Children, however, vary immensely in comparative size for age; and I must explain how to reconcile all discrepancies. Some healthy children, slow growing, and, perhaps, of short parentage, increase at the rate of 2 inches a year instead of $2\frac{1}{2}$; and, if such a child be up to the average weight for its size, nothing unhealthy is indicated, but prospectively a short adult stature.

In the same way, quick growing strong children of large parentage may grow at the rate of 3 inches a year, and without harm, so long as the weight tallies with that shown in this table.

The slow growing short child will attain 5 feet and weigh 6 st. 6 lbs. at fifteen years of age, whereas the fast growing one will do the same at eleven, and the average one at a little under thirteen years. All three are healthy children that should be left to go on developing at this their normal rate. Any attempt to make the short child grow taller would either fail, or do more harm than good. Gymnastics, plenty of exercise, and good hygienic conditions are all that is necessary or allowable.

Arrest of development, whether in latitude or longitude, is an early sign of disease, and is as demonstrable by the weighing-machine as anything shown by the thermometer, which is an equally homely instrument only of late years used in clinical research.

By means of Table III, it is easy to tell whether a child is overgrowing or undergrowing, either in height, weight, or thickness.

Perhaps it most frequently happens that a child grows up to the average height, but does not weigh accordingly, the limbs and chest being ill-developed. There are no other morbid signs, and consequently such a child is left to grow up in this state, if some disease do not first carry it off, which is more than likely.

What I insist on as the chief value of weight and measurement is, that we gain information during childhood which puts one in a position to combat an unhealthy manner of growth, often the forerunner of disease, resulting in a delicate and debilitated manhood. Some cases will baffle every effort; but a large percentage will repay the care bestowed upon them. Many will grow up big and strong that would otherwise be small and weak; and some live that otherwise would die.

In treating of children over 5 feet, we meet with several difficulties, for we have arrived at a point where fixed adult standards mix in with those of growing children.

As the difference between the weight of a growing child of more than 5 feet and an adult of the same height should be 2 st., and as this proportion holds good for any size, the simplest way of getting over what would require another overlapping table is to give the adult weights, with the understanding that before maturity 28 lbs. should be deducted from that stated in Table IV.

TABLE IV.

[N.B.—For a growing person, deduct 2 st. from any of the above weights.]

Height		Weight.	Rate per inch of total height.
Feet.	In.	st. lbs.	
5	0	8 8	2 lbs. per inch of stature
5	1	8 13	
5	2	9 4	
5	3	9 9	
5	4	10 0	5 lbs. per inch added
5	5	10 5	
5	6	10 10	
5	7	11 1	
5	8	11 6	2½ lbs. per inch of stature
5	9	11 11	
5	10	12 2	
5	11	12 7	
6	0	12 12	6 lbs. per inch added
6	1	13 4	
6	2	13 10	
6	3	14 2	
6	4	14 8	3 lbs. per inch of stature
6	5	15 0	
6	6	15 6	
6	7	15 12	
6	8	16 4	
6	9	16 10	
6	10	17 2	
6	11	17 8	
7	0	18 0	

For instance, a boy of 5 feet 5 inches would weigh 8 st. 5 lbs. instead of 10 st. 5 lbs. as given, and if this boy never grew any taller he would attain the latter weight when a fully developed man. I think no tables could be drawn up to show the rate of transition; but I have hopes that we may be able by statistics to tell beforehand of what height a child will be when fully grown, from the age at which he passes 5 feet.

Table IV has resulted from hundreds of my own carefully taken

records, and is not simply theoretical; nevertheless, it will be seen to follow certain laws which enable one to calculate what would be the proportional weight of a giant of any number of feet.

The weights and measurements which I give are the nearest to what I find consistent with the most perfect development. Perfection is not, perhaps, often attained, but is to be aimed at.

I have said before that *adult* tables are not of the value that has been supposed, and that each individual should be judged only by comparison with his own previously ascertained healthy weight.

Great variations are met with in perfect health, due to temperament or diathesis, for there are human varieties. I know two men of the same height and in perfect health, whose weights differ by 10 st.; and it is, therefore, absurd to talk of vital capacity being affected when weight is 7 per cent. over the average. Captain Webb, when he swam across the Channel, stood 5 feet 8 inches, and weighed $14\frac{1}{2}$ st.; and Dr. Hutchinson only allows 11 st. 12 lbs. to be consistent with health. Samuel Brock, who on October 6th, 1835, swam fifteen miles, and was in the water for seven and a half hours, stood 5 feet 5 inches, and weighed 14 st. Immense power and endurance seem generally to reside in a person of short, thick, bull-dog frame. Large bones and muscles do not injuriously affect vital capacity, but they weigh heavily, and do not usually accompany ideal symmetry. The possessors of them are exceptionally powerful as a rule; but, being exceptional, do not fit into this my last table.

I had not intended to publish this paper in its present crude condition; but the circumstances which I have named at the commencement lead me to think that it may be particularly interesting and *à propos* at this time.

A WORD ON THE ORIGIN OF BACTERIA, AND ON ABIOGENESIS.

By WILLIAM ROBERTS, M.D., F.R.C.P.,

Physician to the Royal Infirmary, and Professor of Medicine in Owens College, Manchester.

DR. BASTIAN has cited my name and quoted my experiments in a way that might make it appear that my investigations lend some support to his views on the origin of bacteria in organic infusions; this is, however, not the case. On the contrary, the weight of my experiments is entirely against him and in favour of Pasteur's conclusions. I found, as others have done, that some infusions and organic mixtures produced bacteria after having been boiled for a certain time; but I also invariably found that, if the boiling were sufficiently prolonged, no such result followed. All were rendered permanently barren—even Dr. Bastian's favourite turnip-and-cheese mixture. In quoting my experiments on alkalised infusion of hay, Dr. Bastian omitted to add, that in the same paper from which he was quoting, I furnished decisive *experimental proof* (not a mere explanation) that the germination which took place after boiling was due to a survival of pre-existing germs, and *not* to a *de novo* generation.

The results obtained by me were in substance identical with those obtained by Pasteur and Professor Tyndall. The circumstance that Professor Tyndall did not encounter those examples of great resistance to sterilisation by heat that I encountered, involves no contradiction in our results. His procedure was different from mine, but our results were the same. We both succeeded by boiling in sterilising our infusions without impairing their aptitude for the growth of bacteria. As well might we say that two chemists contradict each other when they obtain the same metal from the same ore by different processes.

It appears to me, that the attitude of Dr. Bastian on the question of the origin of bacteria arises from what I may call the inverted significance which he attaches to the two contrasted results—barrenness or fertility—which follow after boiling an organic infusion. Throughout the controversy, Dr. Bastian speaks of the barren tubes and flasks as “failures” or “negative results”; and he evidently regards the fertile tubes and flasks as “successful” experiments, having the force and authority of “positive” results. The true view is just the reverse of this, and his misunderstanding on this point makes him blind to the overwhelming cogency of the case against him. When the matter is duly considered, it is the barren flask that has the character of a positive result. For what does the experimenter set himself to do in these experiments? He seeks to destroy by boiling all pre-existing bacteria in these infusions, and to leave unimpaired their powers of promoting the growth of bacteria. And it is found in fact that this latter quality is perfectly preserved in boiled infusions; for they breed bacteria with the utmost luxuriance when they are reinfected from an extraneous source.

When the experimenter finds that his infusions germinate after boiling, the *primâ facie* probability is, that he has either applied the heat insufficiently, or has permitted extraneous infection after boiling; for this is exactly what would occur if he failed in either of these points. But if the infusions remain barren, this is a new and unexpected consequence, and carries with it the weight and cogency of a positive result. The explanation of the fertile flask is thus ready at hand; it is simply a faulty experiment; but what possible explanation can be given of the barren flask, except that supplied by the panspermic theory? When I take up one of the flasks or bulbs which have remained barren in my chamber for three or four years, though supplied with air (filtered through cotton-wool) and suitable heat, my wonder never ceases. Each one is a new experiment, every day repeated, and multiplied indefinitely; day after day I ask myself, Why does it not germinate? I compare it to a field in spring not yet sown, but ready for the reception of the seed: for if I withdraw the plug of cotton-wool and admit the dust of the air, or introduce a drop of water, all is changed; in a few hours the stillness of years gives place to life and activity. I repeat, it is the fertile flask, and not the barren flask, that wears the complexion of a failure and of a negative result.

The reluctance of some evolutionists to give up the spontaneous origin of bacteria is evidently due to the notion that this question is bound up with that of abiogenesis generally. This is a wholly erroneous idea. The question of abiogenesis will still remain after all have acquiesced in Pasteur's views of the origin of bacteria: indeed, to a logical evolutionist there would appear to be a strong *à priori* improbability in the abiogenic origin of bacteria. They were not wanted, and could not exist, on the earth's surface until after other organisms had lived and died before them. Their special function and feeding-ground lie amid the wreck of living things. And if the survival of the fittest hold good in regard to bacteria, they must be the remote progeny of less perfect organisms of the same class. What can be more perfect than their adaptability to their place and use in the order of Nature? They resist, in certain media, for considerable periods the heat of boiling water; they multiply with incredible speed; their germs survive in countless myriads in the dust of the atmosphere; they float in every drop of water on land and sea; they appear to be omnipresent and almost indestructible. Those who are in search of a case of abiogenesis, should seek among the primitive organisms—if there be any such—which can exist and grow amid inorganic elements, in the water of the sea, or the mineralised springs and streams of the land. When Pasteur says that abiogenesis is a chimera, he prudently adds, "in the present state of science"; and even thus qualified, the expression is perhaps too strong. But it is absolutely certain that up to the present time no case of abiogenesis has been presented which has stood the test of accurate investigation; nor can it be doubted that, in so far as the antiseptic treatment of disease rests on the origin of bacteria, the advocates of that treatment stand on unassailable ground.

CASE OF ANEURISM OF THE THORACIC AORTA, WITH A DISTINCT SYPHILITIC HISTORY.

By W. C. MACLEAN, C.B., M.D.,

Professor of Military Medicine in the Army Medical School, Netley.

A MAN, thirty-seven years of age, of indifferent character and intemperate habits, of medium height (5 feet 7 inches), and very good muscular development, by trade a butcher, had served nearly twenty years, of which he had spent six-and-a-half in India, eight in the Mediterranean, and the remainder at home. In 1863, he suffered, while in India, from ague, and again from it in 1865 at home. In the same year (1865), he was for five weeks under treatment for primary syphilis, and, after an interval of two months, for secondaries. In 1866, he was treated for rheumatism and neuralgia. There are no entries recorded from 1866 to 1872, but in the latter year he was for six weeks under treatment for pneumonia; in 1873 and 1875, for bronchitis; and, without being discharged from hospital, his case was changed to one of asthma, for which he was finally invalided, arriving at Netley on October 29th, 1865. In May and June 1875, he was treated for bronchitis, and in July was taken into hospital for asthma. About the 8th July, he had for the first time hæmoptysis, which recurred from time to time before embarkation and during the voyage. This symptom was noticed after his admission, and most prominently on December 20th.

Apart from hæmoptysis, the patient suffered much on arrival from symptoms of asthma, with profuse frothy expectoration, his condition becoming aggravated or ameliorated by the weather. The hæmoptysis,

dyspnoea, hoarseness, and dorsal pain which appeared in succession, led to a diagnosis of aneurism, which became confirmed as dullness, and finally pulsation, became perceptible to the right of the sterno-clavicular articulation. Dyspnoea latterly became the most prominent, as well as the most distressing, symptom, till the 7th January, when, after a short fit of coughing, there was a gush of blood from the mouth, and death took place instantaneously.

On *post mortem* examination, both lungs were found to be in a state of apoplexy. The trachea was full of dark fluid, which had flowed into it from a small ulcerated opening, about a quarter of an inch in diameter, about two inches above the bifurcation, and slightly to the left of the middle line. The heart was small, and showed a considerable deposit of fat on its surface, and a well marked white spot. Its substance was flaccid, its cavities empty, and the left ventricle thickened. The valves were healthy. At the commencement of the arch of the aorta, just beyond the innominate, was an aneurism as large as an orange, and in its wall was an irregular opening, through which a probe could be passed into the trachea. The edges of the aneurism were firm, rounded, and well defined. The lining surface of the aorta was very irregular, marked with nodular elevations and cicatricial loss of substance in patches. The calvarium was thickened. The glans penis bore the mark of a chancre; and, in the head of each epididymis, was a nodule, partly fibrous and partly cystic.

It will be remembered that a paper was read by my colleague, Mr. Welch, Assistant Professor of Pathology at Netley, on Aortic Aneurism, before the Royal Medical and Chirurgical Society, on the 22nd of November, 1875. In this paper, Mr. Welch insisted much on the part played by syphilis in causing the degeneration of the arterial walls, terminating in aneurism, supporting his opinion by evidence derived from a careful examination of the preparations in the museum here, of the *post mortem* registers of this hospital, and his own large experience in the *post mortem* room here and elsewhere. In the course of the interesting debate which followed, my able friend Mr. Myers, of the Coldstream Guards, disputed the opinion put forward by Mr. Welch, and, as I am informed, quoted me as favouring the view supported by himself as to the exclusive, or almost exclusive, action of a faulty system of dressing and accoutring the soldier, in causing aneurisms of the thoracic and abdominal aorta. Now, it is well known that no one can be more impressed than I am with the great mischief done by the old faulty system; and those who take an interest in this subject are aware that, when it was under discussion, I did my utmost to spread a knowledge of the facts bearing on the point, not only in the medical department, but among the combatant ranks of the army, and even incurred some odium for pressing my opinions with what was regarded as excess of zeal. But if my friend Mr. Myers will refer to what I have written on the subject, he will nowhere find that I ever committed myself to the opinion that syphilis is not a factor in causing the degenerations of the great vessels so well described by Mr. Welch, and which do most certainly prepare the way for the action of the faulty system insisted on by Mr. Myers. So far from this, I read a paper, six years ago, before the Medical Society of Southampton, "On Aneurisms of the Great Vessels as seen in Soldiers", in which I brought forward evidence derived from the same sources as those from which Mr. Welch obtained his; and I quoted the observations and statistics of Dr. Davidson, one of Mr. Welch's predecessors in the pathological department here, and showed a large collection of specimens of aortic aneurisms having syphilitic histories like the case I have given above, which is typical of those usually seen at our *post mortem* examinations here. As to Mr. Myers's argument that London hospital surgeons have not been impressed by the view that syphilis is the primary cause of a certain proportion of cases of aortic aneurisms, I submit that they have not the same means of arriving at an exact opinion on such a point that we have in a great military invaliding hospitals. London hospital surgeons learn just as much or as little of the medical history of their patients as they are pleased to reveal; while in the army, as was, indeed, pointed out in the course of the debate at the Royal Medical and Chirurgical Society, we have the invaluable evidence of the medical history sheet, which tells of every admission into hospital from the date of the soldier's enlistment to that of his death or discharge.

No one knows better than Mr. Myers that soldiers are for the most part celibates by compulsion in the most vigorous years of life, when the sexual passion is most exacting in its demands on their ungoverned minds; and that the result is promiscuous intercourse with the lowest class of prostitutes, followed, where certain much abused but beneficial Acts are not in operation, by a proportionate amount of disease, of which one of the results is a proportion of degenerative disease of the coats of the great vessels, far exceeding what is seen in civil life. I by no means wish to undervalue the action of the rheumatic diathesis in the same direction, or the part played by alcohol in increasing the heart's