

on the bosom of the troubled ocean. I have seen those truths illumine with a light from heaven the dim eyes soon to be closed for ever by the cold hand of death. These truths are more dear to me than all that Nature can teach me, because they touch my inner life and consciousness. I learned those truths as a little child upon my mother's knee; I cherish them in my heart of hearts; and in defence of them, if opportunity should offer and God should count me worthy, I would gladly lay down my life.

## DUST AND DISEASE:

BEING

*Part of a Lecture delivered at the Royal Institution of Great Britain.\**

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AFTER some preliminary experiments and observations on the polarisation of light by fine dust, by the sky, and the coarser particles of smoke, Professor Tyndall proceeded:

In looking at this illuminated dust, we may ask ourselves what it is. How does it act, not upon a beam of light, but upon our own lungs and stomachs? The question at once assumes a practical character. We find, on examination, that this dust is of organic matter—in part living, in part dead. There are among it particles of ground straw, torn rags, smoke, the pollen of flowers, the spores of fungi, and the germs of other things. But what have they to do with the animal economy? Let me give you an illustration to which my attention has been lately drawn by Mr. George Henry Lewes, who writes to me thus.

"I wish to direct your attention to the experiments of von Recklinghausen, should you happen not to know them. They are striking confirmations of what you say of dust and disease. Last spring, when I was at his laboratory in Würzburg, I examined with him blood that had been three weeks, a month, and five weeks, out of the body, preserved in little porcelain cups under glass-shades. This blood was living and growing. Not only were the amoeba-like movements of the white corpuscles present, but there were abundant evidences of the growth and development of the corpuscles. I also saw a frog's heart still pulsating which had been removed from the body (I forget how many days, but certainly more than a week). There were other examples of the same persistent vitality or absence of putrefaction. Von Recklinghausen did not attribute this to the absence of germs—germs were not mentioned by him; but when I asked him how he represented the thing to himself, he said the whole mystery of his operation consisted in keeping the blood *free from dirt*. The instruments employed were raised to a red heat just before use, the thread was silver-thread and was similarly treated, and the porcelain cups, though not kept free from air, were kept free from currents. He said that he often had failures, and these he attributed to particles of dust having escaped his precautions."

Professor Lister, who has founded upon the removal or destruction of this "dirt" great and numerous improvements in surgery, tells us of the effect of its introduction into the blood of wounds. He informs us what would happen with the extracted blood should the dust get at it. The blood would putrefy and become foetid; and when you examine more closely what putrefaction means, you find the putrefying substance swarming with organic life, the germs of which have been derived from the air.

Another note which I received a day or two ago has a bearing particularly significant at the present time upon this question of dust and dirt, and the wisdom of avoiding them. The note is from Mr. Ellis of Sloane Street, to whom I owe a debt of gratitude for advice given to me when sorely wounded in the Alps. "I do not know", writes Mr. Ellis, "whether you happened to see the letters, of which I enclose you a reprint, when they appeared in the *Times*. But I want to tell you this in reference to my method of vaccination as here described, because it has, as I think, a relation to the subject of the intake of organic particles from without into the body. Vaccination in the common way is done by scraping off the epidermis, and thrusting into the punctures made by the lancet the vaccine virus. By the method I use (and have used for more than twenty years) the epidermis is lifted by the effusion of serum from below, a result of the irritant cantharidine applied to the skin. The little bleb thus formed is pricked, a drop of fluid let out, and then a fine vaccine point is put into this spot, and after a

minute of delay it is withdrawn. The epidermis falls back on the skin and quite excludes the air—and not the air only, but what the air contains.

"Now mark the result: out of hundreds of cases of revaccination which I have performed, I have never had a single case of blood-poisoning or of abscess. By the ordinary way the occurrence of secondary abscess is by no means uncommon, and that of pyæmia is occasionally observed. I attribute the comparative safety of my method, first, to the exclusion of the air and what it contains; and, secondly, to the greater size of the apertures for the inlet of mischief made by the lancet."

I bring these facts forward that they may be sifted and challenged if they be not correct. If they be correct, it is needless to dwell upon their importance; nor is it necessary to say that, if Mr. Ellis resigned himself wholly to the guidance of the germ-theory, he could not have acted more in accordance with the requirements of that theory than he has actually done. It is what the air contains that does the mischief in vaccination. Mr. Ellis's results fall in with the general theory of putrefaction propounded by Schwann, and developed in this country with such striking success by Professor Lister. They point, if true, to a cause distinct from bad lymph for the failures and occasional mischief incidental to vaccination; and, if followed up, they may be the means of leaving the irrational opposition to vaccination no ground to stand upon, by removing even the isolated cases of injury on which the opponents of the practice rely.

We are now assuredly in the midst of practical matters. With your permission, I will recur once more to a question which has recently occupied a good deal of public attention. You know that as regards the lowest forms of life, the world is divided, and has for a long time been divided, into two parties, the one affirming that you have only to submit absolutely dead matter to certain physical conditions to evolve from it living things; the others, without wishing to set bounds to the power of matter, affirming that in our day no life has ever been found to arise independently of pre-existing life. Many of you are aware that I belong to the party which claims life as a derivative of life. The question has two factors: the evidence, and the mind that judges of the evidence; and you will not forget that it may be purely a mental state or bias on my part that causes me throughout this discussion, from beginning to end, to see on the one side dubious facts and defective logic, and, on the other side, firm reasoning and a knowledge of what rigid experimental inquiry demands. But, judged of practically, what, again, has the question of spontaneous generation to do with us? Let us see. There are numerous diseases of men and animals that are demonstrably the products of parasitic life, and such disease may take the most terrible epidemic forms, as in the case of silkworms of France in our day. Now, it is in the highest degree important to know whether the parasites in question are spontaneously developed, or are wafted from without to those afflicted with the disease. The means of prevention, if not of cure, would be widely different in two cases.

But this is by no means all. Besides these universally admitted cases, there is the broad theory now broached and daily growing in strength and clearness—daily, indeed, gaining more and more of assent from the most successful workers and profound thinkers of the medical profession itself—the theory, namely, that contagious disease generally is of this parasitic character. If I had heard or read anything since to cause me to regret having introduced this theory to your notice more than a year ago, I should here frankly express that regret. I would renounce in your presence whatever leaning towards the germ-theory my words might then have betrayed. Let me state in two sentences the grounds on which the supporters of the theory rely. From their respective viruses you may plant typhoid fever, scarlatina, or small-pox. What is the crop that arises from this husbandry? As surely as a thistle rises from a thistle-seed, as surely as the fig comes from the fig, the grape from the grape, the thorn from the thorn, so surely does the typhoid virus increase and multiply into typhoid fever, the scarlatina virus into scarlatina, the small-pox virus into small-pox. What is the conclusion that suggests itself here? It is this:—That the thing which we vaguely call a virus is to all intents and purposes a *seed*; that in the whole range of chemical science you cannot point to an action which illustrates this perfect parallelism with the phenomena of life—this demonstrated power of self-multiplication and reproduction. There is, therefore, no hypothesis to account for the phenomena but that which refers them to parasitic life.

And here you see the bearing of the doctrine of spontaneous generation upon the question. For if the doctrine continue to be discredited as it has hitherto been, it will follow that the epidemics which spread havoc amongst us from time to time are not spontaneously generated, but that they arise from an ancestral stock whose habitat is the human body itself. It is not on bad air or foul drains that the attention of the physician will primarily be fixed, but upon disease-germs which no bad

\* For this report, we are indebted to our contemporary *Nature*.

air or foul drains can create, but which may be pushed by foul air into virulent energy of reproduction. You may think that I am treading on dangerous ground, that I am putting forth views that may interfere with salutary practice. No such thing. If you wish to learn the impotence of medical science and practice in dealing with contagious diseases, you have only to refer to a recent Harveian oration by Dr. Gull. Such diseases defy the physician. They must burn themselves out. And, indeed, this, though I do not specially insist upon it, would favour the idea of their vital origin. For if the seeds of contagious disease be themselves living things, it will be difficult to destroy either them or their progeny without involving their living habitat in the same destruction.

And I would ask you to be cautious in accepting the statement which has been often made, and which is sure to be repeated, that I am quitting my own *métier* when I speak of these things. I am not dealing with professional questions. I am writing no prescription, nor should I venture to draw any conclusion from the condition of your pulse and tongue. I am dealing with a question on which minds accustomed to weigh the value of experimental evidence are alone competent to decide, and regarding which, in its present condition, minds so trained are as capable of forming an opinion as on the phenomena of magnetism and radiant heat. I cannot better conclude this portion of my story than by reading to you an extract from a letter addressed to me some time ago by Dr. William Budd of Clifton, to whose insight and energy the town of Bristol owes much in the way of sanitary improvement.

"As to the germ-theory itself," writes Dr. Budd, "that is a matter on which I have long since made up my mind. From the day when I first began to think of these subjects, I have never had a doubt that the specific cause of contagious fevers must be living organisms. It is impossible, in fact, to make any statement bearing upon the essence or distinctive characters of these fevers, without using terms which are of all others the most distinctive of life. Take up the writings of the most violent opponent of the germ-theory, and, ten to one, you will find them full of such terms as 'propagation', 'self-propagation', 'reproduction', 'self-multiplication', and so on. Try as he may—if he have anything to say of those diseases which is characteristic of them—he cannot evade the use of these terms or the exact equivalents to them. While perfectly applicable to living things, these terms express qualities which are not only inapplicable to common chemical agents, but, as far as I can see, actually inconceivable of them."

Once, then, established within the body, this evil form of life, if you will allow me to call it so, must run its course. Medicine as yet is powerless to arrest its progress, and the great point to be aimed at is to prevent its access to the body. It was with this thought in my mind that I ventured to recommend, more than a year ago, the use of cotton-wool respirators in infectious places. I would here repeat my belief in their efficacy if properly constructed. But I do not wish to prejudice the use of these respirators in the minds of its opponents by connecting them indissolubly with the germ-theory. There are too many trades in England where life is shortened and rendered miserable by the introduction into the lungs of matters which might be kept out of them. Dr. Greenhow has shown the stony grit deposited in the lungs of stone-cutters. The black lung of colliers is another case in point. In fact, a hundred obvious cases might be cited, and others that are not obvious might be added to them. We should not, for example, think that printing implied labours where the use of cotton-wool respirators might come into play; but I am told that the dust arising from the sorting of the type is very destructive of health. I went some time ago into a manufactory in one of our large towns, where iron vessels are enamelled by coating them with a mineral powder and subjecting them to a heat sufficient to fuse the powder. The organisation of the establishment was excellent, and one thing only was needed to make it faultless. In a large room a number of women were engaged covering the vessels. The air was laden with the fine dust, and their faces appeared as white and bloodless as the powder with which they worked. By the use of cotton-wool respirators, these women might be caused to breathe air more free from suspended matters than that of the open street. Over a year ago I was written to by a Lancashire seedsman, who stated that during the seed-season of each year his men suffered horribly from irritation and fever, so that many of them left his service. He asked me could I help him, and I gave him my advice. At the conclusion of the season this year he wrote to me that he had simply folded a little cotton-wool in muslin, and tied it in front of the mouth; and that he had passed through the season in comfort, and without a single complaint from one of his men.

The substance has also been turned to other uses. An invalid tells me that at night he places a little of the wool before his mouth, slightly moistening it to make it adhere; that he has thereby prolonged his

sleep, abated the irritation of his throat, and greatly mitigated a hacking cough from which he had long suffered. In fact, there is no doubt that this substance is capable of manifold useful applications. An objection was urged against the use of it: that it became wet and heated by the breath. While I was casting about for a remedy for this, a friend forwarded to me from Newcastle a form of respirator invented by Mr. Carrick, an hotel-keeper at Glasgow, which meets the case effectually, and, by a slight modification, may be caused to meet it perfectly. It consists of a space under a partition of wire-gauze intended by Mr. Carrick for "medicated substances", and which may be filled with cotton-wool. The mouth is placed against an aperture, which fits closely round the lips; and the air enters thy mouth through the cotton-wool by a light valve, which is lifted by the act of inhalation. During exhalation, this valve closes; another breath escapes by a second valve into the open air. The wool is thus kept dry and cool; the air passing through it being filtered of everything it holds in suspension.\*

## EXFOLIATION OF THE LINING OF THE BLADDER.

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THE interesting case recorded in the JOURNAL of June 10th, by Dr. Wardell of Tunbridge Wells, in which there was exfoliation of the mucous membrane of a female urinary bladder, although doubtless rare, is not, as Dr. Wardell supposes, unique. At a meeting of the Obstetrical Society, held on December 4th, 1861, Mr. Spencer Wells exhibited a cast of a bladder, expelled by a patient who a few weeks previously had been delivered of her first child with instrumental assistance after considerable difficulty. Symptoms of cystitis followed; but after the expulsion of the membrane recovery rapidly ensued. The specimen was examined by Dr. Harley and Mr. Wells, and was found to consist of the whole of the mucous membrane and some portions of the muscular coat of a dilated bladder. At a subsequent meeting, Dr. Tanner referred to a specimen in the museum of the College of Surgeons, which was found in the interior of a male bladder after death, and which is described as "a structure exactly resembling the mucous membrane of a bladder separated as a slough in one piece".

A case presenting many points of resemblance to that recorded by Mr. Wells was brought before the Pathological Society by Dr. Martyn in 1863, in which exfoliation followed long retention of urine during labour; and in the course of the same year Dr. Wilks and Mr. Henry Lee exhibited specimens of the kind found after death within the bladder, perfectly detached from the muscular coat. In the year 1868, the following case, which has not hitherto been published, came under my notice.

M. J., aged 21, was seen by me in consultation on the 11th September. She had been delivered of her first child on the 8th of that month, and the medical man who was called in on that day said that he found her with the child's head firmly wedged in the pelvis, and her bladder extending to the umbilicus. It was said that she had been in labour four days. Considerable extractile force with the forceps had to be used to complete delivery. On the 11th, the lower part of the vagina and the perinæum were in a state of superficial slough from the long-continued pressure. These parts, however, healed well; but the patient was admitted under my care into the Waterloo Road Infirmary on the 7th of October on account of the urinary troubles from which she suffered. There was at that time almost complete incontinence of urine, but no communication existed between the bladder and the vagina or uterus. The meatus urinarius was so dilated as almost to admit the tip of the little finger, and a small portion of the anterior part of the urethra had sloughed away. The day after admission she passed from the bladder a membranous substance, the whole of one surface of which was coated with a phosphatic deposit, and which, when floated in water, appeared to form a nearly complete cast of the bladder. This patient improved under treatment; and a week later, when the bladder was being washed out, small shreds of a similar character were expelled. The last exfoliation took place on the 31st October, when a piece of the mucous membrane and its submucous tissue came away, having its inner surface covered with phosphates.

Judging from the great frequency with which retroversion of the gravid uterus produces retention of urine, and also leads to expulsion of its own contents between the third and fourth months of gestation, and guided by the history of Dr. Wardell's case, I venture to suggest that this condition may have existed.

\* Mr. Ladd, of Beak Street, sells these respirators.