

often than not we use the word "congenital" to mask our ignorance of the real cause.

In the letters of Mr. Edward Deanesly and Mr. R. W. Murray in THE LANCET of Nov. 24th, 1906, p. 1470, and of Mr. Hamilton Russell in your issue of March 9th, 1907, p. 683, we have, I conclude, a full statement of all the facts on which the saccular theory of hernia is based. Concerning these facts all three writers are unanimous. Briefly stated they are: 1. Complete removal of the hernial sac (with, of course, replacement of its contents) works a cure quite as effectively as removal of the sac with suture of the belly wall. They infer, therefore, that it is the presence of a peritoneal sac, not of a lacuna or weakness in the belly wall, which constitutes the essential condition necessary for the formation of a hernia. The sac is not brought down by the hernia; it must therefore be of congenital origin. 2. That the sac, which is usually regarded as of post-natal origin, is similar in form to that which is *known* to be of prenatal origin; all are therefore of prenatal origin. Further, the shape of the sac is often such that it can only be accounted for by supposing that it is of prenatal origin. On these two grounds is based the saccular theory of hernia. As to the first point: I am quite prepared to believe that the supporters of the saccular theory obtain, as they maintain they do, quite as good results by a complete removal of the sac as those who not only remove the sac but also repair the abdominal wall by operations such as those of Macewen or Basini. But it is not necessary for me to point out that those who merely remove the sac really do much more than remove the sac; by their operation they necessarily set up a reparative inflammatory process along the hernial tract which probably leads to a more effective repair of the belly wall with less destruction of its muscular mechanism than the wiring and suturing employed in more radical operations. It is one thing to admit the success of their method of treatment; it is quite another matter to infer from it that hernial sacs are of prenatal origin. As to the second point: the supporters of the saccular theory, on what grounds I cannot conceive, have concluded that the shape of the hernial sac indicates a prenatal origin. We only know of two diverticula of the peritoneum which are formed before birth—namely, the processus vaginalis and one which frequently occurs at the umbilicus. In no instance have these diverticula been seen of an irregular or of a saccular form; a prenatal diverticulum of the peritoneum in the shape of a hernial sac is unknown; so far as its shape goes, then, it indicates a post-natal development for hernial sacs. Everyone will admit that a hernial sac which is freely continuous with the tunica vaginalis, or one that is connected with the tunica by a patent or fibrous funicular process which is decidedly shorter than the inguinal canal, must be of prenatal origin; but these are the only two forms which can be recognised with certainty as such. So far as I can learn the supporters of the saccular theory have not troubled to seek to distinguish such forms from others.

Having now dealt with the slender evidence they have brought forwards in support of their theory I now proceed to bring forward a few details which have escaped their consideration. Since all hernial sacs are pre-formed, why is it that the inguinal sacs fill so readily in the first few years of life, while the femoral sacs do not fill until puberty is reached? Out of 500 consecutive cases of hernia, Mr. Jonathan Hutchinson, junior, found that the inguinal occurred to the femoral form as 3 to 1; Mr. Murray estimates their ratio as 6 to 1; yet in 360 cases of hernia in children Stiles found only one case of femoral hernia—in a girl, aged eight years; in 1424 cases of hernia in children Bull and Coley found only 35 cases of femoral hernia. On the theory that hernia is the result of a high abdominal pressure acting on a weak part of the abdominal wall, I can explain those figures, but I should very much like to know the explanation offered of them by those who hold that all hernial sacs are of congenital origin.

Mr. Deanesly has asked me the question, Why is it, since we all possess weak points in our belly wall and have our abdominal viscera subjected to a high pressure, that we do not all suffer from hernia? In that Mr. Deanesly is in error. We do not all—most fortunately very few of us do—possess any point in our belly wall that cannot resist a pressure much higher than it ever has to bear, even in the stress of a labourer's life. In return I should like to put a question to Mr. Deanesly, Why is it that 50 per cent. of children

under four months of age do not suffer from inguinal hernia? He himself has quoted the result of Sach's observations—namely, that the funicular process was found patent in 59 per cent. of inguinal canals examined in children under four months of age. They have the pre-formed sacs; why do they not have the hernia? Hernia is very common during the first year of life, but is this due to the fact that there is a process of patent peritoneum in the inguinal canal? Is it not rather due to the fact that there is a patency or gap in the resisting structures of the belly wall, which the presence of the open diverticulum necessitates? When he argues that the sac is the cause of hernia he forgets that there cannot be a sac unless there be also an opening in the belly wall for the mouth of that sac.

Mr. Russell has kindly supplied descriptions and illustrations of four cases of femoral hernia for my consideration. One is a particularly remarkable case; its sac was continuous with the peritoneum, yet it was not lined by peritoneum; how that sac was formed and how it became continuous with the abdominal cavity—unless it had been formed out of a "congenital" abscess cavity—I really cannot tell. My difficulty in accepting Mr. Russell's explanation—namely, that all four are of congenital origin—is that (1) we have no evidence in the thousands of bodies that have been dissected that there ever exists an empty congenital diverticulum of the peritoneum in the positions occupied by these sacs; and (2) no embryologist or anatomist has seen a trace of them in embryo or foetus. But my chief reason for refusing Mr. Russell's explanation is that such sacs can be produced experimentally in the dead body under the influence of agencies and conditions which we know are to be found in the living body. It would occupy too much space and time to detail these experiments now, but at some future time I hope space may be accorded me for the purpose in this journal. Meantime, I would recommend the supporters of the saccular theory to make observations on the following points if they wish to reach the truth about hernia: (1) The degree of intra-abdominal pressure which occurs in lifting heavy weights or in vigorous body movements; (2) the resistance power of the loosely fixed peritoneum in the regions where hernia occurs; (3) the resistance power of the femoral sheath—the sole structure which prevents the formation of a femoral hernia; (4) the functional meaning of the femoral canal.

There is one matter concerning which I would thankfully receive information. Is it true, as Mr. Russell asserts, that there is no relationship between the incidence of hernia and the occupation of those who suffer? I cannot lay my hand on exact data, but I have the impression, given me, I think, by Mr. W. McAdam Eccles, that bakers suffer in an undue proportion, and I believe the condition is also very frequent among labourers in brickfields.

I am, Sirs, yours faithfully,

March 18th, 1907.

ARTHUR KEITH.

\* \* Mr. Hamilton Russell has asked us to say that he feels that he cannot engage on equal terms in a newspaper controversy because of the time which must elapse before his communications can reach us, a point which we are sure those interested in this matter will recognise.—ED. L.

## THE LATE PROFESSOR BUDIN'S METHOD OF REARING INFANTS.

To the Editors of THE LANCET.

SIRS,—In your review of my translation of Professor Budin's work, "The Nursling," the following statement is made: "The author excludes from his statistics all infants who have not attended at his clinic for *at least one month*. Would it be uncharitable to suggest that the newly-born infant who can survive a month's ordeal of undiluted sterilised milk might survive anything?"

As systematic supervision throughout infancy was the essence of Professor Budin's method of rearing children his statistics had to exclude all cases of less than a certain minimum duration. One month represented three to five attendances at the clinic, and no reliable conclusions could have been made as to the value of medical supervision if observation over shorter periods had been included. A mother needs to be educated to the fact that the fortnightly weighing and inspection of her healthy infant is worth the trouble it entails on her; the cases eliminated were almost without exception thriving infants whose mothers ceased to attend

after the clinic had lost its novelty. No infant was ever compelled to conform to any dietetic dogma by Professor Budin. He studied the feeding of each new patient as a fresh problem. I can confidently assert that none "were wrecked before the end of thirty days" from exposure to "an ordeal of undiluted sterilised milk," and if your reviewer wishes I shall gladly obtain for him the details of every case which attended Professor Budin's consultations for less than one month.

Thanking you for this opportunity to correct a false impression for which, perhaps, I am somewhat to blame,

I am, Sirs, yours faithfully,  
W. J. MALONEY, M.B. Edin.

Brook-street, Grosvenor-square, W., March 12th, 1907.

## AN OPEN-AIR SCHOOL.

*To the Editors of THE LANCET.*

SIRS,—In the article by Mr. C. H. Garland and Dr. T. D. Lister on a National School for Consumptives we are told of the necessity for education in hygiene.<sup>1</sup> The education is to be confined, however, to the working class and to those suffering from tuberculosis. Should not a National School of Hygiene take into consideration a method of living which would prevent the commencement of disease? and is not the well-to-do class equally in need of instruction as to the duty and methods of healthy living? It is with the idea of forwarding such an education that I am erecting in Garden City the building which I am calling an open-air school. It is an attempt to demonstrate the possibility of truly healthy conditions of living, not for invalids but for perfectly healthy persons, the result aimed at being to teach the possibility of preventing the first awakening of the germs of consumption.

This life is not to be (as was suggested in the article) "soft" more than any other life lived in the world. Open-air sleeping, eating, and bathing are to be combined with absolutely normal activities. It is proposed by rational feeding and reasonable intellectual and artistic development to produce a finer type of physique and a higher degree of mental and moral capacity than is at present known. I should gladly welcome any inquiry or any visit, though the building will not be complete for several months.

I am, Sirs, yours faithfully,

March 18th, 1907.

A. J. LAWRENCE.

## GRADUATED LABOUR IN SANATORIUMS.

*To the Editors of THE LANCET.*

SIRS,—Your readers may be interested to know that the system of graduated labour advocated by Mr. C. H. Garland and Dr. T. D. Lister in your issue of March 9th for patients in sanatoriums has been in force for two years in this institution with satisfactory results. I have had the pleasure of demonstrating this system both to Mr. Garland and Dr. Lister, and am glad to find they now advocate its general adoption. Again, the suggestion of these gentlemen that patients should be taught to take an intelligent interest in their treatment, and that the cause of the disease and methods of its prevention and arrest should be explained fully to each individual has been adopted here since the institution was opened. We have found, however, that "talks in the recreation room," when combined with close supervision of individual patients and continuous coöperation between doctors and patients, yield far more satisfactory results than any pamphlets or other literature, and in my experience amongst the working classes short individual conversations will do more good in a day than teaching by pamphlet will do in a month.

There is one paragraph in the article which appears to me to be original. I refer to the statement that "it is generally humiliating to a man to perform domestic duties ..... like bed-making, floor-scrubbing, washing-up, and similar duties." An ounce of practice is worth a ton of theory. In our experience here we have never met this feeling of humiliation. Our male patients, when well enough to do so, make their own beds and keep their wards and crockery clean. They do this work cheerfully and well. The men of our navy, army, and mercantile marine, and men in other occupations daily perform such work as "bed-making,

floor-scrubbing, and washing-up." Are all these men suffering from a sense of humiliation?

I am, Sirs, yours faithfully,

M. S. PATERSON.

Brompton Hospital Sanatorium, Frimley, March 11th, 1907.

## TREATMENT OF DISTENSION OF THE INTESTINES.

*To the Editors of THE LANCET.*

SIRS,—The treatment of distension of the intestines, whether following operation or otherwise, is always a matter of difficulty. There has been a considerable amount of correspondence upon the subject in your columns from time to time showing that the question is of importance. My object in writing is to protest against the treatment by puncture. I imagined that such a procedure was relegated to the past and that a student who suggested it at his examination would be sent down for six months. To my surprise I find that this is not so and I have just read in a clinical lecture by a physician at one of the leading London hospitals: "The prognosis of acute gastric dilatation is, however, much better than that of tympanites, for while it is easy to evacuate a distended stomach by the tube it is very difficult to deal mechanically with distension of the intestines. The distended coils of intestine may, it is true, be punctured '*with impunity*' (italics are mine!) by a trocar and cannula, but the escape of gas by the cannula is barely sufficient to give permanent relief."

If this is the teaching which students receive it is not surprising that they should act in accordance with it. That they do is supported by the fact that I was recently called upon to operate on a case of septic peritonitis due to this cause. This, I think, is sufficient evidence to justify me in drawing attention to the subject.

I am, Sirs, yours faithfully,

J. LIONEL STRETTON,

Senior Surgeon, Kidderminster Infirmary and Children's Hospital.

March 15th, 1907.

## ON THE CONDITION OF THE BLOOD-VESSELS DURING SHOCK.

*To the Editors of THE LANCET.*

SIRS,—Though firmly persuaded of the truth of Mark Pattison's saying that "of all the ways of wasting time controversy is the most unprofitable," I feel that I must beg space to reply to Mr. J. D. Malcolm's letter on this subject which appears in your issue of March 16th.

In any sphere of thought it is rare; in medicine it is conspicuously rare, for a theory to be found which will at once embrace all the facts. In the present state of our knowledge we have often to decide between two explanations which are diametrically opposed to one another, and in making our selection between them we have nothing to aid us except the weight of available evidence. Even where the preponderance of that evidence is overwhelmingly on one side, there are not infrequently irritating little facts which, for a time at any rate, decline to be included in the fold. It is facts of this nature upon which, in his tilt against the accepted explanation of the phenomena of shock, Mr. Malcolm has seemed to me unduly to dwell, to the exclusion of those matters of real import upon which the accepted explanation is based. If he wishes to persuade us not only that this explanation is wrong, but that it represents the exact opposite of the true one instead, of insisting upon differences in detail between two observers who are in complete consonance on the main question, Mr. Malcolm ought surely to begin by showing that there are fallacies in the arguments by which the present view is supported. Of these arguments I select one for his demolition. It is possibly not the most cogent of the many which might be adduced; it has, at any rate, the merit of simplicity. It is this. The sphygmomanometer shows that the blood pressure in shock is invariably subnormal.<sup>1</sup> The state of the blood pressure depends upon (a) the heart, and (b) the arteries. Mr. Malcolm has expressed himself satisfied<sup>2</sup> that the heart is not affected in shock. There remain therefore the arteries, including, of course, the arterioles. When these

<sup>1</sup> THE LANCET, March 9th, 1907, p. 677.

<sup>2</sup> THE LANCET, March 16th, 1907, p. 762.