INAUGURAL ADDRESS OF THE PRESIDENT, THOMAS H. HUXLEY, LL.D., F.R.S., ETC., BEFORE THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

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 direction 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 endeavor 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 endeavor 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 mold; 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." The axiom of ancient science, "that 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 widespread 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."

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 widespread 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 now-a-days be rendered "a vegetable 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. In fact, 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 cotemporary, 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 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 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 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 the gauze will not keep away aeriform bodies or fluids. This something must, therefore, exist in the form of solid particles too big to get through the gauze. Nor is one left in doubt what these solid particles are; for the blowflies, attracted by the odor 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 guaze. 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 on the model furnished by the Italian philosopher. As the results of his experiments were the same, however varied the nature of the material 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. And thus the hypotbesis that living 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 impunging the authority of the Scriptures; for his adversaries declared that the generation of bees from the carcass of a dead lion is affirmed, in the book of Judges, to have been the origin of the famous riddle with which Sampson 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 of 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.

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 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.

This 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, Lyonet, Vallisnieri, Reaumur, 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.

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 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 $\frac{1}{25}$ of an inch in diameter has, at ten inches from the eye, the same apparent size as an object $\frac{1}{1000}$ of an inch in diameter, when magnified 400 times; but forms of living matter abound the diameter of which is not more than $\frac{1}{4000}$ 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 $\frac{1}{3200}$ 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 all 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 that day, Buffon and Needham doubted the applicability of Redi's hypothesis to the infusorial animalcules, and Needham very properly endeavored 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 holds 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 of 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 hppothesis 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 a mold 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 association 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 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, that 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, no animalcules ever made their appearance within them. It must be admitted that the experiments and arguments of Spallanzani furnish a complete and 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 marvelous 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 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.

Cotemporaneously 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 or 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 microscopial 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 at 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 a development of these organisms must lie in something which cannot pass through membrane; 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 cotten-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 Abiogenesists that, if the doctrine of Biogeny is true, the air must be thick with germs; and they regard this as the hight of absurdity. But nature is occasionally 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 stirabout of excessively minute solid particles; but these particles are almost wholly destructible by heat; and that they are strained off, and the air rendered optically pure, by 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, 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 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 had 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 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 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 mold 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 an 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 mold *fungi*. And I have not yet been able to meet with any advocate of Abiogenesis who seriously 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.

[Remainder next week].

Another Case of Spontaneous Combustion.

The recent great fire in Chicago is now supposed to have been spontaneously originated in a bundle of greasy rags. How long will it be before people generally understand that such rags are dangerous? The general carelessness in the storage of these and similar dangerous substances is only equaled by that in the domestic use of matches. We saw a business man the other day throw without thinking an unextinguished match into his paper waste basket. We not unfrequently step on matches in walking through public buildings or on the ferry boats which detonate under our feet. How many men, women, or children when they drop a match never think of stooping to pick it up, but take a new one from the box, rather than subject themselves to a slight inconvenience, which might perhaps prevent the destruction of thousands of dollars' worth of property. To always extinguish matches before throwing them away, and always pick them up when dropped, are habits which should be taught to every child.

THE MANUFACTURE OF SOLUBLE GLASS.

[From Feuchtwanger's "Treatise on Soluble Glass."]

The potash soluble glass is obtained by mixing 15 parts powdered quartz or pure sand with 10 parts purified pearl ashes, and 1 part charcoal in a Hessian crucible, and exposing the mixture so long to a heat until the mass after six hours has become vitrified. Charcoal is employed for assisting, by its decomposition, the production of carbonic acid, as also some sulphuric acid which may have been produced. It is at present, however, omitted, and if manufactured on a large scale the vitrification is done in a reverberatory furnace capable of holding from 1,200 to 1,500 pounds. The ashes and sand must be well mixed togther for some time and the furnace must be very hot before throwing the mixture in it, and the heat must be constantly kept up until the entire mass is in a liquid condition. The tough mass is then raked out and thrown upon a stone hearth and left to cool. The glass mass so obtained appears to be hard and blistery, of blackish gray color, and if the ashes were not quite pure it will also be adulterated with foreign salts. By pulverizing and exposing it to the air it will absorb the acidity, and by degrees the foreign salts will, after frequent agitation and stirring, be completely separated, particularly after pouring over the mass some cold water, which dissolves them, but not the soluble glass. The purified mass is now put into an iron cauldron, containing five times the quantity of hot water, in small portions, and with constant agitation, and replacing occasionally hot water for that which evaporated during the boiling, and after five or six hours the entire mass is dissolved; the liquid is removed and left to settle over night, in order to be able to separate any undecomposed silex. The next day it is evaporated still more until it has assumed the consistency of a sirup, and standing 28° B. and is composed of 28 per cent potash, 62 per cent silica, and 12 per cent water. It has an alkaline taste, and is soluble in all proportions of water, and is precipitated by alcohol, and if any salts do effervesce they may be wiped off. The color is not quite white, but assumes a greenish or yellowish white color.

MANUFACTURE OF SODA SOLUBLE GLASS.

To 45 parts silica or white river sand are added 23 parts carbonate of soda fully calcined, and 3 parts charcoal, and is then treated in the same manner as the other glass. The proportions of the mixture are altered by the different manufacturers, some propose to 100 parts silex, 60 parts anhydrous glauber salt, and 15 to 20 parts charcoal. By the addition of some copper scales to the mixture, the sulphur will be separated. Another method is proposed by dissolving the fine silex in caustic soda lye. Kuhlman employs the powdered flint, which is dissolved in an iron cauldron under a pressure of 7 to 8 atmospheres. According to Liebig the infusorial earth is recommended in place of sand on account of being readily soluble in caustic lye, and he proposes to use 120 parts infusorial earth to 75 parts caustic soda, from which 240 parts silica jelly may be obtained. His mode is to calcine the earth so as to become of white colors, and passing it through sieves. The lye he prepares from 75 ounces calcined soda, dissolved in five times the quantity of boiling water, and then treated by 56 ounces of dry slacked lime; this lye is concentrated by boiling down to 48 deg. B.; in this boiling lye 120 ounces of the prepared infusorial earth are added by degrees, and very readily dissolved, leaving scarcely any sediment. It has then to undergo several operations for making it suitable for use, such as treating again with lime water, boiling it, and separate any precipate forming thereby, which by continued boiling forms into balls, and which can then be separated from the liquid. This clear liquid is then evaporated to consistency of sirup, forms a jelly slightly colored, feels dry and not sticky, and is easily soluble in boiling water.

The difference between potash and soda soluble glass is not material; the first may be preferred in whitewashing with plaster of Paris, while the soda glass is more fluidly divisible.

It may be observed that before applying either soluble glass, it ought to be exposed to the air for ten to twelve days, in order to allow an efflorescence of any excess of alkali, which might act injuriously.

DOUBLE SOLUBLE GLASS.

This is a compound of potash and soda, and is prepared from 100 parts quartz, 28 parts purified pearl ashes, 22 parts anhydrous bicarbonate of soda, and 6 parts of charcoal, which are spread in such manner as already described. If the mass is fully evaporated to dryness, it forms a vitreous solid glass which cannot be scratched by steel, has a conchoidal fracture, of sea-green color, translucent and even transparent, and has specific gravity of 1·43.

Soluble glass, after Kaulbach, for the use of sterro-chromic painting, is obtained by fusing 3 parts of pure carbonate soda and 2 parts powdered quartz, from which a concentrated solution is prepared, and 1 part of which is then added to 4 parts of a concentrated and fully saturated solution of potash glass solution, by which it assumes a more condensed amount of silica with the alkalies; and which solution has been found to work well for paint. Siemen's patent for the manufacture of soluble glass, consists in the production of a *liquid quartz* by digesting the sand or quartz in a steam boiler tightly closed and at a temperature corresponding to 4–5 atmospheres, with the common caustic alkalies, which are hereby capacitated to dissolve from three to four times the weight of silica to a thin liquid. The apparatus, which was patented in 1845, is well known in this country; as some persons, many years later, obtained a patent for the same apparatus in the United States, which on inspection does not differ from that of Siemens Brothers.