beyond doubt that the symptom-group called cancer is always due to one and the same cause; and that it is not due to parasitism, poisoning, or traumatism; in other words, that it is not due to a cause of extrinsic origin, to any interference from the environment. The manner of growth that characterises the cancerous tumour is, to my mind, to be accounted for only by some disturbance or dislocation, temporary or permanent, of the normal processes of growth; there is, as the older physician would have said, some constitutional flaw at work. The hopeful aspect of the situation lies in the fact that we already know a great deal concerning the antecedent conditions that lead to this constitutional dislocation in individual cases. It is enough to mention sweep's cancer, but of the "cause" we have as yet no positive knowledge. This ignoring of the constitution, of the intrinsic causes of disease, is not confined to the case of cancer; it has been a feature of modern medicine ever since "pathology" and its "organic diseases" took possession of our science. The search of our scientists is now for "the anatomical seat of the disease," to quote from the official "Nomenclature of Diseases," a phrase to which I defy anyone to attach a rational, not to mention a scientific, meaning.

In many cases of diabetes, asthma, epilepsy, insanity, hysteria, it is obvious to all that the cause of the symptoms shown does not arise in the environment of the patient; it must therefore arise in his constitution, it must be intrinsic in origin. Instead of recognising this fact, the cause is simply ignored in the definition of all such "diseases"; we call recurrent symptom-groups by the same name as our true diseases, with the result that one-half of our science is left unsystematised. An exception is made in the case of one group of diseases of intrinsic causation; incomplete development is recognised as a cause of disease on a par with parasitism, poisoning, and traumatism. The diseases of incomplete development are true diseases, because on the basis of correlated cause and effect we frame definite conceptions of them; how long will it be before the other intrinsic causes are recognised? If cancerous symptoms prove, as I believe they will, to be of intrinsic causation, it will not be possible to differentiate "the disease cancer" sharply from other conceptions, as typhoid fever is differentiated; it will merge insensibly into other diseases, such as keloid. There are always indefinite and arbitrary lines, and borderland cases, amongst constitutional diseases.

My case may be summarised by saying that three conditions, not one of which is at present recognised, must be complied with before the solution of the cancer problem can be reached; the definition of "a disease" as "a mental conception, drawn from the observation of a series of symptom-groups of determinate and similar causation," must be accepted as a definite and universal standard in medical science; the intrinsic causes of disease must be recognised as being on an equality, scientifically speaking, with the extrinsic causes. It is also essential that antecedent conditions—which belong to the individual, actual attack, and are variable—be clearly differentiated from the "cause," which is constant, and on the correlation of which with a recurrent recognisable symptom-group depends the recognition, the definition of every true disease. At its present stage the solution of the cancer problem demands, above all things, broad and clear thinking.

I am, Sir, yours faithfully,

Clevedon, Dec. 17th, 1911.

SYDNEY W. MACILWAINE.

To the Editor of THE LANCET.

SIR,—Sir Henry Butlin—I think truly—says "students of cancer, research workers, biologists who have studied cancer, are all practically agreed that the cancer cell is derived from the cells of the part in which it takes its origin." At the same time I think the majority of scientists who are not in the above category feel that the parasitic theory of cancer falls more into line with the broader principles of medical knowledge. Personally I feel that the latter view is more likely to be correct, and welcomed Sir Henry Butlin's paper as supporting it. But after coming to the conclusion that the unicellula cancri is "an independent creature," it is a little surprising to find that he is obliged to bring in heterogenesis as the explanation of this. Surely one must make very certain that there is no other explanation possible before adopting a theory which is now almost universally discarded.

The ground on which he bases his belief that the cancer

cell is formed within the body is the close resemblance of its appearance and reactions to those of the cells around. Cannot this, however, be explained as being an analogy in parasitology to what occurs in zoology in the case of protective adaptation to environment? In other words, may it not be that the cancer cell, by closely resembling in its reactions the normal cells around, escapes the vigilance of the phagocyte, and so is enabled to gain a footing and proliferate?

I am, Sir, yours faithfully,

Brook-street, W., Dec. 29th, 1911.

J. BRUNTON BLAICKIE.

THE HEALTH SEASON IN EGYPT.

To the Editor of THE LANCET.

SIR,—Many people who are more or less invalids find it desirable to leave England during the winter months for some place where the climate is warmer and drier, and of these not a few naturally think of Egypt. I find that there is a more or less prevalent idea that there are reasons why, *this year*, Egypt should not be selected as a place of residence. So far as I can make out it is supposed that travellers run some risk: (1) from the war between Italy and Turkey; and (2) from the possibility of cholera breaking out in Egypt. As regards the war, a few minutes' consideration will convince any reasonable person that Englishmen run no risk whatever. Egypt, in spite of alarmist statements in some of the newspapers, has never been more tranquil than at present. The very slight disturbance in the lower quarters of Alexandria, which lasted for a few hours, some weeks ago, though much exaggerated in one of the London newspapers, was not to be compared for one moment with the riots which occurred in many English towns a few months ago and did not affect English residents or tourists at all. No other disturbance of any kind has occurred. The danger of any attack on life or property exists only in the minds of alarmists in England and is not seriously contemplated in Egypt.

As regards cholera, the precautions taken at the Egyptian ports are so stringent that an outbreak of cholera is practically impossible. I am pleased to find, too, from practical experience that the precautions are not confined to the ports, but that the address of every traveller who has touched at an infected port is taken and notified to the sanitary officer of the district or town he goes to. So that should a sporadic case by any chance pass the frontier it would be dealt with very quickly, and I believe effectively. Some discomfort—i.e., one or two days' detention on the steamer which has conveyed them—may be inflicted on tourists who have come from an infected port. But those coming from England by sea (not touching at an infected port) or those coming overland by Marseilles or by the excellent service *via* Trieste and Austrian Lloyd steamers would suffer no inconvenience whatever.

I am so convinced that residence in Egypt is as safe this year as any other year that I have brought out my wife and family, and I write these lines to do what is possible to dissipate the rumours I have alluded to.

I am, Sir, yours faithfully,

Helouan, Egypt, Dec. 11th, 1911.

R. ACKERLEY.

THE EVOLUTION OF MODERN GYNÆCOLOGY.

To the Editor of THE LANCET.

SIR.—In the very interesting article on the Evolution of Modern Gynæcology, by Dr. Arthur E. Giles, in THE LANCET of Nov. 25th, 1911 (p. 1470), the honour of having performed the first complete extirpation of the uterus and ovaries by the abdominal route, and of having done the first successful hysterectomy for fibroid tumour, was given to Charles Clay of Manchester. Without desiring to minimise the remarkable achievements of Charles Clay in the early days of abdominal surgery, the above statement seems to demand some correction.

On Nov. 17th, 1843, Nov. 21st, 1843, and on Jan. 16th, 1844, Charles Clay opened the abdomen and removed fibroid tumours of the uterus. The third operation is the first recorded abdominal pan-hysterectomy. All of these operations were based upon the erroneous diagnosis of ovarian tumour, so that in no case was the operation started with

the intention of doing a hysterectomy. In 1842 Charles Clay had likewise made a similar mistake in diagnosis, but he abandoned the operation when he discovered the tumour was uterine.

The first deliberate hysteromyomectomy was that performed in June, 1846, by John Bellinger, of South Carolina, U.S.A. The patient, a coloured woman, died upon the fifth day from peritonitis. The first successful hysterectomy was performed by Walter Burnham, of Lowell, Massachusetts, on June 26th, 1853. This was not a deliberate operation, as Burnham considered the tumour to be ovarian. After the incision was made the patient vomited, forcing the uterus, the fibroid, and associated ovarian tumours out of the wound, and as Burnham was unable to replace the mass he did a supravaginal removal of the uterus, with ligation of the lateral arteries. A left ovarian tumour was removed and a right ovarian tumour also incised. The ligation of the ovarian and uterine arteries in the broad ligaments is similar to that of the modern operation. Burnham continued to do the operation from time to time, performing altogether 15 operations, but only three of his patients recovered.

The first deliberate and successful hysterectomy for fibroid tumour was performed by G. Kimball, of Lowell, Massachusetts, on Sept. 1st, 1853. Kimball had a clear concept of the diagnosis, and in his writings advocated the operation. Previous to 1856 Kimball operated four times for fibroid tumours of the uterus, with but one recovery. The successful operation of E. Koeberlé was not performed until April 20th, 1863. The first successful abdominal myomectomy is apparently that of W. L. Atlee, of Pennsylvania, performed on August 28th, 1844. This was not a deliberate myomectomy as the tumour was supposed to be ovarian, but the ovaries were found to be normal at the necropsy three years later.

The honour of having performed the first deliberate hysterectomies for fibroid tumour seems to belong to the following surgeons in the order named:—Bellinger, Kimball, Burnham, Clay, and Koeberlé. For a fuller discussion of the subject and a careful tabulated bibliography of the early operations for fibroid tumours, reference may be made to the article by Noble in the *American Journal of Obstetrics*, Vol. XL., No. 2, 1899.—I am, Sir, yours faithfully,

Philadelphia, U.S.A., Dec. 18th, 1911. W. WAYNE BABCOCK.

THE PRESCRIPTION OF TUBERCULIN.

To the Editor of THE LANCET.

SIR,—I can see nothing in Mr. Gamble's letter to modify my opinion. Whatever terms are used, and however explicit the physician may be to indicate his intentions, one comes back to the fact that he is ordering a specific fluid, and the dilution of that fluid must be calculated in measures. In my last communication I referred to Koch's bacillary emulsion, because at the time I was making dilutions of that fluid, but it applies with even greater force to Koch's new tuberculin T.R. 1 c.c. of this fluid contains the active bacterial matter of 10 milligrammes of tubercle bacilli, which represents 2 milligrammes of solid substance. I have seen the dose of T.R. indicated in decimals or fractions of a c.c. of that fluid, in fractions of a milligramme of tubercle bacilli, and in fractions of a milligramme of solid substance. Therefore it would be far better, and would avoid any possibility of confusion if it were always ordered in decimals or fractions of a c.c. of the original fluid. Personally, I prefer to work in decimals. I am, Sir, yours faithfully,

London, Dec. 19th, 1911.

P. W. SQUIRE.

"GREEN" TEA.

To the Editor of THE LANCET.

SIR,—I have had a good deal to do with China tea, and therefore I am much interested in your annotation on p. 1786 of THE LANCET of Dec. 23rd. Your correspondent says that formerly Prussian blue was used for colouring the leaf in "green" tea. I should be very much obliged if you would ask him to supplement his interesting letter by stating what evidence there is of the former use of this pigment. The statement was freely made in this country some 35 or more years ago, and to it was largely due the disuse of green tea and the ruin of the trade in green tea in this country; but I do not think it was made on any sure basis of fact. For all

that it may have been true, and I should be glad to know if your correspondent has any evidence to offer.

I am, Sir, yours faithfully,

Duchess-street, W., Dec. 29th, 1911. E. MUIRHEAD LITTLE.

SPONGE EDUCATION.

To the Editor of THE LANCET.

SIR,—In your issue of Nov. 11th, p. 1348, you have an annotation on "Sponge Education." This I read with much interest, but would point out that this is by no means a new form of treatment, and I do not think it had its origin in America, as you suggest. In 1889 Mr. A. G. Miller, surgeon to the Royal Infirmary, Edinburgh, regularly taught his students the advantages of applying prepared sponge for encouraging the growth of granulations. He termed his system "sponge grafting." His then house surgeon, Mr. Alexander Miles (now surgeon to the Royal Infirmary, Edinburgh), will doubtless recollect these successful experiments. In 1892 I was consulted by a lady with a large callous ulcer on her leg. She informed me that this had been present for over ten years and had resisted all treatment. Following the method taught by Mr. Miller, I sponge-grafted the leg, and in four months the ulcer had disappeared, leaving a firm cicatrix. I met my patient in 1908, and she informed me that the leg was in excellent condition and had given no further trouble. In numerous cases I have tried Mr. Miller's so-called "sponge grafting" to encourage growth of granulation tissue and invariably with good results.—I am, Sir, yours faithfully,

DAVID DONALD, M.D. Durh.,

L.R.C.P. & S. Edin., L.F.P.S. Glasg.

Victoria, British Columbia, Dec. 4th, 1911.

* * In this regard we may mention a letter in THE LANCET of Nov. 25th, 1911 (p. 1516), where the work of Professor D. J. Hamilton is quoted:—ED. L.

THE ACTION OF RADIUM ON MALIGNANT GROWTHS.

To the Editor of THE LANCET.

SIR,—Many explanations have been advanced as to how radium acts in cases of malignant growths. Some authorities, such as Wickham (Paris) and Finzi (London), maintain—and, I think, with good cause—that radium exercises a selective or specific action on cancer cells, while others, among whom may be mentioned Bashford (director of the Imperial Cancer Research) and Mansell Moullin, refuse to see any such selective action, and state the action of radium is merely that of a powerful caustic. Its action in such a case might be comparable to that of arsenic paste, zinc chloride, potassium bichromate, and other chemical caustics the application of which has occasionally given good results in cancerous growth.

A third theory, propounded by Deane Butcher, is that radium possibly acts as a vaccine, its rays exciting the leucocytes to the production of antibodies. In support of this it might be stated that the opsonic index has been found raised after applications both of X rays and radium.

Dominici,¹ in an interesting article, shows by means of microscopic sections the gradual retrogression of a case of sarcoma under the influence of radium. Briefly, the sarcoma is converted into a fibroma, which is eventually changed into scar tissue. The changes described are apparently of the same nature as those occurring in the "spontaneous or natural cure of cancer."²

Dominici, moreover, demonstrates that this fibroma is not made up of the vascular connective tissue which exists between the sarcomatous cells, but is a result of the transformation of the sarcomatous element itself. Briefly, this change, he says, is brought about in the following manner. 1. A metamorphosis of some of the protoplasm of the plasmodial elements as well as the sarcomatous cells into connective tissue fibres. 2. A transformation of the remainder of the protoplasm and of the nuclei of the sarcomatous elements into permanent fibrous tissue cells. 3. The obliteration of the blood-vessels. I have purposely

¹ Archives Générales de Médecine, Radium Number, July, 1909.

² Handley: Brit. Med. Jour., March 6th, 1909.