instance, it is a common experience to find that an infant 3 or 4 months old is fed ten times a day and only obtains 1 to 1½ oz. of milk at a feeding, or about 12 oz. in the twenty-four hours. Evidence from an independent source confirms these observations, for Dr. Eric Pritchard at his consultations at the Marylebone Dispensary has come to exactly the same conclusions.

A starvation diet is hardly likely to promote active nutrition, so that in cases in which the test feed proves that the child is maintaining its weight on such a small amount of milk, I usually supplement the defective supply with additional feeds from the bottle. The following case, however, will show that this is not always necessary.

M., aged 1 month, weighing 8 lb. 5 oz., obtained 2 oz. from the breast. When he was 3 months old he obtained 4 oz. from the breast, and weighed 12 lb. 5 oz. At 4 months, the infant obtained only 2 oz. from the breast, and weighed 12 lb. 14 oz. At 5 months he obtained 2½ oz., and weighed 15 lb. I felt inclined to give some cow's milk as well, but the child was satisfied, vigorous, and had well developed muscles, so I refrained. At 6 months he obtained 3½ oz. from the breast, and weighed 16 lb. At the seventh month he obtained 3 oz. from the breast, and weighed 16 lb. 11 oz.

This child is still attending the consultations, and at present I see no reason why I should supplement this deficient supply of food, because evidently the quality of the milk must be very high. A wasted child, aged 2 months, and only weighing 6 lb. 10 oz., obtained 3 oz. from the breast; this is rather an excessive amount for this age. In a week's time he had lost 4 oz. in weight, and the test feed showed that he still obtained 3 oz. from the breast. In this case the quality of the milk must have been very deficient.

I frequently have infants brought to me who have been artificially fed from the first week of life, owing to the belief that the breast milk had "dried up" on the fourth or fifth day. I regret to say that some of these cases came from maternity institutions. I am sure that no one, however skilled in maternity work, can possibly tell, apart from the test feed, whether the infant obtains a small quantity from the breast or not. To show how mistakes can be made I will quote the case of an infant born in one of our maternity institutions.

The baby was 2 months old when I saw it, very wasted and was having the bottle. The mother told me her milk had disappeared on the fourth or fifth day, and that the nurse said she must feed the baby on the bottle. The financial problem on leaving the institution worried the mother a good deal, so she put the child to the breast now and then "when the nurse was not looking." The test feed showed that the child obtained 2 oz. from the breast. I told the mother to stop the bottle and feed only by the breast. The child did remarkably well, and there was no further trouble.

Mistakes such as this could easily be avoided if the test feed was employed in all doubtful cases.

During the colostrum period the amount of fluid secreted is always extremely limited. The milk is thought to "dry up" on the third or fourth day, because at about this time the breasts, which have been previously hard, often become soft and smaller owing to the resolution of the gland cells into a colostrum-like secretion.

I have seen many cases of dyspepsia and wasting in infants who have never had a chance of passing through the colostrum period (which usually lasts from ten to fourteen days), but have been fed on milk mixtures from the very commencement.

Colostrum is a bland, unirritating, non-coagulable fluid, and at the appointed time Nature adds the casein very gradually. The whole process may be regarded, in Dr. Pritchard's words, as an "educational" one, in which we can imagine the digestive processes being trained to deal with the clot at a later date. It is easy to see that cow's milk cannot be a suitable food at this early age. I have notes of several cases in which vomiting persisted for two or three months after this initial mistake was made. Infant foods were tried one after the other with a like result, and I only succeeded in arresting the vomiting by giving peptonized milk for some time, and gradually weaning the child on to citrated undiluted milk.

When cow's milk is added to the diet, the knowledge we obtain from the test feed is valuable. Take the case of a child 5 months old, who only gets 2 oz. from the breast—if the regular allowance of 4 or 5 oz. of modified milk were given in such a case gastric disturbance would be a likely consequence. I have seen many cases of mixed feeding,

in which the mother has followed the printed instructions supplied to her with regard to the quantity of milk which should be given at a particular age, and where the result has been disastrous. I have in some of these cases proved by the test feed that the mixtures were in these particular instances far in excess of what the child required; and when I made the quantity of cow's milk to correspond to the amount obtained from the breast a more peaceful state of things resulted.

Up to the present time 360 infants have attended my consultations. During the first two years there were 5 deaths due to gastro-intestinal disease; during the last year no deaths from disease of the alimentary canal have occurred. The number of attendances has been 595 during the last year.

OVARIAN GRAFTING AND MENSTRUATION.

BY

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ONE of us has been making a special study during the last few years of the question of ovarian grafting in the female subject. This grafting consists in implanting under the skin of the abdominal wall one of the ovaries which has been removed during a subtotal hysterectomy. The question of grafting an ovary from one woman into the abdominal wall of another subject, and also of grafting an ovary which has been preserved in ice during a period of from one to twenty-five days, has also been studied.

In all these cases the grafting has succeeded. The ovary has retained its original size for a period varying from one month to three years, and has exhibited at different times certain congestive phenomena which have stimulated those which normally take place just prior to menstruation. The results of these inquiries have been published in a thesis by Scheurer.¹

The question of interest at the present moment is to know what actually takes place in a woman in whom both ovaries and tubes have been removed while the uterus has been left in place, and in whose abdominal wall one of the ovaries, apparently in a healthy state, has been implanted. Such a patient, after recovery from the operation, possesses a uterus in a normal position and an ovary in an abnormal position. Is it possible for such a utero-ovarian combination to present the congestive phenomena associated with menstruation?

We have already published two cases with details upon the subject without being able to formulate any definite conclusions. In the case published below normal menstruation occurred, accompanied by enlargement of an ovary which had been implanted four and a half months previously:

E. G., aged 29, was admitted to the Hertford British Hospital on October 28th, 1909, with a history of abdominal pain on the left side of one month's duration. She was unable to assign any cause for the onset of the pain, and there was no history of miscarriage. There was slight leucorrhoea, but no examination was made to demonstrate the gonococcus. The patient had had one child, who was aged 4 years. Menstruation, which had commenced when she was 13 years of age, had been regular. Examination showed that she was suffering from double salpingitis, the disease being most marked on the left side. Treatment by rest in bed, daily douches, and enemata was undertaken and continued up to December 10th, 1909. On that day both tubes and ovaries were removed, and the right ovary, which appeared to be normal, was grafted into the abdominal wall on the right side. For some days following the operation there was complaint of pain on the right side of the wound, and the grafted ovary was tender upon palpation. These symptoms, however, quickly subsided, and she was dismissed well on February 10th, 1910.

On May 4th the patient came back with the complaint that the abdominal pain had returned, and then she gave the history of the following events, which had occurred since her discharge on February 10th. There had been slight yellowish-white vaginal discharge, which at times had had an offensive odour, and had necessitated her taking vaginal douches. Six weeks prior to her return—that is, on February 20th, or ten weeks after the operation—she had acute pain on the right side of the abdomen. This pain had come on suddenly and continued six

She noticed that there was a swelling on the right side of the abdominal wound; there was no vaginal discharge of blood. The pain gradually passed off, and her state of health again became satisfactory. On April 26th, 1910—that is to say, four and a half months after the operation—she observed a vaginal discharge of blood, accompanied by acute pain in the abdomen, and she noticed that the swelling on the right side of the abdominal wound had again appeared. She suffered also from a dull pain across the sacrum, from headache, and from lassitude. This discharge, which was thus associated with the usual signs and symptoms of a normal menstrual period, continued up to April 30th, 1910, and subsequently gradually diminished.

When the patient was readmitted on May 4th, 1910, there was slight leucorrhoea, but no discharge of blood. The os was unduly soft. The ovary in the right abdominal wall was tender and much swollen, measuring approximately 5 in. in diameter. There was no redness nor oedema of the overlying tissues. The temperature was 99° F., the pulse was 76, and the respirations averaged 24. The heart and lungs appeared to be normal, and the urine, which had a specific gravity of 1005, was acid in reaction and free from albumen and sugar.

On the following days the ovary steadily diminished in size, and by May 10th it was only just palpable, only very slightly tender, and the patient was practically free from all pain.

If we are to venture, in view of the facts of this case, to put forward any hypothesis regarding the nature of menstruation, we would say that the female organism is, in the first place, during a month under the influence of a toxin the origin of which is at present unknown but whose action is elective for the ovary; that the toxin produces an intense congestion of the ovary and perhaps a special metabolism; and that then an internal secretion derived from the ovary is formed which has a special action on the uterus, and which determines the discharge of blood. In the above case, for a period of four and a half months there were present certain signs such as the swelling of the ovary, flushings, and general lassitude, which were strong evidence of the presence in the organism of a poison having an elective action for the vasomotor system. After this period the grafted ovary had become permeated by new vessels originating from the parent body, newly-formed blood vessels had penetrated into the central portions of the ovary, and the menstruation, which up to that time had been in abeyance, was influenced by the grafted ovary, and occurred again in the usual manner.

REFERENCE.

¹ *Les greffes ovariennes*, Thèse de Paris, 1910.

A CASE OF SLEEPING SICKNESS SHOWING REGULAR PERIODICAL INCREASE OF THE PARASITES DISCLOSED.*

BY

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[Prefatory Note by R. Ross.—For a long time it has appeared to me that much light might be thrown on infectious diseases, immunity, and treatment, by more exact enumeration of the infecting organisms, and that we might even be able ultimately to apply mathematical reasoning to the study of these subjects. In 1903¹ I elaborated a method of blood examination, called the *thick-film process*, which enables us to detect small organisms in the blood about twenty times more easily than in ordinary preparations; but for the lack of the necessary assistance I was long unable to apply the method to the laborious enumeration of such organisms. Recently, however, the Advisory Committee for the Tropical Diseases' Research Fund has placed considerable funds at the disposal of the Liverpool School of Tropical Medicine for the study of cases in the tropical ward of the Royal Southern Hospital, Liverpool, with the result that the investigations referred to were commenced by Dr. David Thomson and myself from the beginning of this year. As I expected, methodical counting of the parasites has at once verified or disclosed several facts of importance in connexion with malaria and trypanosomiasis.]

In this communication we limit ourselves to a brief

description of the remarkable periodical increase of *Trypanosoma gambiense* revealed by careful daily counting in a case in the Tropical Ward, Royal Southern Hospital; it is still under treatment, and the eventual outcome will be reported later.

W. A., male, aged 26 years, a strong young man born in Northumberland, was infected in North-East Rhodesia in September, 1909, the trypanosomes being found in his blood in Africa on November 17th. He was admitted into the Southern Hospital, Liverpool, on December 4th. From then until February 16th (seventy-three days) the number of trypanosomes in his blood was estimated only by the rough methods in common use—that is, by the proportion of trypanosomes to red cells or leucocytes, or to "fields" of the microscope examined. These methods are obviously open to such great error that they can scarcely be depended upon to indicate any but very large differences in the numbers of objects counted. During the seventy-three days forty-six counts were made; but on several occasions none was attempted for three or four days in succession, so that, even if the methods of counting employed had been more accurate, sudden fluctuations might easily have been missed. Hence, as was to be expected, the graph during this period is very irregular and almost worthless. On admission on December 4th the patient was reported to contain about 6,000 trypanosomes per cubic millimetre of blood, and large numbers, amounting to about 3,000 per cubic millimetre, were found on December 17th and 28th and on January 16th. All this time the patient was given the usual treatment with atoxyl and mercury, and received altogether ten doses of 2 to 4 grains of the former. Nevertheless, the parasites never fell below about 200 per cubic millimetre, as roughly estimated.

It was then found, however, that atoxyl was injuring the patient's sight (as sometimes happens), and other treatment was substituted. At the same time we elaborated a much more correct method of counting all the parasites in measured quantities (one quarter to 1 c.mm.) of blood taken in thick film; and from February 16th onward the trypanosomes were estimated daily by this method by one of us (D. T.). The attached chart gives the remarkable graph obtained up to the present (April 30th).

The numbers of trypanosomes found were scrupulously recorded, and the smoothness and regularity of the graph suggest that there was no very great error of observation. The blood was taken every day at about 10 a.m., but on April 5th and 6th several counts were made daily.

It will be seen that between February 16th and April 30th (seventy-three days) there were eleven rises in the number of the parasites. Up to April 7th there were seven rises, at intervals of seven or eight days. During this period the patient was given no atoxyl, but was treated with large doses of quinine (30 to 40 grains) daily, with frequent doses of methylene blue, and with trypan red on March 17th, 18th, 19th, and 20th.

On April 5th it was decided to administer atoxyl again, as shown on the chart, together with mercury and other treatment. The temperatures were taken by the Sister of the ward, and it will be seen that there has always been a tendency to a slight rise in temperature concurrent with the rise in the number of parasites—the two curves thus confirming each other. Only the maximum and minimum temperatures are entered in the chart.

The great regularity of the rises can scarcely be compatible with a mere chance distribution. It will also be observed that the rises were of two kinds—namely, high rises and low rises; and that the two kinds alternated with regular periodicity until April 18th, at which point the cycle appears to have become distorted—probably in consequence of the treatment. The regularity of alternation of the high and low rises is so well marked as to recall the picture of a double tertian malaria, and to suggest that two independent sets of parasites may exist in the patient, just as often happens in malaria.

The large dose of atoxyl given on April 5th seems to have had no effect whatever on the following rise; but the succeeding rises were apparently modified for some reason. The value of the enumerative method for therapeutical research is obvious.

Of course many other facts in connexion with the case have been recorded, and parallel work is being done on subinoculated animals and on the parasites. It is, therefore, inadvisable to attempt at present any discussion of the many interesting theoretical questions which arise.

We are much indebted to the Director of the Sleeping Sickness Bureau (Dr. Bagshawe) for having given us references to the literature on the subject of such fluctuations. In the original case of Dutton and Ford it was noted that the parasites varied in numbers, and that a parallel rise in the patient's temperature occurred. Manson and Daniels² chart the number of parasites compared with 500 leucocytes; but the error of this method is very large, and their graph is quite irregular. They abandoned

* Read before the Royal Society, June 16th, 1910.