EXCESSIVE LIABILITY OF EUROPEAN WOMEN IN AFRICA TO TRYPANOSOMIASIS:

ITS CAUSE AND PREVENTION.

By SIR PATRICK MANSON, K.C.M.G., M.D., F.R.S.,
PHYSICIAN TO THE SEAMEN'S HOSPITAL SOCIETY; MEDICAL
ADVISER TO THE COLONIAL OFFICE.

So far as I am aware, there is no anatomical or physiological reason for thinking that the European female is more susceptible to trypanosome infection than the European male. Considering the very small number of European women and their relatively slight exposure as compared to European men in tropical Africa, one would expect a case of trypanosomiasis in the female European to be a rare event. Of every hundred Europeans there, I do not suppose that five are females. I have no accurate statistics, but I believe that 5 per cent. is an overestimate. Nevertheless, of the 14 cases of trypanosomiasis in Europeans (2 from the River Gambia Colony, males; 2 from Northern Nigeria, males; 2 from Uganda, males; 5 from the Congo, 1 male and 4 females; 1 from the Congo-Rhodesia Frontier, male; 2 from Loanda, 1 male and 1 female) with which I have had to deal, 5 were females. Assuming that the above estimate of the proportion of the sexes of Europeans in these districts of Africa is approximately correct, and assuming that the susceptibility of the sexes to this terrible disease is equal, then one would expect that instead of 9 males afflicted with the disease, I should have encountered at least 95.

Having recently met with the fourth and fifth cases (included in the above) of trypanosomiasis in European ladies from Africa, the conviction has been borne in on me that this relatively enormous disproportion in liability to this very grave infection in the European female is no mere accidental coincidence. I am induced to call attention to the subject in the hope that, the fact and the probable cause being indicated, European ladies in tropical Africa will see to it that by quite simple measures a terrible risk may be obviated in the future.

Of my 5 trypanosome cases in females, 3 gave a definite history of a bite by some insect on the leg. Immediately on being bitten the part became very painful, inflamed, and swollen, and within a few days the initial fever and eruption symptomatic of trypanosome infection supervened. When I saw the patients some months later there was still distinct evidence in the local erythema and swelling of what had occurred. In one case only the infecting bite was apparently on the finger. With regard to the fifth case my impression was, though I have no record of the fact, that the bite was, as in the other three cases, on the leg. However that may be, it is certain that in three-fifths of the cases the infection was introduced by a bite on the leg.

These facts indicate that in the present European female dress *Glossina palpalis*, or possibly some other biting vector, has its opportunity, and that the petticoat and skirt should be discarded for some other type of garment, and one more resembling the male trouser, which apparently confers a relative immunity to the wearer. Skirts and petticoats should be discarded in Africa, and a Bloomer costume, or loose trousers of some sort, securely tied round the top of the boots, as well as loose sleeves similarly tied round the wrists, substituted. Thus, in my opinion, the grave and excessive risk to which the European female is unnecessarily exposed in tropical Africa might be obviated. Men, too, should have their trousers and jackets similarly secured. One of my male patients was infected by a bite on the leg while lying asleep with legs exposed on the deck of a river steamer. Manifestly the bathing-drawers style of trouser, so much affected by European travellers, residents, and sportsmen in Africa, should be abandoned for a safer kind of leg covering.

THE number of medical practitioners in Russia in 1907 was 18,315, of whom 986 were women. Of lower grade practitioners there were 20,279, of whom 3,150 were women.

RESULTS OF ONE HUNDRED OPERATIONS FOR CARCINOMA MAMMAE.

By PRIESTLEY LEECH, M.D., B.S.LOND., F.R.C.S.ENG.,
SENIOR SURGEON TO THE ROYAL HALIFAX INFIRMARY.

It has been in my mind for some time to put down in actual figures the results of my operations for cancer of the breast, but although I had made some steps towards that end it was the coming of the Yorkshire Branch of the British Medical Association to Halifax that gave the necessary impetus to the finishing of the task, which has been no light one.

The impressions formed by a surgeon as regards the results of a certain operation are not to be trusted unless these impressions are verified by a complete overhauling of the cases during some years. The successful cases are very apt to be remembered and the unsuccessful to be forgotten—not necessarily through any natural human desire to forget the unpleasant things in life, but because the old proverb, "Out of sight, out of mind," applies to patients who have died just as forcibly as it does to other persons and things. I confess that in my case I had formed an impression that my results in cases of breast cancer were much better than they actually are, simply because I have come in contact at intervals with cases which were operated on several years ago and have had no recurrence; the cases which had died some time after operation did not present themselves for review, and it was only those who happened to have been under my personal charge that occasionally made me wonder what the percentage of non-recurrence after operation really was. At one period during the collection of the statistics for this paper I was forcibly reminded of the saying of a French physician about tuberculosis, that *l'étude de la tuberculose est une méditation sur la mort*.

In this connexion I should like to draw the attention of both medical and lay journalists to a comparison of these two diseases from the point of causation and cure. The profession has been subjected to a good deal of obloquy and cheap sarcasm by newspapers because it has not yet discovered either the cause of or a cure for cancer, and even the medical papers continually lament the apparent want of progress in this direction. I well remember the enthusiasm with which Koch's discovery of the tubercle bacilli was hailed, but I should like to ask how many cases of tubercle even now die, although we know the cause. When the cause of cancer is discovered—and some day I believe it will be discovered—we shall yet be a long way from curing most cases of cancer, unless both the profession and the public are much better educated as to the insidious commencement of most cases and our means of diagnosis improve.

In certain cases of cancer a practical cure can be assured by operation. As regards other means of cure I have had a limited personal experience of the treatment of external cancer by the x rays; by oöphorectomy and thyroid gland; the results have not been satisfactory. In rodent ulcer, mammary carcinoma and sarcoma the growths have diminished in size under treatment by x rays, but the patients have died from internal recurrence although life has been rendered more comfortable, and perhaps lengthened by a period of some weeks. I have also seen cases which have been treated by cancer curers by injections, with appalling consequences as I will show later. How any one with the most elementary knowledge of the pathology of cancer could expect to cure a malignant growth by injections round it passes my comprehension. The results of trypsin and similar injections have been equally fruitless. As far as can be seen at the present time successful results can only be hoped for from a properly planned operation and possibly from the use of radium if the favourable results of its action recently published can be obtained.

From the very beginning of my surgical career I was thoroughly imbued with the idea that cancer had a local origin at first, and that its later manifestations were due to infection from the primary focus, just as in syphilis. This idea had a profound influence on my practice, and from the writings of Gross, Mitchell Banks (who never received the recognition due to him for his work on cancer of the breast), and Heidenhain, I have always extended my

Fig. 1.—Skin incision. The dark line round the nipple shows apparent size of new growth.

Fig. 2.—Shows wound after removal of breast and pectoral muscles. Ribs and intercostal muscles shown.

Fig. 3.—Incision sewn up. Shows the two drainage tubes.

Fig. 6.—Shows movement of arm two to three weeks after operation.

Fig. 4.—One position in which the arm is placed after operation.

Fig. 5.—Alternative position of arm after operation. The arm may be placed first in one and then the other position to relieve the patient.

incision in removal of the breast from the insertion of the pectoral to the external oblique in the epigastrium; and dissected up the skin as far as the sternum on the one side to the latissimus dorsi on the other. My idea was that separate lobules of the mammary gland were sometimes found lying outside the gland proper, and that the dissection described would remove such lobules if they were present. At the same time most of the skin over the mamma was removed.

Up to the present time the incision has remained the same, and the operation I perform to-day is the same as I performed fifteen years ago, except that I remove the deep fascia more completely over the epigastric region, and the axilla is more fully exposed by removal of lesser pectoral. Rodman, of Philadelphia, has lately described an operation for removal of the carcinomatous mamma in which he commences the operation by division of the insertion of the pectorals and dissection of the axilla; he claims that this avoids the further entrance of cancer cells or germs into the circulation by way of the axillary lymphatics and glands. Personally I cannot see that this amounts to much, for if the axillary glands are infected, handling of the mammary gland is not likely to cause any appreciable further infection, and the axillary route is by no means the only route by which, in an operation, cancer cells or germs can obtain an entrance into the lymphatic circulation.

Before giving the results of my operations, I will describe the various steps of the operation itself. The incision commences over the tendon of the pectoralis major, and is continued along the anterior fold of the axilla until it reaches the edge of the mamma; the further course of the incision is regulated by the situation of the growth in the breast. It is so planned that the growth forms the centre of a more or less round or oval incision, so that there is (as far as can be ascertained) a margin of healthy skin of a breadth of 2 in. to 3 in. round the growth (Fig. 1). If the skin is adherent, more is taken away, no consideration being paid to the subsequent closing of the wound. The incision is carried down over the epigastrium, as shown in the slides. The skin is then dissected back as far as the clavicle above, the sternum on the inner side (or even further if the growth is near the inner margin of the mamma) beyond the latissimus dorsi on the outer side, and the deep fascia over the upper part of the abdominal rectus and external oblique are bared. As little fat as possible is left on the skin. The handle of the knife is then pushed down between the fibres of the pectoralis major so as to separate the fibres actually attached to the clavicle, and the left forefinger follows it, passing underneath the tendon of the pectoralis major, which is then divided on the finger by knife or scissors; the left hand pushes the muscle downwards with a piece of gauze, and the pectoralis minor is divided near its insertion into the coracoid process. The two pectoral muscles are then separated from the thoracic wall by knife and gauze-covered fingers; the deep fascia is dissected off the external oblique and rectus below. The axilla can be cleared, if desired, before the breast and underlying muscles are separated from the thorax or afterwards. Generally I separate the breast and pectoral muscles from the chest wall downwards nearly to the subscapularis muscle, removing the digitations of the serratus magnus, and then dissect away the tissues from the outer incision above, exposing the axillary vein and clearing the axilla out from above downwards (Fig. 2).

The incision is then sewn up with a continuous button-hole suture of linen thread; a few silkworm gut sutures are inserted perhaps every 4 in. and two drainage tubes are used, one in the usual situation in the axilla, and the other over the rectus muscle at the abdominal extremity of the incision (Fig. 3). As a rule the edges of the incision come together very well without undermining. If they will not meet with undermining, a flap can often be cut from the skin below, and on twisting it the whole of the incision can be closed.

This incision has given a very low percentage of local recurrence, which is the great test of a successful operation; the surgeon has no command over internal recurrence or metastasis, but he has over local recurrence, unless the skin is widely infected at the time of operation.

To minimize shock as far as possible during the opera-

tion, sterilized towels wrung out of hot sterile saline solution are placed over the chest and over the wound; these are renewed before they become cold, and the wound is exposed as little as possible, bleeding vessels being closed immediately with forceps. No antiseptic solutions come in contact with the wound, and rubber gloves are worn as at all my operations. There is a good deal of post-operative shock, but with the above precautions there has only been one death from shock.

After operation the arm is not bandaged to the side, but is slung up (as shown by the photographs, Figs. 4 and 5) at right angles to the body, by a broad bandage passing round the upper arm and the head of the bedstead. I have located my patients in this manner during the last eleven years. The late Mr. Cotterill, of London, suggested the use of a right angled splint some eleven years ago, and it appeared to me that this method of slinging the arm would be quite as efficacious. This has proved to be the case, and at the end of a fortnight the patients are able to do their own hair and can raise the arm (as seen in the photographs, Figs. 6 and 7) straight up above the body. The drainage tubes are removed at the end of thirty-six or forty-eight hours, according to the amount of serous discharge.

I have removed the breast and cleared out the axilla in this manner in 118 cases. Of these cases 97 were carcinomata; 1 duct papilloma; 11 proved to be cases of chronic mastitis, in 3 of which the microscopical report said it was wiser to remove the whole breast, although there were no definite signs of malignancy; 4 proved on microscopical examination to be cystic adenomata, and in 1 of these the report said it would be wiser to remove the whole breast; 4 proved to be abscess, and 1 of these was thought to be tuberculous; 1 (in a very stout woman) was found to be tuberculous mastitis on microscopical examination. Out of these 118 cases 8 died in hospital.

Fig. 7.—Shows upward range of movement of arm two to three weeks after operation.

Deaths.

- 1 of emphysematous gangrene.
- 1 of pneumonia and empyema.
- 1 a month after operation from acute mania.
- 1 was a bleeder, and died twelve hours after operation.
- 1 died of cerebral softening.
- 1 of shock two days after operation.
- 1 from infection of the breast and femoral thrombosis.
- 1, a case with extensive ulceration, died of exhaustion and trichinosis (I shall refer to this later).

Two of these deaths (those due to emphysematous gangrene and femoral thrombosis) were both due to sepsis, and ought to have been avoided. The only death which can be ascribed to the operation itself was the one due to shock. The death-rate for such an extensive operation is therefore low, and should not be more than 2 per cent. The time taken over an operation has, in my opinion, a great influence on the amount of shock produced. The time usually taken in my cases has been from forty-five minutes to an hour; if possible, this should not be exceeded.

The ages of the patients have varied from 24 to 75 years; the latter, in spite of her age, made a good recovery, and is still living.

A microscopical examination of the breast and glands has been made in almost all the cases, and the results are interesting from the point of view of diagnosis, and of treatment other than by operation. The classical signs of cancer of the breast as given in the textbooks are absolutely useless and more than useless—they are dangerous—as, when they are present, the case has already passed the early stage when a good result from operation might be looked for. Enlarged glands in the axilla, retraction of the nipple, adhesion of the skin, all show that the growth is no longer in an early stage. The differential diagnosis

of carcinoma from mastitis, innocent adenoma, and deep-seated cysto-abscess is in some cases impossible, and the pathologist in some of these cases cannot give a definite answer, but suggests that removal would be the wiser course to pursue. The suggestion that during the course of an operation a piece of the tumour should be excised and examined by the microscope is, in my opinion, futile. In the first place, a good pathologist is not always to be obtained except in a hospital with a medical school attached. Secondly, where his assistance is most needed—namely, in those cases which have just crossed the borderland dividing the innocent from the malignant tumours—the piece excised might not show the change. Thirdly, in doubtful cases it is wiser to act as if they were malignant.

Carcinoma of the mamma is often preceded by chronic mastitis or a cysto-adenomatous condition of the breast, and who can say when the change will take place? The operation in competent hands shows a very high percentage of recoveries, and it is far better to remove a breast the seat of mastitis than leave one, or only do a partial operation on one, which is malignant. Again, as regards cysts of the breast, I am not sure whether most medical men would not regard a cyst of the mamma as quite an innocent sort of tumour, but in one case of mine it was due to breaking down of a carcinoma, and cysts are frequently associated with papillary growths which are, or may become, malignant.

Even the microscope may fail the surgeon. Halstead, in one of his papers, says that for the microscopical investigation of supposed cancerous lymphatic glands it requires a pathologist of rare devotion, and those who are inclined to pin their faith to microscopical diagnosis might with advantage read a paper by Bonhôte Henderson,¹ where he describes a series of tumours of the breast which were doubtful before operation; after operation some of these were proved to be innocent, some malignant, and some remained doubtful after microscopical examination. As these occurred at the Brompton Cancer Hospital there can be no doubt that an adequate investigation was made, and this lends additional support to my contention that in any doubtful tumour of the breast in a woman over 40 it is much wiser to regard it as cancer and perform a radical operation. It must also be remembered that the mamma may be the seat of a so-called mastitis, cystic disease, or adenoma, as well as carcinoma.

Unfortunately, I have not kept notes of all the inoperable cases I have seen, but I have records of thirteen which so I regarded. Of these one was a case of inoperable scirrhus with ulceration; she was seen in 1902, when the disease had been in existence at least three and a half years, and at the present time the disease seems very little, if any, more advanced, though both the axillary and supraclavicular glands are enlarged.

Another case was sent to the hospital for carcinoma of the left breast, and locally it was not too far advanced for operation, but on examination there was found to be disease of the other breast and some dullness at the bases of the lungs; she was sent home and died in three weeks. A third one was treated by x rays; the breast sloughed, the tumour disappeared, and the skin healed, but the axillary glands remained enlarged, and although the patient lived over two years, she finally died of internal metastases. It is impossible in any individual case to make an accurate prognosis as to the probability of early or late internal recurrence, and one less frequently makes mistakes as to local recurrence.

Ulceration in many cases may be regarded as a contra-indication to operation, but I have operated on 8 cases in which there was ulceration, and of these 2 are living—one seven years and the other two years and four months after operation, free from recurrence.

To illustrate again the difficulty, if not impossibility, of giving a prognosis as to freedom from recurrence of the disease I may mention the case of a patient who was operated on in September, 1907, is still living with three nodules of local recurrence and internal lung and spine deposits. Two other patients are living, after seven years and two years and five months, without any recurrence.

The final results are not good; the results as to local recurrence are, however, very good, and show that if cases were operated on in the early stage the results would be much better than they are. The cases have not been

picked in any way and only cases that were inoperable from the local extent of the disease or from internal metastases or infection of the supraclavicular glands have been refused operation. The number of patients in whom there is local recurrence is 9 out of 98; of these in 2 the chest wall was affected at the time of operation, 3 were very advanced cases in which early recurrence was predicted, and 1 is still living with three small nodules over two years after operation. In some cases the occurrence of a local return of the disease is very difficult to explain unless we accept the theory that there is a local infection at the time of operation with a lessened resistance to the disease. Even then why should one case be locally infected and not another when the same operation is done by the same operator? Moreover, in cases where ulceration is present local recurrence does not seem to be any more frequent than where there is no local ulceration.

Out of the 98 cases there are only 34 living, of whom 16 have been operated on under two years; 6 of the 98 cannot be traced, and have been counted as dead; 1 lived five years free from recurrence, and was then lost sight of. Of the rest, 20 only lived one year or under; 19 two years or under; 6 lived three years or under; 2 lived four years; 1 lived seven years, and died of cerebral haemorrhage; 1 lived ten and a half years, and died of internal recurrence. Of the causes of death in these 49 cases, 5 died of bronchitis, 1 of heart failure, 1 of pneumonia, 2 of cerebral haemorrhage, 2 of cancer of breast, 18 with internal recurrence, 1 recurrence in neck, 9 in lungs, 7 in spine, 2 in liver, 1 cancer of uterus; from these figures it follows that 40 deaths out of 49 were due to the original disease. If the three years' limit be taken, there are 66 cases with 14 living more than three years after operation. Of these 14, 2 were operated on more than three years ago, 3 more than four years, 4 more than seven years, 2 more than eight years, 2 more than eleven years, and 1 more than twelve years ago. In one of these the supraclavicular glands were removed, and she is living four years after the operation; in one living seven and a quarter years after the operation microscopical examination showed the axillary glands were infected, and also in another who is alive eleven years after operation.

The lesson to be learnt from these statistics is that until the public and the profession are better educated as to the difficulty, and even, in many cases, the impossibility (without a microscopical examination) of diagnosing early cancer from other conditions of the breast the results after operation will be far from satisfactory. The patients seen in private practice give a much better percentage of freedom from recurrence. In my own private cases of 11 patients operated on more than three years ago 7 are living, 4 dead; of these, 2 died from recurrence—these were both bad cases, 1 having been operated on before by another surgeon; 1 died of cerebral haemorrhage seven years after operation, and 1 from heart failure. The contrast between the two sets of figures is more than striking, and shows the great care that must be taken in compiling statistics, for of the 7 who are living 3 were operated on more than eleven years ago.

It also shows what good results could be obtained if the surgeon only got the cases early enough. The best planned operation is useless if there are metastases in the bones or lungs, and it would appear that if further progress is to be made it must be made along the lines of earlier diagnosis and complete removal in doubtful cases. If the supraclavicular glands are enlarged, it is rare to find any lengthened freedom from recurrence, even if they are removed, as probably there are already deposits in the mediastinum or the liver; but the only other addition to the modern operation is the routine removal of these glands, and in some cases it is possible their removal might prevent or postpone a return of the disease.

I am unable to give a thoroughly detailed account of the histology of all the cases, as full notes of the pathology have not been kept; but from the notes in the operating book the cases are divided as follows: 20 were registered as carcinoma only; 54 were scirrhus, among which 2 were colloid, 1 Paget's disease, and 1 which had been treated by a cancer curer with injections of permanganate of potassium or soda, had trichinosis in the growth and axillary glands and other parts of the body; 15 were spheroidal-celled carcinomata, among which was 1 mucoid degeneration; 1 with tubercle in the breast and also in the axillary

glands, and 1 probably originated in the ducts; 4 were duct carcinomata; 1 duct papilloma; 2 acinous carcinoma; 1 adeno-carcinoma, and 1 was returned as sarcoma by one pathologist and carcinoma by another; 1 was colloid.

In conclusion, my thanks are due to the medical men under whose care the patients have been, and to Mr. A. T. Longbotham, the Superintendent Registrar of Births and Deaths, without whose help I should not have been able to trace as many of the cases as I have done.

REFERENCE.

¹ *Lancet*, September 18th, 1909.

THE TREATMENT OF CHRONIC DUODENAL ULCER.

By E. C. HORT, F.R.C.P. EDIN.*

In the last three years it has fallen to my lot to be directly concerned in the treatment of over 70 cases of gastric and duodenal ulcer, mostly in the chronic stage. Of this number, 24 have been diagnosed as duodenal by myself and other physicians. I venture to hope, therefore, that a very brief summary of what experience has taught me personally in these cases may illustrate one or two of the points raised by this debate.

In the past our surgical colleagues have undoubtedly had cause for complaint that often we have not invoked their assistance sufficiently early. To-day, with no uncertain voice, we are told that all chronic cases of duodenal ulcer should be submitted to operation as soon as diagnosed. But, before counselling our patients to seek the aid of the surgeon, we may surely inquire:

1. If the eventual results of operation, especially of the posterior no-loop variety, are as uniformly good as the immediate issues sometimes appear to show.
2. If we yet know sufficient of the essential nature of the disease itself to justify us in making *ex cathedra* statements as to what in these cases is the correct treatment to adopt.
3. If it is really true that medical science can do little to avert the necessity for what must always, even granting a low operative mortality, be a somewhat formidable procedure.

1. With regard to the first of these inquiries, it happens that in the last few months there have been sent to me 5 cases of duodenal ulcer which had been submitted to operation. In each case the surgeon was a man of high reputation in connexion with recent developments in gastric and duodenal surgery, so there can be no question of the degree of surgical skill involved. Very briefly these are the results:

The first patient, operated on twenty months ago, says: "Since the operation I have had no real peace or relief. My general health is perhaps improved, but I have the old pain always."

The second, operated on last winter, says he cannot believe the ulcer is really healed. He never had vomiting until after operation, and then not for many weeks. He now has severe pain at the old site and at the point of junction.

The third, operated on eighteen months ago, is in a lamentable condition, with frequent attacks of severe pain, sometimes melaena, and constant distress from an aggravated form of flatulent dyspepsia.

The fourth had an alarming attack of malaena several months after operation, and intermittent pain in the left subcostal area.

The fifth, operated on nearly two years ago, had recurrence lately of pain and haemorrhage, and, in addition, uncontrollable retching and sometimes vomiting when the hand is laid even gently on the abdominal wall.

I have in addition notes of 3 gastric cases short-circuited for ulcer presenting a similar history.

In these cases, and in the gastric ones, ulceration has apparently persisted or recurred, and in most there is the additional distress caused by the profound disturbance in the mechanics and hydrostatics of stomach and bowel caused by the new connexion. Viewed from any standpoint, the post-operative condition now is a most difficult one to deal with, as I find all treatment practically useless, beyond temporary relief of the associated catarrh

So much so, that I now decline to undertake the treatment of such cases.

It would of course be most unfair to quote a few isolated cases of failure of relief by operation, and from them to belittle the brilliant work of Mr. Moynihan, the Mayo brothers, and their colleagues. But it is most unlikely that mine has been a solitary experience, and the question arises if operation may sometimes perhaps be undertaken unduly early as well as unduly late. However this may be, an impression remains that a considerable time should elapse after operation before one can judge of the likelihood of permanence of results.

2. That we know absolutely nothing of the essential nature of the origin of duodenal ulcer few will deny, however much we may think we know of the causes of the maintenance of their patency. But—and the question is of more than academic interest—the problem whether duodenal ulcer is a disease *sui generis* or a mere symptom only has a most direct bearing on the question of surgical intervention. If it be a disease bearing no particular relation to other observed incidents in the life-history of the alimentary canal, universal excision of the ulcer, or of a cylinder of duodenum bearing the ulcer, as urged by Mr. Moynihan and his followers, would be on a similar footing to removal of a lipoma, and equally commendable. If, on the other hand, ulcer of the duodenum be only a symptom of a general disease, as yet unidentified, bearing very close relations to other ulcerative lesions in the whole alimentary tract, the matter assumes a very different aspect. Let us consider for a moment what these relations are.

Ulcer of the duodenum is very often a multiple lesion; it is frequently associated with a similar lesion on the other side of the pyloric gate; it is, as I have often had occasion to note, often associated with ulcers, sometimes of great depth and extent, in the tongue, gums, or cheeks; it is not by any means unknown in association with ulceration of the oesophagus; it has an inveterate tendency to recur, often through a long series of years; and, finally, there is hardly any toxæmic condition in which it may not occur as a purely symptomatic event. What can all this mean except that solitary or multiple ulcers of the duodenum are a local expression of some dyscrasia, we know not what, though my original suggestion of a floating mucolytic (gastrolytic or duodenolytic) poison is gradually gaining ground?

If only a fraction of this collateral reasoning be sound, how can the mere excision of a symptomatic lesion ensure the cure of the disease? It is as if by amputating a foot with a perforating ulcer one could eliminate the spirochaete, restore the integrity of trophic neurons, and prevent recurrence of ulcer in the opposite limb. So long as the unknown factor originally responsible for the appearance of an ulcer be present, so long will the ulcer tend to recur. If we could be sure that that factor were no longer present surgery could offer much, and the fact that operation is often followed by the happiest results is strong evidence in such cases of the disappearance of that factor. In such event it can only be supposed that, the exciting cause being gone, natural cure is made possible from exclusion from the lymph stream, by cicatrization, of the ulcer's base and sides. In such cases, therefore, there is for surgery a quite legitimate field.

On the other hand, the frequency of recurrence and of co-existence of multiple ulcers in different sections of the canal as well as near the primary focus is strong evidence of persistence or reappearance, in numerous cases, of the unknown factor. In the untreated disease we often see acute phases of suffering interspaced by considerable periods of relative or absolute ease. In quiescent intervals we have evidence that sometimes the lesion never completely heals; and yet it is only at times, often associated with some lowering of general health, as from anxiety and strain, or at certain seasons of the year, such as spring and autumn, that manifestations of activity occur. In all such cases the eroding influence of digestive fluids from the secreting glands in stomach and bowel, and from leucocytes and other tissue cells, is always present so long as the ulcer remains. Hence there must be some other factor to account for the transition from quiescence to activity. Surely, therefore, it is not unreasonable to surmise that renewed activity may be due, *inter alia*, either to a fresh activity of specific poisons or to the fact that they have

* The author was asked to take part in the discussion on the treatment of duodenal ulcer opened by Mr. Moynihan at a meeting of the Surgical Section of the Royal Society of Medicine on December 14th, 1909 (see BRITISH MEDICAL JOURNAL, December 25th, 1909), but was unavoidably prevented from being present. The remarks which Dr. Hort intended to make are embodied in this paper.