics—the popular repute of that learned theologian and subtle casuist not being such as to make his works a likely place of refuge for liberality of thought. But in these days, when Judas Iscariot and Robespierre, Henry VIII. and Catiline, have all been shown to be men of admirable virtue, far in advance of their age, and consequently the victims of vulgar prejudice, it was obviously possible that Jesuit Suarez might be in like case. And, spurred by Mr. Mivart's unhesitating declaration, I hastened to acquaint myself with such of the works of the great Catholic divine as bore upon the question, hoping, not merely to acquaint myself with the true teachings of the infallible Church, and free myself of an unjust prejudice; but, haply, to enable myself, at a pinch, to put some Protestant bibliolater to shame, by the bright example of Catholic freedom from the trammels of verbal inspiration.

I regret to say that my anticipations have been cruelly disappointed. But the extent to which my hopes have been crushed can only be fully appreciated by citing, in the first place, those passages of Mr. Mivart's work by which they were excited. In his introductory chapter I find the following passages:—

"The prevalence of this theory [of evolution] need alarm no one, for it is, without any doubt, perfectly consistent with the strictest and most orthodox Christian[1] theology" (p. 5).

"Mr. Darwin and others may perhaps be excused if they

[1] It should be observed that Mr. Mivart employs the term "Christian" as if it were the equivalent of "Catholic."

have not devoted much time to the study of Christian philosophy; but they have no right to assume or accept without careful examination, as an unquestioned fact, that in that philosophy there is a necessary antagonism between the two ideas 'creation' and 'evolution,' as applied to organic forms.

"It is notorious and patent to all who choose to seek, that many distinguished Christian thinkers have accepted, and do accept, both ideas, *i.e.* both 'creation' and 'evolution.'

"As much as ten years ago an eminently Christian writer observed: 'The creationist theory does not necessitate the perpetual search after manifestations of miraculous power and perpetual "catastrophes." Creation is not a miraculous interference with the laws of nature, but the very institution of those laws. Law and regularity, not arbitrary intervention, was the patristic ideal of creation. With this notion they admitted, without difficulty, the most surprising origin of living creatures, provided it took place by *law*. They held that when God said, "Let the waters produce," "Let the earth produce," He conferred forces on the elements of earth and water, which enabled them naturally to produce the various species of organic beings. This power, they thought, remains attached to the elements throughout all time.' The same writer quotes St. Augustin and St. Thomas Aquinas, to the effect that, 'in the institution of nature, we do not look for miracles, but for the laws of nature.' And, again, St. Basil speaks of the continued operation of natural laws in the production of all organisms.

"So much for the writers of early and mediæval times. As to the present day, the author can confidently affirm that there are many as well versed in theology as Mr. Darwin is in his own department of natural knowledge,

who would not be disturbed by the thorough demonstration of his theory. Nay, they would not even be in the least painfully affected at witnessing the generation of animals of complex organization by the skilful artificial arrangement of natural forces, and the production, in the future, of a fish by means analogous to those by which we now produce urea.

"And this because they know that the possibility of such phenomena, though by no means actually foreseen, has yet been fully provided for in the old philosophy centuries before Darwin, or even centuries before Bacon, and that their place in the system can be at once assigned them without even disturbing its order or marring its harmony.

"Moreover, the old tradition in this respect has never been abandoned, however much it may have been ignored or neglected by some modern writers. In proof of this, it may be observed that perhaps no post-mediæval theologian has a wider reception amongst Christians throughout the world than Suarez, who has a separate section[1] in opposition to those who maintain the distinct creation of the various kinds—or substantial forms—of organic life" (pp. 19—21).

Still more distinctly does Mr. Mivart express himself, in the same sense, in his last chapter, entitled "Theology and Evolution" (pp. 302-5).

"It appears, then, that Christian thinkers are perfectly free to accept the general evolution theory. But are there any theological authorities to justify this view of the matter?

"Now, considering how extremely recent are these biological speculations, it might hardly be expected *à priori* that writers of earlier ages should have given expression to doctrines harmonizing in any degree with

[1] Suarez, Metaphysica. Edition Vivés. Paris, 1868, vol. i. Disput. xv. § 2.

such very modern views; nevertheless, this is certainly the case, and it would be easy to give numerous examples. It will be better, however, to cite one or two authorities of weight. Perhaps no writer of the earlier Christian ages could be quoted whose authority is more generally recognized than that of St. Augustin. The same may be said of the mediæval period for St. Thomas Aquinas: and since the movement of Luther, Suarez may be taken as an authority, widely venerated, and one whose orthodoxy has never been questioned.

"It must be borne in mind that for a considerable time even after the last of these writers no one had disputed the generally received belief as to the small age of the world, or at least of the kinds of animals and plants inhabiting it. It becomes, therefore, much more striking if views formed under such a condition of opinion are found to harmonize with modern ideas concerning 'Creation' and organic Life.

"Now St. Augustin insists in a very remarkable manner on the merely derivative sense in which God's creation of organic forms is to be understood; that is, that God created them by conferring on the material world the power to evolve them under suitable conditions."

Mr. Mivart then cites certain passages from St. Augustin, St. Thomas Aquinas, and Cornelius à Lapide, and finally adds:—

"As to Suarez, it will be enough to refer to Disp. xv. sec. 2, No. 9, p. 508, t. i. edition Vivés, Paris; also Nos. 13—15. Many other references to the same effect could easily be given, but these may suffice.

"It is then evident that ancient and most venerable theological authorities distinctly assert *derivative* creation, and thus their teachings harmonize with all that modern science can possibly require."

It will be observed that Mr. Mivart refers solely to

Suarez's fifteenth Disputation, though he adds, "Many other references to the same effect could easily be given." I shall look anxiously for these references in the third edition of the "Genesis of Species." For the present, all I can say is, that I have sought in vain, either in the fifteenth Disputation, or elsewhere, for any passage in Suarez's writings which, in the slightest degree, bears out Mr. Mivart's views as to his opinions.[1]

The title of this fifteenth Disputation is "De causa formali substantiali," and the second section of that Disputation (to which Mr. Mivart refers) is headed, "Quomodo possit forma substantialis fieri in materia et ex materia?"

The problem which Suarez discusses in this place may be popularly stated thus: According to the scholastic philosophy every natural body has two components—the one its "matter" (*materia prima*), the other its "substantial form" (*forma substantialis*). Of these the matter is everywhere the same, the matter of one body being indistinguishable from the matter of any other body. That which differentiates any one natural body from all others is its substantial form, which inheres in the matter of that body, as the human soul inheres in the matter of the frame of man, and is the source of all the activities and other properties of the body.

Thus, says Suarez, if water is heated, and the source of heat is then removed, it cools again. The reason of this is that there is a certain "*intimius principium*" in the water, which brings it back to the cool condition when the external impediment to the existence of that condition is removed. This *intimius principium* is the "substantial form" of the water. And the substantial

[1] The edition of Suarez's "Disputationes" from which the following citations are given, is Birckmann's, in two volumes folio, and is dated 1630.

form of the water is not only the cause (*radix*) of the coolness of the water, but also of its moisture, of its density, and of all its other properties.

It will thus be seen that "substantial forms" play nearly the same part in the scholastic philosophy as "forces" do in modern science; the general tendency of modern thought being to conceive all bodies as resolvable into material particles and forces, in virtue of which last these particles assume those dispositions and exercise those powers which are characteristic of each particular kind of matter.

But the Schoolmen distinguished two kinds of substantial forms, the one spiritual and the other material. The former division is represented by the human soul, the *anima rationalis;* and they affirm as a matter, not merely of reason, but of faith, that every human soul is created out of nothing, and by this act of creation is endowed with the power of existing for all eternity, apart from the *materia prima* of which the corporeal frame of man is composed. And the *anima rationalis*, once united with the *materia prima* of the body, becomes its substantial form, and is the source of all the powers and faculties of man—of all the vital and sensitive phenomena which he exhibits—just as the substantial form of water is the source of all its qualities.

The "material substantial forms" are those which inform all other natural bodies except that of man; and the object of Suarez in the present Disputation, is to show that the axiom "*ex nihilo nihil fit,*" though not true of the substantial form of man, is true of the substantial forms of all other bodies, the endless mutations of which constitute the ordinary course of nature. The origin of the difficulty which he discusses is easily comprehensible. Suppose a piece of bright iron to be exposed to the air. The existence of the iron depends

on the presence within it of a substantial form, which is the cause of its properties, *e.g.* brightness, hardness, weight. But, by degrees, the iron becomes converted into a mass of rust, which is dull, and soft, and light, and, in all other respects, is quite different from the iron. As, in the scholastic view, this difference is due to the rust being informed by a new substantial form, the grave problem arises, how did this new substantial form come into being? Has it been created? or has it arisen by the power of natural causation? If the former hypothesis is correct, then the axiom, "*ex nihilo nihil fit*," is false, even in relation to the ordinary course of nature, seeing that such mutations of matter as imply the continual origin of new substantial forms are occurring every moment. But the harmonization of Aristotle with theology was as dear to the Schoolmen, as the smoothing down the differences between Moses and science is to our Broad Churchmen, and they were proportionably unwilling to contradict one of Aristotle's fundamental propositions. Nor was their objection to flying in the face of the Stagirite likely to be lessened by the fact that such flight landed them in flat Pantheism.

So Father Suarez fights stoutly for the second hypothesis; and I quote the principal part of his argumentation as an exquisite specimen of that speech which is a "darkening of counsel."

"13. Secundo de omnibus aliis formis substantialibus [sc. materialibus] dicendum est non fieri proprie ex nihilo, sed ex potentia præjacentis materiæ educi: ideoque in effectione harum formarum nil fieri contra illud axioma, *Ex nihilo nihil fit*, si recte intelligatur. Hæc assertio sumitur ex Aristotele 1. Physicorum per totum et libro 7. Metaphyss. et ex aliis authoribus, quos statim referam. Et declaratur breviter, nam fieri ex nihilo duo dicit, unum est fieri absolute et simpliciter, aliud est quod talis effectio fit ex nihilo. Primum propriè dicitur de re subsistente, quia ejus est fieri, cujus est esse: id autem

proprie quod subsistit et habet esse; nam quod alteri adjacet, potius est quo aliud est. Ex hac ergo parte, formæ substantiales materiales non fiunt ex nihilo, quia proprie non fiunt. Atque hanc rationem reddit Divus Thomas 1 parte, quæstione 45, articulo 8, et quæstione 90, articulo 2, et ex dicendis magis explicabitur. Sumendo ergo ipsum *fieri* in hac proprietate et rigore, sic fieri ex nihilo est fieri secundum se totum, id est nulla sui parte præsupposita, ex quo fiat. Et hac ratione res naturales dum de novo fiunt, non fiunt ex nihilo, quia fiunt ex præsupposita materia, ex qua componuntur, et ita non fiunt, secundum se totæ, sed secundum aliquid sui. Formæ autem harum rerum, quamvis revera totam suam entitatem de novo accipiant, quam antea non habebant, quia vero ipsæ non fiunt, ut dictum est, ideo neque ex nihilo fiunt. Attamen, quia latiori modo sumendo verbum illud *fieri* negari non potest: quin forma facta sit, eo modo quo nunc est, et antea non erat, ut etiam probat ratio dubitandi posita in principio sectionis, ideo addendum est, sumpto *fieri* in hac amplitudine, fieri ex nihilo non tamen negare habitudinem materialis causæ intrinsecè componentis id quod fit, sed etiam habitudinem causæ materialis per se causantis et sustentantis formam quæ fit, seu confit. Diximus enim in superioribus materiam et esse causam compositi et formæ dependentis ab illa: ut res ergo dicatur ex nihilo fieri uterque modus causalitatis negari debet; et eodem sensu accipiendum est illud axioma, ut sit verum: *Ex nihilo nihil fit*, scilicet virtute agentis naturalis et finiti nihil fieri, nisi ex præsupposito subjecto per se concurrente, et ad compositum et ad formam, si utrumque suo modo ab eodem agente fiat. Ex his ergo rectè concluditur, formas substantiales materiales non fieri ex nihilo, quia fiunt ex materia, quæ in suo genere per se concurrit, et influit ad esse, et fieri talium formarum; quia, sicut esse non possunt nisi affixæ materiæ, a qua sustententur in esse: ita nec fieri possunt, nisi earum effectio et penetratio in eadem materia sustentetur. Et hæc est propria et per se differentia inter effectionem ex nihilo, et ex aliquo, propter quam, ut infra ostendemus, prior modus efficiendi superat vim finitam naturaliam agentium, non vero posterior.

"14. Ex his etiam constat, proprie de his formis dici non creari, sed educi de potentia materiæ."[1]

If I may venture to interpret these hard sayings, Suarez conceives that the evolution of substantial forms in the ordinary course of nature, is conditioned not only by the existence of the *materia prima*, but also by a certain "concurrence and influence" which that *materia*

[1] Suarez, *loc. cit.* Disput. xv. § ii.

exerts; and every new substantial form being thus conditioned, and in part, at any rate, caused, by a pre-existing something, cannot be said to be created out of nothing.

But as the whole tenor of the context shows, Suarez applies this argumentation merely to the evolution of material substantial forms in the ordinary course of nature. How the substantial forms of animals and plants primarily originated, is a question to which, so far as I am able to discover, he does not so much as allude in his "Metaphysical Disputations." Nor was there any necessity that he should do so, inasmuch as he has devoted a separate treatise of considerable bulk to the discussion of all the problems which arise out of the account of the Creation which is given in the Book of Genesis. And it is a matter of wonderment to me that Mr. Mivart, who somewhat sharply reproves "Mr. Darwin and others" for not acquainting themselves with the true teachings of his Church, should allow himself to be indebted to a heretic like myself for a knowledge of the existence of that "Tractatus de opere sex Dierum,"[1] in which the learned Father, of whom he justly speaks, as "an authority widely venerated, and whose orthodoxy has never been questioned," directly opposes all those opinions, for which Mr. Mivart claims the shelter of his authority.

In the tenth and eleventh chapters of the first book of this treatise, Suarez inquires in what sense the word "day," as employed in the first chapter of Genesis, is to be taken. He discusses the views of Philo and of Augustin on this question, and rejects them. He suggests that the approval of their allegorizing inter-

[1] "Tractatus de opere sex Dierum, seu de Universi Creatione, quatenus sex diebus perfecta esse, in libro Genesis cap. i. refertur, et præsertim de productione hominis in statu innocentiæ." Ed. Birckmann, 1622.

pretations by St. Thomas Aquinas, merely arose out of St. Thomas's modesty, and his desire not to seem openly to controvert St. Augustin—"voluisse Divus Thomas pro sua modestia subterfugere vim argumenti potius quam aperte Augustinum inconstantiæ arguere."

Finally, Suarez decides that the writer of Genesis meant that the term "day" should be taken in its natural sense; and he winds up the discussion with the very just and natural remark that "it is not probable that God, in inspiring Moses to write a history of the Creation which was to be believed by ordinary people, would have made him use language, the true meaning of which it is hard to discover, and still harder to believe." [1]

And in chapter xii. 3, Suarez further observes:—

"Ratio enim retinendi veram significationem diei naturalis est illa communis, quod verba Scripturæ non sunt ad metaphoras transferenda, nisi vel necessitas cogit, vel ex ipsa scriptura constet, et maximè in historica narratione et ad instructionem fidei pertinente: sed hæc ratio non minus cogit ad intelligendum propriè dierum numerum, quam diei qualitatem, QUIA NON MINUS UNO MODO QUAM ALIO DESTRUITUR SINCERITAS, IMO ET VERITAS HISTORIÆ. Secundo hoc valde confirmant alia Scripturæ loca, in quibus hi sex dies tanquam veri, et inter se distincti commemorantur, ut Exod. 20 dicitur, *Sex diebus operabis et facies omnia opera tua, septimo autem die Sabbatum Domini Dei tui est.* Et infra: *Sex enim diebus fecit Dominus cœlum et terram et mare et omnia quæ in eis sunt*, et idem repetitur in cap. 31. In quibus locis sermonis proprietas colligi potest tum ex æquiparatione, nam cum dicitur: *sex diebus operabis*, propriissimè intelligitur: tum quia non est verisimilé, potuisse populum intelligere verba illa in alio sensu, et è contrario incredibile est, Deum in suis præceptis tradendis illis verbis ad populum fuisse loquutum, quibus deciperetur, falsum sensum concipiendo, si Deus non per sex veros dies opera sua fecisset."

[1] "Propter hæc ergo sententia illa Augustini et propter nimiam obscuritatem et subtilitatem ejus difficilis creditu est: quia verisimile non est Deum inspirasse Moysi, ut historiam de creatione mundi ad fidem totius populi adeò necessariam per nomina dierum explicaret, quorum significatio vix inveniri et difficillime ab aliquo credi posset." (*Loc. cit.* Lib. I. cap. xi. 42.)

These passages leave no doubt that this great doctor of the Catholic Church, of unchallenged authority and unspotted orthodoxy, not only declares it to be Catholic doctrine that the work of creation took place in the space of six natural days; but that he warmly repudiates, as inconsistent with our knowledge of the Divine attributes, the supposition that the language which Catholic faith requires the believer to hold that God inspired, was used in any other sense than that which He knew it would convey to the minds of those to whom it was addressed.

And I think that in this repudiation Father Suarez will have the sympathy of every man of common uprightness, to whom it is certainly "incredible" that the Almighty should have acted in a manner which He would esteem dishonest and base in a man.

But the belief that the universe was created in six natural days is hopelessly inconsistent with the doctrine of evolution, in so far as it applies to the stars and planetary bodies; and it can be made to agree with a belief in the evolution of living beings only by the supposition that the plants and animals, which are said to have been created on the third, fifth, and six days, were merely the primordial forms, or rudiments, out of which existing plants and animals have been evolved; so that, on these days, plants and animals were not created actually, but only potentially.

The latter view is that held by Mr. Mivart, who follows St. Augustin, and implies that he has the sanction of Suarez. But, in point of fact, the latter great light of orthodoxy takes no small pains to give the most explicit and direct contradiction to all such imaginations, as the following passages prove. In the first place, as regards plants, Suarez discusses the problem:—

"*Quomodo herba virens et cætera vegetabilia hoc [tertio] die fuerint producta.*[1]

"Præcipua enim difficultas hîc est, quam attingit Div. Thomas 1, par. qu. 69, art. 2, an hæc productio plantarum hoc die facta intelligenda sit de productione ipsarum in proprio esse actuali et formali (ut sic rem explicerem) vel de productione tantum in semine et in potentia. Nam Divus Augustinus libro quinto Genes. ad liter. cap. 4 et 5 et libro 8, cap. 3, posteriorem partem tradit, dicens, terram in hoc die accepisse virtutem germinandi omnia vegetabilia quasi concepto omnium illorum semine, non tamen statim vegetabilia omnia produxisse. Quod primo suadet verbis illis capitis secundi. *In die quo fecit Deus cœlum et terram et omne virgultum agri priusquam germinaret.* Quomodo enim potuerunt virgulta fieri antequam terra germinaret nisi quia causaliter prius et quasi in radice, seu in semine facta sunt, et postea in actu producta? Secundo confirmari potest, quia verbum illud *germinet terra* optimè exponitur potestativè ut sic dicam, id est, accipiat terra vim germinandi. Sicut in eodem capite dicitur *crescite et multiplicamini.* Tertio potest confirmari, quia actualis productio vegetabilium non tam ad opus creationis, quam ad opus propagationis pertinet, quod postea factum est. Et hanc sententiam sequitur Eucherius lib. 1, in Gen. cap. 11, et illi faveat Glossa, interli. Hugo. et Lyran. dum verbum *germinet* dicto modo exponunt. NIHILOMINUS CONTRARIA SENTENTIA TENENDA EST: SCILICET, PRODUXISSE DEUM HOC DIE HERBAM, ARBORES, ET ALIA VEGETABILIA ACTU IN PROPRIA SPECIE ET NATURA. Hæc est communis sententia Patrum.—Basil. homil. 5; Exæmer. Ambros. lib. 3; Exæmer. cap. 8,11, et 16; Chrysost, homil. 5 in Gen. Damascene. lib. 2 de Fid. cap. 10; Theodor. Cyrilli. Bedæ, Glossæ ordinariæ et aliorum in Gen. Et idem sentit Divus Thomas, *supra*, solvens argumenta Augustini, quamvis propter reverentiam ejus quasi problematicè semper procedat. Denique idem sentiunt omnes qui in his operibus veram successionem et temporalem distinctionem agnoscant."

Secondly, with respect to animals, Suarez is no less decided:—

"*De animalium ratione carentium productione quinto et sexto die facta.*[1]

"32. Primo ergo nobis certum sit hæc animantia non in virtute tantum aut in semine, sed actu, et in seipsis, facta fuisse his diebus in quibus facta narrantur. Quanquam Augustinus lib. 3, Gen. ad liter. cap. 5 in sua persistens sententia contrarium sentire videatur."

[1] *Loc. cit.* Lib. II. cap. vii. et viii. 1, 32, 35.

But Suarez proceeds to refute Augustin's opinions at great length, and his final judgment may be gathered from the following passage :—

"35. Tertio dicendum est, hæc animalia omnia his diebus producta esse, IN PERFECTO STATU, IN SINGULIS INDIVIDUIS, SEU SPECIEBUS SUIS, JUXTA UNIUSCUJUSQUE NATURAM . . . . ITAQUE FUERUNT OMNIA CREATA INTEGRA ET OMNIBUS SUIS MEMBRIS PERFECTA."

As regards the creation of animals and plants, therefore, it is clear that Suarez, so far from "distinctly asserting derivative creation," denies it as distinctly and positively as he can; that he is at much pains to refute St. Augustin's opinions; that he does not hesitate to regard the faint acquiescence of St. Thomas Aquinas in the views of his brother saint as a kindly subterfuge on the part of Divus Thomas; and that he affirms his own view to be that which is supported by the authority of the Fathers of the Church. So that, when Mr. Mivart tells us that Catholic theology is in harmony with all that modern science can possibly require; that "to the general theory of evolution, and to the special Darwinian form of it, no exception . . . need be taken on the ground of orthodoxy;" and that "law and regularity, not arbitrary intervention, was the Patristic ideal of creation," we have to choose between his dictum, as a theologian, and that of a great light of his Church, whom he himself declares to be "widely venerated as an authority, and whose orthodoxy has never been questioned."

But Mr. Mivart does not hesitate to push his attempt to harmonize science with Catholic orthodoxy to its utmost limit; and, while assuming that the soul of man "arises from immediate and direct creation," he supposes that his body was "formed at first (as now in each separate individual) by derivative, or secondary creation, through natural laws" (p. 331).

This means, I presume, that an animal, having the corporeal form and bodily powers of man, may have been developed out of some lower form of life by a process of evolution; and that, after this anthropoid animal had existed for a longer or shorter time, God made a soul by direct creation, and put it into the manlike body, which, heretofore, had been devoid of that *anima rationalis*, which is supposed to be man's distinctive character.

This hypothesis is incapable of either proof or disproof, and therefore may be true; but if Suarez is any authority, it is not Catholic doctrine. "Nulla est in homine forma educta de potentia materiæ,"[1] is a dictum which is absolutely inconsistent with the doctrine of the natural evolution of any vital manifestation of the human body.

Moreover, if man existed as an animal before he was provided with a rational soul, he must, in accordance with the elementary requirements of the philosophy in which Mr. Mivart delights, have possessed a distinct sensitive and vegetative soul, or souls. Hence, when the "breath of life" was breathed into the manlike animal's nostrils, he must have already been a living and feeling creature. But Suarez particularly discusses this point, and not only rejects Mr. Mivart's view, but adopts language of very theological strength regarding it.

"Possent præterea his adjungi argumenta theologica, ut est illud quod sumitur ex illis verbis Genes. 2. *Formavit Deus hominem ex limo terræ et inspiravit in faciem ejus spiraculum vitæ et factus est homo in animam viventem:* ille enim spiritus, quam Deus spiravit, anima rationalis fuit, et PER EADEM FACTUS EST HOMO VIVENS, ET CONSEQUENTER, ETIAM SENTIENS.

"Aliud est ex VIII. Synodo Generali quæ est Constantinopolitana IV. can. 11, qui sic habet. *Apparet quosdam in tantum impietatis venisse ut homines duas animas habere dogmatizent: talis igitur impietatis inventores et similes sapientes, cum Vetus et Novum Testamentum*

[1] Disput. xv. § x. No. 27

omnesque Ecclesiæ patres unam animam rationalem hominem habere asseverent, Sancta et universalis Synodus anathematizat."[1]

Moreover, if the animal nature of man was the result of evolution, so must that of woman have been. But the Catholic doctrine, according to Suarez, is that woman was, in the strictest and most literal sense of the words, made out of the rib of man.

"Nihilominus sententia Catholica est, verba illa Scripturæ esse ad literam intelligenda. AC PROINDE VERE, AC REALITER, TULISSE DEUM COSTAM ADÆ, ET, EX ILLA, CORPUS EVÆ FORMASSE."[2]

Nor is there any escape in the supposition that some woman existed before Eve, after the fashion of the Lilith of the rabbis; since Suarez qualifies that notion, along with some other Judaic imaginations, as simply "damnabilis."[3]

After the perusal of the "Tractatus de Opere" it is, in fact, impossible to admit that Suarez held any opinion respecting the origin of species, except such as is consistent with the strictest and most literal interpretation of the words of Genesis. For Suarez, it is Catholic doctrine, that the world was made in six natural days. On the first of these days the *materia prima* was made out of nothing, to receive afterwards those "substantial forms" which moulded it into the universe of things; on the third day, the ancestors of all living plants suddenly came into being, full-grown, perfect, and possessed of all the properties which now distinguish them; while, on the fifth and sixth days, the ancestors of all existing animals were similarly caused to exist in their complete and perfect state, by the infusion of their appropriate material substantial forms into the matter which had already been created. Finally on the sixth day, the

[1] Disput. xv. "De causa formali substantiali," § x. No. 24.
[2] "Tractatus de Opere," Lib. III. "De hominis creatione," cap. ii. No. 3.
[3] Ibid. Lib. III. cap. iv. Nos. 8 and 9.

anima rationalis—that rational and immortal substantial form which is peculiar to man—was created out of nothing, and "breathed into" a mass of matter which, till then, was mere dust of the earth, and so man arose. But the species man was represented by a solitary male individual, until the Creator took out one of his ribs and fashioned it into a female.

This is the view of the "Genesis of Species," held by Suarez to be the only one consistent with Catholic faith: it is because he holds this view to be Catholic that he does not hesitate to declare St. Augustin unsound, and St. Thomas Aquinas guilty of weakness, when the one swerved from this view and the other tolerated the deviation. And, until responsible Catholic authority—say, for example, the Archbishop of Westminster—formally declares that Suarez was wrong, and that Catholic priests are free to teach their flocks that the world was *not* made in six natural days, and that plants and animals were *not* created in their perfect and complete state, but have been evolved by natural processes through long ages from certain germs in which they were potentially contained, I, for one, shall feel bound to believe that the doctrines of Suarez are the only ones which are sanctioned by Infallible Authority, as represented by the Holy Father and the Catholic Church.

I need hardly add that they are as absolutely denied and repudiated by Scientific Authority, as represented by Reason and Fact. The question whether the earth and the immediate progenitors of its present living population were made in six natural days or not, is no longer one upon which two opinions can be held.

The fact that it did not so come into being stands upon as sound a basis as any fact of history whatever. It is not true that existing plants and animals came into being within three days of the creation of the earth out

of nothing, for it is certain that innumerable generations of other plants and animals lived upon the earth before its present population. And when, Sunday after Sunday, men who profess to be our instructors in righteousness read out the statement, "In six days the Lord made heaven and earth, the sea, and all that in them is," in innumerable churches, they are either propagating what they may easily know, and, therefore, are bound to know, to be falsities; or, if they use the words in some non-natural sense, they fall below the moral standard of the much-abused Jesuit.

Thus far the contradiction between Catholic verity and Scientific verity is complete and absolute, quite independently of the truth or falsehood of the doctrine of evolution. But, for those who hold the doctrine of evolution, all the Catholic verities about the creation of living beings must be no less false. For them, the assertion that the progenitors of all existing plants were made on the third day, of animals on the fifth and sixth days, in the forms they now present, is simply false. Nor can they admit that man was made suddenly out of the dust of the earth; while it would be an insult to ask an evolutionist whether he credits the preposterous fable respecting the fabrication of woman to which Suarez pins his faith. If Suarez has rightly stated Catholic doctrine, then is evolution utter heresy. And such I believe it to be. In addition to the truth of the doctrine of evolution, indeed, one of its greatest merits in my eyes, is the fact that it occupies a position of complete and irreconcilable antagonism to that vigorous and consistent enemy of the highest intellectual, moral, and social life of mankind—the Catholic Church. No doubt, Mr. Mivart, like other putters of new wine into old bottles, is actuated by motives which are worthy of respect, and even of sympathy; but his

attempt has met with the fate which the Scripture prophesies for all such.

Catholic theology, like all theologies which are based upon the assumption of the truth of the account of the origin of things given in the Book of Genesis, being utterly irreconcilable with the doctrine of evolution, the student of science, who is satisfied that the evidence upon which the doctrine of evolution rests, is incomparably stronger and better than that upon which the supposed authority of the Book of Genesis rests, will not trouble himself further with these theologies, but will confine his attention to such arguments against the view he holds as are based upon purely scientific data—and by scientific data I do not merely mean the truths of physical, mathematical, or logical science, but those of moral and metaphysical science. For, by science, I understand all knowledge which rests upon evidence and reasoning of a like character to that which claims our assent to ordinary scientific propositions. And if any one is able to make good the assertion that his theology rests upon valid evidence and sound reasoning, then it appears to me that such theology will take its place as a part of science.

The present antagonism between theology and science does not arise from any assumption by the men of science that all theology must necessarily be excluded from science; but simply because they are unable to allow that reason and morality have two weights and two measures; and that the belief in a proposition, because authority tells you it is true, or because you wish to believe it, which is a high crime and misdemeanour when the subject matter of reasoning is of one kind, becomes under the *alias* of "faith" the greatest of all virtues, when the subject matter of reasoning is of another kind.

The Bishop of Brechin said well the other day:—"Liberality in religion—I do not mean tender and generous allowances for the mistakes of others—is only unfaithfulness to truth."[1] And, with the same qualification, I venture to paraphrase the Bishop's dictum: "Ecclesiasticism in science is only unfaithfulness to truth."

Elijah's great question, "Will you serve God or Baal? Choose ye," is uttered audibly enough in the ears of every one of us as we come to manhood. Let every man who tries to answer it seriously, ask himself whether he can be satisfied with the Baal of authority, and with all the good things his worshippers are promised in this world and the next. If he can, let him, if he be so inclined, amuse himself with such scientific implements as authority tells him are safe and will not cut his fingers; but let him not imagine he is, or can be, both a true son of the Church and a loyal soldier of science.

And, on the other hand, if the blind acceptance of authority appears to him in its true colours, as mere private judgment *in excelsis*, and if he have the courage to stand alone, face to face with the abyss of the Eternal and Unknowable, let him be content, once for all, not only to renounce the good things promised by "Infallibility," but even to bear the bad things which it prophesies; content to follow reason and fact in singleness and honesty of purpose, wherever they may lead, in the sure faith that a hell of honest men will, to him, be more endurable than a paradise full of angelic shams.

Mr. Mivart asserts that "without a belief in a personal God, there is no religion worthy of the name." This is a matter of opinion. But it may be asserted, with less reason to fear contradiction, that the worship of a

[1] Charge at the Diocesan Synod of Brechin. *Scotsman*, Sept. 14, 1871.

personal God, who, on Mr. Mivart's hypothesis, must have used language studiously calculated to deceive His creatures and worshippers, is "no religion worthy of the name." "Incredibile est, Deum illis verbis ad populum fuisse locutum quibus deciperetur," is a verdict in which, for once, Jesuit casuistry concurs with the healthy moral sense of all mankind.

Having happily got quit of the theological aspect of evolution, the supporter of that great truth who turns to the scientific objections which are brought against it by recent criticism, finds, to his relief, that the work before him is greatly lightened by the spontaneous retreat of the enemy from nine-tenths of the territory which he occupied ten years ago. Even the Quarterly Reviewer not only abstains from venturing to deny that evolution has taken place, but he openly admits that Mr. Darwin has forced on men's minds "a recognition of the probability, if not more, of evolution, and of the certainty of the action of natural selection" (p. 49).

I do not quite see, myself, how, if the action of natural selection is *certain*, the occurrence of evolution is only *probable;* inasmuch as the development of a new species by natural selection is, so far as it goes, evolution. However, it is not worth while to quarrel with the precise terms of a sentence which shows that the high watermark of intelligence among those most respectable of Britons, the readers of the *Quarterly Review*, has now reached such a level that the next tide may lift them easily and pleasantly on the once-dreaded shore of evolution. Nor, having got there, do they seem likely to stop, until they have reached the inmost heart of that great region, and accepted the ape ancestry of, at any rate, the body of man. For the Reviewer admits that Mr. Darwin can be said to have established:

"That if the various kinds of lower animals have been evolved one from the other by a process of natural generation or evolution, then it becomes highly probable, *d priori*, that man's body has been similarly evolved; but this, in such a case, becomes equally probable from the admitted fact that he is an animal at all" (p. 65).

From the principles laid down in the last sentence, it would follow that if man were constructed upon a plan as different from that of any other animal as that of a sea-urchin is from that of a whale, it would be "equally probable" that he had been developed from some other animal as it is now, when we know that for every bone, muscle, tooth, and even pattern of tooth, in man, there is a corresponding bone, muscle, tooth, and pattern of tooth, in an ape. And this shows one of two things—either that the Quarterly Reviewer's notions of probability are peculiar to himself; or, that he has such an overpowering faith in the truth of evolution, that no extent of structural break between one animal and another is sufficient to destroy his conviction that evolution has taken place.

But this by the way. The importance of the admission that there is nothing in man's physical structure to interfere with his having been evolved from an ape, is not lessened because it is grudgingly made and inconsistently qualified. And instead of jubilating over the extent of the enemy's retreat, it will be more worth while to lay siege to his last stronghold—the position that there is a distinction in kind between the mental faculties of man and those of brutes; and that, in consequence of this distinction in kind, no gradual progress from the mental faculties of the one to those of the other can have taken place.

The Quarterly Reviewer entrenches himself within formidable-looking psychological outworks, and there is no getting at him without attacking them one by one.

He begins by laying down the following proposition: "'Sensation' is not 'thought,' and no amount of the former would constitute the most rudimentary condition of the latter, though sensations supply the conditions for the existence of 'thought' or 'knowledge'" (p. 67).

This proposition is true, or not, according to the sense in which the word "thought" is employed. Thought is not uncommonly used in a sense co-extensive with consciousness, and, especially, with those states of consciousness we call memory. If I recall the impression made by a colour or an odour, and distinctly remember blueness or muskiness, I may say with perfect propriety that I "think of" blue or musk; and, so long as the thought lasts, it is simply a faint reproduction of the state of consciousness to which I gave the name in question, when it first became known to me as a sensation.

Now, if that faint reproduction of a sensation, which we call the memory of it, is properly termed a thought, it seems to me to be a somewhat forced proceeding to draw a hard and fast line of demarcation between thoughts and sensations. If sensations are not rudimentary thoughts, it may be said that some thoughts are rudimentary sensations. No amount of sound constitutes an echo, but for all that no one would pretend that an echo is something of totally different nature from a sound. Again, nothing can be looser, or more inaccurate, than the assertion that "sensations supply the conditions for the existence of thought or knowledge." If this implies that sensations supply the conditions for the existence of our memory of sensations or of our thoughts about sensations, it is a truism which it is hardly worth while to state so solemnly. If it implies that sensations supply anything else, it is

obviously erroneous. And if it means, as the context would seem to show it does, that sensations are the subject-matter of all thought or knowledge, then it is no less contrary to fact, inasmuch as our emotions, which constitute a large part of the subject-matter of thought or of knowledge, are not sensations.

More eccentric still is the Quarterly Reviewer's next piece of psychology.

"Altogether, we may clearly distinguish at least six kinds of action to which the nervous system ministers :—

"I. That in which impressions received result in appropriate movements without the intervention of sensation or thought, as in the cases of injury above given.—This is the reflex action of the nervous system.

"II. That in which stimuli from without result in sensations through the agency of which their due effects are wrought out. —Sensation.

"III. That in which impressions received result in sensations which give rise to the observation of sensible objects.—Sensible perception.

"IV. That in which sensations and perceptions continue to coalesce, agglutinate, and combine in more or less complex aggregations, according to the laws of the association of sensible perceptions.—Association.

"The above four groups contain only indeliberate operations, consisting, as they do at the best, but of mere *presentative* sensible ideas in no way implying any reflective or *representative* faculty. Such actions minister to and form *Instinct.* Besides these, we may distinguish two other kinds of mental action, namely :—

"V. That in which sensations and sensible perceptions are reflected on by thought, and recognized as our own, and we ourselves recognized by ourselves as affected and perceiving.—Self-consciousness.

"VI. That in which we reflect upon our sensations or perceptions, and ask what they are, and why they are.—Reason.

"These two latter kinds of action are deliberate operations, performed, as they are, by means of representative ideas implying the use of a *reflective representative* faculty. Such actions distinguish the *intellect* or rational faculty. Now, we assert that possession in perfection of all the first four (*presentative*) kinds of action by no means implies the possession of the last two (*representative*) kinds. All persons, we think, must admit the truth of the following proposition :—

"Two faculties are distinct, not in degree but *in kind*, if we may possess the one in perfection without that fact implying that we possess the other also. Still more will this be the case if the two faculties tend to increase in an inverse ratio. Yet this is the distinction between the *instinctive* and the *intellectual* parts of man's nature.

"As to animals, we fully admit that they may possess all the first four groups of actions—that they may have, so to speak, mental images of sensible objects combined in all degrees of complexity, as governed by the laws of association. We deny to them, on the other hand, the possession of the last two kinds of mental action. We deny them, that is, the power of reflecting on their own existence, or of inquiring into the nature of objects and their causes. We deny that they know that they know or know themselves in knowing. In other words, we deny them *reason*. The possession of the presentative faculty, as above explained, in no way implies that of the reflective faculty; nor does any amount of direct operation imply the power of asking the reflective question before mentioned, as to 'what' and 'why.'" (*Loc. cit.* pp. 67, 68.)

Sundry points are worthy of notice in this remarkable account of the intellectual powers. In the first place the Reviewer ignores emotion and volition, though they are no inconsiderable "kinds of action to which the nervous system ministers," and memory has a place in his classification only by implication. Secondly, we are told that the second "kind of action to which the nervous system ministers" is "that in which stimuli from without result in sensations through the agency of which their due effects are wrought out.—Sensation." Does this really mean that, in the writer's opinion, "sensation" is the "agent" by which the "due effect" of the stimulus, which gives rise to sensation, is "wrought out"? Suppose somebody runs a pin into me. The "due effect" of that particular stimulus will probably be threefold; namely, a sensation of pain, a start, and an interjectional expletive. Does the Quarterly Reviewer really think that the "sensation" is the "agent" by which the other two phenomena are wrought out?

But these matters are of little moment to anyone but the Reviewer and those persons who may incautiously take their physiology, or psychology, from him. The really interesting point is this, that when he fully admits that animals "may possess all the first four groups of actions," he grants all that is necessary for the purposes of the evolutionist. For he hereby admits that in animals "impressions received result in sensations which give rise to the observation of sensible objects," and that they have what he calls "sensible perception." Nor was it possible to help the admission; for we have as much reason to ascribe to animals, as we have to attribute to our fellow-men, the power, not only of perceiving external objects as external, and thus practically recognizing the difference between the self and the not-self; but that of distinguishing between like and unlike, and between simultaneous and successive things. When a gamekeeper goes out coursing with a greyhound in leash, and a hare crosses the field of vision, he becomes the subject of those states of consciousness we call visual sensation, and that is all he receives from without. Sensation, as such, tells him nothing whatever about the cause of these states of consciousness; but the thinking faculty instantly goes to work upon the raw material of sensation furnished to it through the eye, and gives rise to a train of thoughts. First comes the thought that there is an object at a certain distance; then arises another thought—the perception of the likeness between the states of consciousness awakened by this object to those presented by memory, as, on some former occasion, called up by a hare; this is succeeded by another thought of the nature of an emotion—namely, the desire to possess the hare; then follows a longer or shorter train of other thoughts, which end in a volition and an act—the loosing of the greyhound from the leash. These several

thoughts are the concomitants of a process which goes on in the nervous system of the man. Unless the nerve-elements of the retina, of the optic nerve, of the brain, of the spinal chord, and of the nerves of the arms went through certain physical changes in due order and correlation, the various states of consciousness which have been enumerated would not make their appearance. So that in this, as in all other intellectual operations, we have to distinguish two sets of successive changes—one in the physical basis of consciousness, and the other in consciousness itself; one set which may, and doubtless will, in course of time, be followed through all their complexities by the anatomist and the physicist, and one of which only the man himself can have immediate knowledge.

As it is very necessary to keep up a clear distinction between these two processes, let the one be called *neurosis*, and the other *psychosis*. When the gamekeeper was first trained to his work, every step in the process of neurosis was accompanied by a corresponding step in that of psychosis, or nearly so. He was conscious of seeing something, conscious of making sure it was a hare, conscious of desiring to catch it, and therefore to loose the greyhound at the right time, conscious of the acts by which he let the dog out of the leash. But with practice, though the various steps of the neurosis remain—for otherwise the impression on the retina would not result in the loosing of the dog—the great majority of the steps of the psychosis vanish, and the loosing of the dog follows unconsciously, or as we say, without thinking about it, upon the sight of the hare. No one will deny that the series of acts which originally intervened between the sensation and the letting go of the dog were, in the strictest sense, intellectual and rational operations. Do they cease to be so

when the man ceases to be conscious of them? That depends upon what is the essence and what the accident of those operations, which, taken together, constitute ratiocination.

Now ratiocination is resolvable into predication, and predication consists in marking, in some way, the existence, the coexistence, the succession, the likeness and unlikeness, of things or their ideas. Whatever does this, reasons; and if a machine produces the effects of reason, I see no more ground for denying to it the reasoning power, because it is unconscious, than I see for refusing to Mr. Babbage's engine the title of a calculating machine on the same grounds.

Thus it seems to me that a gamekeeper reasons, whether he is conscious or unconscious, whether his reasoning is carried on by neurosis alone, or whether it involves more or less psychosis. And if this is true of the gamekeeper, it is also true of the greyhound. The essential resemblances in all points of structure and function, so far as they can be studied, between the nervous system of the man and that of the dog, leave no reasonable doubt that the processes which go on in the one are just like those which take place in the other. In the dog, there can be no doubt that the nervous matter which lies between the retina and the muscles undergoes a series of changes, precisely analogous to those which, in the man, give rise to sensation, a train of thought, and volition.

Whether this neurosis is accompanied by such psychosis as ours, it is impossible to say; but those who deny that the nervous changes, which, in the dog, correspond with those which underlie thought in a man, are accompanied by consciousness, are equally bound to maintain that those nervous changes in the dog, which correspond with those which underlie sensation in a man, are also

unaccompanied by consciousness. In other words, if there is no ground for believing that a dog thinks, neither is there any for believing that he feels.

As is well known, Descartes boldly faced this dilemma, and maintained that all animals were mere machines and entirely devoid of consciousness. But he did not deny, nor can anyone deny, that in this case they are reasoning machines, capable of performing all those operations which are performed by the nervous system of man when he reasons. For even supposing that in man, and in man only, psychosis is superadded to neurosis —the neurosis which is common to both man and animal gives their reasoning processes a fundamental unity. But Descartes's position is open to very serious objections, if the evidence that animals feel is insufficient to prove that they really do so. What is the value of the evidence which leads one to believe that one's fellow-man feels? The only evidence in this argument of analogy, is the similarity of his structure and of his actions to one's own. And if that is good enough to prove that one's fellow-man feels, surely it is good enough to prove that an ape feels. For the differences of structure and function between men and apes are utterly insufficient to warrant the assumption, that while men have those states of consciousness we call sensations, apes have nothing of the kind. Moreover, we have as good evidence that apes are capable of emotion and volition as we have that men other than ourselves are. But if apes possess three out of the four kinds of states of consciousness which we discover in ourselves, what possible reason is there for denying them the fourth? If they are capable of sensation, emotion, and volition, why are they to be denied thought (in the sense of predication)?

No answer has ever been given to these questions.

And as the law of continuity is as much opposed, as is the common sense of mankind, to the notion that all animals are unconscious machines, it may safely be assumed that no sufficient answer ever will be given to them.

There is every reason to believe that consciousness is a function of nervous matter, when that nervous matter has attained a certain degree of organization, just as we know the other "actions to which the nervous system ministers," such as reflex action and the like, to be. As I have ventured to state my view of the matter elsewhere, "our thoughts are the expression of molecular changes in that matter of life which is the source of our other vital phenomena."

Mr. Wallace objects to this statement in the following terms:—

"Not having been able to find any clue in Professor Huxley's writings to the steps by which he passes from those vital phenomena, which consist only, in their last analysis, of movements by particles of matter, to those other phenomena which we term thought, sensation, or consciousness; but, knowing that so positive an expression of opinion from him will have great weight with many persons, I shall endeavour to show, with as much brevity as is compatible with clearness, that this theory is not only incapable of proof, but is also, as it appears to me, inconsistent with accurate conceptions of molecular physics."

With all respect for Mr. Wallace, it appears to me that his remarks are entirely beside the question. I really know nothing whatever, and never hope to know anything, of the steps by which the passage from molecular movement to states of consciousness is effected; and I entirely agree with the sense of the passage which he quotes from Professor Tyndall, apparently imagining that it is in opposition to the view I hold.

All that I have to say is, that, in my belief, consciousness and molecular action are capable of being expressed by one another, just as heat and mechanical action are

capable of being expressed in terms of one another. Whether we shall ever be able to express consciousness in foot-pounds, or not, is more than I will venture to say; but that there is evidence of the existence of some correlation between mechanical motion and consciousness, is as plain as anything can be. Suppose the poles of an electric battery to be connected by a platinum wire. A certain intensity of the current gives rise in the mind of a bystander to that state of consciousness we call a "dull red light"—a little greater intensity to another which we call a "bright red light;" increase the intensity, and the light becomes white; and, finally, it dazzles, and a new state of consciousness arises, which we term pain. Given the same wire and the same nervous apparatus, and the amount of electric force required to give rise to these several states of consciousness will be the same, however often the experiment is repeated. And as the electric force, the light-waves, and the nerve-vibrations caused by the impact of the light-waves on the retina, are all expressions of the molecular changes which are taking place in the elements of the battery; so consciousness is, in the same sense, an expression of the molecular changes which take place in that nervous matter, which is the organ of consciousness.

And, since this, and any number of similar examples that may be required, prove that one form of consciousness, at any rate, is, in the strictest sense, the expression of molecular change, it really is not worth while to pursue the inquiry, whether a fact so easily established is consistent with any particular system of molecular physics or not.

Mr. Wallace, in fact, appears to me to have mixed up two very distinct propositions: the one, the indisputable truth that consciousness is correlated with molecular

changes in the organ of consciousness; the other, that the nature of that correlation is known, or can be conceived, which is quite another matter. Mr. Wallace, presumably, believes in that correlation of phenomena which we call cause and effect as firmly as I do. But if he has ever been able to form the faintest notion how a cause gives rise to its effect, all I can say is that I envy him. Take the simplest case imaginable—suppose a ball in motion to impinge upon another ball at rest. I know very well, as a matter of fact, that the ball in motion will communicate some of its motion to the ball at rest, and that the motion of the two balls after collision is precisely correlated with the masses of both balls and the amount of motion of the first. But how does this come about? In what manner can we conceive that the *vis viva* of the first ball passes into the second? I confess I can no more form any conception of what happens in this case, than I can of what takes place when the motion of particles of my nervous matter, caused by the impact of a similar ball, gives rise to the state of consciousness I call pain. In ultimate analysis everything is incomprehensible, and the whole object of science is simply to reduce the fundamental incomprehensibilities to the smallest possible number.

But to return to the Quarterly Reviewer. He admits that animals have "mental images of sensible objects, combined in all degrees of complexity, as governed by the laws of association." Presumably, by this confused and imperfect statement the Reviewer means to admit more than the words imply. For mental images of sensible objects, even though "combined in all degrees of complexity," are, and can be, nothing more than mental images of sensible objects. But judgments, emotions, and volitions cannot by any possibility be included under the head of "mental images of sensible objects."

If the greyhound had no better mental endowment than the Reviewer allows him, he might have the "mental image" of the "sensible object"—the hare—and that might be combined with the mental images of other sensible objects, to any degree of complexity, but he would have no power of judging it to be at a certain distance from him; no power of perceiving its similarity to his memory of a hare; and no desire to get at it. Consequently he would stand stock still, and the noble art of coursing would have no existence. On the other hand, as that art is largely practised, it follows that greyhounds alone possess a number of mental powers, the existence of which, in any animal, is absolutely denied by the Quarterly Reviewer.

Finally, what are the mental powers which he reserves as the especial prerogative of man? They are two. First, the recognition of "ourselves by ourselves as affected and perceiving.—Self-consciousness."

Secondly. "The reflection upon our sensations and perceptions, and asking what they are and why they are.—Reason."

To the faculty defined in the last sentence, the Reviewer, without assigning the least ground for thus departing from both common usage and technical propriety, applies the name of reason. But if man is not to be considered a reasoning being, unless he asks what his sensations and perceptions are, and why they are, what is a Hottentot, or an Australian black fellow; or what the "swinked hedger" of an ordinary agricultural district? Nay, what becomes of an average country squire or parson? How many of these worthy persons who, as their wont is, read the *Quarterly Review*, would do other than stand agape, if you asked them whether they had ever reflected what their sensations and perceptions are, and why they are?

So that if the Reviewer's new definition of reason be correct, the majority of men, even among the most civilized nations, are devoid of that supreme characteristic of manhood. And if it be as absurd as I believe it to be, then, as reason is certainly not self-consciousness, and as it, as certainly, is one of the "actions to which the nervous system ministers," we must, if the Reviewer's classification is to be adopted, seek it among those four faculties which he allows animals to possess. And thus, for the second time, he really surrenders, while seeming to defend, his position.

The Quarterly Reviewer, as we have seen, lectures the evolutionists upon their want of knowledge of philosophy altogether. Mr. Mivart is not less pained at Mr. Darwin's ignorance of moral science. It is grievous to him that Mr. Darwin (and *nous autres*) should not have grasped the elementary distinction between material and formal morality; and he lays down as an axiom, of which no tyro ought to be ignorant, the position that "acts, unaccompanied by mental acts of conscious will directed towards the fulfilment of duty," are "absolutely destitute of the most incipient degree of real or formal goodness."

Now this may be Mr. Mivart's opinion, but it is a proposition which, really, does not stand on the footing of an undisputed axiom. Mr. Mill denies it in his work on Utilitarianism. The most influential writer of a totally opposed school, Mr. Carlyle, is never weary of denying it, and upholding the merit of that virtue which is unconscious; nay, it is, to my understanding, extremely hard to reconcile Mr. Mivart's dictum with that noble summary of the whole duty of man—"Thou shalt love the Lord thy God with all thy heart, and with all thy soul, and with all thy strength; and thou shalt love thy neighbour as thyself." According to

Mr. Mivart's definition, the man who loves God and his neighbour, and, out of sheer love and affection for both, does all he can to please them, is, nevertheless, destitute of a particle of real goodness.

And it further happens that Mr. Darwin, who is charged by Mr. Mivart with being ignorant of the distinction between material and formal goodness, discusses the very question at issue, in a passage which is well worth reading (vol. i. p. 87), and also comes to a conclusion opposed to Mr. Mivart's axiom. A proposition which has been so much disputed and repudiated, should, under no circumstances, have been thus confidently assumed to be true. For myself, I utterly reject it, inasmuch as the logical consequence of the adoption of any such principle is the denial of all moral value to sympathy and affection. According to Mr. Mivart's axiom, the man who, seeing another struggling in the water, leaps in at the risk of his own life to save him, does that which is "destitute of the most incipient degree of real goodness," unless, as he strips off his coat, he says to himself, "Now mind, I am going to do this because it is my duty and for no other reason;" and the most beautiful character to which humanity can attain, that of the man who does good without thinking about it, because he loves justice and mercy and is repelled by evil, has no claim on our moral approbation. The denial that a man acts morally because he does not think whether he does so or not, may be put upon the same footing as the denial of the title of an arithmetician to the calculating boy, because he did not know how he worked his sums. If mankind ever generally accept and act upon Mr. Mivart's axiom, they will simply become a set of most unendurable prigs; but they never have accepted it, and I venture to hope that evolution has nothing so terrible in store for the human race.

But, if an action, the motive of which is nothing but affection or sympathy, may be deserving of moral approbation and really good, who that has ever had a dog of his own will deny that animals are capable of such actions? Mr. Mivart indeed says:—"It may be safely affirmed, however, that there is no trace in brutes of any actions simulating morality which are not explicable by the fear of punishment, by the hope of pleasure, or by personal affection" (p. 221). But it may be affirmed, with equal truth, that there is no trace in men of any actions which are not traceable to the same motives. If a man does anything, he does it either because he fears to be punished if he does not do it, or because he hopes to obtain pleasure by doing it, or because he gratifies his affections[1] by doing it.

Assuming the position of the absolute moralists, let it be granted that there is a perception of right and wrong innate in every man. This means, simply, that when certain ideas are presented to his mind, the feeling of approbation arises; and when certain others, the feeling of disapprobation. To do your duty is to earn the approbation of your conscience, or moral sense; to fail in your duty is to feel its disapprobation, as we all say. Now, is approbation a pleasure or a pain? Surely a pleasure. And is disapprobation a pleasure or a pain? Surely a pain. Consequently all that is really meant by the absolute moralists is that there is, in the very nature of man, something which enables him to be conscious of these particular pleasures and pains. And when they talk of immutable and eternal principles of morality, the only intelligible sense which I can put upon the words, is that the nature of man being what it is, he always has been, and always will be, capable of feeling these particular

[1] In separating pleasure and the gratification of affection, I simply follow Mr. Mivart without admitting the justice of the separation.

pleasures and pains. *A priori*, I have nothing to say against this proposition. Admitting its truth, I do not see how the moral faculty is on a different footing from any of the other faculties of man. If I choose to say that it is an immutable and eternal law of human nature that "ginger is hot in the mouth," the assertion has as much foundation of truth as the other, though I think it would be expressed in needlessly pompous language. I must confess that I have never been able to understand why there should be such a bitter quarrel between the intuitionists and the utilitarians. The intuitionist is, after all, only a utilitarian who believes that a particular class of pleasures and pains has an especial importance, by reason of its foundation in the nature of man, and its inseparable connection with his very existence as a thinking being. And as regards the motive of personal affection: Love, as Spinoza profoundly says, is the association of pleasure with that which is loved.[1] Or, to put it to the common sense of mankind, is the gratification of affection a pleasure or a pain? Surely a pleasure. So that whether the motive which leads us to perform an action is the love of our neighbour, or the love of God, it is undeniable that pleasure enters into that motive.

Thus much in reply to Mr. Mivart's arguments. I cannot but think that it is to be regretted that he ekes them out by ascribing to the doctrines of the philosophers with whom he does not agree, logical consequences which have been over and over again proved not to flow from them: and when reason fails him, tries the effect of an injurious nickname. According to the views of Mr. Spencer, Mr. Mill, and Mr. Darwin, Mr. Mivart tells us, "*virtue is a mere kind of retrieving:*" and, that we may not miss the point of the joke, he

[1] "Nempe, Amor nihil aliud est, quam Lætitia, concomitante idea causæ externæ."—*Ethices*, III. xiii.

puts it in italics. But what if it is? Does that make it less virtue? Suppose I say that sculpture is a "mere way" of stone-cutting, and painting a "mere way" of daubing canvas, and music a "mere way" of making a noise, the statements are quite true; but they only show that I see no other method of depreciating some of the noblest aspects of humanity, than that of using language in an inadequate and misleading sense about them. And the peculiar inappropriateness of this particular nickname to the views in question, arises from the circumstance which Mr. Mivart would doubtless have recollected, if his wish to ridicule had not for the moment obscured his judgment—that whether the law of evolution applies to man or not, that of hereditary transmission certainly does. Mr. Mivart will hardly deny that a man owes a large share of the moral tendencies which he exhibits to his ancestors; and the man who inherits a desire to steal from a kleptomaniac, or a tendency to benevolence from a Howard, is, so far as he illustrates hereditary transmission, comparable to the dog who inherits the desire to fetch a duck out of the water from his retrieving sire. So that, evolution, or no evolution, moral qualities are comparable to a "kind of retrieving;" though the comparison, if meant for the purposes of casting obloquy on evolution, does not say much for the fairness of those who make it.

The Quarterly Reviewer and Mr. Mivart base their objections to the evolution of the mental faculties of man from those of some lower animal form, upon what they maintain to be a difference in kind between the mental and moral faculties of men and brutes; and I have endeavoured to show, by exposing the utter unsoundness of their philosophical basis, that these objections are devoid of importance.

The objections which Mr. Wallace brings forward to

the doctrine of the evolution of the mental faculties of man from those of brutes by natural causes, are of a different order, and require separate consideration.

If I understand him rightly, he by no means doubts that both the bodily and the mental faculties of man have been evolved from those of some lower animal; but he is of opinion, that some agency beyond that which has been concerned in the evolution of ordinary animals, has been operative in the case of man. "A superior intelligence has guided the development of man in a definite direction and for a special purpose, just as man guides the development of many animal and vegetable forms."[1] I understand this to mean that, just as the rock-pigeon has been produced by natural causes, while the evolution of the tumbler from the blue rock has required the special intervention of the intelligence of man, so some anthropoid form may have been evolved by variation and natural selection; but it could never have given rise to man, unless some superior intelligence had played the part of the pigeon-fancier.

According to Mr. Wallace, "whether we compare the savage with the higher developments of man, or with the brutes around him, we are alike driven to the conclusion, that, in his large and well-developed brain, he possesses an organ quite disproportioned to his requirements" (p. 343); and he asks, "What is there in the life of the savage but the satisfying of the cravings of appetite in the simplest and easiest way? What thoughts, idea, or actions are there that raise him many grades above the elephant or the ape?" (p. 342). I answer Mr. Wallace by citing a remarkable passage which occurs in his instructive paper on "Instinct in Man and Animals."

[1] "The limits of Natural Selection as applied to Man" (*loc. cit.* p. 359).

"Savages make long journeys in many directions, and, their whole faculties being directed to the subject, they gain a wide and accurate knowledge of the topography, not only of their own district, but of all the regions round about. Everyone who has travelled in a new direction communicates his knowledge to those who have travelled less, and descriptions of routes and localities, and minute incidents of travel, form one of the main staples of conversation around the evening fire. Every wanderer or captive from another tribe adds to the store of information, and, as the very existence of individuals and of whole families and tribes depends upon the completeness of this knowledge, all the acute perceptive faculties of the adult savage are directed to acquiring and perfecting it. The good hunter or warrior thus comes to know the bearing of every hill and mountain range, the directions and junctions of all the streams, the situation of each tract characterized by peculiar vegetation, not only within the area he has himself traversed, but perhaps for a hundred miles around it. His acute observation enables him to detect the slightest undulations of the surface, the various changes of subsoil and alterations in the character of the vegetation that would be quite imperceptible to a stranger. His eye is always open to the direction in which he is going; the mossy side of trees, the presence of certain plants under the shade of rocks, the morning and evening flight of birds, are to him indications of direction almost as sure as the sun in the heavens" (pp. 207-8).

I have seen enough of savages to be able to declare that nothing can be more admirable than this description of what a savage has to learn. But it is incomplete. Add to all this the knowledge which a savage is obliged to gain of the properties of plants, of the characters and habits of animals, and of the minute indications by which their course is discoverable: consider that even an Australian can make excellent baskets and nets, and neatly fitted and beautifully balanced spears; that he learns to use these so as to be able to transfix a quartern loaf at sixty yards; and that very often, as in the case of the American Indians, the language of a savage exhibits complexities which a well-trained European finds it difficult to master: consider that every time a savage tracks his game, he employs a minuteness of observation, and an accuracy of inductive and deductive

reasoning which, applied to other matters, would assure some reputation to a man of science, and I think we need ask no further why he possesses such a fair supply of brains. In complexity and difficulty, I should say that the intellectual labour of a "good hunter or warrior" considerably exceeds that of an ordinary Englishman. The Civil Service Examiners are held in great terror by young Englishmen; but even their ferocity never tempted them to require a candidate to possess such a knowledge of a parish, as Mr. Wallace justly points out savages may possess of an area a hundred miles, or more, in diameter.

But suppose, for the sake of argument, that a savage has more brains than seems proportioned to his wants, all that can be said is that the objection to natural selection, if it be one, applies quite as strongly to the lower animals. The brain of a porpoise is quite wonderful for its mass, and for the development of the cerebral convolutions. And yet since we have ceased to credit the story of Arion, it is hard to believe that porpoises are much troubled with intellect: and still more difficult is it to imagine that their big brains are only a preparation for the advent of some accomplished cetacean of the future. Surely, again, a wolf must have too much brains, or else how is it that a dog, with only the same quantity and form of brain, is able to develop such singular intelligence? The wolf stands to the dog in the same relation as the savage to the man; and, therefore, if Mr. Wallace's doctrine holds good, a higher power must have superintended the breeding up of wolves from some inferior stock, in order to prepare them to become dogs.

Mr. Wallace further maintains that the origin of some of man's mental faculties by the preservation of useful variations is not possible. Such, for example, are "the capacity to form ideal conceptions of space and time, of

eternity and infinity; the capacity for intense artistic feelings of pleasure in form, colour, and composition; and for those abstract notions of form and number which render geometry and arithmetic possible." "How," he asks, "were all or any of these faculties first developed, when they could have been of no possible use to man in his early stages of barbarism?"

Surely the answer is not far to seek. The lowest savages are as devoid of any such conceptions as the brutes themselves. What sort of conceptions of space and time, of form and number, can be possessed by a savage who has not got so far as to be able to count beyond five or six, who does not know how to draw a triangle or a circle, and has not the remotest notion of separating the particular quality we call form, from the other qualities of bodies? None of these capacities are exhibited by men, unless they form part of a tolerably advanced society. And, in such a society, there are abundant conditions by which a selective influence is exerted in favour of those persons who exhibit an approximation towards the possession of these capacities.

The savage who can amuse his fellows by telling a good story over the nightly fire, is held by them in esteem and rewarded, in one way or another, for so doing—in other words, it is an advantage to him to possess this power. He who can carve a paddle, or the figure-head of a canoe better, similarly profits beyond his duller neighbour. He who counts a little better than others, gets most yams when barter is going on, and forms the shrewdest estimate of the numbers of an opposing tribe. The experience of daily life shows that the conditions of our present social existence exercise the most extraordinarily powerful selective influence in favour of novelists, artists, and strong intellects of all kinds; and it seems unquestionable that all forms of

social existence must have had the same tendency, if we consider the indisputable facts that even animals possess the power of distinguishing form and number, and that they are capable of deriving pleasure from particular forms and sounds. If we admit, as Mr. Wallace does, that the lowest savages are not raised "many grades above the elephant and the ape;" and if we further admit, as I contend must be admitted, that the conditions of social life tend, powerfully, to give an advantage to those individuals who vary in the direction of intellectual or æsthetic excellence, what is there to interfere with the belief that these higher faculties, like the rest, owe their development to natural selection?

Finally, with respect to the development of the moral sense out of the simple feelings of pleasure and pain, liking and disliking, with which the lower animals are provided, I can find nothing in Mr. Wallace's reasonings which has not already been met by Mr. Mill, Mr. Spencer, or Mr. Darwin.

I do not propose to follow the Quarterly Reviewer and Mr. Mivart through the long string of objections in matters of detail which they bring against Mr. Darwin's views. Everyone who has considered the matter carefully will be able to ferret out as many more "difficulties;" but he will also, I believe, fail as completely as they appear to me to have done, in bringing forward any fact which is really contradictory of Mr. Darwin's views. Occasionally, too, their objections and criticisms are based upon errors of their own. As, for example, when Mr. Mivart and the Quarterly Reviewer insist upon the resemblances between the eyes of *Cephalopoda* and *Vertebrata*, quite forgetting that there are striking and altogether fundamental differences between them; or when the Quarterly Reviewer corrects Mr. Darwin for saying

that the gibbons, "without having been taught, can walk or run upright with tolerable quickness, though they move awkwardly, and much less securely than man." The Quarterly Reviewer says, "This is a little misleading, inasmuch as it is not stated that this upright progression is effected by placing the enormously long arms behind the head, or holding them out backwards as a balance in progression."

Now, before carping at a small statement like this, the Quarterly Reviewer should have made sure that he was quite right. But he happens to be quite wrong. I suspect he got his notion of the manner in which a gibbon walks from a citation in "Man's Place in Nature." But at that time I had not seen a gibbon walk. Since then I have, and I can testify that nothing can be more precise than Mr. Darwin's statement. The gibbon I saw walked without either putting his arms behind his head or holding them out backwards. All he did was to touch the ground with the outstretched fingers of his long arms now and then, just as one sees a man who carries a stick, but does not need one, touch the ground with it as he walks along.

Again, a large number of the objections brought forward by Mr. Mivart and the Quarterly Reviewer apply to evolution in general, quite as much as to the particular form of that doctrine advocated by Mr. Darwin; or, to their notions of Mr. Darwin's views and not to what they really are. An excellent example of this class of difficulties is to be found in Mr. Mivart's chapter on "Independent Similarities of Structure." Mr. Mivart says that these cannot be explained by an "absolute and pure Darwinian," but "that an innate power and evolutionary law, aided by the corrective action of natural selection, should have furnished like needs with like aids, is not at all improbable" (p. 82).

I do not exactly know what Mr. Mivart means by an "absolute and pure Darwinian;" indeed Mr. Mivart makes that creature hold so many singular opinions that I doubt if I can ever have seen one alive. But I find nothing in his statement of the view which he imagines to be originated by himself, which is really inconsistent with what I understand to be Mr. Darwin's views.

I apprehend that the foundation of the theory of natural selection is the fact that living bodies tend incessantly to vary. This variation is neither indefinite, nor fortuitous, nor does it take place in all directions, in the strict sense of these words.

Accurately speaking, it is not indefinite, nor does it take place in all directions, because it is limited by the general characters of the type to which the organism exhibiting the variation belongs. A whale does not tend to vary in the direction of producing feathers, nor a bird in the direction of developing whalebone. In popular language there is no harm in saying that the waves which break upon the sea-shore are indefinite, fortuitous, and break in all directions. In scientific language, on the contrary, such a statement would be a gross error, inasmuch as every particle of foam is the result of perfectly definite forces, operating according to no less definite laws. In like manner, every variation of a living form, however minute, however apparently accidental, is inconceivable except as the expression of the operation of molecular forces or "powers" resident within the organism. And, as these forces certainly operate according to definite laws, their general result is, doubtless, in accordance with some general law which subsumes them all. And there appears to be no objection to call this an "evolutionary law." But nobody is the wiser for doing so, or has thereby contributed, in the

least degree, to the advance of the doctrine of evolution, the great need of which is a theory of variation.

When Mr. Mivart tells us that his "aim has been to support the doctrine that these species have been evolved by ordinary *natural laws* (for the most part unknown), aided by the *subordinate* action of 'natural selection'" (pp. 332-3), he seems to be of opinion that his enterprise has the merit of novelty. All I can say is that I have never had the slightest notion that Mr. Darwin's aim is in any way different from this. If I affirm that "species have been evolved by variation [1] (a natural process, the laws of which are for the most part unknown), aided by the subordinate action of natural selection," it seems to me that I enunciate a proposition which constitutes the very pith and marrow of the first edition of the "Origin of Species." And what the evolutionist stands in need of just now, is not an iteration of the fundamental principle of Darwinism, but some light upon the questions, What are the limits of variation? and, If a variety has arisen, can that variety be perpetuated, or even intensified, when selective conditions are indifferent, or perhaps unfavourable, to its existence? I cannot find that Mr. Darwin has ever been very dogmatic in answering these questions. Formerly, he seems to have inclined to reply to them in the negative, while now his inclination is the other way. Leaving aside those broad questions of theology, philosophy, and ethics, by the discussion of which neither the Quarterly Reviewer nor Mr. Mivart can be said to have damaged Darwinism—whatever else they have injured—this is what their criticisms come to. They confound a struggle for some rifle-pits with an assault on the fortress.

In some respects, finally, I can only characterize the Quarterly Reviewer's treatment of Mr. Darwin as alike

[1] Including under this head hereditary transmission

unjust and unbecoming. Language of this strength requires justification, and on that ground I add the remarks which follow.

The Quarterly Reviewer opens his essay by a careful enumeration of all those points upon which, during the course of thirteen years of incessant labour, Mr. Darwin has modified his opinions. It has often and justly been remarked, that what strikes a candid student of Mr. Darwin's works is not so much his industry, his knowledge, or even the surprising fertility of his inventive genius; but that unswerving truthfulness and honesty which never permit him to hide a weak place, or gloss over a difficulty, but lead him, on all occasions, to point out the weak places in his own armour, and even sometimes, it appears to me, to make admissions against himself which are quite unnecessary. A critic who desires to attack Mr. Darwin has only to read his works with a desire to observe, not their merits, but their defects, and he will find, ready to hand, more adverse suggestions than are likely ever to have suggested themselves to his own sharpness, without Mr. Darwin's self-denying aid.

Now this quality of scientific candour is not so common that it needs to be discouraged; and it appears to me to deserve other treatment than that adopted by the Quarterly Reviewer, who deals with Mr. Darwin as an Old Bailey barrister deals with a man against whom he wishes to obtain a conviction, *per fas aut nefas*, and opens his case by endeavouring to create a prejudice against the prisoner in the minds of the jury. In his eagerness to carry out this laudable design, the Quarterly Reviewer cannot even state the history of the doctrine of natural selection without an oblique and entirely unjustifiable attempt to depreciate Mr. Darwin. "To Mr. Darwin," says he, "and (through Mr. Wallace's

reticence) to Mr. Darwin alone, is due the credit of having first brought it prominently forward and demonstrated its truth." No one can less desire than I do, to throw a doubt upon Mr. Wallace's originality, or to question his claim to the honour of being one of the originators of the doctrine of natural selection; but the statement that Mr. Darwin has the sole credit of originating the doctrine because of Mr. Wallace's reticence is simply ridiculous. The proof of this is, in the first place, afforded by Mr. Wallace himself, whose noble freedom from petty jealousy in this matter, smaller folk would do well to imitate; and who writes thus:—"I have felt all my life, and I still feel, the most sincere satisfaction that Mr. Darwin had been at work long before me, and that it was not left for me to attempt to write the 'Origin of Species.' I have long since measured my own strength, and know well that it would be quite unequal to that task." So that if there was any reticence at all in the matter, it was Mr. Darwin's reticence during the long twenty years of study which intervened between the conception and the publication of his theory, which gave Mr. Wallace the chance of being an independent discoverer of the importance of natural selection. And, finally, if it be recollected that Mr. Darwin's and Mr. Wallace's essays were published simultaneously in the *Journal of the Linnæan Society* for 1858, it follows that the Reviewer, while obliquely depreciating Mr. Darwin's deserts, has in reality awarded to him a priority which, in legal strictness, does not exist.

Mr. Mivart, whose opinions so often concur with those of the Quarterly Reviewer, puts the case in a way, which I much regret to be obliged to say, is, in my judgment, quite as incorrect; though the injustice may be less glaring. He says that the theory of natural

selection is, in general, exclusively associated with the name of Mr. Darwin, "on account of the noble self-abnegation of Mr. Wallace." As I have said, no one can honour Mr. Wallace more than I do, both for what he has done and for what he has not done, in his relation to Mr. Darwin. And perhaps nothing is more creditable to him than his frank declaration that he could not have written such a work as the "Origin of Species." But, by this declaration, the person most directly interested in the matter repudiates, by anticipation, Mr. Mivart's suggestion that Mr. Darwin's eminence is more or less due to Mr. Wallace's modesty.

XII.

THE GENEALOGY OF ANIMALS.[1]

CONSIDERING that Germany now takes the lead of the world in scientific investigation, and particularly in biology, Mr. Darwin must be well pleased at the rapid spread of his views among some of the ablest and most laborious of German naturalists.

Among these, Professor Haeckel, of Jena, is the Coryphæus. I know of no more solid and important contributions to biology in the past seven years than Haeckel's work on the *Radiolaria*, and the researches of his distinguished colleague Gegenbaur, in vertebrate anatomy; while in Haeckel's *Generelle Morphologie* there is all the force, suggestiveness, and, what I may term the systematizing power, of Oken, without his extravagance. The *Generelle Morphologie* is, in fact, an attempt to put the doctrine of Evolution, so far as it applies to the living world, into a logical form; and to work out its practical applications to their final results. The work before us, again, may be said to be an exposition of the *Generelle Morphologie* for an educated public, consisting, as it does, of the substance of a series of lectures

[1] "The Natural History of Creation." By Dr. Ernst Haeckel. [*Natürliche Schöpfungs-Geschichte.*—Von Dr. Ernst Haeckel, Professor an der Universität Jena.] Berlin, 1868.

delivered before a mixed audience at Jena, in the session 1867-8.

"The Natural History of Creation,"—or, as Professor Haeckel admits it would have been better to call his work, "The History of the Development or Evolution of Nature,"—deals, in the first six lectures, with the general and historical aspects of the question, and contains a very interesting and lucid account of the views of Linnæus, Cuvier, Agassiz, Goethe, Oken, Kant, Lamarck, Lyell, and Darwin, and of the historical filiation of these philosophers.

The next six lectures are occupied by a well-digested statement of Mr. Darwin's views. The thirteenth lecture discusses two topics which are not touched by Mr. Darwin, namely, the origin of the present form of the solar system, and that of living matter. Full justice is done to Kant, as the originator of that "cosmic gas theory," as the Germans somewhat quaintly call it, which is commonly ascribed to Laplace. With respect to spontaneous generation, while admitting that there is no experimental evidence in its favour, Professor Haeckel denies the possibility of disproving it, and points out that the assumption that it has occurred is a necessary part of the doctrine of Evolution. The fourteenth lecture, on "Schöpfungs-Perioden und Schöpfungs-Urkunden," answers pretty much to the famous disquisition on the "Imperfection of the Geological Record" in the *Origin of Species*.

The following five lectures contain the most original matter of any, being devoted to "Phylogeny," or the working out of the details of the process of Evolution in the animal and vegetable kingdoms, so as to prove the line of descent of each group of living beings, and to furnish it with its proper genealogical tree, or "phylum."

The last lecture considers objections and sums up the evidence in favour of biological Evolution.

I shall best testify to my sense of the value of the work thus briefly analysed if I now proceed to note down some of the more important criticisms which have been suggested to me by its perusal.

I. In more than one place, Professor Haeckel enlarges upon the service which the *Origin of Species* has done, in favouring what he terms the "causal or mechanical" view of living nature as opposed to the "teleological or vitalistic" view. And no doubt it is quite true that the doctrine of Evolution is the most formidable opponent of all the commoner and coarser forms of Teleology. But perhaps the most remarkable service to the philosophy of Biology rendered by Mr. Darwin is the reconciliation of Teleology and Morphology, and the explanation of the facts of both which his views offer.

The Teleology which supposes that the eye, such as we see it in man or one of the higher *Vertebrata*, was made with the precise structure which it exhibits, for the purpose of enabling the animal which possesses it to see, has undoubtedly received its death-blow. Nevertheless it is necessary to remember that there is a wider Teleology, which is not touched by the doctrine of Evolution, but is actually based upon the fundamental proposition of Evolution. That proposition is, that the whole world, living and not living, is the result of the mutual interaction, according to definite laws, of the forces possessed by the molecules of which the primitive nebulosity of the universe was composed. If this be true, it is no less certain that the existing world lay, potentially, in the cosmic vapour; and that a sufficient intelligence could, from a knowledge of the properties of the molecules of that vapour, have predicted, say the state of the Fauna of Britain in 1869, with as much certainty as one

can say what will happen to the vapour of the breath in a cold winter's day.

Consider a kitchen clock, which ticks loudly, shows the hours, minutes, and seconds, strikes, cries "cuckoo!" and perhaps shows the phases of the moon. When the clock is wound up, all the phenomena which it exhibits are potentially contained in its mechanism, and a clever clockmaker could predict all it will do after an examination of its structure.

If the evolution theory is correct, the molecular structure of the cosmic gas stands in the same relation to the phenomena of the world as the structure of the clock to its phenomena.

Now let us suppose a death-watch, living in the clock-case, to be a learned and intelligent student of its works. He might say, "I find here nothing but matter and force and pure mechanism from beginning to end," and he would be quite right. But if he drew the conclusion that the clock was not contrived for a purpose, he would be quite wrong. On the other hand, imagine another death-watch of a different turn of mind. He, listening to the monotonous "tick! tick!" so exactly like his own, might arrive at the conclusion that the clock was itself a monstrous sort of death-watch, and that its final cause and purpose was to tick. How easy to point to the clear relation of the whole mechanism to the pendulum, to the fact that the one thing the clock did always and without intermission was to tick, and that all the rest of its phenomena were intermittent and subordinate to ticking! For all this, it is certain that kitchen clocks are not contrived for the purpose of making a ticking noise.

Thus the teleological theorist would be as wrong as the mechanical theorist, among our death-watches; and, probably, the only death-watch who would be right

would be the one who should maintain that the sole thing death-watches could be sure about was the nature of the clock-works and the way they move; and that the purpose of the clock lay wholly beyond the purview of beetle faculties.

Substitute "cosmic vapour" for "clock," and "molecules" for "works," and the application of the argument is obvious. The teleological and the mechanical views of nature are not, necessarily, mutually exclusive. On the contrary, the more purely a mechanist the speculator is, the more firmly does he assume a primordial molecular arrangement, of which all the phenomena of the universe are the consequences; and the more completely is he thereby at the mercy of the teleologist, who can always defy him to disprove that this primordial molecular arrangement was not intended to evolve the phenomena of the universe. On the other hand, if the teleologist assert that this, that, or the other result of the working of any part of the mechanism of the universe is its purpose and final cause, the mechanist can always inquire how he knows that it is more than an unessential incident—the mere ticking of the clock, which he mistakes for its function. And there seems to be no reply to this inquiry, any more than to the further, not irrational, question, why trouble oneself about matters which are out of reach, when the working of the mechanism itself, which is of infinite practical importance, affords scope for all our energies?

Professor Haeckel has invented a new and convenient name, "Dysteleology," for the study of the "purposelessnesses" which are observable in living organisms—such as the multitudinous cases of rudimentary and apparently useless structures. I confess, however, that it has often appeared to me that the facts of Dysteleology cut two ways. If we are to assume, as evolutionists

in general do, that useless organs atrophy, such cases as the existence of lateral rudiments of toes, in the foot of a horse, place us in a dilemma. For, either these rudiments are of no use to the animal, in which case, considering that the horse has existed in its present form since the Pliocene epoch, they surely ought to have disappeared; or they are of some use to the animal, in which case they are of no use as arguments against Teleology. A similar, but still stronger, argument may be based upon the existence of teats, and even functional mammary glands, in male mammals. Numerous cases of "Gynæcomasty," or functionally active breasts in men, are on record, though there is no mammalian species whatever in which the male normally suckles the young. Thus, there can be little doubt that the mammary gland was as apparently useless in the remotest male mammalian ancestor of man as in living men, and yet it has not disappeared. Is it then still profitable to the male organism to retain it? Possibly; but in that case its dysteleological value is gone.

II. Professor Haeckel looks upon the causes which have led to the present diversity of living nature as twofold. Living matter, he tells us, is urged by two impulses: a centripetal, which tends to preserve and transmit the specific form, and which he identifies with heredity; and a centrifugal, which results from the tendency of external conditions to modify the organism and effect its adaptation to themselves. The internal impulse is conservative, and tends to the preservation of specific, or individual, form; the external impulse is metamorphic, and tends to the modification of specific, or individual, form.

In developing his views upon this subject, Professor Haeckel introduces qualifications which disarm some of the criticisms I should have been disposed to offer; but

I think that his method of stating the case has the inconvenience of tending to leave out of sight the important fact—which is a cardinal point in the Darwinian hypothesis—that the tendency to vary, in a given organism, may have nothing to do with the external conditions to which that individual organism is exposed, but may depend wholly upon internal conditions. No one, I imagine, would dream of seeking in the direct influence of the external conditions of his life for the cause of the development of the sixth finger and toe in the famous Maltese.

I conceive that both hereditary transmission and adaptation need to be analysed into their constituent conditions by the further application of the doctrine of the Struggle for Existence. It is a probable hypothesis, that what the world is to organisms in general, each organism is to the molecules of which it is composed. Multitudes of these, having diverse tendencies, are competing with one another for opportunity to exist and multiply; and the organism, as a whole, is as much the product of the molecules which are victorious as the Fauna, or Flora, of a country is the product of the victorious organic beings in it.

On this hypothesis, hereditary transmission is the result of the victory of particular molecules contained in the impregnated germ. Adaptation to conditions is the result of the favouring of the multiplication of those molecules whose organizing tendencies are most in harmony with such conditions. In this view of the matter, conditions are not actively productive, but are passively permissive; they do not cause variation in any given direction, but they permit and favour a tendency in that direction which already exists.

It is true that, in the long run, the origin of the organic molecules themselves, and of their tendencies, is

to be sought in the external world; but if we carry our inquiries as far back as this, the distinction between internal and external impulses vanishes. On the other hand, if we confine ourselves to the consideration of a single organism, I think it must be admitted that the existence of an internal metamorphic tendency must be as distinctly recognized as that of an internal conservative tendency; and that the influence of conditions is mainly, if not wholly, the result of the extent to which they favour the one, or the other, of these tendencies.

III. There is only one point upon which I fundamentally and entirely disagree with Professor Haeckel, but that is the very important one of his conception of geological time, and of the meaning of the stratified rocks as records and indications of that time. Conceiving that the stratified rocks of an epoch indicate a period of depression, and that the intervals between the epochs correspond with periods of elevation of which we have no record, he intercalates between the different epochs, or periods, intervals which he terms "Ante-periods." Thus, instead of considering the Triassic, Jurassic, Cretaceous, and Eocene periods, as continuously successive, he interposes a period before each, as an "Antetrias-zeit," "Antejura-zeit," "Antecreta-zeit," "Ant-eocen-zeit," &c. And he conceives that the abrupt changes between the Faunæ of the different formations are due to the lapse of time, of which we have no organic record, during their "Ante-periods."

The frequent occurrence of strata containing assemblages of organic forms which are intermediate between those of adjacent formations, is, to my mind, fatal to this view. In the well-known St. Cassian beds, for example, Palæozoic and Mesozoic forms are commingled, and, between the Cretaceous and the Eocene formations, there are similar transitional beds. On the other hand,

in the middle of the Silurian series, extensive unconformity of the strata indicates the lapse of vast intervals of time between the deposit of successive beds, without any corresponding change in the Fauna.

Professor Haeckel will, I fear, think me unreasonable, if I say that he seems to be still overshadowed by geological superstitions; and that he will have to believe in the completeness of the geological record far less than he does at present. He assumes, for example, that there was no dry land, nor any terrestrial life, before the end of the Silurian epoch, simply because, up to the present time, no indications of fresh water, or terrestrial organisms, have been found in rocks of older date. And, in speculating upon the origin of a given group, he rarely goes further back than the "Ante-period," which precedes that in which the remains of animals belonging to that group are found. Thus, as fossil remains of the majority of the groups of *Reptilia* are first found in the Trias, they are assumed to have originated in the "Antetriassic" period, or between the Permian and Triassic epochs.

I confess this is wholly incredible to me. The Permian and the Triassic deposits pass completely into one another; there is no sort of discontinuity answering to an unrecorded "Antetrias;" and, what is more, we have evidence of immensely extensive dry land during the formation of these deposits. We know that the dry land of the Trias absolutely teemed with reptiles of all groups except Pterodactyles, Snakes, and perhaps Tortoises; there is every probability that true Birds existed, and *Mammalia* certainly did. Of the inhabitants of the Permian dry land, on the contrary, all that have left a record are a few lizards. Is it conceivable that these last should really represent the whole terrestrial population of that time, and that the development of Mammals, of

Birds, and of the highest forms of Reptiles, should have been crowded into the time during which the Permian conditions quietly passed away, and the Triassic conditions began? Does not any such supposition become in the highest degree improbable, when, in the terrestrial or fresh-water Labyrinthodonts, which lived on the land of the Carboniferous epoch, as well as on that of the Trias, we have evidence that one form of terrestrial life persisted, throughout all these ages, with no important modification? For my part, having regard to the small amount of modification (except in the way of extinction) which the Crocodilian, Lacertilian, and Chelonian *Reptilia* have undergone, from the older Mesozoic times to the present day, I cannot but put the existence of the common stock from which they sprang far back in the Palæozoic epoch; and I should apply a similar argumentation to all other groups of animals.

IV. Professor Haeckel proposes a number of modifications in Taxonomy, all of which are well worthy of consideration. Thus he establishes a third primary division of the living world, distinct from both animals and plants, under the name of the *Protista*, to include the *Myxomycetes*, the *Diatomaceæ*, and the *Labyrinthulæ*, which are commonly regarded as plants, with the *Noctilucæ*, the *Flagellata*, the *Rhizopoda*, the *Protoplasta*, and the *Monera*, which are most generally included within the animal world. A like attempt has been made, by other writers, to escape the inconvenience of calling these dubious organisms by the name of plant or animal; but I confess, it appears to me, that the inconvenience which is eluded in one direction, by this step, is met in two others. Professor Haeckel himself doubts whether the *Fungi* ought not to be removed into his *Protista*. If they are not, indeed, the *Myxomycetes* render the drawing of every line of demarcation between

Protistæ and Plants impossible. But if they are, who is to define the *Fungi* from the *Algæ?* Yet the seaweeds are surely, in every respect, plants. On the other hand, Professor Haeckel puts the sponges among the *Cœlenterata* (or polypes and corals), with the double inconvenience, as it appears to me, of separating the sponges from their immediate kindred, the *Protoplasta*, and destroying the definition of the *Cœlenterata*. So again, the *Infusoria* possess all the characters of animality, but it can hardly be said that they are as clearly allied to the worms as they are to the *Noctilucæ*.

On the whole, it appears to me to be most convenient to adhere to the old plan of calling such of these low forms as are more animal in habit, *Protozoa*, and such as are more vegetal, *Protophyta*.

Another considerable innovation is the proposition to divide the class Pisces into the four groups of *Leptocardia*, *Cyclostomata*, *Pisces*, and *Dipneusta*. As regards the establishment of a separate class for the Lancelet (*Amphioxus*), I think there can be little doubt of the propriety of so doing, inasmuch as it is far more different from all other fishes than they are from one another. And there is much to be said in favour of the same promotion of the *Cyclostomata*, or Lampreys and Hags. But considering the close relation of the Mudfish with the *Ganoidei*, and the wide differences between the *Elasmobranchii* and the *Teleostei*, I greatly doubt the propriety of separating the *Dipneusta*, as a class, from the other *Pisces*.

Professor Haeckel proposes to break up the vertebrate sub-kingdom, first, into the two provinces of *Leptocardia* and *Pachycardia*; *Amphioxus* being in the former, and all other vertebrates in the latter division. The *Pachycardia* are then divided into *Monorhina*, which contains the Cyclostome fishes, distinguished by their

single nasal aperture; and *Amphirhina*, comprising the other *Vertebrata*, which have two nasal apertures. These are further subdivided into *Anamnia* (*Pisces*, *Dipneusta*, *Amphibia*) and *Amniota* (*Reptilia*, *Aves*, *Mammalia*). This classification undoubtedly expresses many of the most important facts in vertebrate structure in a clear and compendious way; whether it is the best that can be adopted remains to be seen.

With much reason the Lemurs are removed altogether from the *Primates*, under the name of *Prosimiæ*. But I am surprised to find the *Sirenia* left in one group with the *Cetacea*, and the *Plesiosauria* with the *Ichthyosauria*; the ordinal distinctness of these having, to my mind, been long since fully established.

V. In Professor Haeckel's speculations on Phylogeny, or the genealogy of animal forms, there is much that is profoundly interesting, and his suggestions are always supported by sound knowledge and great ingenuity. Whether one agrees or disagrees with him, one feels that he has forced the mind into lines of thought in which it is more profitable to go wrong than to stand still.

To put his views into a few words, he conceives that all forms of life originally commenced as *Monera*, or simple particles of protoplasm; and that these *Monera* originated from not-living matter. Some of the *Monera* acquired tendencies towards the Protistic, others towards the Vegetal, and others towards the Animal modes of life. The last became animal *Monera*. Some of the animal *Monera* acquired a nucleus, and became amœba-like creatures; and, out of certain of these, ciliated infusorium-like animals were developed. These became modified into two stirpes: A, that of the worms; and B, that of the sponges. The latter by progressive modification gave rise to all the *Cœlenterata*; the former to all other animals. But A soon broke up into two

principal stirpes, of which one, *a*, became the root of the *Annelida*, *Echinodermata*, and *Arthropoda*, while the other, *b*, gave rise to the *Polyzoa* and *Ascidioida*, and produced the two remaining stirpes of the *Vertebrata* and the *Mollusca*.

Perhaps the most startling proposition of all those which Professor Haeckel puts before us is that which he bases upon Kowalewsky's researches into the development of *Amphioxus* and of the *Ascidioida*, that the origin of the *Vertebrata* is to be sought in an Ascidioid form. Goodsir long ago insisted upon the resemblance between *Amphioxus* and the Ascidians; but the notion of a genetic connection between the two, and especially the identification of the notochord of the *Vertebrate* with the axis of the caudal appendage of the larva of the Ascidian, is a novelty which, at first, takes one's breath away. I must confess, however, that the more I have pondered over it, the more grounds appear in its favour, though I am not convinced that there is any real parallelism between the mode of development of the ganglion of the *Ascidian* and that of the *Vertebrate* cerebro-spinal axis.

The hardly less startling hypothesis that the *Echinoderms* are coalesced worms, on the other hand, appears to be open to serious objection. As a matter of anatomy, it does not seem to me to correspond with fact; for there is no worm with a calcareous skeleton, nor any which has a band-like ventral nerve, superficial to which lies an ambulacral vessel. And, as a question of development, the formation of the radiate *Echinoderm* within its vermiform larva seems to me to be analogous to the formation of a radiate Medusa upon a Hydrozoic stock. But a Medusa is surely not the result of the coalescence of as many organisms as it presents morphological segments.

Professor Haeckel adduces the fossil *Crossopodia* and *Phyllodocites* as examples of the Annelidan forms, by the coalescence of which the Echinoderms may have been produced; but, even supposing the resemblance of these worms to detached starfish arms to be perfect, it is possible that they may be the extreme term, and not the commencement, of Echinoderm development. A pentacrinoid Echinoderm, with a complete jointed stalk, is developed within the larva of *Antedon*. Is it not possible that the larva of *Crossopodia* may have developed a vermiform Echinoderm?

With respect to the Phylogeny of the *Arthropoda*, I find myself disposed to take a somewhat different view from that of Professor Haeckel. He assumes that the primary stock of the whole group was a crustacean, having that *Nauplius*-form in which Fritz Müller has shown that so many *Crustacea* commence their lives. All the *Entomostraca* arose by the modification of some one or other of these Naupliform "*Archicarida*." Other *Archicarida* underwent a further metamorphosis into a *Zoœa*-form. From some of these "*Zoeopoda*" arose all the remaining Malacostracous *Crustacea*; while, from others, was developed some form analogous to the existing *Galeodes*, out of which proceeded, by gradual differentiation, all the *Myriapoda*, *Arachnida*, and *Insecta*.

I should be disposed to interpret the facts of the embryological history and of the anatomy of the *Arthropoda* in a different manner. The *Copepoda*, the *Ostracoda*, and the *Branchiopoda* are the *Crustacea* which have departed least from the embryonic or *Nauplius*-forms; and, of these, I imagine that the *Copepoda* represent the hypothetical *Archicarida* most closely. *Apus* and *Sapphirina* indicate the relations of these Archæocarids with the *Trilobita*, and the *Eurypterida*

connect the *Trilobita* and the *Copepoda* with the *Xiphosura*. But the *Xiphosura* have such close morphological relations with the *Arachnida*, and especially with the oldest known Arachnidan, *Scorpio*, that I cannot doubt the existence of a genetic connection between the two groups. On the other hand, the *Branchiopoda* do, even at the present day, almost pass into the true *Podophthalmia*, by *Nebalia*. By the *Trilobita*, again, the *Archicarida* are connected with such *Edriophthalmia* as *Serolis*. The *Stomapoda* are extremely modified *Edriophthalmia* of the amphipod type. On the other side, the *Isopoda* lead to the *Myriapoda*, and the latter to the *Insecta*. Thus the Arthropod phylum, which suggests itself to me, is that the branches of the *Podophthalmia*, of the *Insecta* (with the *Myriapoda*), and of the *Arachnida*, spring separately and distinctly from the Archæocarid root—and that the *Zoœa*-forms occur only at the origin of the Podophthalmous branch.

The phylum of the *Vertebrata* is the most interesting of all, and is admirably discussed by Professor Haeckel. I can note only a few points which seem to me to be open to discussion. The *Monorhina*, having been developed out of the *Leptocardia*, gave rise, according to Professor Haeckel, to a shark-like form, which was the common stock of all the *Amphirhina*. From this "Protamphirhine" were developed, in divergent lines, the true Sharks, Rays, and *Chimœræ*; the Ganoids, and the *Dipneusta*. The *Teleostei* are modified *Ganoidei*. The *Dipneusta* gave rise to the *Amphibia*, which are the root of all other *Vertebrata*, inasmuch as out of them were developed the first *Vertebrata* provided with an amnion, or the *Protamniota*. The *Protamniota* split up into two stems, one that of the *Mammalia*, the other common to *Reptilia* and *Aves*.

The only modification which it occurs to me to suggest

in this general view of the Phylogeny of the *Vertebrata* is, that the "Protamphirhine" was possibly more ganoid than shark-like. So far as our present information goes the Ganoids are as old as the Sharks; and it is very interesting to observe that the remains of the oldest Ganoids, *Cephalaspis* and *Pteraspis*, have as yet displayed no trace of jaws. It is just possible that they may connect the *Monorhina* with the Sturgeons among the *Amphirhina*. On the other hand, the Crossopterygian Ganoids exhibit the closest connection with *Lepidosiren*, and thereby with the *Amphibia*. It should not be forgotten that the development of the Lampreys exhibits curious points of resemblance with that of the *Amphibia*, which are absent in the Sharks and Rays. Of the development of the *Ganoidei* we have unfortunately no knowledge, but their brains and their reproductive organs are more amphibian than are those of the Sharks.

On the whole, I am disposed to think that the direct stem of ascent from the *Monorhina* to the *Amphibia* is formed by the Ganoids and the Mudfishes; while the Osseous fishes and the Sharks are branches in different directions from this stem.

What the *Protamniota* were like, I do not suppose any one is in a position to say, but I cannot think that the thoroughly Lacertian *Protorosaurus* had anything to do with them. The reptiles which are most amphibian in their characters, and therefore, probably, most nearly approach the *Protamniota*, are the *Ichthyosauria* and the *Chelonia*.

That the *Didelphia* were developed out of some ornithodelphous form, as Professor Haeckel supposes, seems to be unquestionable; but the existing Opossums and Kangaroos are certainly extremely modified and remote from their ancestors the "*Prodidelphia*," of which we have not, at present, the slightest knowledge. The

mode of origin of the *Monodelphia* from these is a very difficult problem, for the most part left open by Professor Haeckel. He considers the *Prosimiæ*, or Lemurs, to be the common stock of the *Deciduata*, and the *Cetacea* (with which he includes the *Sirenia*) to be modified *Ungulata*. As regards the latter question, I have little doubt that the *Sirenia* connect the *Ungulata* with the *Proboscidea*; and none, that the *Cetacea* are extremely modified *Carnivora*. The passage between the Seals and the *Cetacea* by *Zeuglodon* is complete. I also think that there is much to be said for the opinion, that the *Insectivora* represent the common stock of the *Primates* (which passed into them by the *Prosimiæ*), the *Cheiroptera*, the *Rodentia*, and the *Carnivora*. And I am greatly disposed to look for the common root of all the *Ungulata*, as well, in some ancient non-deciduate Mammals which were more like *Insectivora* than anything else. On the other hand, the *Edentata* appear to form a series by themselves.

The latter part of this notice of the *Natürliche Schöpfungs-Geschichte*, brings so strongly into prominence the points of difference between its able author and myself, that I do not like to conclude without reminding the reader of my entire concurrence with the general tenor and spirit of the work, and of my high estimate of its value.

XIII.

BISHOP BERKELEY ON THE METAPHYSICS OF SENSATION.[1]

PROFESSOR FRASER has earned the thanks of all students of philosophy for the conscientious labour which he has bestowed upon his new edition of the works of Berkeley; in which, for the first time, we find collected together every thought which can be traced to the subtle and penetrating mind of the famous Bishop of Cloyne; while the "Life and Letters" will rejoice those who care less for the idealist and the prophet of tar-water, than for the man who stands out as one of the noblest and purest figures of his time: that Berkeley from whom the jealousy of Pope did not withhold a single one of all "the virtues under heaven;" nor the cynicism of Swift, the dignity of "one of the first men of the kingdom for learning and virtue;" the man whom the pious Atterbury could compare to nothing less than an angel; and whose personal influence and eloquence filled the Scriblerus Club and the House of Commons with enthusiasm for the evangelization of the North American Indians;

[1] "The Works of George Berkeley, D.D., formerly Bishop of Cloyne, including many of his Works hitherto unpublished, with Preface, Annotations, his Life and Letters, and an Account of his Philosophy." By A. C. Fraser. Four vols. Oxford: Clarendon Press, 1871.

and even led Sir Robert Walpole to assent to the appropriation of public money to a scheme which was neither business nor bribery.[1]

Hardly any epoch in the intellectual history of England is more remarkable in itself, or possesses a greater interest for us in these latter days, than that which coincides broadly with the conclusion of the seventeenth and the opening of the eighteenth century.

The political fermentation of the preceding age was gradually working itself out; domestic peace gave men time to think; and the toleration won by the party of which Locke was the spokesman, permitted a freedom of speech and of writing such as has rarely been exceeded in later times.

Fostered by these circumstances, the great faculty for physical and metaphysical inquiry, with which the people of our race are naturally endowed, developed itself vigorously; and at least two of its products have had a profound and a permanent influence upon the subsequent course of thought in the world. The one of these was English Freethinking; the other, the Theory of Gravitation.

Looking back to the origin of the intellectual impulses of which these were the results, we are led to Herbert, to Hobbes, to Bacon; and to one who stands in advance of all these, as the most typical man of his time—Descartes. It is the Cartesian doubt—the maxim that assent may properly be given to no propositions but such as are perfectly clear and distinct—which, becoming incarnate, so to speak, in the Englishmen,

[1] In justice to Sir Robert, however, it is proper to remark that he declared afterwards, that he gave his assent to Berkeley's scheme for the Bermuda University only because he thought the House of Commons was sure to throw it out.

Anthony Collins, Toland, Tindal, Woolston, and in the wonderful Frenchman, Pierre Bayle, reached its final term in Hume.

And, on the other hand, although the theory of Gravitation set aside the Cartesian vortices—yet the spirit of the "Principes de Philosophie" attained its apotheosis when Newton demonstrated all the host of heaven to be but the elements of a vast mechanism, regulated by the same laws as those which govern the falling of a stone to the ground. There is a passage in the preface to the first edition of the "Principia" which shows that Newton was penetrated, as completely as Descartes, with the belief that all the phenomena of nature are expressible in terms of matter and motion.

"Would that the rest of the phenomena of nature could be deduced by a like kind of reasoning from mechanical principles. For many circumstances lead me to suspect that all these phenomena may depend upon certain forces, in virtue of which the particles of bodies, by causes not yet known, are either mutually impelled against one another and cohere into regular figures, or repel and recede from one another; which forces being unknown, philosophers have as yet explored nature in vain. But I hope that, either by this method of philosophizing, or by some other and better, the principles here laid down may throw some light upon the matter."[1]

But the doctrine that all the phenomena of nature are resolvable into mechanism is what people have

[1] "Utinam cætera naturæ phænomena ex principiis mechanicis, eodem argumentandi genere, derivare licet. Nam multa me movent, ut nonnihil suspicer ea omnia ex viribus quibusdam pendere posse, quibus corporum particulæ, per causas nondum cognitas, vel in se mutuo impelluntur et secundum figuras regulares cohærent vel ab invicem fugantur et recedunt; quibus viribus ignotis, Philosophi hactenus Naturam frustra tentarunt. Spero autem quòd vel huic philosophandi modo, vel veriori, alicui, principia hic posita lucem aliquam præbebunt."—Preface to First Edition of *Principia*, May 8, 1686.

agreed to call "materialism;" and when Locke and Collins maintained that matter may possibly be able to think, and Newton himself could compare infinite space to the sensorium of the Deity, it was not wonderful that the English philosophers should be attacked as they were by Leibnitz in the famous letter to the Princess of Wales, which gave rise to his correspondence with Clarke.[1]

"1. Natural religion itself seems to decay [in England] very much. Many will have human souls to be material; others make God Himself a corporeal Being.

"2. Mr. Locke and his followers are uncertain, at least, whether the soul be not material and naturally perishable.

"3. Sir Isaac Newton says that space is an organ which God makes use of to perceive things by. But if God stands in need of any organ to perceive things by, it will follow that they do not depend altogether upon Him, nor were produced by Him.

"4. Sir Isaac Newton and his followers have also a very odd opinion concerning the work of God. According to their doctrine, God Almighty wants to wind up His watch from time to time; otherwise it would cease to move.[2] He had not, it seems, sufficient foresight to make it a perpetual motion. Nay, the machine of God's making is so imperfect, according to these gentlemen, that He is obliged to clean it now and then by an extraordinary concourse, and even to mend it as a clockmaker mends his work."

It is beside the mark, at present, to inquire how far

[1] "Collection of Papers which passed between the late learned Mr. Leibnitz and Dr. Clarke."—1717.

[2] Goethe seems to have had this saying of Leibnitz in his mind when he wrote his famous lines—

"Was wär' ein Gott der nur von aussen stiesse
Im Kreis das All am Finger laufen liesse."

Leibnitz paints a true picture, and how far he is guilty of a spiteful caricature of Newton's views in these passages; and whether the beliefs which Locke is known to have entertained are consistent with the conclusions which may logically be drawn from some parts of his works. It is undeniable that English philosophy in Leibnitz's time had the general character which he ascribes to it. The phenomena of nature were held to be resolvable into the attractions and the repulsions of particles of matter; all knowledge was attained through the senses; the mind antecedent to experience was a *tabula rasa*. In other words, at the commencement of the eighteenth century, the character of speculative thought in England was essentially sceptical, critical, and materialistic. Why "materialism" should be more inconsistent with the existence of a Deity, the freedom of the will, or the immortality of the soul, or with any actual or possible system of theology, than "idealism," I must declare myself at a loss to divine. But in the year 1700 all the world appears to have been agreed, Tertullian notwithstanding, that materialism necessarily leads to very dreadful consequences. And it was thought that it conduced to the interests of religion and morality to attack the materialists with all the weapons that came to hand. Perhaps the most interesting controversy which arose out of these questions is the wonderful triangular duel between Dodwell, Clarke, and Anthony Collins, concerning the materiality of the soul, and—what all the disputants considered to be the necessary consequence of its materiality—its natural mortality. I do not think that anyone can read the letters which passed between Clarke and Collins, without admitting that Collins, who writes with wonderful power and closeness of reasoning, has by far the best of the argument, so far as the possible materiality of the soul goes; and

that, in this battle, the Goliath of Freethinking overcame the champion of what was considered Orthodoxy.

But in Dublin, all this while, there was a little David practising his youthful strength upon the intellectual lions and bears of Trinity College. This was George Berkeley, who was destined to give the same kind of development to the idealistic side of Descartes' philosophy, that the Freethinkers had given to its sceptical side, and the Newtonians to its mechanical side.

Berkeley faced the problem boldly. He said to the materialists: "You tell me that all the phenomena of nature are resolvable into matter and its affections. I assent to your statement, and now I put to you the further question, 'What is matter?' In answering this question you shall be bound by your own conditions; and I demand, in the terms of the Cartesian axiom, that in turn you give your assent only to such conclusions as are perfectly clear and obvious."

It is this great argument which is worked out in the "Treatise concerning the Principles of Human Knowledge," and in those "Dialogues between Hylas and Philonous," which rank among the most exquisite examples of English style, as well as among the subtlest of metaphysical writings; and the final conclusion of which is summed up in a passage remarkable alike for literary beauty and for calm audacity of statement.

"Some truths there are so near and obvious to the mind that a man need only open his eyes to see them. Such I take this important one to be, viz., that all the choir of heaven and furniture of the earth—in a word, all those bodies which compose the mighty frame of the world—have not any substance without a mind; that their being is to be perceived or known; that consequently, so long as they are not actually perceived by me, or do not exist in my mind or that of any other created spirit, they must either have no existence at all or else subsist in the mind of some eternal spirit; it being perfectly unintelligible, and involving all the absurdity of abstraction, to

attribute to any single part of them an existence independent of a spirit."[1]

Doubtless this passage sounds like the acme of metaphysical paradox, and we all know that "coxcombs vanquished Berkeley with a grin;" while common-sense folk refuted him by stamping on the ground, or some such other irrelevant proceeding. But the key to all philosophy lies in the clear apprehension of Berkeley's problem—which is neither more nor less than one of the shapes of the greatest of all questions, "What are the limits of our faculties?" And it is worth any amount of trouble to comprehend the exact nature of the argument by which Berkeley arrived at his results, and to know by one's own knowledge the great truth which he discovered—that the honest and rigorous following up of the argument which leads us to materialism, inevitably carries us beyond it.

Suppose that I accidentally prick my finger with a pin. I immediately become aware of a condition of my consciousness—a feeling which I term pain. I have no doubt whatever that the feeling is in myself alone; and if anyone were to say that the pain I feel is something which inheres in the needle, as one of the qualities of the substance of the needle, we should all laugh at the absurdity of the phraseology. In fact, it is utterly impossible to conceive pain except as a state of consciousness.

Hence, so far as pain is concerned, it is sufficiently obvious that Berkeley's phraseology is strictly applicable to our power of conceiving its existence—"its being is to be perceived or known," and "so long as it is not actually perceived by me, or does not exist in my mind, or that of any other created spirit, it must either have no existence at all, or else subsist in the mind of some eternal spirit."

[1] "Treatise concerning the Principles of Human Knowledge," Part I. § 6.

So much for pain. Now let us consider an ordinary sensation. Let the point of the pin be gently rested upon the skin, and I become aware of a feeling or condition of consciousness quite different from the former—the sensation of what I call "touch." Nevertheless this touch is plainly just as much in myself as the pain was. I cannot for a moment conceive this something which I call touch as existing apart from myself, or a being capable of the same feelings as myself. And the same reasoning applies to all the other simple sensations. A moment's reflection is sufficient to convince one that the smell, and the taste, and the yellowness, of which we become aware when an orange is smelt, tasted, and seen, are as completely states of our consciousness as is the pain which arises if the orange happens to be too sour. Nor is it less clear that every sound is a state of the consciousness of him who hears it. If the universe contained only blind and deaf beings, it is impossible for us to imagine but that darkness and silence should reign everywhere.

It is undoubtedly true, then, of all the simple sensations that, as Berkeley says, their "*esse* is *percipi*"—their being is to be "perceived or known." But that which perceives, or knows, is mind or spirit; and therefore that knowledge which the senses give us is, after all, a knowledge of spiritual phenomena.

All this was explicitly or implicitly admitted, and, indeed, insisted upon, by Berkeley's contemporaries, and by no one more strongly than by Locke, who terms smells, tastes, colours, sounds, and the like, "secondary qualities," and observes, with respect to these "secondary qualities," that "whatever reality we by mistake attribute to them [they] are in truth nothing in the objects themselves."

And again: "Flame is denominated hot and light;

snow, white and cold; and manna, white and sweet, from the ideas they produce in us; which qualities are commonly thought to be the same in these bodies; that those ideas are in us, the one the perfect resemblance of the other as they are in a mirror; and it would by most men be judged very extravagant if one should say otherwise. And yet he that will consider that the same fire that at one distance produces in us the sensation of warmth, does at a nearer approach produce in us the far different sensation of pain, ought to bethink himself what reason he has to say that his idea of warmth, which was produced in him by the fire, is actually in the fire; and his idea of pain which the same fire produced in him in the same way, is not in the fire. Why are whiteness and coldness in snow, and pain not, when it produces the one and the other idea in us; and can do neither but by the bulk, figure, number, and motion of its solid parts?"[1]

Thus far then materialists and idealists are agreed. Locke and Berkeley, and all logical thinkers who have succeeded them, are of one mind about secondary qualities—their being is to be perceived or known—their materiality is, in strictness, a spirituality.

But Locke draws a great distinction between the secondary qualities of matter, and certain others which he terms "primary qualities." These are extension, figure, solidity, motion and rest, and number; and he is as clear that these primary qualities exist independently of the mind, as he is that the secondary qualities have no such existence.

"The particular bulk, number, figure, and motion of the parts of fire and snow are really in them, whether anyone's senses perceive them or not, and therefore they may be called real qualities, because they really exist in those bodies; but light, heat, whiteness, or coldness,

[1] Locke, "Human Understanding," Book II. chap. viii. §§ 14, 15.

are no more really in them, than sickness, or pain, is in manna. Take away the sensation of them; let not the eyes see light or colours, nor the ears hear sounds; let the palate not taste, nor the nose smell; and all colours, tastes, odours and sounds, as they are such particular ideas, vanish and cease, and are reduced to their causes, *i.e.* bulk, figure, and motion of parts.

"18. A piece of manna of sensible bulk is able to produce in us the idea of a round or square figure; and, by being removed from one place to another, the idea of motion. This idea of motion represents it as it really is in the manna moving; a circle and square are the same, whether in idea or existence, in the mind or in the manna; and thus both motion and figure are really in the manna, whether we take notice of them or no: this everybody is ready to agree to."

So far as primary qualities are concerned, then, Locke is as thoroughgoing a realist as St. Anselm. In Berkeley, on the other hand, we have as complete a representative of the nominalists and conceptualists—an intellectual descendant of Roscellinus and of Abelard. And by a curious irony of fate, it is the nominalist who is, this time, the champion of orthodoxy, and the realist that of heresy.

Once more let us try to work out Berkeley's principles for ourselves, and inquire what foundation there is for the assertion that extension, form, solidity, and the other "primary qualities," have an existence apart from mind. And for this purpose let us recur to our experiment with the pin.

It has been seen that when the finger is pricked with a pin, a state of consciousness arises which we call pain; and it is admitted that this pain is not a something which inheres in the pin, but a something which exists only in the mind, and has no similitude elsewhere.

But a little attention will show that this state of consciousness is accompanied by another, which can by no effort be got rid of. I not only have the feeling, but the feeling is localized. I am just as certain that the pain is in my finger, as I am that I have it at all.

Nor will any effort of the imagination enable me to believe that the pain is not in my finger.

And yet nothing is more certain than that it is not, and cannot be, in the spot in which I feel it, nor within a couple of feet of that spot. For the skin of the finger is connected by a bundle of fine nervous fibres, which run up the whole length of the arm, with the spinal marrow and brain, and we know that the feeling of pain caused by the prick of a pin is dependent on the integrity of those fibres. After they have been cut through close to the spinal cord, no pain will be felt, whatever injury is done to the finger; and if the ends which remain in connection with the cord be pricked, the pain which arises will appear to have its seat in the finger just as distinctly as before. Nay, if the whole arm be cut off, the pain which arises from pricking the nerve stump will appear to be seated in the fingers, just as if they were still connected with the body.

It is perfectly obvious, therefore, that the localization of the pain at the surface of the body is an act of the mind. It is an *extradition* of that consciousness, which has its seat in the brain, to a definite point of the body—which takes place without our volition, and may give rise to ideas which are contrary to fact. We might call this extradition of consciousness a reflex feeling, just as we speak of a movement which is excited apart from, or contrary to, our volition, as a reflex motion. Locality is no more in the pin than pain is; of the former, as of the latter, it is true that "its being is to be perceived," and that its existence apart from a thinking mind is not conceivable.

The foregoing reasoning will be in no way affected, if, instead of pricking the finger, the point of the pin rests gently against it, so as to give rise merely to a tactile sensation. The tactile sensation is referred outwards to

the point touched, and seems to exist there. But it is certain that it is not and cannot be there really, because the brain is the sole seat of consciousness; and, further, because evidence, as strong as that in favour of the sensation being in the finger, can be brought forward in support of propositions which are manifestly absurd.

For example, the hairs and nails are utterly devoid of sensibility, as everyone knows. Nevertheless, if the ends of the nails or hairs are touched, ever so lightly, we feel that they are touched, and the sensation seems to be situated in the nails or hairs. Nay more, if a walking-stick a yard long is held firmly by the handle and the other end is touched, the tactile sensation, which is a state of our own consciousness, is unhesitatingly referred to the end of the stick; and yet no one will say that it *is* there.

Let us now suppose that, instead of one pin's point resting against the end of my finger, there are two. Each of these can be known to me, as we have seen, only as a state of a thinking mind, referred outwards, or localized. But the existence of these two states, somehow or other, generates in my mind a host of new ideas, which did not make their appearance when only one state was present.

For example, I get the ideas of co-existence, of number, of distance, and of relative place or direction. But all these ideas are ideas of relations, and imply the existence of something which perceives those relations. If a tactile sensation is a state of the mind, and if the localization of that sensation is an act of the mind, how is it conceivable that a relation between two localized sensations should exist apart from the mind? It is, I confess, quite as easy for me to imagine that redness may exist apart from a visual sense, as it is to suppose

that co-existence, number, and distance can have any existence apart from the mind of which they are ideas.

Thus it seems clear that the existence of some, at any rate, of Locke's primary qualities of matter, such as number and extension, apart from mind, is as utterly unthinkable as the existence of colour and sound under like circumstances.

Will the others—namely, figure, motion and rest, and solidity—withstand a similar criticism? I think not. For all these, like the foregoing, are perceptions by the mind of the relations of two or more sensations to one another. If distance and place are inconceivable, in the absence of the mind of which they are ideas, the independent existence of figure, which is the limitation of distance, and of motion, which is change of place, must be equally inconceivable. Solidity requires more particular consideration, as it is a term applied to two very different things, the one of which is solidity of form, or geometrical solidity; while the other is solidity of substance, or mechanical solidity.

If those motor nerves of a man by which volitions are converted into motion were all paralysed, and if sensation remained only in the palm of his hand (which is a conceivable case), he would still be able to attain to clear notions of extension, figure, number, and motion, by attending to the states of consciousness which might be aroused by the contact of bodies with the sensory surface of the palm. But it does not appear that such a person could arrive at any conception of geometrical solidity. For that which does not come in contact with the sensory surface is non-existent for the sense of touch; and a solid body, impressed upon the palm of the hand, gives rise only to the notion of the extension of that particular part of the solid which is in contact with the skin.

Nor is it possible that the idea of outness (in the sense of discontinuity with the sentient body) could be attained by such a person; for, as we have seen, every tactile sensation is referred to a point either of the natural sensory surface itself, or of some solid in continuity with that surface. Hence it would appear that the conception of the difference between the Ego and the non-Ego could not be attained by a man thus situated. His feelings would be his universe, and his tactile sensations his "mœnia mundi." Time would exist for him as for us, but space would have only two dimensions.

But now remove the paralysis from the motor apparatus, and give the palm of the hand of our imaginary man perfect freedom to move, so as to be able to glide in all directions over the bodies with which it is in contact. Then with the consciousness of that mobility, the notion of space of three dimensions—which is "*Raum*," or "room" to move with perfect freedom—is at once given. But the notion that the tactile surface itself moves, cannot be given by touch alone, which is competent to testify only to the fact of change of place, not to its cause. The idea of the motion of the tactile surface could not, in fact, be attained, unless the idea of change of place were accompanied by some state of consciousness, which does not exist when the tactile surface is immoveable. This state of consciousness is what is termed the muscular sense, and its existence is very easily demonstrable.

Suppose the back of my hand to rest upon a table, and a sovereign to rest upon the upturned palm, I at once acquire a notion of extension, and of the limit of that extension. The impression made by the circular piece of gold is quite different from that which would be made by a triangular, or a square, piece of the same

size, and thereby I arrive at the notion of figure. Moreover, if the sovereign slides over the palm, I acquire a distinct conception of change of place or motion, and of the direction of that motion. For as the sovereign slides, it affects new nerve-endings, and gives rise to new states of consciousness. Each of them is definitely and separately localized by a reflex act of the mind, which, at the same time, becomes aware of the difference between two successive localizations; and therefore of change of place, which is motion.

If, while the sovereign lies on the hand, the latter being kept quite steady, the fore-arm is gradually and slowly raised; the tactile sensations, with all their accompaniments, remain exactly as they were. But, at the same time, something new is introduced; namely, the sense of effort. If I try to discover where this sense of effort seems to be, I find myself somewhat perplexed at first; but, if I hold the fore-arm in position long enough, I become aware of an obscure sense of fatigue, which is apparently seated either in the muscles of the arm, or in the integument directly over them. The fatigue seems to be related to the sense of effort, in much the same way as the pain which supervenes upon the original sense of contact, when a pin is slowly pressed against the skin, is related to touch.

A little attention will show that this sense of effort accompanies every muscular contraction by which the limbs, or other parts of the body, are moved. By its agency the fact of their movement is known; while the direction of the motion is given by the accompanying tactile sensations. And, in consequence of the incessant association of the muscular and the tactile sensations, they become so fused together that they are often confounded under the same name.

If freedom to move in all directions is the very essence

of that conception of space of three dimensions which we obtain by the sense of touch; and if that freedom to move is really another name for the feeling of unopposed effort, accompanied by that of change of place, it is surely impossible to conceive of such space as having existence apart from that which is conscious of effort.

But it may be said that we derive our conception of space of three dimensions not only from touch, but from vision; that if we do not feel things actually outside us, at any rate we see them. And it was exactly this difficulty which presented itself to Berkeley at the outset of his speculations. He met it, with characteristic boldness, by denying that we do see things outside us; and, with no less characteristic ingenuity, by devising that "New Theory of Vision" which has met with wider acceptance than any of his views, though it has been the subject of continual controversies.[1]

In the "Principles of Human Knowledge," Berkeley himself tells us how he was led to those views which he published in the "Essay towards the New Theory of Vision."

"It will be objected that we see things actually without, or at a distance from us, and which consequently do not exist in the mind; it being absurd that those things which are seen at the distance of several miles, should be as near to us as our own thoughts. In answer to this, I desire it may be considered that in a dream we do oft perceive things as existing at a great distance off, and yet, for all that, those things are acknowledged to have their existence only in the mind.

"But for the fuller clearing of this point, it may be worth while to consider how it is that we perceive distance and things placed at a distance by sight. For that we should in truth see external space and bodies actually existing in it, some nearer, others further off,

[1] I have not specifically alluded to the writings of Bailey, Mill, Abbott, and others, on this vexed question, not because I have failed to study them carefully, but because this is not a convenient occasion for controversial discussion. Those who are acquainted with the subject, however, will observe that the view I have taken agrees substantially with that of Mr. Bailey.

seems to carry with it some opposition to what hath been said of their existing nowhere without the mind. The consideration of this difficulty it was that gave birth to my 'Essay towards the New Theory of Vision,' which was published not long since, wherein it is shown that distance, or outness, is neither immediately of itself perceived by sight, nor yet apprehended, or judged of, by lines and angles or anything that hath any necessary connection with it; but that it is only suggested to our thoughts by certain visible ideas and sensations attending vision, which, in their own nature, have no manner of similitude or relation either with distance, or with things placed at a distance; but by a connection taught us by experience, they come to signify and suggest them to us, after the same manner that words of any language suggest the ideas they are made to stand for; insomuch that a man born blind and afterwards made to see, would not, at first sight, think the things he saw to be without his mind or at any distance from him."

The key-note of the Essay to which Berkeley refers in this passage is to be found in an italicized paragraph of section 127:—

"*The extensions, figures, and motions perceived by sight are specifically distinct from the ideas of touch called by the same names; nor is there any such thing as an idea, or kind of idea, common to both senses.*"

It will be observed that this proposition expressly declares that extension, figure, and motion, and consequently distance, are immediately perceived by sight as well as by touch; but that visual distance, extension, figure, and motion, are totally different in quality from the ideas of the same name obtained through the sense of touch. And other passages leave no doubt that such was Berkeley's meaning. Thus in the 112th section of the same Essay, he carefully defines the two kinds of distance, one visual, the other tangible:—

"By the distance between any two points nothing more is meant than the number of intermediate points. If the given points are visible, the distance between them is marked out by the number of interjacent visible points; if they are tangible, the distance between them is a line consisting of tangible points."

Again, there are two sorts of magnitude or extension :—

"It has been shown that there are two sorts of objects apprehended by sight, each whereof has its distinct magnitude or extension: the one properly tangible, *i.e.* to be perceived and measured by touch, and not immediately falling under the sense of seeing; the other properly and immediately visible, by mediation of which the former is brought into view."—§ 55.

But how are we to reconcile these passages with others which will be perfectly familiar to every reader of the "New Theory of Vision"? As, for example :—

"It is, I think, agreed by all, that distance of itself, and immediately, cannot be seen."—§ 2.

"Space or distance, we have shown, is no otherwise the object of sight than of hearing."—§ 130.

"Distance is in its own nature imperceptible, and yet it is perceived by sight. It remains, therefore, that it is brought into view by means of some other idea, that is itself immediately perceived in the act of vision."—§ 11.

"Distance or external space."—§ 155.

The explanation is quite simple, and lies in the fact that Berkeley uses the word "distance" in three senses. Sometimes he employs it to denote visible distance, and then he restricts it to distance in two dimensions, or simple extension. Sometimes he means tangible distance in two dimensions; but most commonly he intends to signify tangible distance in the third dimension. And it is in this sense that he employs "distance" as the equivalent of "space." Distance in two dimensions is, for Berkeley, not space, but extension. By taking a pencil and interpolating the words "visible" and "tangible" before "distance" wherever the context renders them necessary, Berkeley's statements may be made perfectly consistent; though he has not always extricated himself from the entanglement caused by his own loose phraseology, which rises to a climax in the last ten

sections of the "Theory of Vision," in which he endeavours to prove that a pure intelligence able to see, but devoid of the sense of touch, could have no idea of a plane figure. Thus he says in section 156 :—

"All that is properly perceived by the visual faculty amounts to no more than colours with their variations and different proportions of light and shade; but the perpetual mutability and fleetingness of those immediate objects of sight render them incapable of being managed after the manner of geometrical figures, nor is it in any degree useful that they should. It is true there be divers of them perceived at once, and more of some and less of others; but accurately to compute their magnitude, and assign precise determinate proportions between things so variable and inconstant, if we suppose it possible to be done, must yet be a very trifling and insignificant labour."

If, by this, Berkeley means that by vision alone, a straight line cannot be distinguished from a curved one, a circle from a square, a long line from a short one, a large angle from a small one, his position is surely absurd in itself and contradictory to his own previously cited admissions; if he only means, on the other hand, that his pure spirit could not get very far on in his geometry, it may be true or not; but it is in contradiction with his previous assertion, that such a pure spirit could never attain to know as much as the first elements of plane geometry.

Another source of confusion, which arises out of Berkeley's insufficient exactness in the use of language, is to be found in what he says about solidity, in discussing Molyneux's problem, whether a man born blind and having learned to distinguish between a cube and a sphere, could, on receiving his sight, tell the one from the other by vision. Berkeley agrees with Locke that he could not, and adds the following reflection :—

"Cube, sphere, table, are words he has known applied to things perceivable by touch, but to things perfectly intangible he never knew them applied. Those words in their wonted application always

marked out to his mind bodies or solid things which were perceived by the resistance they gave. But there is no solidity, no resistance or protrusion perceived by sight."

Here "solidity" means resistance to pressure, which is apprehended by the muscular sense; but when in section 154 Berkeley says of his pure intelligence—

"It is certain that the aforesaid intelligence could have no idea of a solid or quantity of three dimensions, which follows from its not having any idea of distance"—

he refers to that notion of solidity which may be obtained by the tactile sense, without the addition of any notion of resistance in the solid object; as, for example, when the finger passes lightly over the surface of a billiard ball.

Yet another source of difficulty in clearly understanding Berkeley arises out of his use of the word "outness." In speaking of touch he seems to employ it indifferently, both for the localization of a tactile sensation in the sensory surface, which we really obtain through touch; and for the notion of corporeal separation, which is attained by the association of muscular and tactile sensations. In speaking of sight, on the other hand, Berkeley employs "outness" to denote corporeal separation.

When due allowance is made for the occasional looseness and ambiguity of Berkeley's terminology, and the accessories are weeded out of the essential parts of his famous Essay, his views may, I believe, be fairly and accurately summed up in the following propositions:—

1. The sense of touch gives rise to ideas of extension, figure, magnitude, and motion.

2. The sense of touch gives rise to the idea of "outness," in the sense of localization.

3. The sense of touch gives rise to the idea of resist-

ance, and thence to that of solidity, in the sense of impenetrability.

4. The sense of touch gives rise to the idea of "outness," in the sense of distance in the third dimension, and thence to that of space, or geometrical solidity.

5. The sense of sight gives rise to ideas of extension, of figure, magnitude, and motion.

6. The sense of sight does not give rise to the idea of "outness," in the sense of distance in the third dimension, nor to that of geometrical solidity, no visual idea appearing to be without the mind, or at any distance off (§§ 43, 50).

7. The sense of sight does not give rise to the idea of mechanical solidity.

8. There is no likeness whatever between the tactile ideas called extension, figure, magnitude, and motion, and the visual ideas which go by the same names; nor are any ideas common to the two senses.

9. When we think we see objects at a distance, what really happens is that the visual picture suggests that the object seen has tangible distance; we confound the strong belief in the tangible distance of the object with actual sight of its distance.

10. Visual ideas, therefore, constitute a kind of language, by which we are informed of the tactile ideas which will, or may, arise in us.

Taking these propositions into consideration *seriatim*, it may be assumed that everyone will assent to the first and second; and that for the third and fourth we have only to include the muscular sense under the name of sense of touch, as Berkeley did, in order to make it quite accurate. Nor is it intelligible to me that anyone should explicitly deny the truth of the fifth proposition, though some of Berkeley's supporters, less careful than himself, have done so. Indeed, it must be confessed that it is

only grudgingly, and as it were against his will, that Berkeley admits that we obtain ideas of extension, figure, and magnitude by pure vision, and that he more than half retracts the admission; while he absolutely denies that sight gives us any notion of outness in either sense of the word, and even declares that "no proper visual idea appears to be without the mind, or at any distance off." By "proper visual ideas," Berkeley denotes colours, and light, and shade; and, therefore, he affirms that colours do not appear to be at any distance from us. I confess that this assertion appears to me to be utterly unaccountable. I have made endless experiments on this point, and by no effort of the imagination can I persuade myself, when looking at a colour, that the colour is in my mind, and not at a "distance off," though of course I know perfectly well, as a matter of reason, that colour is subjective. It is like looking at the sun setting, and trying to persuade oneself that the earth appears to move and not the sun, a feat I have never been able to accomplish. Even when the eyes are shut, the darkness of which one is conscious, carries with it the notion of outness. One looks, so to speak, into a dark space. Common language expresses the common experience of mankind in this matter. A man will say that a smell is in his nose, a taste in his mouth, a singing in his ears, a creeping or a warmth in his skin; but if he is jaundiced, he does not say that he has yellow in his eyes, but that everything looks yellow; and if he is troubled with *muscæ volitantes*, he says, not that he has specks in his eyes, but that he sees specks dancing before his eyes. In fact, it appears to me that it is the special peculiarity of visual sensations, that they invariably give rise to the idea of remoteness, and that Berkeley's dictum ought to be reversed. For I think that anyone who interrogates his consciousness carefully will find that

"every proper visual idea" appears to be without the mind and at a distance off.

Not only does every *visibile* appear to be remote, but it has a position in external space, just as a *tangibile* appears to be superficial and to have a determinate position on the surface of the body. Every *visibile*, in fact, appears (approximately) to be situated upon a line drawn from it to the point of the retina on which its image falls. It is referred outwards, in the general direction of the pencil of light by which it is rendered visible, just as, in the experiment with the stick, the *tangibile* is referred outwards to the end of the stick.

It is for this reason that an object, viewed with both eyes, is seen single and not double. Two distinct images are formed, but each image is referred to that point at which the two optic axes intersect; consequently, the two images exactly cover one another, and appear as completely one as any other two exactly similar superimposed images would be. And it is for the same reason, that, if the ball of the eye is pressed upon at any point, a spot of light appears apparently outside the eye, and in a region exactly opposite to that in which the pressure is made.

But while it seems to me that there is no reason to doubt that the extradition of sensation is more complete in the case of the eye than in that of the skin, and that corporeal distinctness, and hence space, are directly suggested by vision, it is another, and a much more difficult question, whether the notion of geometrical solidity is attainable by pure vision; that is to say, by a single eye, all the parts of which are immoveable. However this may be, for an absolutely fixed eye, I conceive there can be no doubt in the case of an eye that is moveable and capable of adjustment. For, with the moveable eye, the muscular sense comes into play in exactly the

same way as with the moveable hand; and the notion of change of place, *plus* the sense of effort, gives rise to a conception of visual space, which runs exactly parallel with that of tangible space. When two moveable eyes are present, the notion of space of three dimensions is obtained in the same way as it is by the two hands, but with much greater precision.

And if, to take a case similar to one already assumed, we suppose a man deprived of every sense except vision, and of all motion except that of his eyes, it surely cannot be doubted that he would have a perfect conception of space; and indeed a much more perfect conception than he who possessed touch alone without vision. But of course our touchless man would be devoid of any notion of resistance; and hence space, for him, would be altogether geometrical and devoid of body.

And here another curious consideration arises, what likeness, if any, would there be between the visual space of the one man, and the tangible space of the other?

Berkeley, as we have seen (in the eighth proposition), declares that there is no likeness between the ideas given by sight and those given by touch; and one cannot but agree with him, so long as the term ideas is restricted to mere sensations. Obviously, there is no more likeness between the feel of a surface and the colour of it, than there is between its colour and its smell. All simple sensations, derived from different senses, are incommensurable with one another, and only gradations of their own intensity are comparable. And thus so far as the primary facts of sensation go, visual figure and tactile figure, visual magnitude and tactile magnitude, visual motion and tactile motion, are truly unlike, and have no common term. But when Berkeley goes further than this, and declares that there are no "ideas" common to the "ideas" of touch and those of sight, it appears to

me that he has fallen into a great error, and one which is the chief source of his paradoxes about geometry.

Berkeley in fact employs the word "idea" in this instance to denote two totally different classes of feelings, or states of consciousness. For these may be divided into two groups: the primary feelings, which exist in themselves and without relation to any other, such as pleasure and pain, desire, and the simple sensations obtained through the sensory organs; and the secondary feelings, which express those relations of primary feelings which are perceived by the mind; and the existence of which, therefore, implies the pre-existence of at least two of the primary feelings. Such are likeness and unlikeness in quality, quantity, or form; succession and contemporaneity; contiguity and distance; cause and effect; motion and rest.

Now it is quite true that there is no likeness between the primary feelings which are grouped under sight and touch; but it appears to me wholly untrue, and indeed absurd, to affirm that there is no likeness between the secondary feelings which express the relations of the primary ones.

The relation of succession perceived between the visible taps of a hammer, is, to my mind, exactly like the relation of succession between the tangible taps; the unlikeness between red and blue is a mental phenomenon of the same order as the unlikeness between rough and smooth. Two points visibly distant are so, because one or more units of visible length (*minima visibilia*) are interposed between them; and as two points tangibly distant are so, because one or more units of tangible length (*minima tangibilia*) are interposed between them, it is clear that the notion of interposition of units of sensibility, or *minima sensibilia*, is an idea common to the two. And whether I see a point move across the

field of vision towards another point, or feel the like motion, the idea of the gradual diminution of the number of sensible units between the two points appears to me to be common to both kinds of motion.

Hence, I conceive, that though it be true that there is no likeness between the primary feelings given by sight and those given by touch, yet there is a complete likeness between the secondary feelings aroused by each sense.

Indeed, if it were not so, how could Logic, which deals with those forms of thought which are applicable to every kind of subject-matter, be possible? How could numerical proportion be as true of *visibilia*, as of *tangibilia*, unless there were some ideas common to the two? And to come directly to the heart of the matter, is there any more difference between the relations between tangible sensations which we call place and direction, and those between visible sensations which go by the same name, than there is between those relations of tangible and visible sensations which we call succession? And if there be none, why is Geometry not just as much a matter of *visibilia* as of *tangibilia*?

Moreover, as a matter of fact, it is certain that the muscular sense is so closely connected with both the visual and the tactile senses, that, by the ordinary laws of association, the ideas which it suggests must needs be common to both.

From what has been said it will follow that the ninth proposition falls to the ground; and that vision, combined with the muscular sensations produced by the movement of the eyes, gives us as complete a notion of corporeal separation and of distance in the third dimension of space, as touch, combined with the muscular sensations produced by the movements of the hand, does. The tenth proposition seems to contain a perfectly true statement,

but it is only half the truth. It is no doubt true that our visual ideas are a kind of language by which we are informed of the tactile ideas which may or will arise in us; but this is true, more or less, of every sense in regard to every other. If I put my hand in my pocket, the tactile ideas which I receive prophesy quite accurately what I shall see—whether a bunch of keys or half-a-crown—when I pull it out again; and the tactile ideas are, in this case, the language which informs me of the visual ideas which will arise. So with the other senses: olfactory ideas tell me I shall find the tactile and visual phenomena called violets, if I look for them; taste tells me that what I am tasting will, if I look at it, have the form of a clove; and hearing warns me of what I shall, or may, see and touch every minute of my life.

But while the "New Theory of Vision" cannot be considered to possess much value in relation to the immediate object its author had in view, it had a vastly important influence in directing attention to the real complexity of many of those phenomena of sensation, which appear at first to be simple. And even if Berkeley was, as I imagine he was, quite wrong in supposing that we do not see space, the contrary doctrine makes quite as strongly for his general view, that space can be conceived only as something thought by a mind.

The last of Locke's "primary qualities" which remain to be considered is mechanical solidity, or impenetrability. But our conception of this is derived from the sense of resistance to our own effort, or active force, which we meet with in association with sundry tactile or visual phenomena; and, undoubtedly, active force is inconceivable except as a state of consciousness. This may sound paradoxical; but let anyone try to realize what he means by the mutual attraction of two particles, and I think he will find, either, that he conceives them

simply as moving towards one another at a certain rate, in which case he only pictures motion to himself, and leaves force aside; or, that he conceives each particle to be animated by something like his own volition, and to be pulling as he would pull. And I suppose that this difficulty of thinking of force except as something comparable to volition, lies at the bottom of Leibnitz's doctrine of monads, to say nothing of Schopenhauer's "Welt als Wille und Vorstellung;" while the opposite difficulty of conceiving force to be anything like volition, drives another school of thinkers into the denial of any connection, save that of succession, between cause and effect.

To sum up. If the materialist affirms that the universe and all its phenomena are resolvable into matter and motion, Berkeley replies, True; but what you call matter and motion are known to us only as forms of consciousness; their being is to be conceived or known; and the existence of a state of consciousness, apart from a thinking mind, is a contradiction in terms.

I conceive that this reasoning is irrefragable. And therefore, if I were obliged to choose between absolute materialism and absolute idealism, I should feel compelled to accept the latter alternative. Indeed, upon this point Locke does, practically, go as far in the direction of idealism as Berkeley, when he admits that "the simple ideas we receive from sensation and reflection are the boundaries of our thoughts, beyond which the mind, whatever efforts it would make, is not able to advance one jot."—Book II. chap. xxiii. § 29.

But Locke adds, "Nor can it make any discoveries when it would pry into the nature and hidden causes of these ideas."

Now, from this proposition, the thorough materialists dissent as much, on the one hand, as Berkeley does, upon the other hand.

The thorough materialist asserts that there is a something which he calls the "substance" of matter; that this something is the cause of all phenomena, whether material or mental; that it is self-existent and eternal, and so forth.

Berkeley, on the contrary, asserts with equal confidence that there is no substance of matter, but only a substance of mind, which he terms spirit; that there are two kinds of spiritual substance, the one eternal and uncreated, the substance of the Deity, the other created, and, once created, naturally eternal; that the universe, as known to created spirits, has no being in itself, but is the result of the action of the substance of the Deity on the substance of those spirits.

In contradiction to which bold assertion, Locke affirms that we simply know nothing about substance of any kind.[1]

"So that if anyone will examine himself concerning his notion of pure substance in general, he will find he has no other idea of it at all, but only a supposition of he knows not what support of such qualities, which are capable of producing simple ideas in us, which qualities are commonly called accidents.

"If anyone should be asked, what is the subject wherein colour or weight inheres? he would have nothing to say but the solid extended parts; and if he were demanded what is it that solidity and extension inhere in? he would not be in much better case than the Indian before mentioned, who, urging that the world was supported by a great elephant, was asked what the elephant rested on? to which his answer was, a great tortoise. But being again pressed to know what gave support to the broad-backed tortoise? replied, something, he knew

[1] Berkeley virtually makes the same confession of ignorance, when he admits that we can have no idea or notion of a spirit ("Principles of Human Knowledge," § 138); and the way in which he tries to escape the consequences of this admission, is a splendid example of the floundering of a mired logician.

not what. And thus here, as in all other cases when we use words without having clear and distinct ideas, we talk like children, who, being questioned what such a thing is, readily give this satisfactory answer, that it is something; which in truth signifies no more when so used, either by children or men, but that they know not what, and that the thing they pretend to talk and know of is what they have no distinct idea of at all, and are, so, perfectly ignorant of it and in the dark. The idea, then, we have, to which we give the general name substance, being nothing but the supposed but unknown support of those qualities we find existing, which we imagine cannot exist *sine re substante*, without something to support them, we call that support *substantia*, which, according to the true import of the word, is, in plain English, standing under or upholding."[1]

I cannot but believe that the judgment of Locke is that which Philosophy will accept as her final decision.

Suppose that a piano were conscious of sound, and of nothing else. It would become acquainted with a system of nature entirely composed of sounds, and the laws of nature would be the laws of melody and of harmony. It might acquire endless ideas of likeness and unlikeness, of succession, of similarity and dissimilarity, but it could attain to no conception of space, of distance, or of resistance; or of figure, or of motion.

The piano might then reason thus: All my knowledge consists of sounds and the perception of the relations of sounds; now the being of sound is to be heard; and it is inconceivable that the existence of the sounds I know, should depend upon any other existence than that of the mind of a hearing being.

This would be quite as good reasoning as Berkeley's, and very sound and useful, so far as it defines the limits of the piano's faculties. But for all that, pianos have an existence quite apart from sounds, and the auditory consciousness of our speculative piano would be dependent, in the first place, on the existence of a "substance" of brass, wood, and iron, and, in the second, on that of a

[1] Locke, "Human Understanding," Book II. chap. xxiii. § 2.

musician. But of neither of these conditions of the existence of his consciousness would the phenomena of that consciousness afford him the slightest hint.

So that while it is the summit of human wisdom to learn the limit of our faculties, it may be wise to recollect that we have no more right to make denials, than to put forth affirmatives, about what lies beyond that limit. Whether either mind, or matter, has a "substance" or not, is a problem which we are incompetent to discuss; and it is just as likely that the common notions upon the subject should be correct as any others. Indeed, Berkeley himself makes Philonous wind up his discussions with Hylas, in a couple of sentences which aptly express this conclusion :—

"You see, Hylas, the water of yonder fountain, how it is forced upwards in a round column to a certain height, at which it breaks and falls back into the basin from whence it rose; its ascent as well as its descent proceeding from the same uniform law or principle of gravitation. Just so, the same principles which, at first view, lead to scepticism, pursued to a certain point, bring men back to common sense."

THE END.

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