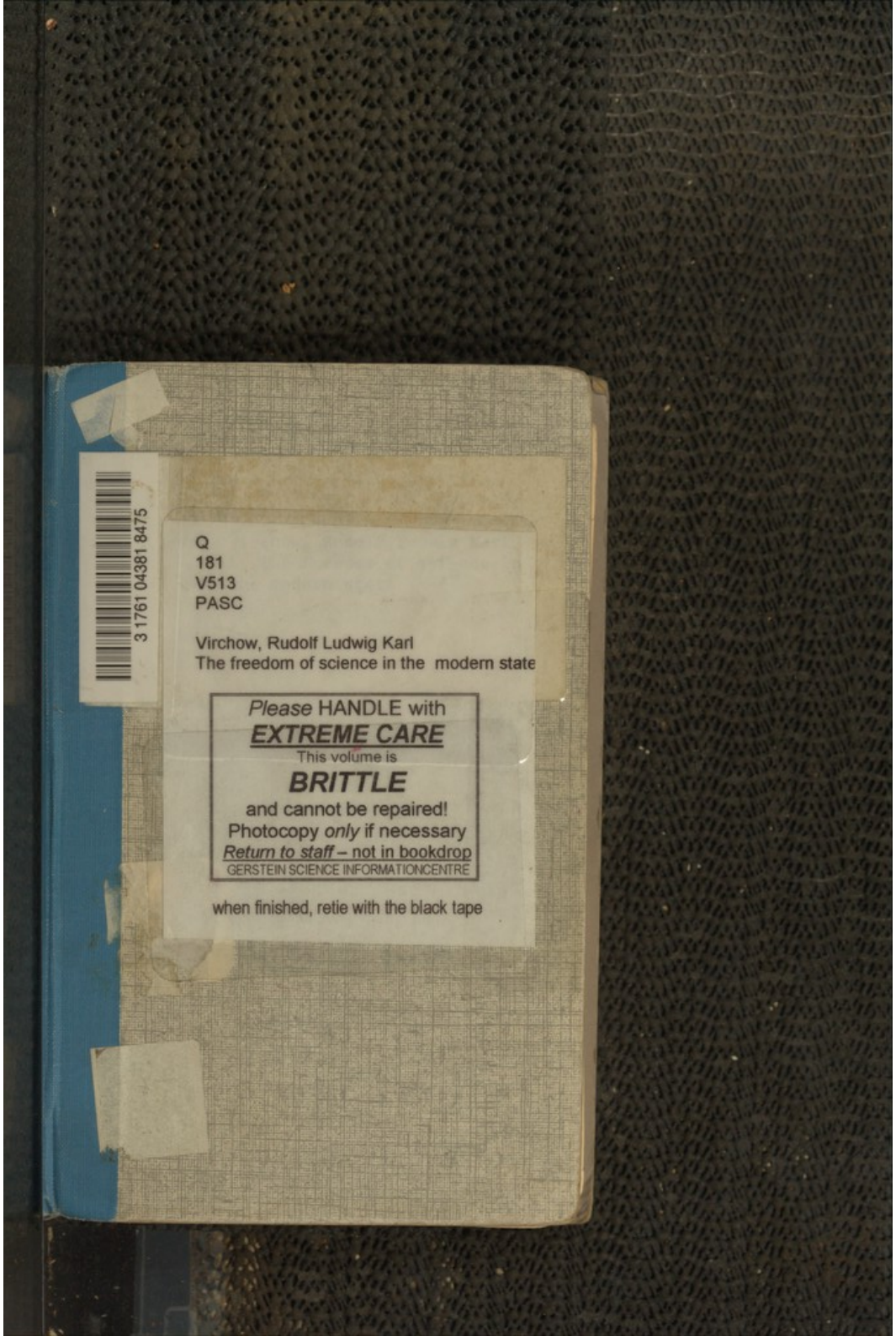
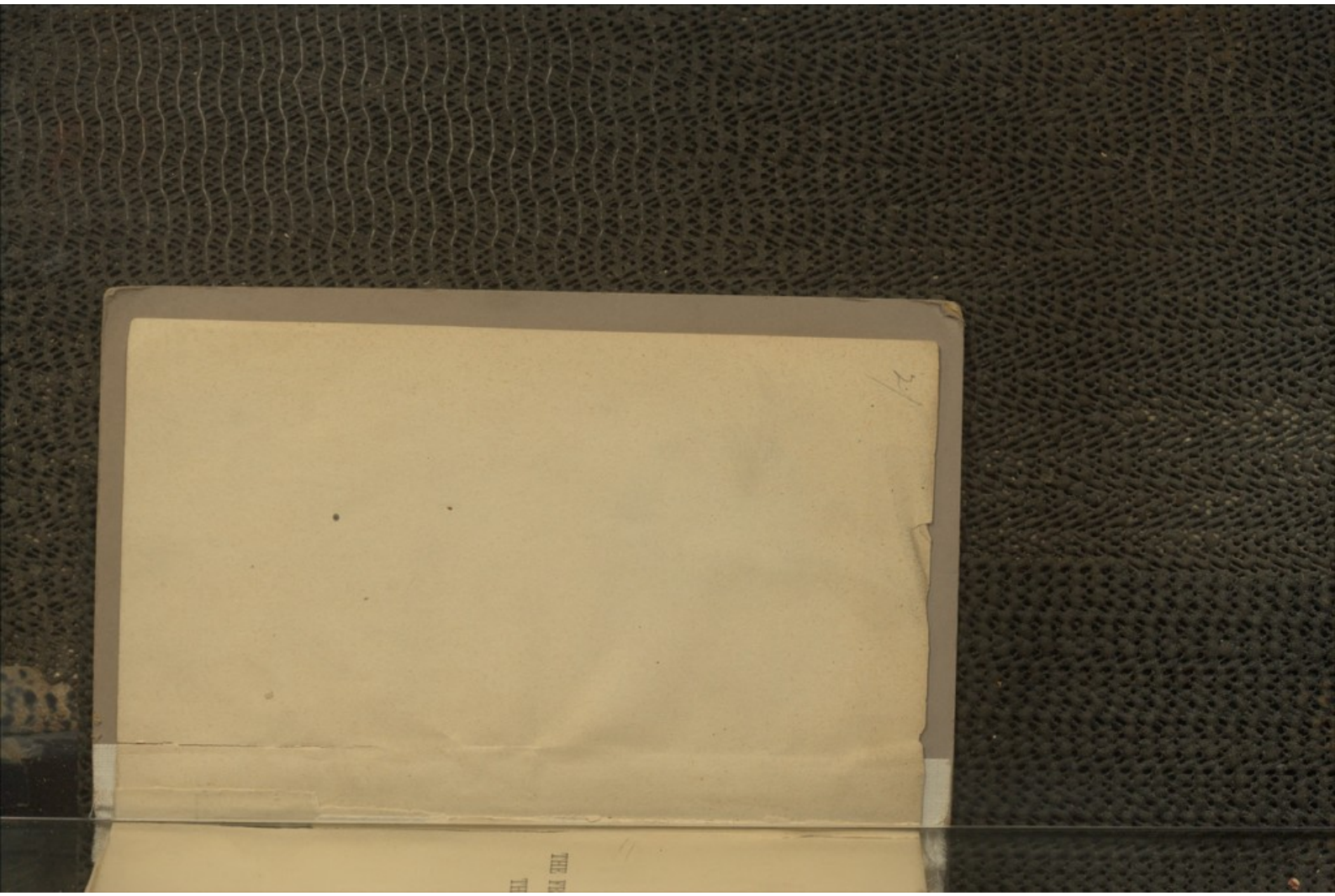
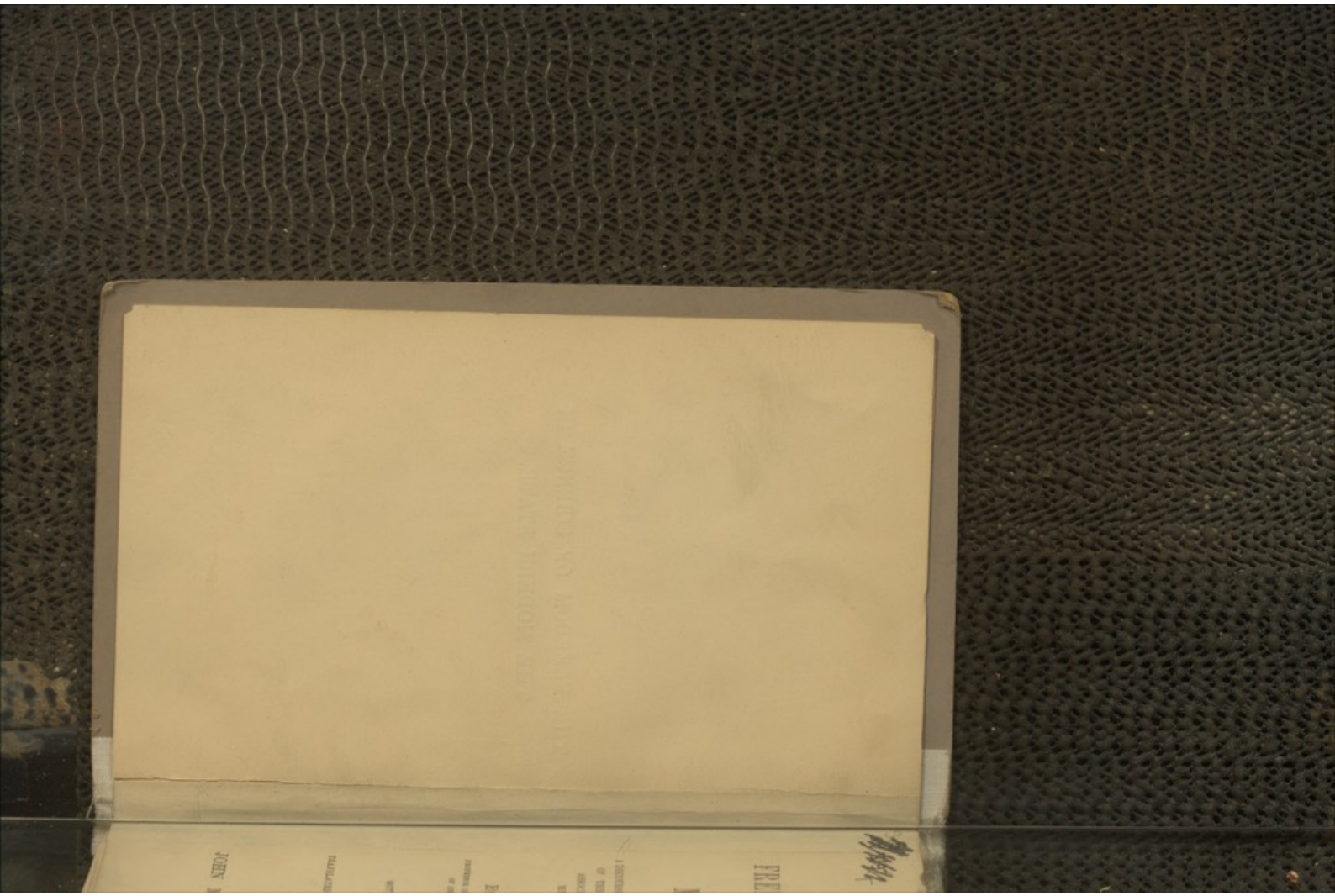
The freedom of science in the modern state : A discourse delivered at the third general meeting of the 50th conference of the German Association of Naturalists and Physicians at Munich, on the 22nd of September, 1877. With a new pref. by the author. Translated from the German, with the author's sanction.





THE FREEDOM OF SCIENCE IN THE MODERN STATE.



THE

FREEDOM OF SCIENCE

IN THE

MODERN STATE.

A DISCOURSE DELIVERED AT THE THIRD GENERAL MEETING OF THE FIFTIETH CONFERENCE OF THE GERMAN ASSOCIATION OF NATURALISTS AND PHYSICIANS AT MUNICH, ON THE 22ND OF SEPTEMBER, 1877.

By RUDOLF VIRCHOW, M.D.,

PROFESSOR IN THE UNIVERSITY AND MEMBER OF THE ACADEMY OF SCIENCES OF BERLIN; MEMBER OF THE PRUSSIAN HOUSE OF DEPUTIES.

WITH A NEW PREFACE BY THE AUTHOR.

TRANSLATED FROM THE GERMAN, WITH THE AUTHOR'S SANCTION.

LONDON:
JOHN MURRAY, ALBEMARLE STREET.
1878.

LONDON:
BRADBURY, AGNEW, & CO., PRINTERS, WHITEFRIARS.

DR. VIRCHOW'S PREFACE

FOR THE ENGLISH TRANSLATION.

THE following Discourse was spoken extemporaneously at the third general meeting of the German Association of naturalists and physicians at Munich last September, when the papers read by Drs. Häckel, Nægeli, and Klebs, had caused no little agitation both in the assembly itself, and particularly in the press and among the public.

The position taken up by the speaker has been firmly held and reiterated by him for many years, and has been maintained, especially, in articles of the Journal which he edits.*

* "Archiv für Pathologische Anatomie und Physiologie und für clinische Medecin." His most recent article on the subject is in Vol. 70.

Nothing was further from the speaker's intention than any wish to disparage the great services rendered by Mr. Darwin to the advancement of biological science, of which no one has expressed more admiration than himself. It was quite needless to give to the German public a particular explanation of those services, so fully are they known and esteemed by every educated man in Germany.

On the other hand, it seemed to him high time to utter an energetic protest against the attempts that are made to proclaim the problems of research as actual facts, the opinions of scientists as established science, and thereby to set in a false light, before the eyes of the less informed masses, not merely the methods of science, but also its whole position in regard to the intellectual life of men and nations.

With a few individual exceptions, this protest has met with cordial assent from German

naturalists. They feel themselves set free again from the tyranny of dogmatism. They have regained the certainty that, in Natural Science as in all else, real work, even if it produces only isolated results, is a better security for the durability of progress than the most ingenious speculation.

Let us hope that men of science in England also will not fail to examine this most serious question, whether the authority of Science will not be better secured, if it confines itself strictly to its own province, than if it undertakes to master the whole view of nature by the premature generalizing of theoretical combinations.

RUD. VIRCHOW.

BERLIN, *February 14th*, 1878.

NOTE.

The following are the titles of the Papers referred to by Dr. Virchow as read at the previous meetings of the Association :—

1. Professor HÆCKEL :—
   Ueber die heutige Entwickelungslehre im Verhaltnisse zur Gesammtwissenschaft. "On the modern Theory of Evolution in its relation to Science in general."—September 18.

2. Professor NÆGELI :—
   Ueber die Schranken der naturwissenschaftliche Erkenntniss.—"On the Limits of Knowledge in the Natural Science."—September 20.

3. Professor KLEBS :—
   Ueber die Umgestaltung der medecinischen Anschauungen in den letzten drei Jahrhunderten."—"On the Transformation of our views of Medicine in the las three centuries."—September 20.

TRANSLATOR'S PREFACE.

THE English public are indebted to the Prussian correspondent of the *Times** for calling attention to the following Discourse, and for a statement of the circumstances under which it was delivered. It has also been noticed in an article in the *Quarterly Review* for January, 1878, on the Use and Abuse of Scientific Lectures, and reviewed with copious extracts in the *Times* of Jan. 30: and is now offered to the English reader in a full translation, with Dr. Virchow's sanction.

The speculations of Mr. Darwin on the "Origin of Species" and the "Descent of Man"

* The *Times*, Nov. 30, 1877; in an article entitled "Darwinism in Germany."

have been eagerly received by German teachers and disciples, whose theorizing habits of mind have led them to rush to those conclusions concerning the origin and development of organic life, which are known also among ourselves as the theories of "advanced" Darwinism, though going far beyond Mr. Darwin's own doctrines. The extreme lengths to which such conjectures have been pushed in Germany are represented by the works of Professor Häckel, of Jena, one of which has lately been translated into English. In fact, as Dr. Virchow has remarked in a letter to the translator, the speculations of Häckel are founded rather on the ideas of Lamarck than on those of Darwin.

In the more philosophic atmosphere of Germany there seems to have been less eagerness, than has of late years been shown in England, to obtrude such "favourite fancies

and personal opinions"—as Dr. Virchow designates them—upon general assemblies of scientific men, or to force them on the bewildered minds of popular audiences, however freely they are taught in books and lecture-rooms. But this proper and respectful reserve was thrown aside at the meeting of the German Association of naturalists and physicians, — a society somewhat resembling our British Association — whose conference at Munich, in September, 1877, derived a certain solemnity from being their fiftieth annual assembly.

Professor Häckel deemed this a proper occasion, not only for putting forth the opinions which the Prussian correspondent describes in the subjoined terms, but for demanding that such views should be received as a settled part of the nation's knowledge, and taught in all the public schools.

"Having contended that the Biblical account of this planet's creation has long been demolished by geology, Herr Häckel wondered that morphology should have been so slow to come forward and explain the origin and diversity of the animal world. According to him, the two principles of inheritance and adaptation explain the development of the manifold existing organisms from a single organic cell; while, were further argument needed to disprove supernatural intervention, we have only to turn to the frequent occurrence of undeveloped and useless organs in many types of the animal world, to realize the truth. In this way the Creator is disposed of, not only as superfluous, but as a being who, if He existed, instead of being all-wise, would every now and then have committed the indiscretion of attempting to create eyes and wings which His power did not suffice to perfect.' Then, passing on to the omnipotent cell constituting the groundwork of animal bodies, he referred his audience to certain zoological enquiries proving the possession of motion and sensibility, of perception and will, even by those primary organisms consisting of but a single cell.

"Everything being thus dependent upon the cell, the lecturer at this stage became interested in the matter forming this marvellous organism. The cell, then, consists of matter called protoplasm, composed chiefly of carbon, with an admixture of hydrogen, oxygen, nitrogen, and sulphur. These component parts, pro-

perly united, produce the body and soul of the animated world, and, suitably nursed, become man. With this simple argument the mystery of the universe is explained, the Divinity annulled, and a new era of infinite knowledge ushered in. It was a fitting conclusion to such a scientific *pronunciamiento* that the lecturer, who regarded his argument as incontrovertible, insisted that it should be taught in every school of the land. In a previous part of his speech he had certainly admitted that the theory of organic evolution could not be experimentally proved; but as he asserted in the same breath that no such demonstration was required, and that the facts observed enabled any one in his senses to draw the crowning inferences, this deficiency had nothing in it to shake his assurance."

The bold utterance of these "wilful and despotic" views—the phrase is Dr. Virchow's, though he leaves the application to his readers—did not pass unchallenged. The correspondent tells us that "the extreme bias of the views expounded formed too marked a contrast to the lofty tone that pervaded the assembly, to be ignored by the more

moderate elements present. It was felt that, sceptically inclined as the nation and its learned professors might be, the majority were hardly disposed to adopt the materialist philosophy recommended to them as the only teaching consistent with the rational enlightenment of the times."

Accordingly, at the third meeting of the Association, four days later, Dr. Virchow delivered, and afterwards published, the Discourse, of which a translation is offered in the following pages.

The English reader should be made aware, as indeed he will clearly see from the Discourse itself, that Professor Virchow came forward, not as a champion of orthodoxy, but simply to utter an emphatic protest against the violence done to truth by claiming that mere conjectures should be accepted and taught as the very foundations of science, and

an equally emphatic warning against the dangers which such a claim involves to the advance of science and its estimation with the public. There are passages in the Discourse which assuredly qualify the correspondent's description of the orator as not only "opposed to every species of orthodoxy" but "altogether innocent of faith;" but thus much at least is certain, that, as Dr. Virchow, the eloquent deputy to the Prussian parliament, is known throughout Europe as a decided liberal in politics, so in science he maintains unbounded freedom of enquiry. Only he insists on *verification*, not as a catchword opposed to *faith*, but as a *practical test*, by which every speculation and hypothesis—however captivating or even probable—must be fully tried and proved to stand, *before* it is announced and taught as scientific truth.

And if Dr. Virchow cannot be taunted with

orthodox partisanship, much less can any reproach be cast on his competence to speak with authority on the present state of the evidence respecting the theory of evolution and the descent of man. The description given of him as "a luminary of science" has been more than confirmed to the present writer by very high scientific opinion, formed partly in his own lecture-room. We have been told that, if he has one equal, he has certainly no superior in his own department of science throughout Europe.

In accordance with this character and reputation was the profound impression made by Dr. Virchow's discourse, not only on his scientific audience, but throughout all Germany. As the Prussian correspondent says:—"The cautious distinction he drew between fact and conjecture went far to convince the uninitiated that the production of man in

the chemist's retort was not likely to be recorded among the discoveries of the age. The cold water the Professor dashed into the face of these vain imaginings has sobered public opinion, and contributed to a wholesome reaction."

The application of Professor Virchow's cautions and warnings on behalf of the true principles and interests of science to questions warmly agitated among ourselves is so obvious, as to supersede all explanation of the motive for offering them to the English reader.

The translator has made no alteration, at least knowingly; and the typographical marks of emphasis are faithfully transferred from the original.

Since the above was in type we have been favoured by Dr. Virchow with his own brief but pointed Preface, which will certainly

enhance the value of the Discourse to English readers. Dr. Virchow has also revised the proofs and expressed his approval of the translation.

CONTENTS.

THE FREEDOM OF SCIENCE IN THE MODERN STATE.

When I was honoured with the request of our executive Committee to address the Conference from this place, I proposed to myself the question, whether I should not lay before you a special province of the latest development of our science, according to the point of view suggested formerly by myself, and lately recalled to remembrance by Dr. Klebs.

But I have come to the decision rather, on this occasion, to give expression to a want generally felt; and chiefly so because, as it seems to me, the time has come when some mutual explanation must be arrived at between Science, as it is represented and pursued by us, and the common life of men; and because in our history—as the continental nations of Europe—the moment is impending, ever nearer and nearer,

when the intellectual destinies of these peoples will have to be determined for a long time in their highest and most critical points.

Gentlemen, it is not now for the first time that I have had the opportunity, at such an assembly as this, of raising a warning voice with regard to those almost dramatic events which are being accomplished in a neighbouring land. On repeated occasions, at the meeting of a congress of naturalists, I have had occasion to refer to recent events beyond the Rhine, which, however far they might appear to lie from our province, yet always come in the end to touch on the same disputed ground, namely, to determine the question now at issue—what weight modern science is to have in the modern state.

Let us be frank—we can probably be doubly so here—it is the question of Ultramontanism and of Orthodoxy which agitates us ever more and more. I can truly say that I am awaiting with real anxiety the events which will be accomplished among our neighbours during the coming year. Here we can at this moment look

round with a certain pride, and contemplate the course of things with some tranquillity. But to-day, when we are engaged in celebrating the fiftieth anniversary of this Association, it is assuredly not out of place to recal to memory what great changes have been accomplished in Germany, and especially in Munich, since the time when OKEN gathered the first assembly of German naturalists and physicians.

I will touch but briefly on two facts, well known indeed, but important enough to be brought again to remembrance. The first is that, in 1822, when the few persons who composed the first Conference of German naturalists met at Leipzig, it still appeared so dangerous to hold such an assembly, that it actually met in the darkness of a secret session. It was not till thirty-nine years later, in 1861, that the names of those members who came to the meeting from Austria could be safely published. The second fact, of which the mention of Oken's name recals an affecting remembrance, is this—that that esteemed and

venerated teacher, that ornament of the High School of Munich, was fated to die in exile, in the same Swiss canton in which Ulrich von Hutten closed his life of trouble and contention.

Gentlemen, the bitter exile which crushed Oken in his last days, and left him to languish and die far from the state to which he had devoted the best powers of his life,—this exile will remain the significant stamp of the age which we have outlived and overcome. And so long as a conference of German naturalists shall be gathered, so long shall we gratefully remember that this man bore to his dying day all the signs of martyrdom,—so long shall we point to him as one of those witnesses unto blood, who have won for us the battle of free enquiry in Science.

To-day, gentlemen, in this our German land, it is easy to speak of the freedom of science. We are now in safety here, where, but a few decades since, men were troubled with anxiety lest suddenly some new revolution should bring

to light the most extreme reaction; and we can discuss in perfect tranquillity the highest and hardest problems of life and of the future state. Assuredly the discussions which have been held in our first and second general sessions furnish conspicuous proofs, that Munich is now a place at which the representatives of science can be allowed to speak with the most perfect freedom. I had not the opportunity of hearing all those speeches, but I have since read those both of Professors HÆCKEL and NÆGELI, and I feel bound to say that we can ask for nothing beyond such freedom of discussion.

Were the question only about our enjoyment of this liberty which we possess, I should not have chosen to speak upon such a subject. But, gentlemen, we find ourselves at a crisis, when the real question for us is, to ask whether we have a right to hope for the duration and security of the freedom which we actually enjoy. The mere fact, that we are at this moment free to discuss in such a spirit, is for any one who can look back on so long an experience of public life

as I can, no sufficient security that it will be so for ever. I think, therefore, that we have not only to exert ourselves to secure general support for the moment, but I am of opinion that we are bound also to ask ourselves what we have to do in order to secure this position.

Gentlemen, I will at once tell you what I should most desire to lay before you as the chief result of my meditations, and what I would here, above all, prove. I would desire, in a word, to maintain that for ourselves we have now nothing more to ask, but rather that we have reached the point at which we must especially impose on ourselves the task of making it possible, *through our moderation, through a certain abnegation of fond fancies and personal opinions,* to escape the danger of subverting that favourable feeling of the nation which we now enjoy. My own opinion is, that we are actually in danger of imperilling the future through a too unbounded use of the freedom which existing circumstances afford us; and I am anxious to utter a warning against further progress in

that wilfulness of advancing favourite speculations of our own, which now prevails widely in many departments of natural science.

The expositions which my predecessors have given you, especially those of Dr. NÆGELI, have furnished to all who read them a series of the most important views in relation to the progress and the boundaries of the knowledge of Natural Science. These I cannot undertake to recapitulate; but, on the other hand, I must cite two or three practical examples from our experience in the natural sciences, in order to emphasize the difference between that which we put forth as real science, in the strictest sense of the word,—and on behalf of which alone (in my opinion) we can claim the full sum of all those liberties which may be described as the *freedom of science*, or, to speak perhaps with greater precision, *freedom of scientific teaching*,—between this, I say, and that wider province which belongs rather to speculative expansion: which states the problems, and discovers the questions, to which new investigation must be directed; which formulates by

anticipation a set of propositions that have still to be proved and whose reality has still to be established, but which yet may in the meantime be put forward, with a degree of probability, for the filling up of certain gaps in our knowledge.

We must not forget that there is a line of demarcation between the speculative province of Science and the domain which she has actually won and fully settled. What is required of us is, that this boundary shall be marked with continually greater precision, not only occasionally, but that in general it shall be so far fixed, that every individual shall be always more and more conscious where the boundary lies, and how far he can demand of others the admission, that what he teaches is the truth. This, gentlemen, is the task at which we have to work *among ourselves*.

The practical questions which are bound up with this consideration are obvious. It is self-evident that for what we regard as certain scientific truth we are bound to claim a full reception into the treasury of the nation's know-

ledge. *This the nation must accept as its own;* this it must feed on and digest: with this it must carry on its work. Herein lies precisely the twofold benefit which Science offers to the nation. On the one hand is that material progress, that enormous advance, which our modern age displays. All that has been done by the steam-engine, the telegraph, photography, and so forth; chemical discoveries, the technic arts of colour, and the like;—all this is based essentially on the fact, that we men of science have brought the principles to perfection, and when they are thoroughly prepared and made sure, so that we know for certain that "this is a truth of natural science," then they are handed over to the community; then others can work upon them and create new inventions, of which no one had an inkling before, which no one ventured to dream of, which come as new powers into the world, and change the condition of society and of states.

Such is the material significance of our services; and much the same is it, on the other

hand, with their intellectual significance. If I deliver to the nation an established truth of science, which is certainly attested and of which there cannot remain the least doubt,—if I request every one to convince himself of the certainty of this truth, to accept it for his own, and make it a part of the substance of his thought,—I assume, by necessary implication, that his view of things in general must be harmonized with this truth. Every essential novelty of this sort must work some influence on the whole system of human ideas, and on the *method* of thought.

Let us take, for example, a case that just now presents itself, and contemplate the advances made of late years in the knowledge of the human eye, from the first days when the various component parts of the eye were more accurately discovered by anatomy. These different parts, thus anatomically distinguished, were next subjected to microscopical investigation, and their several purposes were proved. From that time we have gradually come to

know the vital properties and the physiological functions of these different parts, until at last, in the discovery of the purple colouring of the retina (*des Sehpurpurs*), and of its photographic properties, a step forward has been taken, of which there was scarcely an anticipation a year ago.

Now it is obvious that with every such advance a certain part of optical science, and especially of the theory of vision, is made more precise and more accurate. We thereby learn definitively, that the operation of light takes place in the interior of the human body, and that it is a mere superficial organ of the human body—not in fact the brain, but the eye itself—which experiences this operation. We learn that this photography is, in fact, not a mental operation, but a chemical procedure, which is perfected by the co-operation of certain vital processes, and that, in reality, we do not see external objects, but the pictures of our eye. We are thus placed in a position to gain a new starting-point of analysis for the under-

standing of our relations to the external world, and for discriminating more precisely the purely mental part of vision from the part of it which is purely physical.

Thus a certain part of optical science, and, at the same time, of physiology, is constructed quite anew. Chemistry comes forward to take its share in the investigation of problems with which it has hitherto had no concern, namely, in the highly important questions—What is this purple colouring (*Sehpurpur*)? What sort of a substance is it? How is it formed, how destroyed? how restored again? The solution of these questions cannot fail to lay open a new province of research: we may hope soon to make fresh progress in the department of technical photography, and to learn how to obtain coloured photograms.

Thus do we make mingled advances, half within the intellectual, and half within the physical region. And therefore it is that I say, with every real advance in the knowledge of nature, there must needs be a number of

changes effected in the inner as well as the outward relations of mankind, and no one can withdraw himself from the influence of new knowledge on his own being. Every new fragment of real knowledge works within men, begets new ideas and new trains of thought, and none can choose but finally, in his own experience, to bring the highest problems of the mind into a certain connection with the processes of nature.

But there is still another aspect of these practical considerations, which comes far more nearly home to us. Throughout our whole German Fatherland men are busied in renovating, extending, and developing the system of education, and inventing fixed forms in which to mould it. On the threshold of coming events stands the Prussian law of education. In all the German States larger schools are being built, new educational establishments are set up, the Universities are extended, "higher" and "middle" schools are founded. Finally comes the question, What is to be the chief substance of the teaching? To what are

the schools to lead? In what direction are they to work? While natural science demands, while we have all for years urged the claim, that we should gain an influence over the schools; while we require that the knowledge of nature should be admitted in a greater measure into the regular course of teaching, that this fruitful material should be presented betimes to the youthful mind as the groundwork of new views; so assuredly we must also confess it to be high time that we should ourselves come to an understanding as to what we *can*, and what we *will* demand.

When Dr. Häckel says that it is a question for the educators, whether the theory of evolution (*die Descendenztheorie*) * should be at once laid down as the basis of instruction, and the protoplastic soul (*die Plastidul-Seele*) † be assumed as

* We add Professor Virchow's own word, which is the term used by Dr. Häckel, to guard against any misunderstanding of the extent to which his remarks apply to the evolution theory as a whole.—Tr.

† As to the precise meaning of the term *Plastidul*, see the note on p. 23.

the foundation of all ideas concerning spiritual being;—whether the teacher is to trace back the origin of the human race to the lowest classes of the organic kingdom, nay, still further, to spontaneous generation;—this is, in my opinion, an inversion of the questions at issue.* If the evolution theory is as certain as Dr. Häckel assumes it to be, then we must demand, then it is a necessary claim, that it should be introduced into the schools. How could it be conceivable that a doctrine of such moment,—which lays hold of every one's mind as a complete revolutionary force,—the direct result of which is to form a sort of new religion,—should not be imported in its completeness into the scheme of our schools? How would it be possible to keep a dead silence in our schools about such a *revelation* (I may surely call it), or to leave to the discretion of the educator the communication of the greatest and most important advance which our views have made in the whole century?

* That is, as we say, putting the cart before the horse.

Yes, gentlemen, this would be an act of resignation of the hardest kind, and, in fact, it would never be carried out in practice. Every schoolmaster who accepted this doctrine would teach it, however unintentionally. How could it be otherwise? He must play the complete hypocrite, he must most artificially lay aside his own knowledge for the time, if he would not betray the fact that he acknowledges and firmly holds the evolution theory, and that he knows exactly how man comes into being and whence he is derived. If he does not also know whither man goes, he would at least claim to know exactly how the progressive series had been developed in the course of ages! I say, therefore, that even if we did not demand the introduction of the evolution theory into the plan of the schools, it would come in of itself.

We should not however forget that what we utter here, perhaps still with a certain degree of modest reserve, will be carried further by the world without, with a thousandfold increasing confidence. For example, I once advanced the

view—in opposition to the doctrine then prevalent of the development of organic life from inorganic matter—that every cell was derived from a cell, certainly with reference first to pathology, and principally in the case of man. I remark, by the way, that in both respects I still hold this view to be quite right. But when I had put forth this proposition and formulated the origination of cell from cell, there were not wanting others, who not merely extended this statement in the organic world beyond the bounds within which I had maintained it, but who transferred it beyond the bounds of organic life, as holding good universally. I received the most wonderful communications from America and Europe, in which the whole sciences of astronomy and geology were based on the cell theory; because it was held to be impossible, that anything which was an established truth for the life of organic nature on this earth should not be transferred to the stars, which to be sure are also round bodies, which have assumed a spherical form and represent cells, which travel

about in the celestial space and there play a part like that of the cells in our body!

I cannot say that those who made this perversion were mere stark fools and idiots. On the contrary, I have derived the impression from some of their expositions, that many—who were in themselves educated men, who had studied much, and had at last applied themselves to the problems of astronomy—could not conceive that the design* of celestial phenomena could have been established in any other way than the design of the human organisation; so that at last, in order to obtain a consistent view, they came to assume that the heaven must also be an organism—nay, that the whole world must be a regularly constituted organism, in which no other principle could have any force but the principle of the cell! I cite this only to show the form which scientific doctrines take in the outer world, how the "theory" expands, how our proposi-

* *Zweckmässigkeit*, "fitness" or "adaptation" to a purpose; the "teleogy," which is a bye-word for the contempt of "advanced" evolutionists.—Tr.

tions return to us in a form which shocks their authors. Only imagine, then, the shape which the evolution theory assumes at the present day in the brain of a Socialist!

Gentlemen, this may appear ludicrous to many, but it has a serious bearing, and I will only hope that the evolution theory may not bring upon us all the alarm that similar theories have actually roused in the neighbouring country. At all events, this theory, if consistently carried out, has a very serious aspect, and I trust it has not escaped your notice that Socialism has already established a sympathetic relation with it. We must not conceal these facts from ourselves.

Nevertheless, however dangerous the state of things might be, let the confederates be as mischievous as they might, still I do not hesitate to say that, from the moment when we had become convinced that the evolution theory was a perfectly-established doctrine—so certain that we could pledge our oath to it, so sure that we could say "Thus it is,"—from that moment we

could not dare to feel any scruple about introducing it into our actual life, so as not only to communicate it to every educated man, but to impart it to every child, to make it the foundation of our whole ideas of the world, of society, and the State, and to base upon it our whole system of education. This I hold to be a necessity.

In saying this, I do not shrink from the reproach which, to my astonishment, during my absence in Russia, has caused a great sensation in my Prussian fatherland—the reproach of half-knowledge. It is remarkable that one of our so-called Liberal journals has started the question, whether the great mischiefs of these times —and Socialism in particular—have not sprung from half-knowledge. With respect to this I may well affirm, here in the very midst of the conference of students of nature, that *all* human knowledge is but fragmentary. All of us who call ourselves students of nature possess only portions of natural science. None of us can come here and with equal accuracy represent

every department, and take part in the discussions of each. On the contrary, the reason why we esteem individual professors so highly is precisely because they have developed their knowledge in a certain one-sided direction. In other provinces we all find ourselves in the predicament of half-knowledge.

Would that we could only succeed in diffusing this half-knowledge more widely! Would that we might only accomplish thus much—to carry forward the majority of educated men at least so far that they could take a general view of the chief directions which the several branches of natural science are pursuing, so as to be able to follow their development without any great difficulty, and that, even if the evidences of each particular thesis were not at the moment clear to them, their minds might at least be imbued with a knowledge of the general method of science! Much more than this we cannot attain.

I myself, for example, have honestly laboured all my life to acquire a knowledge of Chemistry; I have worked in the laboratory; but I feel by

no means competent at once to take my place in a conference of chemists and to discuss modern chemistry in all its bearings. Nevertheless, I am qualified, with some little time, to bring up my knowledge to such a state, that I should find no new discovery in chemistry to be incomprehensible. But I am obliged to work up this knowledge anew for each occasion. I do not possess it already, and if I want to use it I must first obtain it. That which is my accomplishment is just the *knowledge of my ignorance*. The most important thing is, that I know exactly what I do *not* understand in chemistry. Did I not possess that knowledge, I should be always oscillating hither and thither. But since, as I believe, I know pretty exactly what I do not know, I am prepared to say to myself, whenever I am obliged to enter on a field hitherto closed to me, "Now you must begin to learn; now you must study anew; now you must behave as one who is just entering upon this science."

The great error, which still keeps its hold even on many educated men, consists in their

not realizing to themselves how impossible it is, in the vast magnitude of the natural sciences and in the inconceivable abundance of materials, for any living man to master the sum total of all these details. That each may advance so far as to have a clear conception of the *fundamental principles* of the natural sciences, and to learn to know exactly the void spaces in his own knowledge,—so that whenever he comes upon such a void he may say to himself, "Now you are entering on an unknown province:"—this is what it behoves us to attain. If each of us were but sufficiently clear on this point, many a man would smite upon his breast and confess, that it is a serious matter to draw universal conclusions in respect of the history of all things while the theorist has not yet himself completely mastered the very materials from which he attempts to draw these conclusions.

It is easy to say that "a cell consists of small portions, and these we call *Plastidules*,* and

* The term *Plastidul* (in German, pl. *Plastidüle*) denotes the most minute independent mass of living protoplasm.

that plastidules are composed of carbon, hydrogen, oxygen, and nitrogen, and are endowed with a special soul; which soul is the product or sum of the forces which the chemical atoms possess." To be sure this is possible. I cannot form an exact judgment about it. It is one of the positions which are for me still unapproachable. I feel like a sailor who puts forth into an abyss, the extent of which he cannot see. But I must plainly say that, so long as no one can define for me the properties of carbon, hydrogen, oxygen, and nitrogen, in such a way that I can conceive how from the sum of them a soul arises, so long am I unable to admit that we should be at all justified in importing the "plastidulic soul" into the course of our education, or in requiring every educated man to receive it as scientific truth, so as to argue from it as a logical premiss, and to found his whole view of the world upon it. This we really cannot demand. On the contrary, I am of opinion that, before we designate such hypotheses as the voice of science, before we say, "This is modern

science,"—we should first have to conduct a long series of elaborate investigations. *We must therefore say to the teachers in schools,* "*Do not teach it.*"

This, gentlemen, is the resignation which, in my opinion, must be practised even by those who regard such a solution as the probable issue of scientific investigation. On one point, at least, we cannot for a moment dispute—that, if this doctrine of the soul were really true, it could only be certainly established by a long course of scientific enquiry. There is a series of facts in the history of natural science, from the example of which we are able to show how long certain problems remain unsettled, before it becomes possible to find their true solution. When this solution is at last found, in a sense, perhaps, which had been divined centuries before, it does not therefore follow that during those ages—the times of mere expectation and speculation—the hypothesis ought to have been taught as a scientific reality.

Dr. KLEBS has recently maintained the

doctrine of *contagium animatum;* the view, namely, that the contagion of diseases is effected by living beings, and that these beings are the primary germs of disease. The doctrine of *contagium animatum* is lost in the darkness of the Middle Ages. We have received this name from our forefathers; and it already appears distinctly in the 16th century. We possess several works of that time, which put forward *contagium animatum* as a scientific doctrine, with the same confidence, with the same sort of proof, with which the "plastidulic soul" is now set forth.

Nevertheless no one was able, throughout a long time, to discover these living germs of disease. The 16th century did not find them, nor did the 17th, nor the 18th. At last, in the 19th century, we have begun, little by little, really to find *contagia animata.* Both Zoology and Botany have made their contributions to the discovery. We have come to know of animals and plants which produce contagions; and a certain part of the doctrine of contagion has

thus found its solution in Zoology and Botany, quite in the sense of the theories of the 16th century. But you have already seen, from the discourse of Herr Klebs, that we are still far from the end of the course of proofs. However much any one may be disposed to grant the universal validity of the old doctrine, now that a number of new living contagions have been discovered, and we have ascertained that anthrax and diphtheria are diseases caused by particular organisms, we cannot, however, as yet venture to affirm that *all* contagious or indeed all infectious diseases are caused by living germs.

Since it has been shown that a doctrine, which was already put forward in the 16th century and has from that time been always emerging obstinately in the notions of some men, has at last, since the second decade of this century, received continually more and more positive proofs of its correctness, we might easily think it our duty to maintain, on the principle of the inductive generalisation of our knowledge, that all sources of contagion and miasma were living germs.

Nay, gentlemen, I am willing to admit that this conception has very great probability in its favour. Even those enquirers, who during the interval referred to did not go so far as to hold contagions and miasmata to be really living beings, yet always said that they were very nearly allied to living beings, that they had properties which we only see elsewhere in living beings—they propagate, they multiply, they generate under special conditions; they seem like bodies really organic. Nevertheless, in spite of all this, they have rightly waited until evidence of the infecting organisms was furnished. And thus now also, prudence enjoins reserve.

We should not forget that the history of our sciences exhibits a great number of facts which teach us that very closely related phenomena may be produced in very different ways. When fermentation was traced to a species of fungus, when it was discovered that fermentation depends on the development of this particular fungus, it was certainly a very obvious step, to

hold that all the processes allied to fermentation —to which the name of "catalytic" was given— which present themselves so frequently in the bodies of men and animals, as well as in plants —took place in the same way as fermentation itself. There have not been wanting men of science who held that digestion—one of the processes which have a great resemblance to that of fermentation—takes place in the following manner: that certain fungi, which are often found in the stomach (the question has been discussed practically in the special case of cattle), are the media of digestion, just as the yeast-fungi are the media of fermentation.

We now know that the gastric juices have absolutely nothing to do with fungi. However much they possess catalytic properties, we are still certain that their operative substance consists of chemical bodies, which we extract, which we isolate from the other substances, and when isolated we can make them work without any admixture of living forms. When the human saliva has to perform the function of trans-

muting starch and gum into sugar in the shortest time, and when, every time that we eat bread, this new production of "sweet" bread is effected in our mouth, no fungus has any share in the process, no organized ferment; but there are chemical substances which accomplish the transformation of the material, in a way precisely similar to what takes place in the interior of a fungus. Thus we see that two nearly allied processes—the one in the interior of a yeast-fungus, the other in the human digestive system—are carried on in different ways: the similar process, in the one case, depends on a particular vegetable organism; in the other case it is effected without anything of the kind, simply by means of free fluids.

I should esteem it a great misfortune if men of science were unwilling to proceed in a similar manner to that which has been followed in this case; namely, to enquire whether the *supposition*, which any one makes, the *idea* which he has formed to himself, and which may be highly probable, is also really true—whether it

is justified by *facts*. In this connection I wish also to remind you that there are cases of infectious disease in which without any doubt we find a similar contrast. I must beg my friend Klebs to pardon me if, notwithstanding the late advances made by the doctrine of infectious fungi, I still persist in my reserve so far as to admit only the fungus which is really proved, while I deny all other fungi so long as they are not actually brought before me.

There is found among infectious diseases a certain group arising from organic poisons. I will adduce only one among them, which, in my opinion, is very instructive—poisoning through the bite of a snake, a well known and very remarkable form. When this sort of poisoning is compared with those kinds of poisoning which we commonly called infectious diseases (for infection means nothing else than poisoning), it must be granted that the closest analogies are exhibited by the process in the two cases. With regard to the process, there would be nothing to contradict the assumption, that all the

symptoms which follow a snake's bite in the human body take place in consequence of the invasion of the body by fungi, which produce changes in the various organs. In fact we know certain processes, such, for example, as sephthemia (a kind of blood-poisoning), which show phenomena exactly similar; and it is indisputable that certain forms of poisoning by a snake's bite and certain forms of septic infection are as like as one egg is to another. And yet we have not the least ground for supposing the intrusion of fungi in the case of the snake's bite, while on the contrary we recognise such an intrusion in the septic diseases.

The history of our natural science furnishes many examples, which should induce us more and more to confine the application of our theories to the province within which we can really make them good, and not to advance so far on the path of induction, as forthwith to give an unmeasured extension to propositions which are proved for one or a few cases. Nowhere is the necessity of such a limitation more con-

spicuous, than in the very province of the doctrine of generation. The question of the first origin of organic beings, which lies at the basis of advanced Darwinism, is of very high antiquity. Who first tried to discover its solution is absolutely unknown. But when we figure to ourselves the old popular doctrine, according to which all possible living beings, whether animals or plants, sprang always from a clod of earth,—in some cases a very little clod —we should at the same time remember that the famous doctrine of *generatio æquivoca*, or *epigenesis*, is closely connected with the former, and has appeared in all the notions held for centuries.

The doctrine of spontaneous generation has now again been taken up in connection with Darwinism; and I cannot deny that there is a sort of strong temptation to adopt the ultimate conclusion of the evolution theory, and, after setting forth the whole series of living forms, from the lowest *protista* to the highest human organism, to proceed to link on this long series to the in-

organic world. It is in harmony with that tendency to generalization which is so natural to man, that it has found its place in the speculations of various peoples up to the most venerable antiquity. We feel it an undeniable necessity, not to sever the organic world from the whole, as if it were something disjoined from that whole, but rather to establish firmly its connection with the whole. In this sense there is something soothing in being able to say that the group of atoms, Carbon and Company—(this phrase is, perhaps, rather too brief, but still correct, inasmuch as carbon is probably the essential element)—that this firm of Carbon and Co. has at some time or other dissolved partnership from the common carbon, and founded under special conditions the first plastidule, and that they still continue to establish new branch companies.*

But in opposition to this it must be emphatically stated, that all really scientific knowledge

* The word used here (*gründen*) contains a humorous allusion to the "founders" of joint-stock companies, who are as much a bye-word in Germany as our "promoters."—Tr.

respecting the beginning of life has followed a course exactly contrary. We date the foundation of our actual knowledge of the generation of the higher organisms from the day on which Harvey announced the famous thesis "*Omne vivum ex ovo*,"—"Every living being springs from an egg." This proposition, as we now know, is not true universally. We can no longer at this day acknowledge it as absolutely correct: on the contrary, we know that there are a great many cases of generation and propagation without an egg. From Harvey to our celebrated Von Siebold, who has contributed to the full acknowledgment of Parthenogenesis, there have been discovered a whole series of constantly narrowing limitations, which prove that the thesis "*Omne vivum ex ovo*" is incorrect as a universal proposition. Nevertheless, we should be most ungrateful did we not acknowledge, that the opposition made by Harvey to the old *generatio æquivoca* involved the greatest advance which science has made in this province.

We have since come to know a large number

of new ways in which the propagation of different kinds of living beings is effected and new individuals are produced—such as simple fission, germination, and alternation of generations. All these discoveries are additions to our knowledge which have led us to abandon that simple formula for the procreation of living beings. In the place of that simple theory we have admitted several theories of the process. We have now no longer one simple formula by which to make clear to every one, for all time, how a new animal being has its origin.

The *generatio æquivoca*, too, which has been so often contested and so often contradicted, is nevertheless always meeting us afresh. To be sure, we know not a single *positive fact* to prove that a *generatio æquivoca* has ever been made, that there ever has been a case of procreation in this way, that inorganic masses—such as the firm of Carbon and Co.—have ever spontaneously developed themselves into organic masses. Nevertheless, I grant that, if any one *is determined* to form for himself an *idea* of how the first

organic being *could* have come into existence of itself, nothing further is left than to go back to spontaneous generation.

Thus much is evident. If I do not choose to accept a theory of creation; if I refuse to believe that there was a special Creator, who took the clod of earth and breathed into it the breath of life; if I prefer to make for myself a verse after my own fashion [in the place of the verse in Genesis]; then I must make it in the sense of *generatio æquivoca*. *Tertium non datur*. No alternative remains when once we say, "I do not accept creation, but I *will* have an explanation." If that first thesis is laid down, you must go on to the second thesis and say, "*Ergo*, I assume the *generatio æquivoca*." But of this we do not possess any actual proof. No one has ever seen a *generatio æquivoca* really effected, and whoever supposes that it has occurred is contradicted by the naturalist, and not merely by the theologian.

Gentlemen, I adduce this argument in order to set our impartiality in its true light, which

indeed is sometimes very necessary. We have always the weapons in ourselves, as well as ready at hand, to fight against every unjustified hypothesis.

I am obliged then, I say, to acknowledge the theoretical correctness of such a formula. *Whoever will have a formula,* whoever says—"I have absolute need of a formula; I must make all clear to myself; I am resolved to have a consistent view of the universe;"—he must assume either a *generatio æquivoca* or creation: there remains for him nothing else. If we would speak frankly, we must admit that naturalists may well have some little sympathy for the *generatio æquivoca.* If it were capable of proof, it would indeed be beautiful! But, we must acknowledge, it has not yet been proved. The proofs of it are still wanting. If, however, any proof should be successful, we would give in our adhesion. But even then it must first be settled to what extent the *generatio æquivoca* is admissible. We should have to proceed quietly to the investigation; for no one would

think of maintaining that spontaneous generation at all accounts for the whole number of organic beings. It may possibly hold good only for one series of beings. But my opinion is, that we have time to wait for the proof.

Whoever recals to mind the lamentable failure of all the attempts made very recently to discover a decided support for the *generatio æquivoca* in the lower forms of transition from the inorganic to the organic world, will feel it doubly serious to demand that this theory, so utterly discredited, should be in any way accepted as the basis of all our views of life. I may assume that the history of the Bathybius is pretty well known to all educated persons: and with the Bathybius the hope has once more subsided, that the *generatio æquivoca* may be capable of proof.

With respect, then, to this first point—the connection of the organic and the inorganic —we must, I think, simply confess that, in fact, we know nothing about it. We ought not to represent our conjecture as a certainty,

nor our hypothesis as a doctrine: this is inadmissible. As, in the course of the theories of evolution, it has been found much safer, much more fruitful in results, much more in harmony with the progress of accredited science, to dissect the original simple doctrine part by part, so must we proceed also in this case, first of all to keep apart things organic and inorganic, according to the old and familiar method of analysis, and not to put them together prematurely.

Nothing, gentlemen, has been more hazardous in the natural sciences, nothing has more damaged their progress and their place in the esteem of the people of all countries, than a premature synthesis. In making this emphatic statement I may cite, as an example, how our founder, Oken, has suffered in the opinion, not only of his contemporaries, but also of the succeeding generation, because he was one of those who have given synthesis a wider scope in their ideas than a more severe method would have allowed.

Gentlemen, let us not fail to profit by the

experience of that great naturalist; let us not forget that when the public see a doctrine—which has been exhibited to them as certain, established, positive, and claiming universal acceptance,—proved to be faulty in its very foundations, or discovered to be wilful and despotic in its essential and chief tendencies, many lose faith in science. Then break forth the reproaches, "Ah, you yourselves are not quite sure; your doctrine, which you call truth to-day, is to-morrow a lie: how can you demand that your teaching should form the subject of education and a recognised part of our general knowledge?" From such experience I carry away the warning that, if we wish to maintain our claim on the attention of all, we must firmly withstand the temptation to give such prominence to our hypotheses, our edifices of mere theory and speculation, as if we wished to build up on them a whole system of the universe.

If I was right in saying that, in a certain degree, half-knowledge is the proper condition of all students of nature,—that in many, nay,

perhaps in most collateral branches even of their own science they are but half-informed;—and in adding that the true student of nature is distinguished by this quality, that he is quite clear as to the boundary between his knowledge and his ignorance;—if all this is true, gentlemen, of ourselves, you see plainly that in our relation to the general public also we must limit our claims to the demand, that that alone shall be received into the public teaching which each several enquirer can certify in his own line and department of learning to be truth ascertained and common to all.

In thus circumscribing our knowledge, we have above all things to remember, that what are commonly called the Natural Sciences are made up, like all other knowledge in the world, of three entirely different parts. Though the distinction is commonly drawn only between objective and subjective knowledge, we nevertheless still find a kind of middle province, namely, that of faith, which assuredly has its place in science also, though here it is applied to other

objects than those of religious faith. It is, in my opinion, very unfortunate that the term "faith" has been claimed so especially by the Church, that we can scarcely apply it to secular things without being misunderstood. But, in fact, faith has a certain province in science also, in so far as the individual no longer takes upon himself to prove the truth of what he is taught, but is content to learn in the way of mere tradition: just as is the case in the Church.

I might likewise observe, conversely—and what I mean is not gainsaid by the Church itself—that what the Church teaches is not faith only, but her teaching has also its objective and subjective sides. No Church can refuse to develop itself in the three directions now described—namely, in the middle and sufficiently broad path of faith, alongside of which there lies, on the one hand, a certain quantum of objective historical truth, on the other, a changeful course of subjective and often very fanciful ideas. So far the teaching of the Church and of Science is alike; for the human mind is very uniform, and

consequently it transfers the method which it follows in one domain to all the rest.

But we must always be clear upon this point, how far each of the tendencies now described proceeds, in the several domains of knowledge. Thus, for example, in the sphere of the Church—for it is easier to explain the matter in this case—we have the distinctive dogma, or what is called positive faith, of which I need not speak. But every Church has also its special historical side. It declares: "This has happened; this has taken place; this has come to pass." This historical truth is not merely taught simply as a tradition, but it presents itself in the garb of objective truth supported by definite proofs. This is the case with the Christian religion, as well as with the Turkish, the Jewish, or the Buddhist.

Beside this we have on the other hand, in a certain measure, the left wing, where subjectivism has free play: there are the dreams of the individual; there we find the visions and hallucinations of each several mind—stimulated

in one religion by particular drugs, in another by fasts, and so forth. Thus are developed individual currents, which sometimes appear as independent phenomena beside the hitherto established order of the Church; which are sometimes thrust out as heretical; but which are very often infused into the great stream of the recognised life of the Church.

All this we have also in the Natural Sciences. Here too we have the current of dogma; here too we have the current of objective, and that of subjective teaching. Consequently our task is a complex one. We are always labouring first of all to lessen the dogmatic current. The chief object which science has pursued for centuries has been this:—always more and more to strengthen the right, the conservative side. This side, which accepts the *certain facts* with *full knowledge of their evidences;* this side, which holds fast to *experiment as the highest means of proof;* this side, which keeps possession of the proper treasury of science;—this, I say, has ever been growing broader and swelling greater, and,

that mainly at the expense of the dogmatic current. In fact, we have only to consider the fulness of those natural sciences which have come to perfection since the end of the last century, to see what an incredible revolution has taken place.

In no science is this so visible as in Medicine; because that is the only science which has had a continuous history of near upon 3000 years.* We are in a sense the patriarchs of science, inasmuch as we have followed the dogmatic current for the longest time. This was so strong, that even the Catholic Church in the early Middle Ages took it for her consort, and the heathen Galen appeared to the common view like a Father of the Church; nay, if we read the early medieval poems, we find him often actually in the character of a Father.

The dogmatic age of medicine lasted to the time of the Reformation. Vesalius and Paracelsus appeared as contemporaries with Luther,

* The monumental records of Egypt extend the history of medicine for at least 2000 years more.—Tr.

and made the first attempts to divert the current. They drove piles into the stream of dogma, dammed up its course, and left it only a narrow channel. In every succeeding century since the sixteenth it has become continually narrower and narrower, till at last only a very little waterway is left for the therapeutæ. So passes away the glory of the world!

Thirty years ago the Hippocratic method was still spoken of as a thing so exalted and important, that nothing could be imagined more sacred. To-day it may be said that this method is all but annihilated to its very roots. At least there would be considerable exaggeration in saying that a clinical physician of the present day still proceeds like Hippocrates. Yes! when we compare the medical practice of 1800 with the practice of to-day—for it so happens, that the year 1800 forms a great turning-point in medicine—we find that our science has been completely transformed in the course of seventy years. At that time, under the immediate influence of the French Revolu-

tion, the great Parisian school was formed; and we must give honour to the genius of our neighbours, that they were able at one stroke to discover the principles of an entirely new science. If we now see medicine developing itself in the greater breadth of objective knowledge, we must never forget that the French were the pioneers, as the Germans were in the Middle Ages.

I have wished thus to show you briefly, from the example of our own science, how the methods and the fund of knowledge have been transformed. I am convinced that in Medicine, at the end of this century, no more than a narrow pipe will be left for the last weak waters of the dogmatic stream to run their course,—a sort of 'drainage.' In the coming age, the objective current will probably have quite absorbed the dogmatic.

Probably, too, the subjective current will still exist beside it. Then also, probably, many an individual will be dreaming his fair dreams. The province of objective facts in Medicine, large

as it has become, has still left so many borderlands unoccupied, that abundant opportunities are daily offered for any one who pleases to speculate. This abundance is also fairly used. A multitude of books would remain unwritten, if writers were bound to put forward none but objective facts. But the subjective wants are still so great, that I think we may safely assume, that a full half of our present medical literature might disappear without any harm being done to the objective side of the science.

In our *teaching*, however, we ought not, in my opinion, to regard this subjective side as an essential part of what we teach. I am now almost one of the oldest professors of medicine. I have been teaching my science for more than thirty years, and I venture to say that during those thirty years I have honestly laboured, in my own mind, continually to put off the subjective character more and more, and to bring myself ever more and more into the objective current. Nevertheless, I freely confess that it is impossible for me entirely to renounce

the subjective spirit. Every year I am continually seeing afresh that I myself, on the very points where I thought I had become entirely objective, have still always retained a large portion of subjective ideas. I do not go so far as to make from human nature the impossible demand, that everyone should express himself without a subjective vein of thought; but I do say that we must set ourselves the task, to put forth in the first place what is properly actual knowledge, and whenever we go beyond this we must always say to the learners, "Observe that this is not proved, but it is *my* opinion, *my* idea, *my* theory, *my* speculation."

But we can only take this course with those who are already advanced, who are already cultivated. We cannot carry the same method into the schools for the people: we cannot say to every peasant child,—"This is actual; this is known; and that is only supposed." On the contrary, what we know and what we only suppose blend themselves, as a general rule, so completely into a single picture,

that what is supposed appears to be the principal thing, and what is known seems the accessory. So much the more is it incumbent on us who profess science, whose lives are spent in science, to refrain from putting our mere suppositions into the heads of men, and—I would say here with special emphasis—into the heads of teachers in our schools. Of course we cannot put forth bare facts as mere raw material: that is not to be thought of. The facts must be brought into a certain arrangement, but we must not extend this arrangement beyond what is absolutely necessary.

This is the complaint that I have to make, for example, against Dr. Nägeli. He certainly discussed the difficult questions which he chose for his subject with the greatest moderation, and—as you will see when you read his discourse—in a thoroughly philosophic spirit. Nevertheless, he took a step which I consider most dangerous. In fact, what the *generatio æquivoca* does, he has done in another direction. He demands that the province of mind should not

only be extended from animals to plants, but that we should finally pass over from the organic to the inorganic world with our conceptions of the nature of mental operations.

This mode of thinking, which has its representatives among great philosophers, is in itself very natural. If any one insists on bringing the operations of the mind into connection with the other processes of the universe, he is of necessity led, in the first place, to extend the psychical phenomena, which are observed in man and in the most highly organized vertebrate animals, to the lower and ever lower forms of animal life: next, the plants also obtain their soul: further, the cell is endowed with sensation and thought: and, finally, the transition is made to the chemical atoms, which hate or love, seek or fly from one another.

This is all very fine and admirable, and may ultimately perhaps prove true. It is *possible*. But, meanwhile, have we really any need?—are we bound by any positive scientific necessity?—to extend the province of mental operations beyond

those bodies in which, and in connection with which, they are actually exhibited? I have no objection to your saying that atoms of carbon also possess mind, or that in their connection with the Plastidule-company they acquire mind; only I *do not know how I am to perceive this.* It is a mere playing with words. If I explain attraction and repulsion as exhibitions of mind, as psychical phenomena, I simply throw the Psyche out of window, and the Psyche ceases to be Psyche.*

The processes of the human mind may ultimately find a chemical explanation, but at present, in my opinion, it is not our business to bring these provinces into connection. Much

* 'As an illustration of Professor Virchow's meaning, we may quote the conclusion at which Dr. Tyndall arrives respecting "the hypothesis of a human soul, offered as an explanation or simplification of a series of obscure phenomena"—"*psychical* phenomena," as he also calls them:—"If you are content to make your 'soul' a poetic rendering of a phenomenon which refuses the yoke of ordinary physical laws, *I, for one, would not object to this exercise of ideality!*" —'*Science and Man.* Presidential Address delivered before the Birmingham and Midland Institute, October 1st, 1877.' *Fortnightly Review*, November 1st, 1877, p. 607.—Tr.

rather is it our business to keep them strictly to the limits within which we actually perceive them. And, as I have all along laid stress upon this—that we should not seek, in the first place, the *transition* of the inorganic into the organic, but rather first fix the *contrast* between the inorganic and the organic, and direct our studies to this contrast,—so do I also maintain that this is the only way of progress; and I have the firmest conviction that we shall make no advance, unless we fix the province of mental processes at those limits within which mental phenomena actually present themselves to us, and unless we refrain from *supposing* mental phenomena where they may indeed *possibly* take place, but where we perceive no *visible, audible, tangible,* in a word, no *sensible* phenomena, which could be designated as intellectual.

For us, beyond all doubt, the sum total of psychical phenomena is inseparably connected with certain animals, not with the whole world of organic being, nay, not even with all animals in general: this I unhesitatingly assert. We

have no reason, up to the present time, even to speak of the possession of psychical properties by the lowest animals: we find them only in the higher, and only with full certainty in the highest.

I will, indeed, willingly grant that we can find certain gradations, certain gradual transitions, certain definite points at which we trace a passage from mental processes to processes purely physical or of a physical character. Throughout this discourse I am not asserting that it will never be possible to bring psychical processes into an immediate connection with those which are physical. All I say is, that we have *at present* no right to set up this *possible* connection as a *doctrine* of science: and I must enter my decided protest against the attempt to make a premature extension of our doctrines in this manner, and to be ever anew thrusting into the very foreground of our expositions that which has so often proved an insoluble problem.

We must draw a strict distinction between what we wish to *teach* and what we wish to

search for. The objects of our research are expressed as problems (or hypotheses). We need not keep them to ourselves; we are ready to communicate them to all the world and say, "There is the problem; that is what we strive for:" just as Columbus, when he set sail to discover India, made no absolute secret of his object—though he ended by discovering, not India, but America. Even so does it not infrequently befal us. We set out with the aim of proving a definite problem, which we propose to ourselves as certain, and in the end we find out quite a different one, of which we had had no conception. The investigation of such problems, in which the whole nation may be interested, cannot be restricted to any one. This is *Freedom of Enquiry*. But the problem (or hypothesis) is not, without further debate, to be made a *doctrine*. In our teaching, we must keep to that lesser but still large province, which we have really mastered.

Gentlemen, I am persuaded that only by such resignation, imposed by us on ourselves and

practised towards the rest of the world, shall we be able to conduct the contest with our opponents and to carry it on to victory. Every attempt to transform our problems into doctrines, to introduce our hypotheses as the bases of instruction — especially the attempt simply to dispossess the Church, and to supplant its dogmas forthwith by a religion of evolution— be assured, gentlemen, every such attempt will make shipwreck, and in its wreck will also bring with it the greatest perils for the whole position of science.

Therefore, gentlemen, let us moderate our zeal: let us patiently resign ourselves always to put forward, as problems only, even the most favourite problems that we set up; never ceasing to repeat a hundredfold a hundred times:— "Do not take this for established truth; be prepared to find that it is otherwise; only for the moment we are of opinion that *it may possibly be so*."

I will cite one more example by way of illustration. There are at this time few students

of nature who are not of opinion that man stands in some connection with the rest of the animal kingdom, and that such a connection may possibly be discovered, if not with the apes, yet perhaps, as Dr. Vogt now supposes, at some other point. I freely acknowledge that this is a desideratum in science. I am quite prepared for such a result, and I should neither be surprised nor astonished if the proof were produced that man had ancestors among other vertebrate animals. You are aware that I am now specially engaged in the study of anthropology, but I am bound to declare that every positive advance which we have made in the province of pre-historic anthropology has actually removed us further from the proof of such a connection.

Anthropology is at present occupied with the question of fossil man. We have gone back from the man of the present "period of creation" into the quaternary age, the time respecting which Cuvier still maintained most distinctly that, speaking generally, man did not yet exist.

But in our day the quaternary man is a fact universally accepted: the quaternary man is no longer a problem, but a real doctrine.

The tertiary man, on the contrary, is a problem, the material evidence of which is now under discussion. There already exist objects with regard to which it is disputed, whether they are to be accepted as proofs of the existence of man in the tertiary period. We are no longer making mere speculations on the point, but we are debating about distinct specimens, whether they can be acknowledged as evidence of the activity of man in the tertiary period. The question proposed receives different answers, according as these material objects are deemed sufficient evidence or not. Eminent Churchmen even, such as the Abbé Bourgeois, are convinced that man lived in the tertiary period. For them the tertiary man is now an actual doctrine; for us, who are of a somewhat more critical disposition, the tertiary man is still only a problem, but, we must acknowledge, a problem ready for discussion.

Let us then, in what we have now to say, keep

provisionally to the quaternary man, whom we really find. When we study this fossil man of the quaternary period, who must, of course, have stood comparatively near to our primitive ancestors in the order of descent, or rather of ascent, we always find a *Man*, just such as men are now.

As recently as ten years ago, whenever a skull was found in a peat bog, or in pile dwellings, or in ancient caves, people fancied they saw in it a wonderful token of an inferior state, still quite undeveloped. They smelt out the very scent of the ape: only this has continually been more and more lost. The old troglodytes, pile-villagers, and bog-people, prove to be quite a respectable society. They have heads so large, that many a living person would be only too happy to possess such. Our French neighbours, indeed, have warned us against inferring too much from these big heads. It may have been that their contents were not merely nerve-substance, but that the ancient brains may have had more interstitial tissue than is now usual, and that, in spite of the size of the brain, their nerve-

substance may have remained at a lower stage of development.

This, however, is but the sort of familiar talk which is brought in as a kind of prop for weak minds. On the whole we must really acknowledge, that there is a complete absence of any fossil type of a lower stage in the development of man. Nay, if we gather together the whole sum of the fossil men hitherto known, and put them parallel with those of the present time, we can decidedly pronounce that there are among living men a much greater number of individuals who show a relatively inferior type than there are among the fossils known up to this time. Whether it is just the highest geniuses of the quaternary period that have had the good luck to be preserved to us, I will not venture to surmise!

Our usual course is to argue from the character of a single fossil object to the generality of those not yet found. This, however, I will not do. I will not affirm that the whole race was as good as the few skulls that have survived.

But one thing I must say—that not a single fossil skull of an ape or of an 'ape-man' has yet been found that could really have belonged to a human being. Every addition to the amount of objects, which we have obtained as materials for discussion, has removed us further from the hypothesis propounded. At the same time, we cannot entirely set aside the consideration, that the men of the tertiary period may perhaps have lived only on a particular part of the earth. This might be just as possible as in the case of the remarkable discovery made of late years in North America, that the fossil ancestors of our horses are found in regions where the horse itself has long since entirely disappeared. When America was discovered, it was generally horseless. In the region where the ancestors of our horses lived, there was not a living horse to be found. Just so it may be, that the tertiary man once existed in Greenland or Lemuria,*

* A term invented by Mr. Philip Sclater, to describe a hypothetical continent, of which Madagascar and the Mascarine islands are the supposed remainder.—Tr.

and will still be brought to light somewhere or other out of the depths.

Only, as a matter of fact, we must positively recognise that there still exists as yet a sharp line of demarcation between man and the ape. WE CANNOT TEACH, WE CANNOT PRONOUNCE IT TO BE A CONQUEST OF SCIENCE, THAT MAN DESCENDS FROM THE APE OR FROM ANY OTHER ANIMAL. We can only indicate it as an hypothesis, however probable it may seem, and however obvious a solution it may appear.

From the repeated experience of the past we ought to take a signal warning, lest we should unnecessarily impose on ourselves the obligation, or succumb to the temptation, to draw conclusions at a time when we are not justified in so doing. Believe me, gentlemen, herein lies the great difficulty for every student of nature who addresses the world at large. Whoever speaks or writes for the public is bound, in my opinion, to examine with twofold exactness how much of that which he knows and says is objectively true. He is bound to take the

greatest possible care that all the merely inductive generalizations which he makes, all his extended conclusions according to the laws of analogy—however obvious they may seem—be printed in smaller type under the text, and that in the text itself he put nothing but what is really objective truth.

Thus, gentlemen, we might surely hope to gain an ever enlarging circle of adherents, to obtain an ever greater number of fellow-workers, to see the educated public take a further interest in science, with those fruitful results which have already been witnessed in many of its domains. Or else, gentlemen, I fear that we shall over-estimate our power. With perfect truth did Bacon say of old, "*Scientia est potentia.*" But he also defined that knowledge; and the knowledge he meant was not speculative knowledge, not the knowledge of hypotheses, but it was objective and actual knowledge.

Gentlemen, I think we should be abusing our power, we should be imperilling our power, unless in our teaching we restrict ourselves to

this perfectly legitimate, this perfectly safe and unassailable domain. From this domain we may make incursions into the field of problems, and I am sure that every venture of that kind will then find all needful security and support.

THE END.

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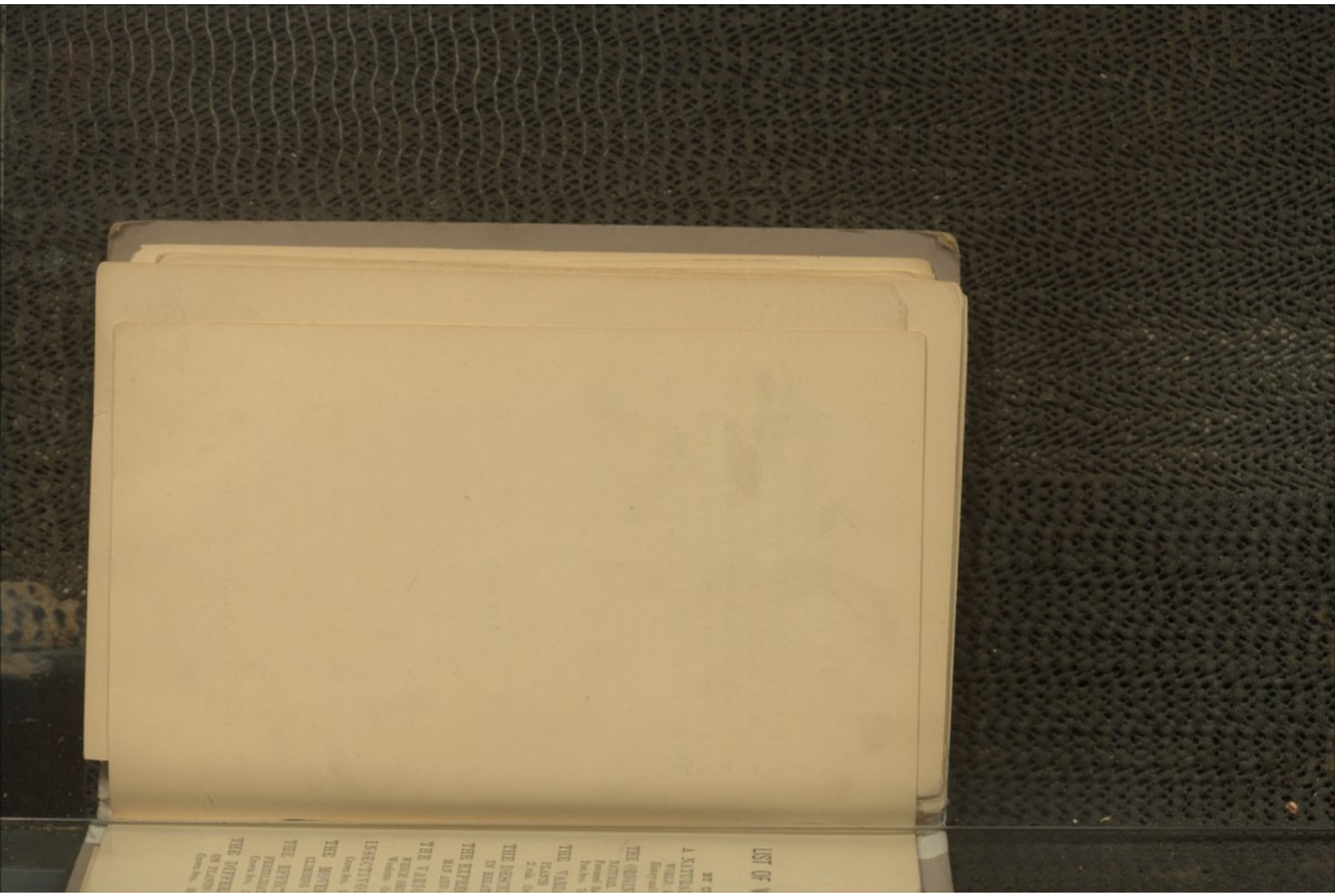

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