

to hear what conclusion he then forms. Mr. Paterson and many others who write upon the subject speak of the anterior operation as being much "simpler" than the posterior and this simplicity is held to be an advantage. I can assure Mr. Paterson that the posterior operation can be made every whit as simple as the anterior. If Mr. Paterson will accept the invitation I formerly gave him, and now cordially repeat, to "come and see," I shall not be without the hope of convincing him that the posterior no-loop operation can be made as easy as it is possible for a gastro-enterostomy operation to be, and that his statement that "the anterior is admittedly the simpler and less complicated procedure" can be successfully refuted.

I hope it will not be thought that, though my views upon the matters we have discussed are diametrically opposed to those held by Mr. Paterson, I wish to be chary in my estimate of the great value of his lectures. They are, I think, of quite unusual excellence.

I am, Sirs, yours faithfully,

Leeds, April 29th, 1906.

B. G. A. MOYNIHAN.

To the Editors of THE LANCET.

SIRS,—In the course of the controversy in the correspondence columns of THE LANCET I have come by degrees to the conclusion that a piece of epoch-marking work in this department of surgery is in danger of losing its identity. And to-day I have read a statement made by Dr. W. J. Mayo in a communication to the *Annals of Surgery*, November, 1905, which I quote: "In the summer of 1903 Mr. Moynihan demonstrated to us the method he was using with the clamps and which goes by the name of the Moynihan operation," &c.

On Oct. 19th, 1900, my colleague, Mr. H. Littlewood, made a communication to the Leeds and West Riding Medico-Chirurgical Society upon a method of performing intestinal suture which he had been led to devise and carry out in consequence of the spontaneous closure of the gastro-enterostomy opening in two cases not in his own practice. I refer your readers to THE LANCET of June 29th, 1901, p. 1817, in which Mr. Littlewood describes the operation he had invented, which is essentially—indeed, with many of us identically—the same operation that is performed to-day, that is, by the aid of rubber-sheathed Doyen's forceps and with the excision of an ellipse of mucous membrane from each viscus.

There are two variant details the value of which is variously estimated, and it may be that Mr. Moynihan attaches so much importance to them and has practised them successfully for so long that he has come to regard them as the core and origin of the whole procedure. I refer to (1) the first line of suture, and (2) the question of "loop or no loop." Mr. Littlewood incised the serous and muscular coats and stripped them up a little in the submucous layer before he sutured them. Mr. Moynihan places his first line of suture through the outer coats before incising them. This is obviously a quite minor point, and some surgeons use either method indifferently. As a matter of fact, Mr. Littlewood now completes his incisions before he puts in any sutures at all, and he claims as the resulting advantage that there is no risk of soiling any suture already *in situ*.

Mr. Moynihan attaches great importance to the establishment of the jejunal opening as high as possible. It may even be that he has come to regard this as the essential feature of the operation, but if so I would point out that this is not what Dr. Mayo refers to as the Moynihan operation; and that the method of intestinal anastomosis by means of Doyen's clamps and the excision of ovals of the mucosa, if it is to be known by the name of its author, should be called the Littlewood operation. The omission to put this matter of authorship and precedence in a clear light is probably, as it may so easily be, a mere inadvertence on Mr. Moynihan's part, and Mr. Littlewood's reticence has led me to recall the sequence of events as we who witnessed these developments so well remember it. In Robson's and Moynihan's book, published ten months after Mr. Littlewood's original paper, no mention is made of clamps.

My reason for asking leave to publish these remarks in your columns is to afford Mr. Moynihan the opportunity, of which I am sure he will be only too willing to avail himself, of publicly stating how far Dr. Mayo and those who accept his statement are in error in withholding from Mr. Littlewood the credit of the pioneer work in this important advance in intestinal surgery. Whether the operation is anterior or posterior; whether the outer coats should be incised before suture or sutured before incision; whether a jejunal loop is undesirable or not—these

points, even the last of them, are, so far as concerns the principle, the method, the technique, of the operation, of lesser importance; the essential features are: (1) the use of rubber-sheathed Doyen's clamps, with the fourfold object of steadying the parts in apposition during suture, preventing puckering and contraction from too tight stitching, preventing the escape of intestinal contents, and controlling hæmorrhage; and (2) the excision of elliptical portions of mucous membrane from each organ, so as to produce and maintain a free oval opening and to prevent the healing of the cut edges across the gap.

By far the worse of my own two cases of *circulus vitiosus* was after a posterior operation in which I took special pains to make the anastomosis as high as possible, short of producing actual tension at the time. The absence of any loop made the later treatment of this case a matter of the greatest possible difficulty and anxiety. In my other case, an anterior operation, the condition was easily converted into a Roux's operation, but, although not a very early convert, I have for some time been a convinced believer in the advantages of the posterior operation in all cases—a very large majority—where it is practicable. It would be very interesting if Dr. Mayo would formally restate what he regards as the important, the essential, points of the operation.

I am, Sirs, yours faithfully,

Leeds, May 2nd, 1906.

EDWARD WARD.

CASEIN AND CASEINOGEN.

To the Editors of THE LANCET.

SIRS,—My attention has been called to a letter from Mr. H. Droop Richmond in THE LANCET of April 28th, in which he repeats the protest contained in his book against the use of the words caseinogen and casein in the sense in which they are employed in my writings on the subject. Proteid nomenclature in general is in such a confused condition just now that a joint committee of the Chemical and Physiological Societies has been appointed to attempt to reduce the chaos to order. Among the recommendations of this committee I find the following: "They strongly urge the continued use of the words caseinogen and casein in the sense now employed in the majority of English text-books." It would be most desirable for analytical chemists to fall into line also. The words are most helpful to teachers and students and have the recommendation of being rational.

The word casein is derived from the Latin word *caseus*, which means cheese. Cheese is the result of the action of rennet and calcium salts on the principal proteid of milk, and so it is right to apply the term casein to the curdled product and not to the original proteid upon which the rennet acts. The original proteid begets the casein; it is the mother substance of the cheese and so is rightly named caseinogen. The termination *gen* is derived from the Latin verb *genero* (I beget or produce). Caseinogen is therefore not a hybrid as Mr. Richmond says. Does Mr. Richmond also propose that we should drop the word fibrinogen and call it fibrin? Such a proposal would be quite on all fours with his suggestion that we should resort to the antique terminology he has not yet abandoned and call caseinogen casein. His statement that Dr. Gustav Mann has confused the two words is not an argument in his favour. The so-called confusion is due to an obvious press error which will be corrected in future editions of Dr. Mann's book.

I may add in conclusion that the word caseinogenate which Mr. Richmond fears may be introduced is already largely employed in physiological literature and bids fair to become as firmly rooted as caseinogen. Surely a chemist is the last person to object to words on account of their length.

I am, Sirs, yours faithfully,

King's College, London, May 4th, 1906. W. D. HALLIBURTON.

THE TRANSMISSION OF LEPROSY.

To the Editors of THE LANCET.

SIRS,—You will be glad to learn that Dr. W. J. Goodhue, medical superintendent of the Molokai Leper Settlement, has after several years' work there been able to demonstrate the bacillus lepræ (Hansen) in the mosquito (*Culex pungens*) and in the bed bug (*Cimex lectularia*). I will quote from my brother's official report to the Hawaiian Board of Health:—

..... We have since been sectioning mosquitoes taken from various leper houses, but until last June [letter dated Feb. 10th, 1906] without any apparent success. At that time it appeared that we had isolated