

malignant new growth from one animal to another of the same species. Quite recently Jensen and Borrel have had successful series of inoculations and graftings also in white mice." And then I give the references to the papers of each of these investigators, as will be seen by looking at THE LANCET of Dec. 12th, 1903, p. 1635. Further, it is within my personal knowledge that Morau's work has been referred to on several occasions in the deliberations of the Executive Committee of the Imperial Cancer Research; and lastly, I have before me as I write a long and full address entitled, "L'État Actuel de la Question du Cancer" by the director of the Imperial Cancer Research (Dr. E. F. Bashford) in the *Revue Scientifique* of June 2nd, 1906, in which there is a distinct reference to Morau "as the first who succeeded in transplanting cancer from mice to other mice." Morau was unfortunate in that his carefully and scientifically conducted researches through 15 series of transplantations in mice, as well as his experiments with other animals and his lucid and interesting report of them, came at a time before the establishment of cancer research laboratories, and in that except for any bearing they were supposed to have in relation to the bacterial origin of cancer they fell flat. Jensen, on the other hand, was fortunate in that his work came just at the time when systematic research into cancer was being established in this country and a little later in Germany. The original mouse tumour from which he transplanted the disease into other mice may be said to be still living, in thousands of mice, in most places where cancer research is being followed within both hemispheres of the globe. The great importance of Jensen's work consists in the fact that he did not restrict himself to reproducing the same form of cancer in mice by transplanting portions of the tumour from the mouse originally affected, but he proved at enormous labour and pains, by following the processes going on step by step at the site of inoculation, that the new formation was actually a continuation of growth of the cells introduced by inoculation.

I am, Sirs, yours faithfully,

Nov. 20th, 1905.

HENRY MORRIS.

To the Editors of THE LANCET.

SIRS,—I shall be obliged if you will allow me to give a short summary of the work of Morau. It was in 1891 that Morau¹ first announced his discovery of this malady, more than ten years before any publication of Jensen's on this subject had appeared. Morau's original tumour developed spontaneously in the subcutaneous tissue of the axilla of a captive white mouse. It was easily enucleable, non-adherent to contiguous structures, and caused no disturbance to the animal's general health. A portion of this tumour, immediately after its aseptic ablation, was by trituration reduced to a magma which was forthwith injected into the subcutaneous tissue of a series of other white mice. About three months later it was found that nearly all the injected animals presented similar tumours at the seats of inoculation and elsewhere, which subsequently slowly increased in size. One of these tumours was then enucleated, and from it fresh magma was prepared, with which another series of mice was injected, and these also after a similar interval acquired the disease. Experiments of this kind were successfully continued through several generations. It is noticeable that the animals employed for these experiments were all of the same strain, closely inter-related, and had all been kept in the same cage. This no doubt explains the very large proportion of successful implantations attained by Morau—over 80 per cent.—which is much in excess of that of any other experimenter: for when animals of foreign strains are used the proportion of successful implantations is far less.

In subsequent experiments Morau found that when the malady had been artificially transmitted through a considerable number of generations the inoculability of the pseudoplasm markedly diminished. In another series of experiments mice were fed with a paste composed of chopped-up tumour and bread crumbs, with the result that these also after some time developed the disease. Morau seldom noticed dissemination of the malady, except after traumatism applied to the tumour. The absence of cachexia also attracted his attention: "J'ai l'honneur de présenter un animal dont

l'état général est assez satisfaisant, malgré l'énorme développement de la tumeur dont il est porteur" (p. 721). Pregnancy was found to check the progress of the malady, but post partum its increase was greatly accelerated. Morau's view as to the nature of this pseudoplasm is shown by the title of his publication, wherein he speaks of it as "an epithelial tumour" ("*Inoculation en série d'une tumeur épithéliale de la souris blanche*"); but in a subsequent passage he claims that it may be classed "dans la catégorie des épithéliomas cylindriques." All attempts to transplant the malady into other animals than mice failed. Moreover, no cultures could be obtained from any of the many tumours examined, nor could any microbes be discriminated.

Some recent observations by Borrel² seem to show that the tumours are formed round certain parasitic helminths which end their existence thus after having lived for a time in the animal's blood; and in this connexion it is interesting to note that the same observer has often found the blood of these tumours teeming with parasitic spirochætae. It would be premature to speculate as to the precise value of these significant finds, but they seem to point to a distinct causative agent. We are evidently only just at the beginning of the investigation as to the real nature of this malady.

I am, Sirs, yours faithfully,

Clifton, Bristol, Nov. 17th, 1906.

W. ROGER WILLIAMS.

THE "SACCULAR THEORY" OF HERNIA.

To the Editors of THE LANCET.

SIRS,—Dr. A. Keith in his criticism of Mr. R. Hamilton Russell's theory brings forward observations on some 70 fetuses in which he has failed to find any sign of a peritoneal diverticulum in the femoral canal such as the theory postulates. The point is, of course, a crucial one, and if Dr. Keith's observations are confirmed it must be admitted that Mr. Russell's theory, at any rate as regards femoral hernia, will be definitely disproved. But a much larger number of observations will be required before the question can be settled. 70 is a very small number of cases when we consider the comparative rarity of femoral hernia. In 350 consecutive operations of my own on inguinal and femoral herniæ the latter do not exceed 5 or 6 per cent. of the total number. I do not know the frequency of femoral hernia with relation to population, but 1 in 5000 probably overstates it. In these circumstances the absence of a congenital femoral sac in 70 cases proves little, even if we ignore the possibility of a sac having been overlooked in a single instance, as might readily happen if the observations were not made for that special purpose but only incidentally. With regard to inguinal hernia, Mr. Russell's theory stands on much stronger ground and Dr. Keith's remarks leave it quite untouched. The frequent presence of a congenital inguinal sac in persons of all ages who have never had a hernia, which was shown many years ago in the case of infants, has now been demonstrated by Mr. R. W. Murray³ in adults of all ages. The pre-formed sac is not, therefore, a theoretical postulate but a matter of direct observation.

Dr. Keith's remarks about the weakness of the internal abdominal ring and the pressure to which it is subjected are quite beside the mark. The really interesting question is—Why does not everyone acquire a hernia at some time of life? Weak spots in the abdominal wall and intra-abdominal strain occur in everyone. Yet herniæ do not always, or even generally, occur in those whose abdominal walls are weak or in those specially exposed to strain. On the contrary, it occurs as often as not in the most unlikely persons and is equally often absent when all the supposed predisposing causes operate most strongly. The theory of "weak spots" and "strain" affords no clue to this apparently capricious occurrence of hernia. It does not account for the fact that a hernia usually appears for the first time *tout d'un coup* and not slowly and gradually. Nor does it explain why inguinal and femoral herniæ have pear-shaped sacs with narrow necks, whereas all herniæ which are indisputably due to "rupture" or strain show no such peculiarity. The saccular theory not only explains these facts perfectly but can be tested by another criterion—that of treatment. Dr. Keith says truly, if Mr. Russell's theory is right, then we must modify profoundly our conception of the cause of hernia and the principles which we

¹ Comptes Rendus de la Société de Biologie de Paris, 1891, tome iii., pp. 229, 721, and 801, &c.

² Infection Vermineuse et Spirochète chez les Souris Cancéreuses, Comptes Rendus de la Société de Biologie, Paris, tome lvi., p. 770.

³ THE LANCET, Feb. 10th, 1906, p. 363.

apply to its cure. I am convinced that Mr. Russell is right and for the last six years I have carried his hypothesis to its logical conclusion by making perfect obliteration of the sac the central aim of the radical cure and by relegating the closure of the inguinal canal to a secondary place or disregarding it altogether. I have published elsewhere⁴ the results of 142 consecutive operations for hernia at all ages, based on this principle, and the results as regards the number and permanence of the cures compare favourably with any similar series yet published. I have now for a considerable time discarded all methods of closing the inguinal canal as unnecessary, even in large herniæ, and content myself with thorough displacement and removal of the sac by Kocher's method. I find the results excellent. I propose at some future time to publish a consecutive series which will, I believe, show conclusively that the sac is the *fons et origo* of the hernia, since if that is efficiently removed the hernia does not return though strain and a weak inguinal region continue to exist as before.

I am, Sirs, yours faithfully,

Wolverhampton, Nov. 17th, 1906.

EDWARD DEANESLY.

To the Editors of THE LANCET.

SIRS,—It is not greatly to the credit of the medical profession that there should still be considerable difference of opinion as to the essential cause of such a common complaint as hernia. There are at present two distinct schools of thought on this question. The disciples of the smaller and more modern school hold to the saccular theory as the article of their faith, and thus explain the occurrence of hernia at all periods of life as being essentially due to the presence of a pre-formed hernia sac, whereas the disciples of the larger and more orthodox school believe that all herniæ occurring during infancy and childhood have a saccular origin, but when a hernia appears for the first time during adult life the hernia sac, with very few exceptions, has been acquired.

The saccular theory which during the last few years has been advanced by Mr. R. Hamilton Russell, chiefly from a clinical point of view, is, in your issue of Nov. 17th, subjected to severe adverse criticism by Dr. A. Keith. The question is considered by Dr. Keith from the standpoint of the embryologist and the anatomist, and as such is entitled to the greatest respect. In his letter Dr. Keith is chiefly concerned in refuting the explanation given by Mr. Russell as to the developmental origin of the sac of a femoral hernia, but also deals with inguinal hernia, and in the course of his remarks says, "It is scarcely conceivable that a 'saccular theory' of hernia could be formulated by anyone familiar with the anatomy of the groin." On the contrary, I am surprised that persons familiar with the anatomy of the groin have not long ago formulated a saccular theory of inguinal hernia. It has long been recognised that the frequent occurrence of inguinal hernia during infancy is due to the presence of a pre-formed sac—a defective obliteration of the processus vaginalis testis. It is also known that during adult life a *scrotal* hernia may suddenly develop owing to the bowel descending for the first time into a patent peritoneal diverticulum. In such circumstances strangulation of the bowel frequently occurs and at the operation for its relief the sac is usually found to be distinct from the tunica vaginalis. Inguinal hernia not infrequently appears for the first time during convalescence from a long illness, and in such circumstances it is surely far more probable that a pre-formed sac existed rather than that the bowel, apart from any violent muscular effort, forced the peritoneum in front of it.

I have operated for the radical cure of inguinal hernia upon a number of infants, older children, and adults, and have been greatly impressed with the marked similarity of the sac and its surroundings at all periods of life, and have yet to learn how to distinguish between the so-called acquired and the pre-formed sac. Again, the fact that during infancy and early childhood radical cure of an inguinal hernia can with something approaching certainty be obtained by simply excising the sac after applying a ligature to the neck of it is strong evidence in favour of the saccular theory so far as the inguinal region is concerned.

Dr. Keith does not spare Mr. Russell respecting the "facts" concerning development which he has discovered and which he subjects to a close examination. However, I hold no brief for Mr. Russell, who will

no doubt look after himself. At the same time when a person asks for facts, he himself should exercise great caution as to the statements he makes, and I trust I have adduced some evidence to show that Dr. Keith is not entirely correct when he says: "All the evidence at our disposal points to the coincident formation or pre-formation of a peritoneal sac as the least essential point in the pathology of hernia."

After all said and done what occasion is there to suppose that the sac of an inguinal hernia in the adult is ever acquired, when we know that during intra-uterine life a peritoneal diverticulum normally exists in this region and from clinical and post-mortem experience that this diverticulum—a pre-formed hernia sac—frequently persists throughout life. I will not now discuss the question further as I hope soon to publish a paper dealing with the etiology and treatment of inguinal hernia based on a second series of 100 post-mortem examinations made at the Mill Road Infirmary upon persons in whom during life there was no evidence of hernia and also upon the results obtained by a method of treatment based upon the saccular theory of inguinal hernia. I am, Sirs, yours faithfully,

Liverpool, Nov. 18th, 1906.

R. W. MURRAY.

THE SOILING OF THE LOAF.

To the Editors of THE LANCET.

SIRS,—Dr. Sandwith says in connexion with the above, "It seems quite possible that public opinion has never been directed to this evil since Englishmen began to consider the question of domestic health." He will be glad to learn I have been advocating a "clean bread delivery" for nearly ten years, both in the medical and lay press. My last communication to the former was about two years ago in THE LANCET, when I advocated that loaves should be delivered in oil papered bags and not opened until they reached the breakfast-table. I am, Sirs, yours faithfully,

Manchester-square, W., Nov. 18th, 1906.

THOMAS DUTTON.

To the Editors of THE LANCET.

SIRS,—A year or more ago you kindly published a protest of mine against the insanitary treatment of bread during its journey from the oven to the consumer. I told you I was in the habit of taking a walk before breakfast and daily passed two well-known bakers' shops and saw most mornings loaves in trays placed on the pavement with dogs prowling round. I saw the much over-loaded hand-carts with baskets full of bread on the handles bumping in the mud. Sometimes I saw loaves tumbling off into the road replaced almost unwiped. I then made inquiries about the bread-supply at this club and ever since the committee has wisely insisted every loaf and roll shall be sent separately in a paper bag—a precaution which should be observed by everyone responsible. I am, Sirs, yours faithfully,

REGINALD BARRATT.

Arts Club, Dover-street, W., Nov. 18th, 1906.

THE BOARD OF EDUCATION AND THE TEACHING OF HYGIENE.

To the Editors of THE LANCET.

SIRS,—In a special article in THE LANCET of Nov. 17th, on page 1393, you notice the reception by the President of the Board of Education of the deputation which waited on him at the office of the Board on Nov. 12th. You state that the object of the deputation was "..... to consider a memorandum presented by the committee of the medical profession by the British Medical Association, and by the Advisory Board for the Teaching of Hygiene and Temperance."

As one of those who on that occasion represented the International Association for the Prevention of Tuberculosis, I should feel obliged to you, Sirs, if you would kindly publish in THE LANCET the inclosed copy of a letter sent by my colleagues and myself to Mr. Birrell on Nov 9th. In that letter is stated what we asked the Board of Education to do, and you will observe that our objects were not identical with those of the other members of the deputation. We had

⁴ Brit. Med. Jour., June, 1905,