Correspondence.

"Audi alteram partem."

THE MORTALITY AFTER OPERATION FOR STRANGULATED HERNIA.

To the Editors of THE LANCET.

SIRS,—In a leading article on the "Treatment of Gangrenous Hernia," published in *THE LANCET* of May 6th last, you conclude your remarks with the following sentence: "One statement made in the discussion is so remarkable that it deserves special notice. Mr. Bowlby stated that, taking all the operations for strangulated hernia performed in St. Bartholomew's Hospital during the last ten years, they showed a mortality of 40 per cent. It would be interesting to know to what this very high death-rate is to be ascribed." In accordance with your suggestion I beg to give you such further information as is at my command; but, in the first place, I should wish to state here what I stated whilst speaking at the Royal Medical and Chirurgical Society in the discussion on Mr. Kendal Franks' paper and what is duly reported in the proceedings of the Society—namely, that the mortality after operation for strangulated hernia is greatly higher than is generally appreciated and that the general average mortality is probably over 40 per cent. In the sentence I have quoted from your article, it would seem as though St. Bartholomew's Hospital enjoyed an unenviable distinction in this respect. But this is not the case, and in my remarks I specially quoted from figures collected by Mr. James Berry and published by him in the St. Bartholomew's Hospital Reports for 1884. These figures show that in 940 cases treated consecutively in St. Thomas's and Guy's Hospitals as well as in St. Bartholomew's Hospital the mortality was 43¹ per cent., the death-rate being nearly equal in all three of these institutions. I considered that these figures justified the conclusions I drew, and, in further support of them, I would allude to the statement made by Mr. Treves in the discussion on Mr. Lockwood's paper in *THE LANCET* of April 4th, 1891, to the effect that the mortality at the London Hospital after operations on strangulated hernia "was nearly 50 per cent." One may therefore conclude that in the four largest hospitals in London such mortality has been within recent years not less than 40 per cent.; but as some of your readers may prefer actual figures from cases still more recently in St. Bartholomew's Hospital I venture to summarise the results of the past ten years: Femoral hernia, 165 operations, 59 deaths—percentage of deaths, 35·7; inguinal hernia, 104 operations, 30 deaths—percentage of deaths, 28·8; umbilical hernia, 24 operations, 14 deaths—percentage of deaths, 58·8—a total of 293 cases with 103 deaths, showing a general mortality of 35·2 per cent. You will thus see that the results obtained at St. Bartholomew's Hospital are, at any rate, no worse than those obtained at other London hospitals (and it is most probable that in the latter also the latest statistics show further improvement); but I think all surgeons will be indebted to you for calling special attention to such a high mortality, and I am personally much obliged for the prominence you have given to this matter, for if properly appreciated and acted upon one may hope that in the future our results may be greatly improved. You say: "It would be interesting to know to what this high mortality is to be ascribed." In a general way this is easy to answer. The high mortality is to be ascribed to the length of time that has elapsed between the act of strangulation and the operation for its relief. The mortality need not be more than 5 or 10 per cent. Most of the deaths are avoidable. Hardly any patients die from the operation itself. Nearly all who succumb are dying or are fatally injured before the operation can be performed. In cases operated on in the first twelve hours after strangulation the mortality is quite trifling. After three or four days of strangulation the chances of recovery are but small. If it were only more fully realised that the death-rate is such as I have shown it to be, surely hospital surgeons might reasonably expect to have cases sent to them at an earlier date, and this is why I wish to insist on a recognition of the high death-rate and the responsibility incurred by those who advocate even the shortest delay.

¹ Not 44 per cent., as I am reported to have said.

With regard to the more exact causes of death, and after having made a large number of post-mortem examinations in cases of hernia, I would say that peritonitis accounts for only a small minority. When it occurs it is usually the result of perforation of the bowel at the seat of stricture and such perforation may not occur for several days after operation. In a case of my own I have known it to occur on the ninth day. Neither peritonitis nor wound complications account for the high mortality. Most of the deaths are due to exhaustion resulting from compulsory starvation of several days' duration as well as from continuous retching, vomiting and pain. In many cases the post-mortem examination reveals basic pneumonia and distension of the right heart; this is the result, in my opinion, of a distended abdomen causing upward displacement of the diaphragm, and with this is combined difficulty of breathing, due to such displacement and to the loss of abdominal respiratory movements. None of these deaths which are due to exhaustion and congestive pneumonia occur in cases operated on early, and they are all avoidable.

Finally, I would say: Let no one think that in resection of gangrenous intestine there exists an efficient means of dealing with cases in which operation has been very long delayed. I understood from Mr. Franks' admirable paper that the patients who survived the operation of resection were operated on not later than the third day and that those operated on after that time all died. If this be so—and I trust Mr. Franks will correct me if I am in error—there is then no reason to believe that after four or five or more days' delay resection of gut will result in any greatly improved statistics; whilst, on the other hand, it is fortunately a comparatively rare occurrence to find gangrenous intestine before the third day of strangulation, and I cannot doubt that in many of the successful cases the gut resected was not gangrenous in the sense in which the term is commonly used by English surgeons. I do not at all wish to condemn the operation of resection in suitable cases, but, on the other hand, I do wish to point out that too much must not be expected from it and that if patients are left too long unrelieved death is most likely to ensue whatever line of treatment is adopted. The routine performance of resection in all cases of gangrenous gut can only bring the operation into disrepute. I am, Sirs, yours truly,

Manchester-square, May 13th, 1893. ANTHONY A. BOWLBY.

"GLYCOSURIA AND AMMONIACAL FERMENTATION."

To the Editors of THE LANCET.

SIRS,—Under the above heading Dr. Bays, in *THE LANCET* of May 13th, publishes a case of glycosuria in which ammoniacal decomposition of the urine took place in the bladder, and he argues that the case stands in contradiction to a statement made by me in *THE LANCET* of Feb. 25th, to the effect that if a saccharine urine be impregnated with both the ammoniacal ferment and with the lactic ferment the lactic fermentation always gets the upper hand, and the development of the ammoniacal fermentation is thereby prevented. Dr. Bays, however, brings forward no evidence that the lactic ferment had gained entrance into the bladder in his patient. The urine, it is true, was saccharine; but a saccharine urine is as susceptible of the ammoniacal fermentation as any other urine provided the ammoniacal ferment be alone in the field. The real inference in Dr. Bays' case is that the ammoniacal ferment alone had effected an entrance into the bladder and that its action was therefore uncontrolled by any antagonistic ferment. To prove his point Dr. Bays must show that the lactic ferment was also present in the bladder with the ammoniacal ferment. His case, as it stands, in regard to its bearing on my statement is therefore entirely beside the mark.

I am, Sirs, yours truly,
Manchester-square, W., May 15th, 1893. WM. ROBERTS.

"BURIAL OR CREMATION?"

To the Editors of THE LANCET.

SIRS,—As you mention the exception taken at Liverpool that my insistence on the burial of the dead as soon as the rigor mortis has passed off may possibly lead to interment before life is extinct, will you kindly—whilst reminding your readers that the cessation of the rigor mortis is in reality the com-

mencement of putrefaction—do me the favour to reprint the annexed cutting from *The Times* of May 19th, 1875 :—

"The following question, arising out of an apprehension admitted to exist lest a curtailment of the interval now allowed between death and burial might lead to the accident of interment before life was extinct, has been proposed to us by Mr. Seymour Haden :—

"As hospital physicians and surgeons of extended experience, has it ever occurred to you to see a case of so-called suspended animation or trance which, in your opinion, could be mistaken for death; and do you think, the present state of our knowledge and resources considered, that the occurrence of such a case or of such a mistake can be regarded as possible?"

"In reply to this question we desire to say that no case of the kind it suggests ever presented itself to any of us and to express our confident belief that the dread of the possible occurrence of such a case is without support in the medical experience of this country; and, further, that the signs of death are as certain after a few hours' suspension of the vital functions as they can be after many days. It would, we feel, be a matter of regret, and an indication of a low state of public intelligence, if the groundless fears to which our attention has been directed by Mr. Seymour Haden should hinder for a moment the adoption of an improvement in our social system, the importance of which cannot be overrated.

GEORGE BURROWS, M.D., President of the
Royal College of Physicians.

WM. FERGUSSON.

WILLIAM W. GULL, M.D.

WILLIAM JENNER.

JAMES PAGET.

THOMAS WATSON, M.D.

"London, March 13th, 1875."

Perhaps, also, at the risk of repeating myself, you will allow me to lay stress once more on the following facts: 1. That it is the retention in the dwelling-house of the body after putrefaction has well set in which necessitates the use of the strong coffin, and again that it is the strong coffin which is the cause of all the evils that surround the subject; but the source of these evils is to be found, in fact, not in the burial of the dead, but in the unreasoning sentiment which prompts us to keep them unburied as long as possible and then to bury them in such a way that the earth can have no access to them. 2. That, were the dead only properly buried, not one of these evils would have any existence, not a single dead body would remain to encumber the soil, and a quantity of land of incalculable value, now hopelessly alienated, would be liberated for purposes of hygiene or of utility. 3. That the remedy for such evils is, therefore, not in cremation or in any of the alternatives that have been proposed for burial, but in a sensible recognition of, and a timely submission to, a well-defined and all-sufficient law of nature. 4. That the destruction by cremation of all demonstrable evidence of the cause of death—of all such evidence, that is to say, as is only to be obtained by the exhumation of the body—is a danger to society and an arbitrary act on the part of an irresponsible body of persons, which ought to be at once made the subject of competent investigation and, if its dangerous character is recognised, to be declared illegal. It would seem, therefore, that what is wanted is not a Bill to regulate cremation—which on the contrary, as a measure of public safety, ought rather at once to be declared a misdemeanour—but a Bill to regulate safe and proper burial, which Bill to be effectual should contain the following provisions: (1) For burial within the earth as the only legal mode of disposing of a dead body; (2) for a limitation of time beyond which it should be illegal to keep a dead body unburied; (3) for the illegality of strong coffins, brick graves and vaults, and all contrivances having for their effect to prevent resolution, and to confer on the dead a tenure, practically illimitable, of the soil which is necessary for the purposes of the living.

I am, Sirs, yours faithfully,

F. SEYMOUR HADEN.

Woodcote Manor, Alresford, Hants, May 15th, 1893.

"DIET IN CHRONIC BRIGHT'S DISEASE."

To the Editors of THE LANCET.

SIRS,—It is to be hoped that any discussion arising with regard to the question of diet in chronic renal disease will have the effect of producing considerable modification by the profession of the indiscriminate and routine prescribing of the so-called milk diet in all cases of albuminuria. I say "the profession" because I cannot agree with your correspondent of last week (Dr. Donkin) that "nephrologists" are responsible for the too general application of theoretic views. On the contrary, I would say that most writers, certainly English, have pointed out that the cases in which benefit is to be obtained by the exclusive use of milk, or the so-called "milk diet," belong almost exclusively to the recent and

acute form. Dr. Saundby, a distinguished "nephrologist," in his published Lectures on Bright's Disease, p. 271, 1889, is "convinced that absolute milk diet is in many cases not only unnecessary and extremely distasteful to most patients, but positively harmful." In my own work on "Diseases of the Kidney" &c. I state of this diet: "Though of little use—indeed, I have in one case found it positively do harm during an early stage of the disease—it is most advantageously employed during the subsidence of the attack" (p. 262); and again (p. 263): "Although absolute milk diet should never be omitted when the albuminuria is persistent, still there are many cases that get well without the necessity for employing such a rigorous measure"; and finally (p. 265) I express my opinion that the "importance of regulating the quantity of the food, and not overloading the digestive organs, is greater than rigidly supervising the quality." These expressions can hardly be taken as applying to a "theoretically strict diet," and I could quote others.

The fact is that, milk in large quantities having been found useful in cases of acute and subacute nephritis for clearing off the dropsy, especially during the subsidence of the attack and whilst the patients are still active and vigorous, the profession have somewhat indiscriminately employed it in all stages of nephritis. The rule as regards dietetic treatment in chronic Bright's disease should always be guided by the condition of the heart and vascular system and not so much by the amount of albumen and excretion of urea. So long as the pulse has a high tension and the left ventricle is still heaving with strong impulse, a light, not necessarily low, diet should be insisted on. On the other hand, when the pulse is failing and the heart muscle degenerating more liberal diet is called for and must be judiciously given; for in these cases one has to steer between two dangers—on the one hand uræmia, on the other cerebral hæmorrhage. When arterial tension falls too low, then there is an increase of uræmic symptoms; if raised too suddenly, then there is a danger of a degenerated vessel giving way. Between these two the medical attendant has to steer, and the slightest deviation to either side may bring him ashore; for, though I have seen many cases of uræmic convulsions induced by an injudicious attempt to push the "milk diet" in chronic Bright's disease, I have seen cerebral hæmorrhage also follow in cases where a meat diet, though limited, had only been resumed a few days. Close and frequent examination of the heart and pulse in these cases furnishes a more satisfactory guide of the requirements of the patient than what can be learned from estimations of albumen or of urea.

I am, Sirs, yours faithfully,

CHARLES HENRY RALFE.

Queen Anne-street, W., May 15th, 1893.

VACCINE LYMPH IN THE TROPICS.

To the Editors of THE LANCET.

SIRS,—In this moist, hot climate the great obstacle to the preservation of animal vaccine paste has hitherto been the difficulty of storing it so as to prevent its contact with the "living air." Here the air, at all times and especially during the rainy season, is laden with vegetable germs; then one's boots acquire overnight a bloom of mildew; paste or glue of any kind soluble in water becomes food for bacteria and moulds, so that book-bindings and instrument cases, if not otherwise securely fastened, fall to pieces, and even glycerine, if exposed for a few days, affords nourishment to a luxuriant growth of black mould. The paste is prepared in the ordinary way by thoroughly mingling the scrapings of the vesicles with glycerine. I have tried several kinds of receptacles—tiny porcelain jars with screw lids, homœopathic phials with corks, or paraffin stoppers and other similar plans—with disappointing results, all these methods involving more or less exposure of the paste to the air when the vessels are opened, necessitating at the same time touching and perhaps contamination of a considerable portion of the mass on the introduction of an instrument to take a portion for use; the result being that the paste loses its active properties in a week or two. I lately had the good fortune to hit upon the idea of using tiny pewter collapsible tubes with screw caps for storing the paste. I obtained a supply from London, and introduced them to the Municipal Vaccination Department here. The result has been most gratifying; the head vaccinator assures me that paste stored in this manner