In a note on "Colonial Grasses as Paper-making Materials," the *Colonies and India* suggests the possibility of utilising some of the coarse grasses which grow with such provoking pertinacity in South Africa, Australia, New Zealand, &c. The *Typha angustifolia*, for example, a large kind of tussock grass (known as *raupo* to the New Zealand natives, who use it for thatching their houses), which grows in enormous quantities in the swampy flats near rivers and lakes, may, like its neighbour, the *Phormium tenax*, prove a rival to Esparto grass; the *wiwi*, a coarse, wiry kind of grass, growing chiefly in the interior of North Island, is also worth an experiment. In New South Wales the grass-cloth plant (*Böhmeria nivea*) has already received some attention, being used for the manufacture of a fine kind of matting. South Africa is probably richest of all in its grasses; in the great Karroo district thousands of square miles are covered with the twaa-grass, the sour-veldt, and the sweet-veldt, the importance of which as fodder may be found equalled by their value as paper-making material. Still more likely to prove valuable is the *Stipa capensis*, a member of the family to which Esparto belongs.

The *Transactions* of the Cumberland Association for the Advancement of Literature and Science, Part III., 1877-78, edited by Mr. Clifton Ward, is a thickish volume containing papers by members of some of the Associated Societies. The first paper, however, after various reports, is that by Sir George Airy, on the "Probable Condition of the Interior of the Earth," a report of which we gave at the time of its delivery; accompanying it is a diagram of an ideal earth. Mr. Ward has a paper on "Quartz in the Lake District;" Mr. C. Smith one on "Boulder Clay;" Mr. Pickering, on a "Submerged Forest at St. Bees;" and Mr. Fisher Crosthwaite gives an interesting account of Peter Crosthwaite, who, at the end of the last and beginning of the present centuries, did much to promote science in the district.

M. J. Poliakoff, who was sent last summer by the St. Petersburg Academy of Sciences to examine the remains of the stone period in the governments of Yaroslaff and Vladimir, gives the following results of his explorations:—Very interesting collections were found in excavating a mound, close by Yaroslaff; numerous skulls of men of the neolithic period were discovered here, together with polished silex hatchets and hammers, and numerous bones of animals of existing species. Far richer collections were found in the valley of the Oka River, in the district of Murom. Here, in the sandy mounds of the valley, as well as in the alluvium of the river, M. Poliakoff has discovered immense quantities of silex implements, polished and rough, of the most varied forms. The implements were always found together with bones of the *Castor fiber*, the *Sus scrofa ferus*, and the *Bos primigenius*, none of which exist now in those regions. Besides, he also discovered vestiges of old wood buildings, very like the lacustrian dwellings of Switzerland. The most important discovery during these explorations was made by M. Poliakoff, in company with Count Uraroff, close by Karacharovo Town, in a very old lake alluvium, being a somewhat washed-up glacier deposit. Here they found rough stone implements of the paleolithic period, together with bones of the mammoth, rhinoceros, and the *Bos priscus*. The character of the deposits proved without doubt the co-existence of man with those extinct mammals in Russia, as well as in other parts of Europe. After having finished his explorations, M. Poliakoff made a journey in Western Europe to study the chief museums, and to compare the implements he has collected during many years in Russia and Siberia, Western and Eastern, with those of England, Sweden, Denmark, France, and Switzerland. We expect that this last journey of M. Poliakoff will accelerate the opening of the projected pre-historic museum at St. Petersburg.

The additions to the Zoological Society's Gardens during the past week include two Black-faced Spider Monkeys (*Ateles ater*), two Rufous-vented Guans (*Penelope cristata*) from U. S. of Columbia, two Horsfield's Tortoises (*Testudo horsfieldi*) from Turkestan, presented by Mr. A. Gonzalez Carazo; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. A. G. Lytton Squires; two Black-eared Marmosets (*Hapale penicillata*) from South-East Brazil, presented by the Countess of Cotterham; two Laughing Kingfishers (*Dacelo gigantea*) from Australia, presented by Mr. Edwin A. B. Crockett; a Ceylon Jungle Fowl (*Gallus stanleyi*) from Ceylon, two Japanese Pheasants (*Phasianus versicolor*) from Japan, a Grey Francolin (*Francolinus ponticerianus*) from India, presented by Mr. Geo. Lyon Bennett; a Rhomb-marked Snake (*Psammophylax rhombeatus*), three Rufescent Snakes (*Leptodira rufescens*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Kinkajou (*Cercoleptes caudivolvulus*) from South America, three Snow Buntings (*Plectrophanes nivalis*), European, purchased.

ROYAL SOCIETY—THE PRESIDENT'S ANNIVERSARY ADDRESS[1]

II.

The modern development of botanical science, being that which occupies my own attention, is naturally that on which I might feel especially inclined to dwell; and I should so far have the excuse that there is, perhaps, no branch of research with the early progress of which this Society is more intimately connected.

One of our earliest secretaries, Robert Hooke, two centuries ago, laboured long and successfully in the improvement of the microscope as an implement of investigation. He was one of the first to reap the harvest of discovery in the new fields of knowledge to which it was the key, and if the results which he attained have rather the aimless air of spoils gathered hither and thither in a treasury, the very fulness of which was embarrassing, we must remember that we date the starting-point of modern histology from the account given by Hooke in his "Micrographia" (1667) of the structure of cork, which had attracted his interest from the singularity of its physical properties. Hooke demonstrated its *cellular structure*, and by an interesting coincidence he was one of the first to investigate, at the request, indeed, of the founder of the Society, Charles II., the movement of the sensitive plant *Mimosa pudica*, one of a class of phenomena which is still occupying the attention of more than one of our Fellows. In attributing the loss of turgescence, which is the cause of the collapse of the petiole and subordinate portions of the compound leaf which it supports, to the escape of a subtle humour, he to some extent foreshadowed the modern view which attributes the collapse of the cells to the escape of water by some mechanism far from clearly understood —whether from the cell-cavities or from the cell-walls into the intercellular spaces.

Hooke having shown the way, Nehemiah Grew, who was also secretary of the Royal Society, and Marcello Malpighi, Professor of Medicine in the University of Bologna, were not slow to follow it. Almost simultaneously (1671-3) the researches of these two indefatigable students were presented to the Royal Society, and the publication of two editions of Malpighi's works in London prove how entirely this country was at that time regarded as the head-quarters of this branch of scientific inquiry. We owe to them the generalisation of the cellular structure, which Hooke had ascertained in cork, for all other vegetable tissues. They described also accurately a host of microscopic structures then made known for the first time. Thus, to give one example, Grew figured and described in several different plants the stomata of the epidermis:—"Passports either for the better avolation of superfluous sap, or the admission of air."

With the exception of Leeuwenhoek no observer attempted to make any substantial addition to the labours of Grew and Malpighi for more than a century and a half, and however remarkable is the impulse which he gave to morphological studies, the view of Caspar Wolff in the middle of the eighteenth cen-

[1] Address of Sir Joseph Hooker, C.B., K.C.S.I., the President, delivered at the Anniversary Meeting of the Royal Society, on Saturday, November 30, 1878. Continued from p. 113.

tury (1759), in regarding cells as the result of the action of an organising power upon a matrix, and not as themselves influencing organisation, were adverse to the progress of histology. It is from Schleiden (1838), who described the cell as the true unit of vegetable structure, and Schwann, who extended this view to all organisms whether plants or animals, and gave its modern basis to biology by reasserting the unity of organisation throughout animated nature, that we must date the modern achievements of histological science. Seldom, perhaps, in the history of science has any one man been allowed to see so magnificent a development of his ideas in the space of his own lifetime as has slowly grown up before the eyes of the venerable Schwann, and it was, therefore, with peculiar pleasure that a letter of congratulation was intrusted by the officers to one of the Fellows of this Society on its behalf on the recent occasion of the celebration of the fortieth anniversary of Schwann's entry into the professoriate.

If we call up in our mind's eye some vegetable organism and briefly reflect on its construction, we see that we may fix on three great steps in the analysis of its structure, the organic, the microscopic, and the molecular, and, although not in the same order, each of the three last centuries is identified with one of these. In the seventeenth century Grew achieved the microscopic analysis of plant tissues into their constituent cells; in the 18th, Caspar Wolff effected the organic analysis (independently but long subsequently expounded by the poet Goethe) of plant structures into stem and leaf. It remained for Nägeli in the present century to first lift the veil from the mysterious processes of plant growth, and by his memorable theory of the molecular constitution of the starch-grain and cell-wall, and their growth by intussusception (1858), to bring a large class of vital phenomena within the limits of physical interpretation. Strasburger has lately (1876) followed Sachs in extending Nägeli's views to the constitution of protoplasm itself, and there is now reason to believe that the ultimate structure of plants consists universally of solid molecules (not, however, identical with chemical molecules) surrounded with areas of water which may be extended or diminished. While the molecules of all the inert parts of plants (starch-grains, cell-wall, &c.) are on optical grounds believed to have a definite crystalline character, no such conclusion can be arrived at with respect to the molecules of protoplasm. In these molecules the characteristic properties of the protoplasm reside, and are more marked in the aggregate mass in proportion to its denseness, and this is due to the close approximation of the molecules and the tenuity of their watery envelopes. The more voluminous the envelopes the more the properties of protoplasm merge in those of all other fluids.

It is, however, to the study of the nuclei of cells that attention has been recently paid with the most interesting results. These well-known structures, first observed by Ferdinand Bauer at the beginning of the century (1802), were only accurately described, thirty years later, by Robert Brown (1833). Up to the present time their function has been extremely obscure. The beautiful investigations of Strasburger (1875) have led him to the conclusion that the nucleus is the seat of a central force which has a kind of polarising influence upon the protoplasm molecules, causing them to arrange themselves in lines radiating outwards. Cell-division he regards as primarily caused by the nucleus becoming bipolar, and the so-called caryolitic figures first described by Auerbach exhibit the same arrangement of the protoplasm molecules in connecting curves as in the case of iron-filings about the two poles of a bar-magnet. The two new centres mutually retire, and each influencing its own tract of protoplasm, the cell-division is thereby ultimately effected. This is but a brief account of processes which are greatly complicated in actual detail, and of which it must be remarked that, while the interest and beauty of the researches are beyond question, caution must be exercised in receiving the mechanical speculations by which Strasburger attempts to explain them. He has himself shown that cell-division presents the same phenomena in the animal kingdom, a result which has been confirmed by numerous observers, amongst whom I may content myself with mentioning one of our own number, Mr. F. Balfour. Strasburger further points out that this affords an argument for the community of descent in animal and vegetable cells; he regards free cell-division as derivable from ordinary cell-division by the suppression of certain stages.

Turning now to the discoveries made during the last five years in physiological botany, we find that no one has advanced this subject so greatly as Mr. Darwin. In 1875 was published his work on insectivorous plants, in which he ascertained the fact that a number of species having elaborate structures adapted for the capture of insects, utilised the nitrogenous matter which these contain as food. The most important principle established in the course of these researches was that such plants as *Drosera*, *Dionæa*, *Pinguicula*, &c., secrete a digestive fluid, which has led, through Gorup Bezanez's investigations on the ferment in germinating seeds, to a recognition of the active agency of ferments in the transmission of food-material, which marks a great advance in our knowledge of the general physiology of nutrition.

The extreme sensitiveness of the glands of *Drosera* to mechanical and chemical stimulus (especially to phosphate of ammonia), the directive power of its tentacles, depending upon the accurate transmission of motor impulses, and the "reflex" excitation of secretion in the glands, were all discoveries of the most suggestive nature in connection with the subject of the irritability and movements of plants. The phenomenon of the aggregation of the protoplasmic cell-contents in the tentacles of *Drosera* is a discovery of a highly remarkable nature, though not yet thoroughly understood. Lastly, Mr. Frank Darwin, following his father's footsteps, as it were crowned the edifice by showing to what an extent insectivorous plants do profit by nitrogenous matter supplied to their leaves.

In close relation to these researches are those, also by Mr. Darwin, on the structure and functions of the bladder of *Utricularia*, which he has shown to have the power of absorbing decaying animal matter; and those of Mr. Frank Darwin on contractile filaments of extraordinary tenuity attached to the glands on the inner surface of the cups formed by the connate bases of the leaves of the teasel, and which filaments exhibit motions suggesting a protoplasmic origin. It is to be hoped that their discoverer will pursue his investigations upon these curious bodies, whose origin and real nature in relation to the plant and its functions is involved in obscurity.

The subject of the cross-fertilisation of plants, which, though a long-known phenomenon, first become a fruitful scientific study in Mr. Darwin's now classical work, "On the Various Contrivances by which Orchids are Fertilised," has within the last few years made rapid advance under its author's hand. The extreme importance of avoiding self-fertilisation might indeed be inferred from the prevalence in flowers of elaborate contrivances for preventing it; but it remained to be shown that direct benefit attended cross-fertilisation, and this has now been proved by an elaborate series of experiments, the results of which are not only that both increased fertility or greater vigour of constitution attend cross-fertilisation, but that the opposite effects attend self-fertilisation. In the course of these experiments it became evident that the good effects of the cross do not depend on the mere fact of the parents being different individuals, for when these were grown together and under the same conditions, no advantage was gained by the progeny; but when grown under different conditions a manifest advantage was gained. As instances, if plants of *Ipomœa* and *Mimulus*, which had been self-fertilised for seven previous generations, were kept together and then intercrossed, their offspring did not profit in the least; whereas, when the parent plants were grown under different conditions, a remarkably vigorous offspring was obtained.

Mr. Darwin's last work, "On the Different Forms of Flowers," though professedly a reprint of his paper on dimorphic plants, published by the Linnean Society, contains many additions and new matter of great importance in reference to the behaviour of polygamous plants, and on cleistogamic flowers. Among other points of great interest is the establishment of very close analogies between the phenomena attending the illegitimate union of trimorphic plants and the results of crosses between distinct species: the sterile offspring of the crosses of the same species exhibiting the closest resemblance to the sterile hybrids obtained by crossing distinct species; while a whole series of generalisations, founded on the results of the one series of experiments, are closely paralleled by those founded on the other. The bearing of this analogy on the origin of species is obviously important.

Besides these investigations, Mr. Darwin has produced within the last five years second editions of his volume on the "Fertilisation of Orchids," and on the "Habits and Movements of Climbing Plants," as also of his early works on "Coral Reefs," and "Geological Observations in South America;" all of them abounding in new matter.

Of special interest to myself, as having been conducted in the

Jodrell Laboratory at Kew, are Dr. Burdon Sanderson's investigations on the exceptional property possessed by the leaves and other organs of some plants which exhibit definite movements in response to mechanical, chemical, or electric stimuli. In 1873 this physiologist showed us in our meeting-room that the closing of the laminæ of the leaf of *Dionæa* is preceded by a preliminary state of excitement, and is attended with a change in the electric conditions of the leaf; and this so closely resembled the change which attends the excitation of the excitable tissues of animals that he did not hesitate to identify the two phenomena.

This remarkable discovery immediately directed the attention of two German observers to the electromotive properties of plants, one, Dr. Kunkel, in the laboratory of Prof. Sachs; the other, Prof. Munk, in that of the University of Berlin.

Prof. Munk, whose researches are of much the greater scope and importance, took as his point of departure Dr. Burdon Sanderson's discovery. The leading conclusion to which he arrived was that in *Dionæa* each of the oblong cells of the parenchyma is endowed with electromotive properties which correspond with those of the "muscle-cylinder" of animals; with this exception, that whereas in the muscle-cylinder the ends are negative to the central zone, in the vegetable cell they are positive; and he endeavours to prove that according to this theory all the complicated electromotive phenomena which had been observed could be shown to be attributable to the peculiar arrangement of the leaf-cells.

During the last two summers Dr. Burdon Sanderson has been engaged in endeavouring to discover the true relations which subsist between the electrical disturbance followed by the shutting of the leaf-valves of *Dionæa* and the latent change of protoplasm which precedes this operation. He has found that though the mechanism of the change of form of the excitable parenchyma which causes the contraction is entirely different from that of muscular contraction, yet that the correspondence between the exciting process in the animal tissues and what represents this in plant tissues appears to be more complete the more carefully the comparison is made; and that whether the stimulus be mechanical, thermal, or electrical, its effects correspond in each case. Again, the excitation is propagated from the point of excitation to distant points in the order of their remoteness, and the degree to which the structure is excited depends upon its temperature. Notwithstanding, however, the striking analogies between the electrical properties of the cells of *Dionæa* and of muscle-cylinders, Dr. Burdon Sanderson is wholly unable to admit with Prof. Munk that these structures are in this respect comparable.

In morphological botany attention has been especially directed of late to the complete life-history of the lower order of cryptogams, since this is seen to be more and more an indispensable preliminary to any attempt at their correct classification.

The remarkable theory of Schwendener, now ten years old, astonished botanists by boldly sweeping away the claims to autonomous recognition of a whole group of highly characteristic organisms—the lichens—and by affirming that these consist of ascomycetal fungi united in a commensal existence with algæ. The controversial literature and renewed investigations which this theory has given rise to is now very considerable. But the advocates of the Schwendenerian view have gradually won their ground, and the success which has attended the experiments of Stahl in taking up the challenge of Schwendener's opponents, and manufacturing such lichens as *Endocarpon* and *Thelidium*, by the juxtaposition of the appropriate algæ and fungi, may almost be regarded as deciding the question. Sachs, in the last edition of his "Lehrbuch," has carried out completely the principle of classification of algæ, first suggested by Cohn, and has proposed one for the remaining thallophytes, which disregards their division into fungi and algæ. He looks upon the former as standing in the same relation to the latter as the so-called saprophytes (*e.g.* *Neottia*) do to ordinary green flowering-plants.

This view has especial interest with regard to the minute organisms known as *Bacteria*, a knowledge of the life-history of which is of the greatest importance, having regard to the changes which they effect in all lifeless and, probably, in all living matter prone to decomposition. This affords a morphological argument (as far as it goes) against the doctrine of spontaneous generation, since it seems extremely probable that just as yeast may be a degraded form of some higher fungus, *Bacteria* may be degraded allies of the *Oscillatoriæ*, which have adopted a purely saprophytal mode of existence.

Your *Proceedings* for the present year contain several important contributions to our knowledge of the lowest forms of life. The Rev. W. H. Dallinger, continuing those researches which his skill in using the highest microscopic powers and his ingenuity in devising experimental methods have rendered so fruitful, has adduced evidence which seems to leave no doubt that the spores or germs of the monad which he has described differ in a remarkable manner from the young or adult monads in their power of resisting heated fluids. The young and adult monads, in fact, were always killed by five minutes' exposure to a temperature of 142° F. (61° C.), while the spores germinated after being subjected to a temperature of 10° above the boiling-point of water (222° F.).

Two years ago, Cohn and Koch observed the development of spores within the rods of *Bacillus subtilis* and *B. anthracis*. These observations have been confirmed, with important additions, in these two species by Mr. Ewart, and have been extended to the *Bacillus* of the infectious pneumo-enteritis of the pig, by Dr. Klein; and to *Spirillum* by Messrs. Geddes and Ewart; and thus a very important step has been made towards the completion of our knowledge of the life-history of these minute but important organisms. Dr. Klein has shown that the infectious pneumo-enteritis, or typhoid fever of the pig, is, like splenic fever, due to a *Bacillus*. Having succeeded in cultivating this *Bacillus* in such a manner as to raise crops free from all other organisms, Dr. Klein inoculated healthy pigs with the fluid containing the *Bacilli*, and found that the disease in due time arose and followed its ordinary course. It is now, therefore, distinctly proved that two diseases of the higher animals, namely, "splenic fever" and "infectious pneumo-enteritis," are generated by a *contagium vivum*.

Finally, Messrs. Downes and Blunt have commenced an inquiry into the influence of light upon *Bacteria* and other *Fungi*, which promises to yield results of great interest, the general tendency of these investigations leaning towards the conclusion that exposure to strong solar light checks and even arrests the development of such organisms.

The practical utility of investigations relating to *Bacillus* organisms as affording to the pathologist a valuable means of associating by community of origin various diseases of apparently different character, is exemplified in the "Loodiana fever," which has been so fatal to horses in the East. The dried blood of horses that had died of this disease in India has been recently sent to the Brown Institution, and there afforded seed from which a crop of *Bacillus anthracis* has been grown, which justified its distant pathological origin by reproducing the disease in other animals. Other equally interesting experiments have been made at the same Institution, showing that the "grains" which are so largely used as food for cattle, afford a soil which is peculiarly favourable for the development and growth of the spore filaments of *Bacillus;* and that by such "grains" when inspected, the anthrax fever can be produced at will, under conditions so simple, that they must often arise accidentally. The bearing of this fact on a recent instance in which anthrax suddenly broke out in a previously uninfected district, destroying a large number of animals, all of which had been fed with grains obtained from a particular brewery, need scarcely be indicated.

In systematic botany, which, in a nation like ours, that is ever extending its dominions and exploring unknown regions of the globe, must always absorb a large share of the energies of its phytologists, I can but allude to two works of great magnitude and importance.

Of these the first is the "Flora Australiensis" of Bentham, completed only a year ago; a work which has well been called unique in botanical literature, whether for the vast area whose vegetation it embraces (the largest hitherto successfully dealt with), or for the masterly manner in which the details of the structure and affinities of upwards of 8,000 species have been elaborated; its value in reference to all future researches regarding the geographical distribution of plants, the southern hemisphere, and the evolution therein of generic and specific types, cannot be over-estimated.

The other great work is the "Flora Braziliensis," commenced by our late foreign Fellow, von Martius, and now ably carried on by Eichler, of Berlin, assisted by coadjutors (amongst whom are most of our leading systematists) under the liberal auspices of His Majesty the Emperor of Brazil. When completed, this gigantic undertaking will have embraced, in a systematic form, the vegetation of the richest botanical region of the globe.

Having now endeavoured to recall to you some of the great advances in science made during the last few years, it remains for me, after the distribution of the medals awarded by your Council, to retire from the Presidency in which I have so long experienced the generous support of your officers and yourselves. This support, for which I tender you my hearty thanks, together with my sense of the trust and dignity of the office, and the interest attached to its duties, has rendered my resignation of it a more difficult step than I had anticipated. My reasons are, however, strong. They are the pressure of official duties at Kew, which annually increase in amount and responsibility, together with the engagements I am under to complete scientific works, undertaken jointly with other botanists, before you raised me to the Presidency, and the indefinite postponement of which works delays the publication of the labours of my coadjutors. I am also influenced by the consideration that, though wholly opposed to the view that the term of the Presidency of the Royal Society should be either short or definitely limited, this term should not be very long; and that, considering the special nature of my own scientific studies, it should, in my case, on this as well as on other grounds, be briefer than might otherwise be desirable. Cogent as these reasons are they might not have been paramount were it not that we have among us one pre-eminently fitted to be your President by scientific attainments, by personal qualifications, and by intimate knowledge of the Society's affairs; and by calling upon whom to fill the proud position which I have occupied, you are also recognising the great services he has rendered to the Society as its treasurer for eight years, and its ofttimes munificent benefactor.

HAECKEL ON THE LIBERTY OF SCIENCE AND OF TEACHING[1]

II.

CHAPTER V. treats of the methods of teaching, and contrasts the *genetic* method, as advocated by Haeckel, with the *dogmatic* one recommended by Virchow. The sensation which Virchow's address caused in wider circles was only partly the result of his opposition to the descent theory; its principal cause was his surprising conclusions with regard to the liberty of teaching. Virchow demands that in the school—from the elementary school up to the university—*nothing should be taught which is not absolutely certain;* only *objective* but no *subjective* knowledge is to be communicated to the pupils by the teacher; only facts, no hypotheses. Haeckel remarks that rarely has an eminent representative of science made such an attack upon the liberty of science as did Virchow at Munich. "Where," he asks, "are we to find the limits between subjective and objective knowledge?" According to his conviction no such limit exists, and all human knowledge as such is subjective. "An objective science consisting only of facts, without subjective theories, cannot be imagined." He then proceeds to review various sciences in turn, and to point out how much objective knowledge and "facts," and how much subjective knowledge and "hypotheses" they contain. He begins with *Mathematics* as the science which is eminently the most *certain* one of all: "What about the simplest and deepest maxims upon the firm basis of which the whole proud building of mathematics rests? Can they be proved for certain? Certainly not! The most fundamental maxims are indeed 'maxims,' and incapable of 'proof.' Only in order to show by an example how even the first mathematical maxims may be attacked by sceptics and shaken by philosophical speculation we recall the recent discussions regarding the three dimensions of space and the possibility of a fourth dimension, discussions which are still continued by a number of the most illustrious mathematicians, physicists, and philosophers. So much is certain that mathematics is absolutely objective as little as any other science, but has a subjective basis in man's own nature. . . . But even if we own that mathematics is an absolutely certain and objective science, how about all other sciences? No doubt those are 'most certain' amongst the 'exact' sciences, the maxims of which are founded on pure mathematics, in the first line therefore a great part of *physics*. We say a great part, because another great part—upon close examination by far the greater—is incapable of an exact mathematical foundation. Or what we do know with certainty about the essence of *matter* or the essence of *force?* What do we know for certain about gravitation, about mass-attraction, about action at a distance, &c.? We look upon Newton's gravitation theory, the basis of mechanics, as the most important and most certain theory of physics, and yet gravitation itself is only a hypothesis. And then the other branches of physics—electricity and magnetism, for instance. The whole knowledge of these important branches is based upon the hypothesis of 'electric fluids' or of imponderable substances, the existence of which is certainly not proved. Or optics? No doubt optics belongs to the most important and most complete branches of physics, yet the vibration theory, which to-day we consider to be its indispensable basis, rests upon a hypothesis which cannot be proved, viz., upon the 'subjective' supposition of the light-ether, the existence of which nobody can objectively prove. Nay, even more; before Young established the vibration theory of light, the emanation theory taught by Newton reigned supreme in physics for centuries; this theory has to-day been abandoned as untenable. According to our view the mighty Newton acquired the greatest merit with regard to the development of optics, as he made the first attempt to connect and explain the mass of objective optical facts by a subjective leading hypothesis. But according to Virchow's view Newton sinned most heavily by teaching this false hypothesis; because in 'exact' physics only *single* and *certain facts* are to be taught and to be ascertained by 'experiment as the highest means of proof;' but physics as a *whole*, resting as it does upon a number of unproved hypotheses, may be the object of research, but must not be taught!" Turning to *Chemistry*, Haeckel shows that its objectiveness stands upon still weaker feet than that of physics. Here the whole of the science is built upon the hypothesis of the existence of atoms, a hypothesis as unproved and as incapable of proof as any. No chemist has ever seen an atom, and yet he thinks the mechanics of atoms the highest problem of his science, and describes and constructs the positions and groupings of atoms, as if they were before him on his dissecting table. According to Virchow, we therefore ought *to banish chemistry from the school* and teach only the properties of bodies and their reactions, which can be shown to the pupils as "certain facts." This matter becomes still more ludicrous when we turn to the other sciences, which are all more or less *historical*, and therefore do not possess that "half-exact" basis upon which chemistry and physics rest. Geology, for instance, would, according to Virchow, have to confine itself to the description of certain facts, *i.e.*, the structure of rocks, the forms of fossils, the shape of crystals, &c., but would in the school have completely to abandon all speculation regarding the development of the earth's crust, *i.e.*, nothing but unproved hypotheses from beginning to end. We might not even teach that fossils are the actual remains of organisms which existed in former periods, because even this is an "unproved" hypothesis. Even down to the eighteenth century many eminent naturalists believed fossils to be "freaks of nature," an enigmatic "*lusus naturæ*." In a later part of his address Virchow admits fossils as "objective material proofs;" but even here we may go no further than our actual experience allows, and we may not draw subjective deductions from the objective facts. Virchow's remark about quaternary man being an "accepted fact" affords Haeckel an opportunity for pointing out his inconsistency, and the uncertainty and vagueness of most hypotheses concerning the age and the first geological occurrence of man; indeed, the distinction of a tertiary and a quartery age in itself is nothing but a *geological hypothesis*. "Virchow tells us that never has a fossil ape skull been found which really belonged to a human proprietor, and that we cannot consider it as a revelation of science, we cannot teach, that man descends from the ape or from any other animal. If that be true, then nothing remains but the descent from a god or from a clod of earth."

Zoology, botany, and other biological doctrines do not fare better, if we consider them in the light in which Virchow would have them taught. Haeckel shows the utter untenability of Virchow's demands, since no science, not even history, and certainly not philosophy, could be tolerated in our schools; indeed, the only one which could remain would be theology. "Incredible as it seems, Virchow, the sceptical antagonist of dogmas, the combatant for the liberty of science, now finds the only certain basis of instruction in the dogma of Church religion. After all that has happened the following phrase leaves no doubt on this point:—'All attempts to transform our problems into doctrines, to introduce our theories as the basis of a plan of education, par-

[1] Freie Wissenschaft und freie Lehre. Eine Entgegnung auf Rudolf Virchow's Münchener Rede über "die Freiheit der Wissenschaft im modernen Staat." Von Ernst Haeckel. Continued from p. 115.