

nutriment, because their number is so great that their combined bulk would be much greater than the germ cells they are supposed to provide with nourishment. They look for all the world like so many messengers, anxious to deliver their messages to the germ cells. Is it not just possible that these cells, so sensitive and active and so vital in many other respects to our economy, may also be the carriers of acquired modifications? Were it so, it would explain why the mere hypertrophy of an organ from use would not be inherited, as this is a mere quantitative change, not a qualitative. And also why mutilations are not inherited, as in this case no message could be expected from an organ that no longer existed.

We must remember that the nerves are not the only transmitters of impulse in animals. This has been proved by Popielski, Wertheimer, and Le Page, who have shown that a flow of pancreatic juice can be induced by introducing acid into the duodenum, even when the nerves supplying the duodenum and pancreas have been cut through. These experiments have been repeated by Starling and Bayliss, who consider that the secretion cannot be reflex, since it occurs after the extirpation of the solar plexus as well as the nerves. "It must therefore be due to direct excitation of the pancreatic cells, by a substance conveyed to the gland from the bowel by the blood-stream. *So many of the connections between organs are made by nerves that we are apt to forget the other messenger, the blood.*" (Halliburton's "Physiology," tenth edition, p. 517.) We know that there are several varieties of leucocytes, and this must imply different functions. But we know very little of their origin. It would be interesting to know what resemblance the various varieties show to the original germ plasm on the supposition that some of them may be near relatives who circulate in the blood for the express purpose of collecting and conveying acquired modifications to the developed germ cells.

I am, Sir, yours faithfully,
GERDA B. JACOBI.

Tavistock-place, London, W.C., Feb. 24th, 1913.

THE STATISTICS OF OPERATION FOR APPENDICITIS.

To the Editor of THE LANCET.

SIR,—May I correct a slight error in your report of my remarks at the meeting of the Medical Society of London on Feb. 10th? I was speaking of my own statistics, not those of the whole of St. George's Hospital. I append a table of my results there during the last four years.

I am, Sir, yours faithfully,

Half Moon-street, Piccadilly Feb. 21st, 1913. G. R. TURNER.

A.D.	Cases.	Drainage.	Deaths.
1909	42	22	0
1910	43	20	2
1911	30	11	1
1912 (up to date)	49	30	0
	164	83	3 = 1·8 % mortality.

144 acute cases. Tube and gauze used in 83. 3 deaths = 2·07 per cent. mortality.

Last 62 cases without a death. One death in last 100 cases. No death in last 50 acute cases.

Analysis of cases of death.—1. One from intestinal obstruction ten days after operation; operated on by another surgeon. This was not a drainage case. The wound was healed, and patient apparently well. 2. General septic peritonitis present on admission before and after operation. 3. Fatal peritonitis followed removal of gauze in a case doing well up to this time.

THE MORTALITY OF THE ABDOMINO-PERINEAL OPERATION FOR RECTAL CANCER

To the Editor of THE LANCET.

SIR,—I have read with interest Mr. Leonard Ley's letter in THE LANCET of Feb. 22nd, p. 563, in which he advocates, as a routine, a preliminary colostomy for a fortnight before excision, with a view to reducing the mortality from sepsis of the abdomino-perineal operation. I agree with him that this course is advisable in certain cases in which the colon cannot be satisfactorily cleansed before operation (the semi-obstruction type). In the majority of cases which are suitable for the combined operation there is no difficulty in thoroughly

cleansing the bowel by means of enemata. Cases in which there is any tendency to obstruction will, when explored, usually be found unsuitable for the major operation, although operation may become possible after the establishment of a colostomy.

The establishment of a preliminary colostomy possesses certain disadvantages: (1) the presence of an artificial anus in the abdominal wall will render sterilisation of the skin a matter of uncertainty, and adds an additional risk of sepsis to the operation; (2) the existence of an inguinal colostomy, so far from shortening the operation, tends to prolong it, renders it more difficult, and adds to the risk of infecting the peritoneal cavity during division of the bowel within the abdomen.

In performing the combined operation the pelvic colon is divided outside the abdomen. This portion of the operation with closure of the ends can be completed very quickly, and the time spent in bringing out the upper end through the inguinal region is less than that spent in dividing and closing the bowel within the abdomen when a colostomy is already in existence. Furthermore, when the colostomy has been previously performed, adhesions may occur which complicate and prolong the second stage of the operation. I have been in the habit of keeping patients in bed for at least a week prior to operation. During this period copious enemata are given daily, and naphthalene tetrachloride is given internally as an intestinal disinfectant. In a few cases I have given prophylactic doses of a coli vaccine prepared from the patient's faeces two or three days prior to the operation.

I believe that in operating on suitable cases with moveable growths careful preliminary treatment followed by operation in one stage is preferable to a two-stage operation, because it enables the operation to be conducted on more aseptic lines and with less difficulty. If the case is considered suitable for the major operation, but symptoms of toxic absorption or faecal accumulation exist and do not clear up by preliminary treatment as laid down, then appendicostomy, followed by irrigation from above and below, should be employed rather than inguinal colostomy. Preliminary colostomy should, in my opinion, be reserved for cases which manifest signs of partial obstruction. The high immediate mortality of the operation in suitable cases is due in the main to two causes, shock and sepsis. Severe shock can best be avoided by rapid operation, a preliminary dose of morphia and atropine, and the use of oxygen passed through warm alcohol during chloroform anaesthesia, followed by continuous saline infusion either *via* the colostomy wound or subcutaneously. Sepsis can only be avoided by scrupulous care and experience in the details of the technique. Another factor in the high mortality of the operation is the selection of unsuitable cases for the major operation—e.g., stout patients over 60 (especially males) and patients with growths which have passed the confines of the bowel wall.

The mortality of the operation is rapidly improving in individual hands with increase of experience. Wertheim's operation for cancer of the uterus when first practised by gynaecologists suffered a far higher mortality than it does at the present day. *Ceteris paribus*, the most important factor of all is experience.—I am, Sir, yours faithfully,

London, W., Feb. 24th, 1913. C. GORDON WATSON.

THE DIVERSE CONSEQUENCES OF ORAL SEPSIS AND ITS TREATMENT.

To the Editor of THE LANCET.

SIR,—Oral sepsis is now commonly recognised, but as the symptoms are very varied, I should like to bring forward in your correspondence pages certain points whose further discussion might be useful. The following cases occurring in my own practice have presented such different symptoms that a very great diversity of feature must be common.

1. A woman, aged 40, developed a swelling of the right ankle-joint, dark red and shiny and extremely painful. After this cleared up four or five patches appeared on the legs about the size of half a crown, and appeared identical with erythema nodosum. Then an almost agonising pain between the shoulders, then extreme pain in the right knee, but no swelling. All these appeared over a period of three months, at the end of which time the patient was agreeable

to undergo any treatment for relief, and so she consented to have all her teeth out as her gums were very septic. She soon regained perfect health and looked years younger.

2. A woman aged 56. I found the arms, hands, and legs covered with cutaneous hæmorrhages, some as large as a shilling. She was very anæmic, had hæmoglobinuria, and her very septic gums were continually bleeding. All her teeth were extracted at a hospital and she nearly succumbed to hæmorrhage afterwards, but when I saw her three months later she was apparently quite well and with a good colour.

3. Male, aged 40. When I saw him he could not move his left arm owing to very severe pain. He had a very septic condition of his gums but would not consent to extraction of the teeth. He went back to work after eight weeks' treatment, but had recurrent attacks of similar pain but not so severe. About a year after his first attack he suffered from loss of memory for five days, followed by prostration for three weeks, but whether this was due to the pyorrhœa or not, I do not know. He was a non-smoker and he told me that friends of his who had had pains similar to his were also non-smokers and had septic gums such as he had.

4. Male, aged 55, had frequent recurrent attacks of agonising pain in the left lumbar region, needing morphia injections; he was X rayed but no stone could be seen. By my advice he had all his teeth removed as his gums were exceedingly septic. He lost all the pain and his anæmia from which he also suffered.

5. Male, aged 60, had continual pain and tenderness on pressure over the duodenum, severe paroxysms of coughing and retching every night, unable to work, no appetite, very depressed, and contemplated suicide. After eight months at home he agreed to have his mouth cleared of septic stumps. Now, three months after, he can enjoy his food, the duodenal pain is gone, sleep is undisturbed, and he says he feels well.

I have under treatment now two patients completely invadled by "rheumatism," who will not, I believe, get any better until they agree to have their teeth seen to. I have had a large number of cases during the last 15 years, and find that the men with this disease are non-smokers, or practically so. The variety of symptoms is, I think, an interesting point, and should form quite a long list in the books on medicine.—I am, Sir, yours faithfully,

Stock, Ingatesone.

HERBERT CARDIN.

PSYCHOLOGY: A NEW SYSTEM.

To the Editor of THE LANCET.

SIR,—I have always been prepared for hostility, for the man who in the realm of philosophy seeks truth, and truth only, is sure to find the big battalions against him. Moreover, although the cry for originality is heard, what is really hoped for is something daring within safe and orthodox lines—an originality which arises in the very mode of treatment disconcerts the conventional mind, and resentment of what is new and strange finds even intellectual expression in the desire to "eave 'arf-a-brick at 'im." And so the popular conscience is vindicated.

Such has been the history of criticism in this country, for I would ask you how many instances you can point to of great works of thought which have not been assailed by the malice of the orthodox or covered by the derision of imbeciles. Nevertheless, I confess I was surprised to read in THE LANCET of Feb. 22nd, p. 540, a review so inept as that which dealt with my "Psychology." I will not further refer to the writer; but after having deliberately abandoned old landmarks, after having toiled in preparation through realms more extensive at least than ever entered upon by any thinker before, and after having established a system which a brilliant Italian psychologist declared to be striking in its entire originality and scope, possibly I may be allowed to say a few words, less in the way of rebutting a foolish criticism than in the desire to indicate the true purport of the book. Out of a hundred possible illustrations I will select one which touches on our own profession. Thus, in regard to localisation there was no principle in physiology held with greater certitude than that which referred aphasia to Broca's lobe. Throughout all the universities and medical schools of the world this doctrine was given with authority, and indeed with that manner, so common in universities and so fatal to progress, of discouraging investigations of orthodox teachings.

My analysis, on the psychological side, of aphasia led me to the conclusion not only that the doctrine of Broca's lobe was untenable, but that the whole theory on which researches in localisation had been conducted was fallacious. In this light I re-examined the work of many—Ferrier, Munk, Goltz, &c.—whose reputations had been built on expositions of the functions of the brain, and I showed not only that there was no concordance of the authorities amongst themselves, but that when they overstepped into the psychological realm their deductions from observations were unwarranted and contradictory. Broca's conclusions are now overthrown, as those who are interested may discover in Moutier's imposing volumes. And this result I am able to quote in confirmation of my analysis; but that analysis is none the less necessary now, since the same incomprehension of the mechanism of thought that allowed too facile acceptance of Broca's dictum will lead to discrediting what is valid in his work.

I am entitled to ask whether, in the whole range of psychology, you can show me a work which can claim anything analogous to this criticism of localisation. Viewed, however, in regard to the intention of the work, this result occupies in the perspective a place of no special importance. It is only one out of innumerable conclusions which arise by way of corollaries in the course of the exposition, and which will eventually be found to interest all branches of science and to throw light on the mode of their development. The investigation of the foundations of mathematics is, on the other hand, an integral part of the book, for this study has been essential in order to ensure rigour in the proofs I have given of the completeness and of the necessity of the fundamental processes.

The pith of the matter consists in the enunciation of these fundamental processes, the illustrations of the modes of their combinations, and the demonstration that by such syntheses the whole range of mental action—in regard to any psychic act whatever—may be expressed. Only two philosophers have hitherto definitely posed the main problem of psychology as I have conceived it—Aristotle and Kant. I have shown, in the course of the analysis, not only that their systems were wrongly based, but also I have pointed out exactly wherein they failed, and I have indicated the origin of their faults. I am making a plea now not merely for myself but for the science itself, which has ever fascinated me, and which, as I apprehend it, is destined to rise as from a new birth and to offer new illumination throughout the world of human thought.

I am, Sir, yours faithfully,

St. Ives, Cornwall, Feb. 24th, 1913.

ARTHUR LYNCH.

EPILEPSY AND DEMENTIA.

To the Editor of THE LANCET.

SIR,—Inasmuch as Dr. W. Alexander, of Liverpool, in a paper entitled "Necessity of a Better Classification of Epileptics" published in THE LANCET of Jan. 25th, 1913, p. 263, questions my statement, "Epilepsy is a disorder leading to dementia," I desire to call attention to the admission Dr. Alexander makes in the same paragraph, "that sane epileptics do not tend to dementia to any serious extent."

It has been my privilege to have closely observed over 3000 epileptics during the past 12 years, during which period I have resided in the Craig Colony for Epileptics. From this experience I have arrived at the conclusion that the epileptic who does not show some evidence of mental deterioration is the exception that proves the rule. Furthermore, it is exceedingly difficult, if not impossible, in many cases of epilepsy to draw the line between sanity and insanity. A careful examination of the mental state of epileptics in the various institutions for their special care will show but a very small number who would be called normal or even approaching normal, mentally.

If it be true, as a great proportion of those experienced in the care of epileptics believe, epilepsy is a condition due in part to, and resulting in, permanent destructive changes in the brain, what could one look for to accompany the other symptoms but a mental impairment, more or less progressive in nature? One must remember that in any condition in which mental deterioration occurs that there are various degrees of impairment and also periods during which this deterioration is apparently in abeyance, such periods lasting for perhaps many years. My statement that "Epilepsy is a