on patients suffering from such affections as gastric catarrh and ulceration, dilatation of the stomach, and so forth. Cabot,⁶ for example, states that digestion leucocytosis often does not occur in chronic gastritis and ulcer, but that its absence is still more constant in carcinoma ventriculi.

To this latter point attention had already been directed by Müller,⁶ who published in 1897 a note of five cases of cancer of the stomach, in all of which digestion leucocytosis was wanting. Then Schneyer⁷ some years later gave an account of the application of this fact clinically to the differential diagnosis of gastric ulcer and cancer. The results he obtained were as follows:

In 18 cases of cancer, digestion leucocytosis was absent in all.

In 3 cases of benign stenosis, digestion leucocytosis was present in all.

In 8 cases of gastric ulcer, digestion leucocytosis was present in 7.

Hartung⁸ also made a series of observations on this point. He found in cases of cancer that as a rule some slight increase in the number of white cells did occur after eating, but that it was small, the maximum rise being 1,000 per c.mm. In the cases he investigated he always gave 1 gram of nuclein along with the test meal. He also mentions the value of the general leucocytosis (so commonly present in malignant disease even in the fasting state) as an important point in diagnosis between cancer of the stomach and ulcer; he usually found the polymorphonuclear cells increased, their percentage being 80 or over.

Capps⁹ also made observations on this point, and obtained results agreeing with those of the German investigators, for he found digestion leucocytosis absent in 12 out of 14 cases of carcinoma ventriculi. He concludes that it is absent in most cases, but not invariably so.

The point is one of some interest alike to those who devote special attention to the clinical examination of the blood and to those who pay special attention to the diagnosis of gastric disorders, and, finding that practically no observations on this subject have been published in Great Britain, I have examined a number of cases during the past two years, with a view to testing the validity of the above statements. I am only able to report on 11 cases, but these were all undoubted instances of malignant disease involving the stomach, the diagnosis being confirmed in all, either by the course of the illness and the localisation of a tumour or by the results of surgical procedure or *post-mortem* examination. I desire to gratefully acknowledge the courtesy of Dr. Finlayson, Dr. Renton, and Dr. Beatson in permitting me to examine cases in their wards, in the Western Infirmary and the Cancer Hospital of this city.

The method employed for ascertaining the presence or absence of digestion leucocytosis was as follows: In each case the blood was carefully examined and the leucocyte count determined in the fasting condition, generally about 11.45 A.M., or shortly before the hour of the hospital dinner. At noon a meal was given sufficiently rich in proteids to occasion a distinct leucocytosis in a healthy person. This meal usually consisted of 10 ounces of milk beaten up with 1 or 2 raw eggs, according to the state of the patient's stomach. The leucocyte count was then made again three hours later. The subjoined table gives the results obtained.

It would appear, then, from these results, that digestion leucocytosis is a broken reed on which to lean in the diagnosis of cancer of the stomach, since only a trifle over 54 per cent. of the cases gave a positive reaction in regard to it. It may be retorted that the number of cases is too small to permit of trustworthy deductions being drawn. There is an element of truth in this, but in the latest monograph on this disease, that published last year by Osler and Macrae,¹⁰ we have records of 22 cases in which digestion leucocytosis was studied, with the result that 12, or 54.5 per cent., showed its absence, while in 10, or 45.5 per cent., it was present. They arrive at the same conclusion as myself, namely, that reliance cannot be placed on this method. It seems probable that the true statement of the case, as Marchetti has recently put it, is that digestion leucocytes essentially depends on the digestive and absorptive powers of the stomach, and that it may or may not occur in cancer of the organ, according to the degree of impairment of its functions. He considers that the presence of the phenomenon is of more value in excluding cancer than is its absence in proving the existence of the disease.

Table of Cases.

Case.	Date.	Sex.	Age.	Disease.	Result of Examination.
1	July, 1898	F.	42	Cancer of pylorus	Digestion leucocytosis absent.
2	Aug., 1898	M.	57	Cancer of pylorus	" " present.
3	Oct., 1898	M.	47	Tumour of body of stomach	" " present.
4	Nov., 1898	M.	53	Cancer of cardiac end	" " inconstant
5	Nov., 1898	M.	47	Tumour of omentum involving stomach	" " present.
6	May, 1899	F.	59	Cancer of stomach	" " absent.
7	June, 1899	F.	60	Cancer of pylorus	" " present.
8	Oct., 1899	M.	54	Cancer of stomach	" " absent.
9	Jan., 1900	F.	36	Cancer of body of stomach	" " absent.
10	July, 1900	F.	62	Cancer of pylorus	" " absent.
11	Oct., 1900	M.	61	Cancer of pylorus	" " absent.

Result: Digestion leucocytosis absent in 6 = 54.54 per cent.
 " " present in 4 = 36.36 "
 " " inconstant in 1 = 9.10 "
 11 cases = 100.00 "

Some may say that my statements partake chiefly of the nature of a destructive criticism, and that I have given little of a positive kind to lay hold of. But destructive criticism is just as needful in many cases as corroborative evidence. I suppose most of us will agree as to the hopelessness of a patient's condition with cancer of the stomach, and many of us recognise that surgical interference, if in time, offers the best chance of amelioration or of cure. But to do good the surgeon must be called early, and it is here that the importance of all methods of clinical examination lies with regard to the early establishment of the nature of the case by the physician.

I may quote once more from Osler and Macrae's¹¹ recent work: "When we remember the importance of an early operation, and consider the saying of Czerny and Rindfleisch that the presence of a positively diagnosed tumour is a contraindication to radical operation, the necessity of more attention to the points of early diagnosis is evident." If we rely on a faulty method, as this appears to me to be, we may let genuine cases slip and send on unsuitable ones to the operating table, bringing discredit alike upon ourselves and on those more refined methods of clinical diagnosis which, when once substantiated, are really of value.

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THE PALLIATIVE TREATMENT OF CARCINOMA UTERI.

By GERALD R. LEIGHTON, M.B., C.M. EDIN.,
 Pontnillas.

EVERY now and then it falls to the lot of the general practitioner to have the care of a case of cancer, in which, for one reason or another, surgical interference is contraindicated. Such cases are, perhaps, the most trying and the saddest that one has to deal with, for the reason that we are helpless to stay the progress of the disease, and are compelled to direct

all our efforts to diminution of suffering until such time as the patient succumbs to sheer exhaustion, or dies from some concomitant complaint. It has been my lot to have such a case under almost daily observation for two and a-half years—from the first diagnosis of the disease to its fatal termination—and the course of the case under the treatment adopted seems to me worth recording, and the treatment itself worth remarking upon.

The problem I wish to draw attention to is this: Given a case of cancer where surgical treatment is impossible, what treatment offers the best means of making the patient's life bearable as long as it lasts?

The case I refer to was as follows:

Mrs. M., widow, aged 71, consulted me in May, 1898, for symptoms pointing to commencing cancer of the cervix.

HISTORY.

As usual, a slight hæmorrhage was the first thing complained of. Shortly afterwards an offensive discharge set in. A microscopic examination of a scraping from the cervix revealed the characteristic cell growth. The patient went to London with a view to being operated upon, and was seen by several surgeons in consultation. After careful consideration, however, it was decided that on account of the age (and other reasons) complete removal of the disease by surgical interference was inadvisable, and she returned home to the country. While in London—a period of some two months—pain began to require relief, and when she returned to my care was becoming severe. The pain increased rapidly in severity, and for a time the patient got much worse; then, under the treatment to be described immediately, she improved much in general health, and lived in comparative ease until October, 1900, when she had fainting fits on several successive days, and died from exhaustion.

TREATMENT.

(A) *Hæmorrhage*.—This was always controlled by ergot internally, and on the few occasions when severe flooding occurred, by subcutaneous injection of ergotin.

(B) *Offensive Discharge*.—In the earlier stages a solution of sulphate of zinc was used, but later I found nothing so satisfactory as a carbolic douche (1 to 40).

(C) *Pain*.—This soon became the great object of treatment, and about six months after the first diagnosis—a period which had been looked upon as the probable duration of life in this case—the pain was constant and intense. It had been treated till then with liq. morph. hydroch. alternating with chloral hydrate and morphine suppositories. But these began to lose their effect, and I was continually called up at night to give hypodermic injections of morphine to procure any sleep at all. The relief thus obtained was only evanescent, and it was evident the patient was rapidly being worn out by the intense suffering. I then remembered a remark of an old teacher of mine at a clinic on a similar case, when he wound up his words on treatment thus: "Then, gentlemen, if everything else fails the best thing you can do is to turn your patient into an unconscious opium eater." At this time (December, 1898) she was taking from 3 to 5 grains of opium a day in one form or another. I then decided to push the opium by internal administration alone, and finding that the tincture of opium had a more lasting effect, dose for dose, than the liquor morphinæ, I discarded the latter and used the tincture entirely. I gave 15m every four hours, or about 6 grains of opium in twenty-four hours; increased this to a dose every three hours in a few days, or 8 grs. a day, and found in a fortnight that the patient got entire freedom from pain for six or eight hours at a stretch, during which refreshing sleep was obtained. She soon began to show a marked improvement in her general condition, took food well, and instead of dying as was expected a few weeks before, began to get out in her carriage a little. The dose had to be constantly increased, however, to secure this freedom from pain, and not to make the description too tedious I may say at once that in six months' time (June, 1899) she was taking 3 ozs. of tincture of opium daily, or about 90 grs. of opium. A month later it had increased to 4 ozs., and on my going away for my summer holiday at that time my *locum tenens* flatly refused to make up the prescription until he had seen me do so and administer it. While away from home I met a well-known professor of surgery, and being rather anxious about the treatment I was pursuing, I told him of the case and asked his advice. He asked me if the patient slept well, took her food well, had regular action of the bowels, whether she had any head symptoms, and as to her general health. On my answering all these queries satisfactorily, he turned and said, "Then what on earth more do you want? Give her a pint if necessary." I took his advice, if not quite literally, and continued to increase the dose as tolerance became established, the only difference being that I added mv of tinct. belladonna night and morning, which I found assisted the action of the opium. By August, 1900, she was taking 6 ozs. of tinct. opii daily, or about 198 grs. of opium a day. Three months later this had risen to 8 ozs. or 264 grs. per day, and on some days, when the pain did threaten to become severe, she took over 12 ozs., or nearly 400 grs. She went on with this dose, one day a little more, the next a little less, till October, 1900, when she collapsed somewhat suddenly, and after a week of extreme prostration died.

The patient had thus lived two years longer than was at first thought probable, and I have no hesitation in saying that the last 18 months of life had been incomparably more comfortable than were the first 12 after the disease showed itself. Of course, I do not mean to say she had no pain at all; she had severe pain at times, but it was bearable, and never so distressing as it had been in the early stages. Throughout the whole of the last two years there was no difficulty with the bowels, no head symptoms, and a plain

milk diet was well taken. Towards the end the growth had extended to the bladder, and finally to the rectum also.

Looking back on the case, the firm impression left on my mind is that in no other way could the patient have lived as comfortably as she did or so long. She never knew that she took opium, and would occasionally ask me if the pain came on if I would give her a little morphine. Under similar circumstances I do not know that one can do better than follow the maxim quoted above, and "turn the patient into an unconscious opium eater."

The only drawback I see to this instrument is its great expense for the poorer class; and I may say that the druggist's bill in this case for tinctura opii alone was over £20. This, however, has nothing to do with the purely medical aspect of the treatment I have described.

THE APPLICATION OF ROMANOWSKY'S STAIN IN MALARIA.

[With Coloured Plate.]

By MAJOR W. B. LEISHMAN, M.B., R.A.M.C.,

Assistant Professor of Pathology, Army Medical School.

[From the Pathological Laboratory, Netley.]

THE following note on the application of Romanowsky's method of staining for the demonstration of the presence of malarial parasites in the blood and, in particular, for the differential diagnosis of the tertian parasite, has been written in the hope that the simplified technique described below may render its great advantages more generally available to those who are brought into every-day contact with the disease.

The recent arrival at Netley of several large batches of invalids from India has given me the opportunity of using this stain in many cases of malaria, and the results obtained by it have proved strikingly superior to those in which other and better-known methods were followed, and I am convinced that those who may be led to give it a trial will appreciate the increased facilities it affords for the investigation of many important points in connection with the diagnosis, prognosis and treatment of malarial fevers. The method which has been followed in most of its essentials is that described with great care and detail by Dr. Georg Maurer¹ as the result of his work in Sumatra.

The chief advantages which the staining method described below holds over others are the following:

1. The specific action of the stain upon all red blood corpuscles infected by the tertian parasite.
2. The greater certainty of the detection of very young intracorpuscular forms of all varieties of the malarial parasite.
3. The facility with which the occurrence of a "mixed" infection may be detected, that is, the presence in the system simultaneously of more than one variety of the parasite.
4. After a little practice, the ease of its application and the certainty of the results obtained.

With regard to the first of these points—the specific action upon the red blood corpuscles infected by tertian parasites—the appearance in the infected cell, when stained by this method, of bright red dots or points was first pointed out by Schüffner.² A red cell infected by one of these tertian parasites forms one of the most striking of microscopical objects, as may be judged by a reference to Figs. 5-23 in the plate, which were all drawn from Nature from films prepared in the wards of this hospital and stained by Romanowsky's method.

The origin and nature of these dots are not by any means clear. The fact that their staining reaction is identical with that of the nuclear chromatin of the parasites might point to their being of similar structure; but, more probably, they are the result of trophic changes in the substance of the red cell brought about by the invading parasite, and are not directly connected with the parasite itself. Whatever their nature, they appear as fine more or less rounded granules scattered irregularly throughout the whole of the red cell, and, in the case of the more fully developed parasites, both over and underlying the parasite itself (see Figs. 5-23). They stain a deep ruby red colour, and are so numerous as to recall