useful. After the second or third year, cold bathing is most useful to counteract that peculiar sensitiveness to cold which is common in scrofulous children. More than ordinary care is needed in watching such children through the usual diseases of infancy and childhood, as whooping-cough, measles, scarlet fever, etc.; for we now know that pulmonary consumption often has its starting-point in the chronic pneumonia which frequently remains after these diseases, or the enlarged and inflamed glands which are a common sequel serve as foci of infection whence tubercular disease may be disseminated. It may be even necessary to extirpate such enlarged glands when they refuse to yield to constitutional treatment.

At the approach of puberty, we should look especially to the proper development of the thorax, we should see that the carriage of the body is such that the chest expands freely.* Gymnastic exercises specially designed to draw the shoulders backwards and dilate the upper part of the chest should be adopted, but we should be exceedingly careful that these do not overtax the imperfect breathing power. Cold ablutions should be associated with these exercises. We should be careful to maintain the nutrition of the body, and especially ward off anæmic conditions. We should select for such persons occupations involving much out-of-door exercise, and avoid all those that are sedentary, or which lead to the breathing of impure air.

In the case of incomplete resolution of inflammations of the respiratory organs in such persons, much good has been obtained by a course of mineral waters containing soda and lime (Ems), and in bathing in such saline springs as those of Reichenhall and Soden. Such courses may be advantageously followed by a residence in an elevated region; but of this I shall have more to say when I deal with the question of climate.

(c.) The third division of this part of my subject, viz., the prevention of phthisis arising from unhealthy social conditions, occupations, habitations, etc., has already been so ably and fully dealt with at this meeting, that any further observation on this head from me would be superfluous.

2. I next pass to the consideration of those remedial measures which modern research has suggested as of use in phthisis. From amongst these I propose to select for our present consideration the following chiefly.

a. The use of the *alkaline hypophosphites*.

b. The use of antiseptic inhalations, and of the sulpho-carbolates internally.

c. The treatment of phthisis by rest (local rest).

d. The removal to mountain climates.

a. Nothing could be more unfortunate than the manner in which the alkaline hypophosphites were introduced to the notice of the medical profession as remedies for pulmonary consumption. An unreasoning, untrustworthy, and injudicious advocacy, provoked a hasty, impatient, and somewhat unjust antagonism. The absurd and extravagant claim for them that they were specific remedies for phthisis, was met by the equally unfounded statement that they were worthless and inert. I hope we are now in a position, disregarding alike their original quackish advocacy, on the one hand, and their hasty rejection, on the other, to form a calm and unbiased opinion as to their real merits. For my own part, knowing how difficult it is in treating cases of phthisis to judge accurately of the influence of the remedies we administer, because of the many counteracting agencies to which such patients are exposed, especially when they are out-door hospital patients, I determined, in forming my own estimate of the value of the hypophosphites to give them perseveringly in a great number of cases, and extend their trial over a long period of time. During the

last five years, at the Brompton Hospital alone, I have given the hypophosphites in one form or another to nearly a thousand patients selected out of more than fifteen thousand cases that have been under my care in that institution.

Any one who cares to look at the records of these cases will be forced to admit that these remedies have had a most patient and persevering trial. From this extended experience of these remedies I have been able to arrive at the following conclusions.

In the first place, they have *no claim whatever* to be regarded as specific remedies for tubercular disease; but they are of considerable value in a certain limited class of cases.

In advanced phthisis, with both lungs involved—a very prevalent condition amongst the out-patients at Brompton—I have never been able to discover that the hypophosphites are of any use. Nor are they of use in less advanced cases which are obviously running a rapid course, and are attended with much fever and cachexia. I have given them in such cases again and again, and I have been compelled to abandon their use, often at the solicitation of the patients themselves. In short, in those cases which we have always been accustomed to regard as hopeless and beyond the reach of all remedies, the hypophosphites are of no avail. I have noticed that they are less successful with dark persons of sanguine or bilious temperament, than with fair persons of phlegmatic temperament. They are much more useful in young than in older persons. I have seen the greatest benefit arise from their use, in the first place, in children in all forms of chronic lung-disease; and, in the second place, in young adults of fair complexion, not very cachectic or emaciated, and with disease limited to the upper part of one lung. In the cases in which they do good, their beneficial effect is generally noticed almost immediately. The patients usually say they feel very much better “in themselves”; they feel stronger, in better spirits, are more active, eat better, and sleep better. The night-sweats disappear, the cough sometimes disappears also; but it will often be observed that, while there is this general amelioration and a subjective feeling of improvement, the cough will continue unrelieved, and even occasionally become more troublesome. Notwithstanding the evident improvement in general health, the physical signs often remain the same, and I have known them actually increase in extent, while the patient has been feeling so much better that he has resumed his occupation, and declared that he felt “as well as ever he did in his life”. The improvement which follows the use of the hypophosphites is frequently only of temporary duration; this is especially the case with hospital out-patients. It is much less so with private patients, who can be more carefully watched and more thoroughly protected from adverse circumstances.

I have seen all the symptoms of phthisis disappear during the use of the hypophosphites, and the general condition as well as the physical signs undergo rapid amelioration. The patient has been, apparently quite well, but an imprudent visit to a theatre has resulted in a fresh catarrh, and the disease has again advanced with unusual rapidity; the former remedies were now quite useless, and, in a few months, the case ended fatally. It has, indeed, seemed to me that, in some of these cases where the hypophosphites led to temporary arrest of the disease, the subsequent advance has been unusually rapid and quickly fatal. I would therefore, urge that patients who are mending under the influence of these remedies should be protected with more than usual care against all those conditions which may possibly lead to a relapse.

Such are some of the results of my own observations with regard to the use of the alkaline hypophosphites in the treatment of pulmonary consumption, and I believe they are consistent with the conclusions of many other recent observers. Time forbids my entering into many interesting details, or quoting the records of illustrative cases.

[To be continued.]

THE CAUSES OF THE SOUNDS OF THE HEART: REPLY TO DR. LEARED.

By W. H. BROADBENT, M.D., F.R.C.P.,
Physician to St. Mary's Hospital, etc.

My friend Dr. Leared attributes to heat what was really due to haste. I found myself at issue with him as to matters of fact, and plainness of speech, I thought, would conduce to clearness of meaning; and we certainly understand each other thoroughly.

We are in flat contradiction on the point whether, in listening to the naked heart from which the blood has been cut off, it is possible to distinguish between sound produced by friction of the stethoscope against the heart and sound produced by sudden tension of the muscu-

* Mr. Liebreich has given much attention to the correction of faulty attitudes and positions arising during school-life; and although his original object was to prevent and correct defects of vision, he has carried his observations further than this, and many of his remarks apply equally well to the rectification of those faulty attitudes which interfere with the healthy action of the organs of respiration, and promote the development of phthisical disease when a tendency to it exists. Speaking of the treatment of tendencies to spinal curvature, he observes: “If a hard surface is to give the back rest, without making it suffer, it must have certain curvatures so adapted to the normal shape of the body that this latter is supported everywhere, and its weight equally distributed. These curvatures I have carefully studied, and, following their outlines, I have designed a couch adapted to the inclination of the body at an angle of 45 deg. For girls who have any predisposition to lateral curvature of the spine, it will be found useful to let them do every work that will admit of it, while reclining on such couch at an angle of 45 deg.” (*School-Life in its Influence on Sight*, by R. Liebreich, Ophthalmic Surgeon at St. Thomas's Hospital). I have examined this couch, which can be seen at Callaghan's, optician, Bond Street; and it seems to me well calculated not only to support the back, but also to take the weight of the arms and shoulders off the upper part of the chest; for when these are allowed to hang forward, as in the stooping attitude habitual to many young people, they compress the upper part of the chest, and prevent due expansion of the apices of the lungs. I have, later on, called attention to some remarks of Professor Rindfleisch on this head.

lar walls of this organ. Dr. Leared says it is absolutely impossible. An universal negative is a very sweeping assertion—absolutely impossible to any and every human being. This is what Dr. Leared says, but I venture to think he means “absolutely impossible to me”; and I hope I shall have his thanks for the alteration I suggest. My own assertion then becomes, “It is easy to me”, and I have only to show how the element of friction is eliminated. Dr. Sibson found, as will be seen by reference to his *Medical Anatomy*, that there is a point on the anterior aspect of the heart towards which all other parts converge, and which is itself almost motionless. In all delicate observations, it was here that the stethoscope, which was flexible and had a small cup, was applied. There was no friction to hear. The statement which he quotes from Dr. Halford, “that neither first nor second sound was heard”, is beside the question. Dr. Leared must have heard sound under these conditions, as I have, or his assertion about it was simply unmeaning. Had he come to the same conclusion as Dr. Halford, I should probably never have written my remarks at all. The second sound disappears altogether, and the first becomes exceedingly feeble; but any sound that is audible must be entirely independent of friction.

As regards the anæmic aortic or pulmonic murmur, Dr. Leared accepts my correction, but endeavours to attenuate its effect. When he has restated the proposition put by him as a *crux*, which I challenged, with the modifications required by his admission that the murmur accompanies instead of replacing the sound, I will again consider it. In the meantime, I may give my own version of the question as it now stands. The argument, as I understood it, was, that the shock-sound being replaced by a murmur in consequence of the altered condition of the blood, the shock and murmur owned the same cause—collision or vibration of the blood. To this, my objection, admitted by Dr. Leared to be well founded, was fatal. The new ground now taken up is that, while it is impossible to deny that the sound and murmur coexist, this is only occasional, and the sound is feeble and degenerated; *i. e.*, apparently the sonorous vibrations are divided into shock and murmur. If, therefore, the shock-sound be not notably impaired when a murmur is present, the theory of the common origin of the first sound and murmur falls to the ground. We are now again in the field of observation. Unfortunately, the issue is not so simple and definite as when the coexistence of a sound and murmur only was in question. If it were, I should confidently expect an early surrender on this point also; for, according to my experience, not only does a first sound accompany an anæmic murmur, but this first sound is not otherwise diminished in intensity than through weak action of the heart. When, indeed, the heart's action is excited, the first sound is very loud, but sharp. The first sound must, of course, be sought where we hear it under other circumstances—at the apex or over the right ventricle, according as the murmur is aortic or pulmonary; and not for this occasion only over the aorta.

Dr. Leared's answers to my questions are before the reader, and there I am content to leave them, with one or two general observations. First, as to “the difficult question of the propagation and conduction of the sounds”, on which a part of these answers turns: I see no end to the discussion if we must cease to accept the recognised methods of localising the source of a sound whenever this is required by the exigencies of Dr. Leared's theory. Errors arising from the conduction of sound do not prevent us from forming a confident diagnosis in disease, and ought not to be invoked to break the force of an adverse observation. Another part of these answers turns on the difficulties attending the production of shock-sounds in fluids. This is exactly the reason why we, the “unbelievers”, are not even “convinced against our will”, and refuse to reject, in favour of so uncertain a cause, the sudden tension of membrane and muscle which must occur and which cannot but be attended with sound. If we deny to the valves any share in the production of the sounds, we must positively explain their silence. I tried in vain to obtain Dr. Leared's attention to this. Dr. Leared kindly informs me that, when I whistle, it is the air issuing from my lips which forms the sound. Quite so. But he wants me to believe also that the boom of the drum is formed in the same way. There is just the same difference between the murmurs and the sounds of the heart as between the “toot” of the fife and the “beat” of the drum.

I ought to conclude here, but my attention is specially called to the fact that there are instances in which the first sound is louder over the base than over the apex. Well, these exceptional cases would be an interesting clinical study; but they do not invalidate the rule, with which this discussion alone is concerned.

An explanation, again, is put in my mouth of the second sound sometimes heard (in the neck), together with an aortic regurgitant murmur, which I repudiate. I do not attribute this second sound, or any second sound, to vibration of the valves only. I am in this a

disciple of Dr. Markham, who showed that the elasticity of the valves, arterial and auriculo-ventricular, alike brings them into position to close the orifices before the pressure of the blood comes into operation; and of Dr. Sibson, who taught and proved that the second sound is produced by the sudden tension of the great vessel and valve together.

I feel that I owe an apology to Dr. Williams for intruding myself into this discussion. I did not consider that the great and imperishable monument of his work was shaken, or that it needed any support from me; but statements were confidently made which I could not but challenge, and I trust Dr. Williams will accept my apology, and with it my tribute of respect and admiration.

ANIMAL VACCINATION.

By JOHN GREENE, L.R.C.P., Birmingham.

AT the present time, when the paucity of vaccine lymph is drawing public attention towards the question of supply, and when an impression is gaining ground that all available means are not employed by Government to that end, the right of anyone having a special and large experience of animal vaccination to express his views on that process will be granted.

I will premise that, in pressing forward, I am impelled by no mere love of change, but by a conviction that certain facts connected with vaccination have not as yet received a due recognition by those in authority, or, at any rate, if received, have not had a proper sequence of action. Further, I am not an advocate for the substitution of ordinary arm-to-arm vaccination by that direct from the calf; I desire merely to secure the advantages which that system affords as a means for the cultivation and renewal of the virus, and as a reserve source of supply in time of temporary pressure, the former being the chief aim; that a certain percentage of the population may, either by preference or direction, be inoculated from animal-vaccine, would be simply a necessary incident. In my pamphlet, *Good Vaccine Lymph*, 1871, I endeavoured to prove the necessity of an occasional renewal of the virus from a primary source, taking as my text an unknown or forgotten paper of Jenner, “Instructions for Vaccine Inoculation”, found amongst the medical papers of my grandfather. The opinion expressed in this pamphlet received the cordial adhesion of many leading members of the profession, particularly of Mr. Robert Ceely, the well known author of those remarkable papers on the “*Variolæ Vaccinæ*”, than which, as evidence of close and accurate scientific observation, it is impossible to conceive anything more complete.

In 1853, the responsibility of keeping the virus up to a high state of efficiency hitherto resting with the profession, which by individual enthusiasm had nobly justified public reliance, was transferred *ipso facto* by Lord Lyttelton's Act to the legislature. That *verve* which responsibility gave to individual effort was extinguished, interest in the question for the time being paled before the universality of the practice and the supposed infallibility of the genius of public law.

This important duty so transferred has, since this Act, been fulfilled on the narrow basis of departmental opinion existing at that time and since. The radical defect of the law is, that, while stringently forcing upon the nation a certain saving virus, it has not taken a sufficiently broad view of the possible necessities of its nature and origin; as the use of this virus is compulsive, it is essential that the ruling idea concerning its source should be perfect at all points and satisfactorily ample in its dimensions, not only for the sake of disarming possibly ignorant suspicions, but for the conservation of its high prophylactic power.

It was, of course, much the pleasanter and smoother road for the learned framers of the Act to adopt the theory of unchangeability in the lymph under official supervision, rather than the weary and difficult path of those who from time to time sought to produce a new stock of high standard in relation to variola. It is not the first time that difficulty has been hidden under the cloak of dogma, and all in good faith has this been done; for the evidence in favour of non-degeneration is certainly strong, till contrasted with the opposing facts and arguments which have accumulated and grown stronger since the time in question.

In all observations on the course of the vaccine in the human subject, it has not been sufficiently stated whether the presence of the typical vesicle in the vaccinations of a given stock has been meant to express that such was the average condition, the numerical majority. An experienced vaccinator is well aware that a number of children may be vaccinated from a weak or doubtful stock, and yet that certain of them will unfailingly exhibit a good vesicle, and *vice versa*. The superiority of “the most active kind of virus” (Jenner) over a poor virus is simply that it raises the average number of vesicles of the actual or ap-