

then we have become more critical. I am quite sure now that the case was one of overdistended lung and not a pneumothorax, for the following simple reason that I have never seen a skiagram of a pneumothorax that did not show a shadow of the collapsed lung. The shadow of the collapsed lung of course varies a good deal, but it is always there. It may show a very considerable shadow, as in Fig. 8 of Dr. Pitt's lecture, due, no doubt, to the tuberculous infiltration preventing complete collapse. In other cases the shadow of the collapsed lung can be seen running beside the spinal shadow, but it is always there. I hope to place these cases on record shortly.

I am, Sir, yours faithfully,

Harley-street, W., Dec. 10th, 1910.

HUGH WALSHAM.

## TREATMENT OF ULCERS OF THE LEG BY A PASTE.

*To the Editor of THE LANCET.*

SIR,—Your annotation in THE LANCET of Dec. 10th, p. 1716, implies that Dr. B. A. Thomas has discovered a new treatment for varicose ulcers. Twenty years ago when an out-patient dresser at St. Thomas's Hospital I spent most of my time dressing varicose ulcers more or less successfully with what we as students called "Unna's"—the composition of this paste being glycerine, gelatin, and zinc oxide—applied warm on carbolic gauze. I still use this preparation, and it is quite the best provided the dressing is applied from the arch of the foot to the tubercle of the tibia.

I am, Sir, yours faithfully,

Bromley, Kent, Dec. 10th, 1910.

C. W. GRANT-WILSON.

## INVALIDITY INSURANCE.

*To the Editor of THE LANCET.*

SIR,—In THE LANCET of Dec. 10th Mr. W. A. Robertson, actuary, Century Insurance Company, raises the question of invalidity insurance in a most interesting manner. In doing so he states that whilst mortality is improving invalidity is increasing, and he bases his argument on the grounds of the available statistics of his company during the past 25 years with reference to sickness insurance. Quite recently, elsewhere, I had the opportunity of raising and discussing this very question with Mr. Robertson, and now that it has appeared in the columns of THE LANCET I would like to add a few remarks. Remembering that I am engaged as medical examiner to insurance companies in London, I cannot accept his deductions as to an increased invalidity as they are based upon selected statistics which are always open to error, and for the same reason "returns" from benefit societies are not a true index as to increase of invalidity. For my purpose it is unnecessary to refer to the conditions under which sickness policies are issued to the public, and the members of the medical profession are well aware of the amount of abuse that arises in connexion with benefit societies. I need only cite the cases where men in addition to having a sickness policy are also members of two or three benefit societies to show how erroneous it would be to draw any conclusion as to increased invalidity from any statistics relating to such persons. I am sure that all students of public health and preventive medicine will require stronger proofs on the question of increased invalidity than those forthcoming from insurance companies and benefit societies. I take it as a significant sign of the times to find the insurance companies appreciating and acknowledging the achievements of public health and preventive medicine, and in my opinion it will not be long before all large commercial undertakings will find it to their advantage to have the assistance of medical men holding the D.P.H. I need hardly say that the statistics published and compiled by the life offices have no relation to those relating to sickness insurance, and if they did not relate to selected lives could be taken as a true index to the mortality of the entire community.

I am, Sir, yours faithfully,

Cannon-street, E.C., Dec. 12th, 1910.

JOHN J. SCANLAN.

*To the Editor of THE LANCET.*

SIR,—Mr. W. A. Robertson's letter on Invalidity Insurance in your last issue contains some interesting statistics of the relative amount of sick benefit paid to

members of different ages under a system of sickness insurance during the past 25 years. The statistics referred to in the letter do not, however, give any support to the assertion that "it is an indisputable fact that sickness is actually on the increase." The returns of the Century Insurance Company, to which Mr. Robertson, as actuary, has access, may quite probably show an increasing rate of claims for sick benefit among the members in recent years, although his letter does not contain any proof of such increase. Moreover, if the existence of such increase be assumed, it could not be accepted as proof of any real increase of sickness, but only as proof that under this system of insurance, as Mr. Robertson admits, "people now lie up where previously they would have continued to work." A satisfactory national system of sickness insurance has long been a desideratum with those interested in sanitary progress, but the differentiation of what constitutes definite sickness presents one of the difficulties that still stand in the way of its realisation. Mr. Robertson remarks on the incongruity of his assumed fact that "while mortality is declining invalidity is increasing," and it would indeed be surprising if a real increase of sickness could be proved to exist coincidentally with the recent steady decrease in the national death-rate. Insurance against sickness, and the resulting benefit during invalidity, is undoubtedly beneficial to public health, and very probably assists in reducing the death-rate, but it is obviously impossible to accept increase of payments by an insurance company for sick benefit as conclusive proof of a real increase of sickness, even among its members. Similarly, experience of the results of the Workmen's Compensation Act suggests a marked increase of accidents and injuries to workmen, but should not be accepted as any proof of actual increase in the proportion of occurring accidents and injuries. If Mr. Robertson has access to statistics showing a marked increase in the ratio of sick benefit paid to the members of his company in recent years, they would be interesting, but they must not be assumed to prove actual increase in the ratio of national sickness.

I am, Sir, yours faithfully,

Surbiton, Dec. 12th, 1910.

NOEL A. HUMPHREYS.

## THE MOTOR FUNCTIONS OF THE STOMACH.

*To the Editor of THE LANCET.*

SIR,—I have been interested in reading the criticisms, by Dr. Arthur F. Hertz, of my recent small article on the Motor Functions of the Stomach. One ought to welcome criticism, but that of Dr. Hertz is based very largely on his own personal observations and interpretations of these, which one has yet to learn have been so widely accepted as he appears to imagine. His quotation of authorities (and manner of speech) would lead one to think that all of them are in agreement with him, and that all phenomena in connexion with this subject are simple, straightforward, easy to demonstrate, and further, that "Mr. Gray's" is the only dissentient voice, which is, however, negligible in that "Mr. Gray" is hopelessly behind the times! One knows that the quoted authorities do not agree on many points with Dr. Hertz, as I shall show in the case of some of them.

I should like to criticise his letter in return, paragraph by paragraph, because he has charged me with "numerous inaccuracies" and "mistakes"!

In the first paragraph he states that I ascribe, in papers published in 1908, "the discovery of secretin to Wertheimer, without even mentioning the names of Bayliss and Starling." The actual words in my first paper (Feb. 22nd, 1908) are "as Wertheimer and others have shown, acid in the duodenum excites the production of secretin." In the same paragraph of my paper an extensive quotation, duly ascribed to Starling, appears.

With regard to the second paragraph, my first observations on the human stomach with the X rays were made in the vertical as well as the horizontal position. If Dr. Hertz had taken the trouble to read accurately my second paper (July 25th, 1908) he would have seen this and been possibly prevented from hinting that I had been less thorough in my methods than he, seeing that his patients up till then

had been examined only in the horizontal posture. This is, however, a small matter! In spite of previously having read Dr. Hertz's paper, which he kindly sent to me, in which he gives "reasons in detail" for being convinced that there is no such thing as a "middle sphincter," I have yet to be convinced that there is not.

I shall next consider paragraph 5, in which Dr. Hertz expresses a wish to know the names of "recent radiographers and others" who "assert that the pylorus during gastric digestion descends naturally below and to the left of the umbilicus." In consideration of the possibility that Dr. Hertz has placed the names of his "recognised authorities" on the subject in what he estimates as order of merit, I am quite pleased to take the combined statements of the first two. Barclay (Proceedings of the Royal Society of Medicine, Electro-Therapeutical Section, February, 1909) says: "The whole organ is placed to the left and seldom transgresses the middle line except when it is full of food, and then only by half an inch or so. Its lower border is usually a little below the umbilicus in the upright position," and Pfahler (quoted by Dr. Hertz in his paper of July, 1910), and also Holzknecht, state that in their "normal" stomach the pylorus is the lowest point! It is but just to remark that Barclay says that it "was very exceptional to find the pylorus as the lowest point." If the two premier authorities of Dr. Hertz's list disagree in this way, whom are we to believe, especially as both disagree with Dr. Hertz?

Reverting to paragraphs 3 and 4, Dr. Hertz has evidently an idea differing from mine as to what is to be taken as the dividing line between the cardiac and pyloric portions of the stomach. I may say that my ideas have been based on the description given by the late Professor Cunningham, which is practically repeated by Dr. Hertz in his paper which was published so late as July, 1910. I stated in my papers that peristalsis occurs in the distal part of the cardiac portion, and this statement has been made by others—for example, some of Dr. Hertz's "recognised authorities." I shall be obliged if Dr. Hertz will quote any "recognised authority" other than himself to justify his remark that it is "now universally acknowledged (except by Mr. Gray) that there is no peristalsis in the cardiac end." It is pleasant to think that Dr. Hertz admits and figures in his paper of July, 1910, that a tonic contraction, sufficient to keep the walls of the empty stomach in close apposition, affects the whole of the stomach beyond the fundus, which is after all but a small part of the cardiac end. Why should peristalsis affect only a small part of this tonically contracted tube? I submit that my observations are supported by the anatomical findings of such an indubitably "recognised authority" as the late Professor Cunningham. So far as his other statements in these two paragraphs are concerned, I can only say that Dr. Hertz appears to have unbounded confidence in the infallibility of his own observations. If they are correct of course I am wrong.

In paragraph 6 Dr. Hertz asks, "Who are the 'recent radiographers' who say that 'carbohydrate food' begins to pass through the pylorus (displaced as described) in 15 to 20 minutes, and proteid in 30 to 45 minutes?" Again I seek to take an average, but quote, for accuracy, Dr. Hertz's first-mentioned "recognised authority"—viz., Barclay—he who says that "the whole organ is placed to the left," and therefore the pylorus must also be. Barclay says that carbohydrate food commences to pass "out of the stomach" in from 5 to 40 minutes. That is to say, he observes the passage of food through a sphincter which is evidently situated to the left of the midline. Dr. Hertz evidently agrees that the pylorus occupies its usually accepted position to the right of the middle line. Does it not appear reasonable to suggest that Barclay mistakes "the middle sphincter for the pylorus," and, further, that these are the times when food begins to pass through the middle sphincter? The statements made by Dr. Hertz in this paragraph, "that food, whatever its nature, begins to pass through the pylorus at once," is news! This may occur in infants, but all his "recognised authorities" differ from him, as well as do the previously recorded statements of Dr. Hertz himself. One supposes, of course, that Dr. Hertz speaks of ordinary food, not ordinary water or even albumin water. Are we to disregard all the work of physiologists, physicians, and even some surgeons with regard to the check action of the pylorus which has hitherto been regarded, under normal conditions,

to require at least a little free hydrochloric acid before its sphincter action is unloosened?

In paragraph 7 Dr. Hertz again refers to Dr. Barclay (who places the pylorus in a wrong position) as confirming his own observations in connexion with duodenal ulcer. "The picture of a normal stomach exhibiting excessive peristalsis and passing the food on very rapidly is always suggestive of trouble in or about the duodenum." It is "universally acknowledged" (possibly there are exceptions) that hyperchlorhydria is a concomitant of ulcer, whether gastric or duodenal. Dr. Hertz agrees (in his paper of July, 1910) that hydrochloric acid is possibly the chief chemical stimulant of peristalsis, and there is little wonder that peristalsis should be excessive if the hydrochloric acid is excessive, but it has also been agreed that hydrochloric acid in marked excess stimulates pyloric spasm. I repeat, therefore, my remark that the interpretations of radiographers require criticism. One cannot allow that Dr. Barclay's observations confirm Dr. Hertz's when they differ on such an important point as the position of the pylorus.

With regard to Dr. Hertz's further remark, that he cannot understand how the period of relief of hunger pain is evidence in favour of a middle sphincter, I reply that it is to my mind just as much so as the relief of hunger pain of duodenal ulcer is evidence of pylorospasm. Destructive criticism, especially in the absence of confirmation, is easy—let Dr. Hertz give a reasonable explanation!

With regard to Dr. Hertz's remarks in paragraph 8, I may say that I have repeatedly seen intestinal segmentation, both as represented by the skiagraphic picture of the intestinal contents and also by direct vision of exposed human intestine. If Professor Cunningham's representation of the muscular coat of the stomach is correct I maintain that *modified* segmentation is possible in the part along the lesser curvature of the cardiac portion, and my observations in certain cases have led me to compare the passage of food masses along this part to the passage of segmented masses along the intestine. These may possibly be the same as the "blobs" referred to by Dr. Hertz and Dr. Barclay.

The literature on this subject is beset with so many contradictory statements that confirmation or criticism is urgently required. I shall be extremely glad if I stimulate others to take up the matter so that the truth may be arrived at as nearly as possible. As I indicated in my paper of Dec. 3rd, I shall not at all object to accept new views, but at present the ones I hold explain most easily and rationally clinical symptoms both before and after the operation of gastro-enterostomy.

I am, Sir, yours faithfully,

Aberdeen, Dec. 10th, 1910.

H. M. W. GRAY.

## THE USE OF "606."

To the Editor of THE LANCET.

SIR,—In view of the fact that the Ehrlich-Hata preparation, or "606," is now being put on the market, it is well to consider how far this preparation has fulfilled its expectations. That it has a rapid healing effect in certain cases appears definite, but the earlier reports of its wonderful effects were premature and probably exaggerated. The claim that it could effect an abortive cure of syphilis, or "sterilisatio magna," has not been substantiated. Professor Neisser, who was at first an ardent supporter of the new treatment, has since modified his opinion and recommends a combination with mercury and iodides. Professor Gaucher is anything but enthusiastic, and considers "606" only indicated in cases which resist mercurial treatment, but that such cases are rare. As regards the toxic effects of the drug, these appear to be less than those of atoxyl and arsacetin, but several deaths have occurred after injections of "606," some of which, if not entirely due to the drug, were probably hastened by it. Under these circumstances, it appears questionable whether we are justified in submitting patients, in the great majority of cases of syphilis, to treatment by "606," and perhaps losing valuable time by neglecting mercury and iodides.

I am, Sir, yours faithfully,

C. F. MARSHALL.

St. John's Wood Park, N.W., Dec. 10th, 1910.