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INTRODUCTORY LECTURE

DELIVERED IN THE

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BY

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INTRODUCTORY LECTURE.[1]

Gentlemen,—I stand before you affected with very mingled feelings. On the one hand, I cannot but feel proud to have been called to occupy a Chair which, without disparagement to others, must be allowed to have been, during the last thirty-six years, the one most influential for good in this the most important medical school in the British dominions. But the exultation which I might otherwise naturally feel is heavily dashed by the thought that the circumstance which led to my promotion was the retirement of the man to whom the lustre of the Edinburgh Chair of Clinical Surgery has been from first to last entirely due. I am well aware that he has made the place, not the place him. And though in his presence I must not say all that I otherwise should, I cannot refrain from expressing my conviction that, whether regarded as a scientific and practical surgeon, or as a teacher of those principles which he has done more than any other man in this century to establish, he has been without a rival in the world. Hence, in addition to the grief which I feel in common with

[1] This lecture was not originally intended for publication, and was for the most part delivered *extempore*.

you all at the cause of his resigning the Chair which he had so long adorned, I am oppressed with a humbling sense of my own insufficiency; of my weakness, compared with his giant strength of mind and purpose; of my utter inability to fill his place. I can only strive, by the blessing of God, to do my best among you, relying, as I know I may, upon your generous sympathy. At the same time, we may all rejoice that our old master is still among us, to cheer us by his presence and aid us by his counsel; and it is a source of great satisfaction to myself that, as I have the privilege of free access to his inexhaustible store of wisdom and experience, he will, in some sense, through me be still your teacher.

But, leaving these personal considerations, let us turn to the subject that lies before us. Clinical Surgery is, strictly speaking, surgery at the bed-side; surgery illustrated by cases in hospital, as distinguished from surgery taught systematically in the class-room. The importance of Clinical or bed-side study cannot be overestimated. It is the very key-stone, without which all the rest of the educational structure, being merely preparatory, would be absolutely useless. It is to surgery or medicine what dissection is to anatomy. It confers a familiar acquaintance with the nature of disease, and an instinctive knowledge of the appropriate treatment, without which, a man, however accomplished otherwise, would be utterly unfit to practise the profession. But how, it may be asked, can a course of lectures be delivered upon this principle? Can it be possible to take a class of the size of my present audience from bed to bed in a ward, and profitably teach them there? To do this would certainly be impossible. Remarks made at the bed-side are doubtless highly valuable to those who hear them and who see their subject; but it is only a few at a time who can be thus taught. Hence Clinical lectures commonly degenerate into the reading of details of cases, with remarks upon them, which, for the great majority of those who hear them, lack the genuine element of Clinical interest.

This difficulty was happily overcome by Mr. Syme. Though it was impossible to take a large class to the bed-side of the patient, it was easy in most instances to bring the patient before the class, collected in the operating theatre, where they could all see the salient features of the case, and hear not only the remarks of the teacher, but the patient's own account of his symptoms, and witness the treatment then and there put in practice; or, if it was thought desirable to defer the operation to another day, they were prepared to watch its various steps with intelligence and profit, after having heard the principles of the procedure fully discussed. Such a course of instruction is truly clinical, and, if rightly conducted, possesses a vividness of interest and permanence of impression peculiar to itself. Having witnessed its advantages when in Edinburgh, I have followed this system in Glasgow, and shall continue to pursue it here. But invaluable as such lectures may be made, you must not suppose that attendance upon them will do all that is needful for you in the way of Clinical study. You must not only see diseases and watch their treatment by others, but handle them and be personally concerned in their management. Facilities for this are presented by the hospital offices of dresser, clerk, and house-surgeon, and no man should consider himself justified in assuming the serious responsibilities of practice without having availed himself largely of such opportunities, either in our Infirmary or in some other similar institution.

But to return to the course before us. There are some details regarding the mode in which you may attend it to the greatest advantage, which I shall reserve till we next meet. And now, as the place where we are assembled forbids my entering at once upon Demonstrative Surgery, I propose to devote the remainder of this hour to the endeavour to convince you, so far as the limited time at our disposal permits, of the truth of the germ-theory of putrefaction, the basis of a new mode of treatment which finds its application in all departments of practice ; so that without understanding it we cannot advance satisfactorily in the consideration of individual cases. I allude to the Antiseptic System. This system of treatment consists of such management of a surgical case as shall effectually prevent the occurrence of putrefaction in the part concerned. When this is really secured, Surgery becomes something totally different from what it used to be ; and injuries and diseases formerly regarded as most formidable, or even hopeless, advance quietly and surely towards recovery. Of this system the germ-theory of putrefaction is the pole-star which will guide you safely through what would otherwise be a navigation of hopeless difficulty.

The germ-theory declares that the putrefaction of organic substances under atmospheric influence is not effected, as used to be supposed, by the oxygen of the air, but by living organisms developed from germs floating in the atmosphere as constituents of its dust.

The first great step towards the establishment of this theory was the discovery of the yeast-plant in 1836 by Cagniard Latour, who, having detected in yeast a microscopic fungus, the *torula cerevisiæ*, which appeared to be the essential constituent of the ferment, attributed the resolution of sugar into alcohol and carbonic acid to the disturbing influence of the growing organism.[1] In the following year, Schwann of Berlin published the results of a remarkable investigation into the cause of putrefaction (in the course of which, by a coincidence such as is not uncommon in the history of science, he, too, had independently discovered the yeast-plant), and he related experiments which showed that a decoction of meat might remain for weeks together free alike from putrefaction and from the development of infusoria or fungi in a flask containing air frequently renewed, provided that the atmosphere was subjected to a high temperature at some part of its course towards the containing vessel.[2] Hence he concluded that putrefaction was caused by the growth of organisms springing from germs in the air, the heat preventing the putrefactive change by depriving the germs of their vitality. In other words, he propounded the germ-theory of putrefaction. These experiments of Schwann's appear to me to prove conclusively that oxygen, as ordinarily understood by chemists, cannot of itself occasion putrefaction. It is true, indeed, that, if you

[1] See *Comptes Rendus*, tom. iv. p. 905.

[2] See Poggendorf's *Annalen*, vol. xli. art. xvi.

attempt to repeat the experiments, you may meet with failure. But it must be remembered that merely negative results go for nothing here, if the positive evidence rests on satisfactory authority. This is a point which has been too little borne in mind in the discussion of this subject. If we consider what the germ-theory assumes, how minute the putrefactive particles are supposed to be, and how universally present in the atmosphere, and in the dust which adheres to all objects exposed to it, it is easy to understand failure in such experiments consistently with the truth of the theory. But it is *impossible* to understand success in any single instance, consistently with the falsehood of the theory. If in any one case it really happened that a decoction of meat remained without putrefaction for weeks together, though freely exposed to air, unaltered, except by having been temporarily subjected to a high temperature, this is enough to show that oxygen, as known to chemists, is not the sole cause of the change in question.[1] One genuine successful experiment out of a thousand is enough to establish that point.

Schwann's observations, however, did not receive the attention which they appear to me to have deserved. The fermentation of sugar was generally allowed to be occasioned by the *torula cerevisiæ;* but it was not admitted that putrefaction was due to an analogous agency. And yet the two cases present a very striking parallel. In each a stable chemical compound, sugar in the one

[1] Such experiments are peculiarly likely to fail in the hands of those who perform them with the object of confuting the germ-theory. In fact, a belief in the theory is almost essential in order that the experimenter may be sufficiently keenly alive to the subtle sources of failure.

case, albumen in the other, undergoes extraordinary chemical changes under the influence of an excessively minute quantity of a substance which, regarded chemically, we should suppose inert. As an example of this in the case of putrefaction, let us take a circumstance often witnessed in the treatment of large chronic abscesses. In order to guard against the access of atmospheric air, we used to draw off the matter by means of a canula and trocar, such as you see here, consisting of a silver tube with a sharp-pointed steel rod fitted into it, and projecting beyond it. The instrument, dipped in oil, was thrust into the cavity of the abscess, the trocar was withdrawn, and the pus flowed out through the canula, care being taken by gentle pressure over the part to prevent the possibility of regurgitation. The canula was then drawn out with due precaution against the reflux of air. This method was frequently successful as to its immediate object, the patient being relieved from the mass of the accumulated fluid, and experiencing no inconvenience from the operation. But the pus was pretty certain to reaccumulate in course of time, and it became necessary again and again to repeat the process. And unhappily there was no absolute security of immunity from bad consequences. However carefully the procedure was conducted, it sometimes happened, even though the puncture seemed healing by first intention, that feverish symptoms declared themselves in the course of the first or second day, and, on inspecting the seat of the abscess, the skin was perhaps seen to be red, implying the presence of some cause of irritation, while a rapid reaccumulation of the fluid was found to have occurred. Under these circumstances, it became necessary to open the abscess by free incision, when a quantity, large in proportion to the size of the abscess, say, for example, a quart, of pus escaped, fetid from putrefaction. Now, how had this change been brought about ? Without the germ theory, I venture to say, no rational explanation of it could have been given. It must have been caused by the introduction of something from without. Inflammation of the punctured wound, even supposing it to have occurred, would not explain the phenomenon. For mere inflammation, whether acute or chronic, though it occasions the formation of pus, does not induce putrefaction. The pus originally evacuated was perfectly sweet, and we know of nothing to account for the alteration in its quality but the influence of something derived from the external world. And what could that something be ? The dipping of the instrument in oil, and the subsequent precautions, prevented the entrance of oxygen. Or even if you allowed that a few atoms of the gas did enter, it would be an extraordinary assumption to make that these could in so short a time effect such changes in so large a mass of albuminous material. Besides, the pyogenic membrane is abundantly supplied with capillary vessels, through which arterial blood, rich in oxygen, is perpetually flowing ; and there can be little doubt that the pus, before it was evacuated at all, was liable to any action which the element might be disposed to exert upon it.

On the oxygen theory, then, the occurrence of putrefaction under these circumstances is quite inexplicable. But if you admit the germ-theory, the difficulty vanishes at once. The canula and trocar having been lying exposed to the air, dust will have been deposited upon them, and will be present in the angle between the trocar and the silver tube, and in that protected situation will fail to be wiped off when the instrument is thrust through the tissues. Then when the trocar is withdrawn, some portions of this dust will naturally remain upon the margin of the canula, which is left projecting into the abscess, and nothing is more likely than that some particles may fail to be washed off by the stream of outflowing pus, but may be dislodged when the tube is taken out, and left behind in the cavity. The germ-theory tells us that these particles of dust will be pretty sure to contain the germs of putrefactive organisms, and if one such is left in the albuminous liquid, it will rapidly develop at the high temperature of the body, and account for all the phenomena.

But striking as is the parallel between putrefaction in this instance and the vinous fermentation, as regards the greatness of the effect produced, compared with the minuteness and the inertness, chemically speaking, of the cause, you will naturally desire further evidence of the similarity of the two processes. You can see with the microscope the torula of fermenting must or beer. Is there, you may ask, any organism to be detected in the putrefying pus? Yes, gentlemen, there is. If any drop of the putrid matter is examined with a good glass, it is found to be teeming with myriads of minute jointed bodies, called vibrios, which indubitably proclaim their vitality by the energy of their movements. It is not an affair of probability, but a fact, that the entire mass of that quart of pus has become peopled with living organisms as the result of the introduction of the canula and trocar; for the matter first let out was as free from vibrios as it was from putrefaction. If this be so, the greatness of the chemical changes that have taken place in the pus ceases to be surprising. We know that it is one of the chief peculiarities of living structures that they possess extraordinary powers of effecting chemical changes in materials in their vicinity, out of all proportion to their energy as mere chemical compounds. And we can hardly doubt that the animalcules which have been developed in the albuminous liquid, and have grown at its expense, must have altered its constitution, just as we ourselves alter that of the materials on which we feed.

The only question, therefore, that remains to be answered is, Whence have these vibrios originated? Have they sprung, like higher animals and plants, from pre-existing similar organisms, or have they arisen spontaneously out of the pus from an alteration in its physical constitution, determined in some inexplicable manner by the introduction of a canula and trocar?

All analogy, gentlemen, is in favour of the former view. The doctrine of spontaneous or equivocal generation has been chased successively to lower and lower stations in the world of organized beings, as our means of investigation have improved. I remember a conversation I once had, when a student, with an elderly gentleman, not indeed belonging to our profession, on the subject of mites in cheese. He believed that they grew out of the cheese from some change in its substance as the result of keeping; and the view which I advocated, that they had sprung from the eggs of pre-existing mites, seemed to him preposterous. But when the microscope is applied to these creatures, and we see that they rank in the type of their organization with spiders or crabs, and that they are similarly provided with organs of reproduction, it seems to us as absurd to suppose that they have arisen from a mere alteration in the cheese as it would be to imagine that crabs could spring spontaneously out of a piece of dead fish or other garbage upon which they prey. Yet though no physiologist doubts that cheese-mites do arise from parentage, it must be confessed that there is some difficulty in accounting for their almost invariable occurrence in some kinds of cheese kept for a sufficient length of time. Whether the eggs are transferred by the hand of the cheesemonger, or whether the adult mites migrate from cheese to cheese, may be matter for curious discussion.

But though with creatures as large, comparatively speaking, as the cheese-mite, it may not be very easy to explain the extensive diffusion of their ova, this difficulty becomes less and less the more minute the organism. If a vessel containing preserved fruit is left exposed to the air, the surface of the preserve soon becomes covered with mould, and it is then found to have a "mouldy" flavour,—implying alteration in its chemical constitution. The mould itself has a flavour of its own, and it has developed, in part at least, at the expense of the preserve. If the mould is examined microscopically, it is seen to be just as distinctly a vegetable as a cabbage is, and far more abundantly provided with reproductive apparatus. Supposing it to be the ordinary blue mould, the blue tint is simply the colour of the fructification. This is in accordance with a general law in the organic world, that so far from any deficiency appearing in the arrangements for reproduction in the lower forms of life, so as to make it difficult to account for their originating from parents, the lower the organism the more lavishly is this provided for. In some animals low in the scale of being we find, besides the formation of ova, a faculty of self-multiplication by segmentation, or, as it is termed, fissiparous generation. For what purpose, I venture to ask, can be this ample provision for reproduction of the lowest species by parentage, if they can spring spontaneously out of the materials in which they grow ?

Now, in the case of the blue mould, the sporules, besides being produced in incalculable multitudes, are of extreme minuteness, and constitute a very fine dust, which cannot fail to be wafted and extensively diffused through the air. If a ray of sun-light were to shoot through this room, we should see the sunbeam peopled with motes. But the particles of dust which are rendered visible to the naked eye by being so illuminated, are gross indeed compared with the sporules of such a fungus. Some of them are complicated organic structures, such as pieces of hair or vegetable fibre ; and if these are suspended in the air, still more must microscopic spores be so, though their extreme minuteness makes it less easy to distinguish them from particles of inorganic matter. Hence it appears that, for the lowest forms of life, as for the highest, the notion of spontaneous generation is simply gratuitous and uncalled for.

But although from these considerations we may be led pretty surely to infer, on the one hand, that the atmosphere is pervaded by the germs of minute organisms, and, on the other hand, that without such germs the organisms could not take their origin, it would be highly desirable to obtain positive evidence on both these points, if indeed it is attainable.

Such evidence has been afforded of late years by the beautiful researches of Pasteur. From among his numerous experiments, I will select one set as peculiarly instructive. A number of glass flasks, with attenuated necks, were partially filled with a decoction of yeast, filtered so as to be perfectly clear and transparent. Each was then boiled for a certain length of time, with the object of destroying any organisms existing in the decoction, or adhering to the interior of the vessel, and during ebullition the neck was hermetically sealed, so that when the vessel cooled, a vacuum was produced in the part previously occupied by air. A certain number of such a series of flasks were then opened in a particular locality, as, for example, a lecture-room such as this, by breaking the narrow neck of each, after scratching it with a file. Air rushed in to fill the vacuum, after which the neck was immediately sealed again with the blowpipe. As the result of the introduction of this limited amount of air, the previously transparent liquid in a considerable proportion of the flasks was seen to present, in the course of the next few days, a cloudiness indicative of the first appearance of the growth of torulæ and other organisms, which afterwards continued to increase. But if a set of such flasks were opened in a situation where atmospheric germs might be expected to be few, if any, a different result was obtained. M. Pasteur was at the pains to take such flasks to the Mont Anvert, in Switzerland, and open them in wind blowing from a glacier, taking special care, by exposing the neck to the flame of a spirit-lamp when filing it, and breaking it with long forceps similarly treated, to guard as much as possible against the introduction of living organisms from the instruments employed, or from his own person. The pure air thus introduced had indeed, in one flask out of twenty, the effect of inducing, very slowly, an appearance of organic development. But in all the rest the liquid remained perfectly unchanged for an indefinite period. On the other hand, if the flasks were opened in a situation where the air, though in one sense pure, might be expected to abound in minute life, viz., under the shade of trees in the country, organisms formed in sixteen out of eighteen flasks, and presented a great variety in their nature.[1] These experiments, which rest not only on the high authority of M. Pasteur, but also on the unimpeachable corroborative testimony of a Committee of the French Academy of Sciences, including the celebrated Milne Edwards, prove conclusively both that the gases of the air cannot of themselves occasion the growth of organisms, even in a very favourable nidus for their development, and also that in regions inhabited by plants or animals, whether in

[1] See *Annales des Sciences Naturelles,* 1861 and 1865.

cities or in the country, nearly every cubic inch of atmosphere really does contain living germs floating in it.

But there is one other experiment related by Pasteur,[1] which is in some respects even more striking. A flask is prepared similar to those already described, except that, after the introduction of the decoction of yeast, the neck is not only drawn out into a pretty narrow tube, but bent at various angles. The fluid is then boiled as in the former experiments; but the end of the neck, instead of being sealed, is left open, so that air passes into the flask on withdrawal of the lamp. The vessel being then left undisturbed, the diurnal changes of temperature, involving alternate expansion by day, and condensation at night of the gases in the flask, necessitate a daily interchange between the air in the body of the flask and the external atmosphere. Yet the fluid, though exposed in this way to air perpetually changed, remains for an indefinite period quite transparent, without trace of organic development. There can be but one interpretation of this fact. The oxygen, whether in its ordinary condition or that of ozone, with all the other atmospheric gases, including any which may exist in such small quantities as to be undiscoverable by the chemist, must pass, each in its own proportion, unchanged into the body of the flask. It is impossible that a dry glass tube can stop any gas. For though the tube is moist from condensation of aqueous vapour in the first instance, it is soon dried by the air that passes in and out through it. It is, therefore, inconceivable that any atmospheric gas can have been arrested by the tube. But it is con-

[1] This experiment is attributed by Pasteur to M. Chevreul.

ceivable, considering the very gradual character of the movements of the air in consequence of the diurnal changes, that dust, even though very fine, may be arrested by the angles. We may, perhaps, wonder that particles of such extreme minuteness as the germs of atmospheric organisms should be so detained. But no one can say it is impossible, and no other possible explanation presents itself. The experiment proves with certainty that the gases of the air, however abundantly supplied, are of themselves unable to originate the growth of torulæ and the other minute organisms which appear in decoction of yeast freely exposed to the atmosphere; and also that the essential source of such development must be suspended particles or germs. But in order to render the experiment, if possible, still more conclusive, the Committee of the Academy completed it by sealing the end of the neck of the flask, after the fluid had remained clear for a sufficient length of time to show that no organisms could grow in it, and inverting and shaking the vessel till some of the liquid passed into the angles of the bent tube, after which the flask was again left to itself. And now, gentlemen, occurred something which you may perhaps be disposed to regard as too good to be true, but which is true nevertheless. In the course of no long time the fluid in the angles of the tube exhibited indications of organic growth, demonstrating that the sources or germs of such development had, as a matter of fact, been arrested there.

This experiment charms us alike by its simplicity and perfect conclusiveness. Here is evidence indeed, which, if the facts be admitted, cannot be gainsaid. But though

I could not doubt the authority on which it rested, I felt desirous, if possible, to bring it to bear more directly upon the subject of putrefaction. The fluid which seemed most likely to answer the purpose, combining transparency with a high degree of putrescibility, was urine, and I accordingly made it the subject of the experiment to which I now desire to direct your attention.[1] Two years ago last month, I introduced portions of the same specimen of fresh urine into four flasks, of which two are before you. The body of each vessel was about one-third filled with the liquid. After the introduction of the fluid, the necks of three of them were drawn out into tubes rather less than a line in diameter, and then bent at various acute angles, as you observe in one of these. In the other the neck was drawn out to a calibre if anything rather finer, but cut short and left vertical, as you see it. The liquid in each flask was then boiled for five minutes, the steam issuing freely from the open end of the narrow neck. The reason for boiling it so long is that, as Pasteur has shown, merely raising this fluid to the temperature of 212° F., and then allowing it to cool, is not enough to kill all the organisms it may contain. It is necessary to maintain the elevated temperature for about five minutes to insure complete destruction of their vitality.[2] The lamp being then re-

[1] Since making the experiment, I have learned that Pasteur had previously performed it with urine.

[2] See *Comptes Rendus*, vol. L. p. 306. It follows that if any germs were drawn into the body of the flask with the air that rushes in on the withdrawal of the lamp, they would retain their vitality in the hot liquid, and develope in it when it had cooled. I have elsewhere expressed the opinion that the germs contained in the air which is thus rapidly admitted in the first instance must be arrested by the drops of water which appear in the

moved, air of course passed in to take the place of the condensed aqueous vapour. And during the two years that have since elapsed, a considerable fraction of a cubic inch of fresh air has entered every night into the body of each flask to exert its influence upon the liquid. In the case of the flasks with contorted neck, the air moving to and fro through the tube soon dried the moisture which was at first deposited within it; and any of you may see, after lecture, that in the one before you the neck is dry as well as open from end to end, so that it could present no obstacle to any gaseous constituent of the atmosphere. Nevertheless, though thus freely exposed to the action of the gases of the air for so long a period, including two unusually hot summers, the urine still retains its original straw colour and perfect transparency, presenting neither cloud, scum, nor sediment; and the only change that I can detect in it is, that of late, as a result, I presume, of the slow evaporation that has been going on in consequence of the perpetual change of air, some very minute shining crystals have been deposited upon the sides of the glass. Similarly unaltered are the contents of the other two similar flasks which I have not thought it needful to bring here. But very different is the appearance of the urine in this other flask, whose neck, short and vertical, was calculated to admit particles of dust as well as gaseous material. The transparent straw colour has given place to a muddy brown, with abundant sediment, including the

angles of the tube immediately on the cessation of ebullition, just as the particles of dust in inspired air are stopped by the mucus of our bronchial tubes. See *British Medical Journal*, July 18, 1868.

débris of different fungi, which have long since ceased to grow, poisoned, no doubt, by the acridity of the liquid, the pungently ammoniacal character of which may be readily ascertained by placing the warm hand for a moment upon the body of the flask, while one nostril is kept above the orifice.

Soon after the commencement of the experiment, this short-necked flask had a really beautiful appearance. Two different kinds of fungi presented themselves—one of exceedingly delicate structure growing rapidly from the bottom of the vessel, so as to occupy in no long time the greater part of the bulk of the liquid; the other a dense blue mould floating at the surface, and extending slowly in concentric rings. Meanwhile the fluid gradually assumed a deeper and deeper amber tint, indicative of progressive change in its chemical composition.

In the case of the flasks with bent necks I was not content with observing the completely unchanged appearance of the contained urine. Half a year after the experiment was begun I poured out about half an ounce of the clear contents of one of them into a wine-glass for examination. Its odour was perfectly sweet, and its reaction faintly acid; and under the microscope a careful search with an excellent glass of high power failed to detect vibrio, bacterium, or any other organism. The lowest known forms of organic development and the slightest approach to putrefactive change had been alike prevented by simply filtering the air of its floating molecules.

Yet the urine which had so long remained unaltered under the free influence of the gaseous constituents of the atmosphere proved as prone as ever to the usual effects of exposure to the air as soon as particles of dust could gain access to it; for the wine-glass having been covered to prevent evaporation, I found the fluid in two days with a dunghill odour, and loaded with minute microscopic organisms, and a few days later different kinds of fungi visible to the naked eye were growing in it.

Gentlemen, I commend these facts to your candid and impartial judgment, beseeching you to form your own opinions regarding them. The minds which you bring to bear upon this subject to-day are very much the same as they will be throughout your lives. An observation which any one of you may make now will serve in after life to illustrate a course of lectures, should he occupy a position corresponding to that which I have now the honour to hold. And you are as competent as you ever will be to draw logical inferences from established data. Do not, then, let any authority shake your confidence in knowledge so obtained.

Throughout the course on which we are entering I shall endeavour, as far as possible, to place before you simple facts,—trusting that, in estimating their significance, you will be ever guided by that which our dear master has so constantly striven to inculcate as our leading principle, the love of Truth.

THE END.