coloured as it is in most cases of malignant disease. The removal of any obstruction of the common duct that may exist is generally followed very shortly by the disappearance of bile from the urine, but in a few cases where the condition is of long standing, bile pigments may be found, although in much diminished quantity, several weeks, or even months, after the operation.

In the course of our observations over 300 specimens of urine have been investigated, but of these about 100 were only employed as controls. The remainder may be divided into an earlier series, on which the preliminary investigations were carried out, and a succeeding series of 170 specimens derived from 111 cases of suspected pancreatic disease where the diagnostic value of the methods described were put to the test.

As I have already sketched the lines along which the investigation proceeded it will not be necessary to refer further to the first series, excepting to state that all the cases were under Mr. Mayo Robson's care and had been found at operation to be suffering from some disease of the pancreas.

In the second series specimens of urine were sent to the laboratory for examination without any indication as to the nature of the report expected, and in many of the earlier ones the condition of the pancreas was ascertained while the examination was being made. By thus separating the clinical and experimental investigations any unconscious bias on the part of the surgeon or the pathologist was avoided, and a true estimate of the reliability of the tests formed.

SCIENTIFIC GRANTS COMMITTEE

OF THE

British Medical Association.

REPORT LXXXII.

ON THE ADAPTATION OF THE PANCREAS.*

By F. A. BAINBRIDGE, M.A., M.R.C.P.,

Research Scholar to the British Medical Association.
(From the Physiological Laboratory, University College, London).

I.—INTRODUCTION.

THE brilliant investigations of Pawlow and his pupils have greatly extended the range of physiological knowledge of the normal mechanism of gastric and pancreatic secretion. One of their most interesting discoveries has been the observation that the pancreas, like most other organs of the body, possesses the power of adaptation to varying conditions, and that the composition of pancreatic juice is not constant, but can be modified by different diets, particularly with respect to the amounts of the enzymes which it contains. Pawlow's work has been dominated throughout by the conception that pancreatic secretion is controlled entirely by a nervous mechanism, whose efferent fibres run in the vagus, and whose afferent fibres have their origin in the duodenal mucous membrane. This view naturally led him to the supposition that the adaptation of the pancreas was also dependent on a nervous mechanism; and Pawlow holds that the intestinal mucosa contains nerve-endings which have a specific excitability, such that they react only to the stimulus of definite substances and call forth a secretion of the pancreas in a specific fashion. Fat, for example, stimulates certain nerve endings in the intestinal mucous membrane, thereby evoking the secretion of steapsin in large amount by the pancreas.

However, the recent work of Bayliss and Starling on secretin has shown that pancreatic secretion does not depend upon a nervous reflex action, but is normally produced by a chemical substance—secretin—acting as a specific stimulus to the cells of the pancreas. The discovery of secretin has not merely rendered it possible to study the work of the pancreas without recourse to the production of a permanent fistula; it has proved that Pawlow's interpretation of his own results can no longer be accepted. Moreover, the fact that the normal mechanism of pancreatic secretion is chemical naturally raises the question as to the nature of the adaptation of the pancreas to food, and throws doubt on the nervous theory maintained by Walther; the results of

* This paper formed part of a thesis offered for the M.D. Cantab.; the expenses of the research were partly defrayed by a grant from the Royal Society.

an investigation of this subject are embodied in the present thesis.

The majority of the observations on the adaptation of the pancreas have been made on the enzymes normally present in pancreatic juice, and considerable difficulties arise in the estimation of the amount of enzyme formed in response to different diets, since the amount of enzyme secreted varies inversely with the rate of secretion of juice, and the rate of secretion is different for each food stuff. For this and other reasons it was found more satisfactory to follow Weinland and work with lactase, which converts lactose into galactose and dextrose. This enzyme is absent from the pancreatic juice of adult dogs, though it is present in extracts of the pancreas of most young animals; consequently, the mere appearance—even in small amount—of lactase in pancreatic juice as the result of a diet containing lactose furnishes conclusive evidence of adaptation on the part of the pancreas. Moreover, lactose has the advantage of being a normal article of food in early life, and can be readily added to or excluded from the diet of animals.

HISTORICAL.

The investigations of Weinland not only furnish an important contribution to our knowledge of lactase, but supply convincing evidence that adaptation of the pancreas does occur. Weinland used chloroform-water extracts of the pancreas (of dogs), added a known quantity of lactose, and also toluol, and kept the mixture at 37° for twenty-four hours. The solution was then acidified and boiled, and the proteids removed by means of potassium-mercuric iodide; the clear filtrate was examined polarimetrically.

He found that, with one or two exceptions, the pancreas of dogs fed on a diet free from milk contained no lactase. On the other hand, extracts of the pancreas of dogs fed for several days on milk, sometimes with the addition of lactose, invariably contained lactase in considerable amount; in some experiments he obtained conversion of 50 per cent. of the lactose added to the pancreatic extract. In one experiment an extract of the pancreas of three sucking puppies three weeks old contained lactase.

In a second paper Weinland attempted to ascertain how a milk diet caused the formation of lactase by the pancreas. He found that the pancreas of dogs fed on meat, with the addition of milk-sugar, contained lactase, whereas lactose injected subcutaneously for several days in succession was ineffective. He also fed a dog for several days on meat, with the addition of galactose, but found no lactase in the pancreatic extracts. From these experiments Weinland concluded that the formation of lactase was not dependent on any direct chemical action exerted by lactose or its products on the pancreas; he therefore accepted the view that the adaptation of the pancreas to milk-sugar was brought about by a nervous mechanism.

METHODS.

The discovery of secretin has made it possible to obtain pure pancreatic juice in large quantities; and the majority of my observations have been made with pancreatic juice. Occasionally chloroform-water extracts of the pancreas were used. The animals used were almost invariably dogs; a few experiments were made on cats. They were fed either on milk or on biscuits. The dogs fed on milk received about two pints of milk daily for two or three weeks as a rule, sometimes for as long as two months. In some experiments 1 oz. of pure lactose was added to the milk each day for a week. The animals were kept without food during the twenty-four hours preceding an experiment, in order that the pancreas might be in a "resting" condition.

At the beginning of each experiment one-half to 1 gr. of morphine was given subcutaneously, and the animal was anaesthetised with A.C.E. mixture. The abdomen was opened, a small metal cannula was placed in the main pancreatic duct, and the abdomen was then closed by ligatures or clips. A cannula was also placed in the jugular vein for the purpose of injecting secretin. As a rule, the blood pressure was not taken. A certain amount of pancreatic juice was collected, and then the animal was killed while still anaesthetised.

When an extract of the intestine was made the gut was opened and washed with water; the mucous membrane was scraped off, ground up with sand in a mortar, and extracted with chloroform water and filtered through muslin. If the extract was to be used for injection into animals no further filtration was made; but if the presence of enzymes was being investigated the extract was

also filtered through filter paper. Extracts of the pancreas were made in a similar manner.

Great care was taken throughout the course of the research to prevent any dogs from having milk, unless they were on a milk diet. The biscuit-fed and milk-fed dogs were kept in separate compartments, and it was impossible for the biscuit dogs to obtain any milk.

The occurrence and degree of conversion of lactose into galactose and dextrose by the enzyme lactase was estimated volumetrically with Pavy's solution in all the experiments. This method has been shown by Croft Hill to be extremely accurate, provided that the experimental conditions are kept uniform, and I have found that it yields very constant and reliable results. In a considerable number of experiments this method was checked and the results controlled by the preparation of osazones and by the fermentation method.

RESULTS.

I.—The Reaction of the Pancreas to a Milk Diet.

In the first place a number of experiments were made on adult dogs fed on biscuits. The pancreatic juice from such dogs never contained lactase.

TABLE I.—Pancreatic Juice of Biscuit-fed Dogs. (The figures represent the number of c.cm. of the solution, which reduced 50 c.cm. Pavy's solution.)

Experiment.	Pure Lactose Solution.	Control. Lactose + Boiled Juice.	Lactose + Pancr. Juice Incubated at 37°.
VI.	6.40 c.cm.	—	6.50 c.cm.
VII.	7.50 c.cm.	—	7.50 c.cm.
VIII.	8.35 c.cm.	—	8.35 c.cm.

In the next place, adult dogs were fed solely on milk for periods varying from two weeks to two months in different experiments. When the milk diet was only given for a short time (say a fortnight) lactase was sometimes added to the milk, an ounce of lactase being added daily for a week. The pancreatic juice was then collected in the usual way and examined for lactase, which was found to be present in every case.

TABLE II.—Action of the Pancreatic Juice of Milk-fed Dogs on Lactose. (The figures represent the number of c.cm. of lactose solution which reduced 50 c.cm. of Pavy's solution.)

Experiment.	Duration of Feeding on Milk.	Lactose Solution.	Control Lactose + Boiled Juice.	Lactose + Pancr. Juice.	Degree of Conversion.
I.	1 month	5.5 c.cm.	—	5.0 c.cm.	Per Cent.
II.	2 weeks milk + 8 oz. lactose	7.3 c.cm.	7.2 c.cm.	6.6 c.cm.	20.0
III.	1 month	9.4 c.cm.	—	8.8 c.cm.	18.9
IV.	2 weeks	8.6 c.cm.	—	7.8 c.cm.	14.6
					21.0

In the experiments given in Table II, Pavy's method was controlled by the preparation of osazones and by the fermentation method. Both these methods yielded positive results in each case.

In a certain number of experiments the pancreatic extracts of young suckling puppies and kittens were examined and were found to contain lactase; and there is every reason to suppose that lactase is a normal constituent of the pancreatic juice of young animals, and gradually disappears when they cease to live on a milk diet.

These experiments show clearly that lactase is absent from the pancreatic juice of adult dogs, unless they are fed on a milk diet, when it invariably makes its appearance in the juice. They furnish satisfactory evidence that the pancreas does adapt itself to differences of diet by modifying the composition of its secretion as regards the enzymes; and probably the formation of lactase in response to a milk diet may be taken as typical of the reaction of the pancreas to all forms of food.

II.—The Specific Nature of the Reaction.

I have made a number of experiments with extracts of the mucous membrane of the small intestine of dogs, usually comparing it simultaneously with pancreatic extracts or juice from the same animal. Some experiments were also made with the alimentary mucous membrane of puppies and kittens and one calf. In all cases lactase was present even in adult

dogs fed on biscuits. One or two observations were made on the mucous membrane of milk-fed dogs, but the amount of lactase was not noticeably larger than in biscuit dogs.

The question whether lactase passed from the duodenum into the blood stream, and was simply picked out and secreted by the pancreas had therefore to be considered. The fact that no lactase occurs in the pancreatic juice of biscuit-fed dogs, although it is abundant in the intestinal mucous membrane is strongly opposed to such a view. But it seemed possible that on a milk diet the amount of lactase in the mucous membrane might be increased to such an extent that some ferment entered the blood stream and appeared in the pancreatic juice. However, this does not occur as is shown by two sets of experiments.

In the first place, extracts of the intestinal mucous membrane of biscuit-fed dogs were injected subcutaneously into other biscuit-fed dogs on one or more days in succession. The juice of the latter was then collected and examined for lactase, but invariably with a negative result. Such extracts always contained lactase, and in all probability the enzyme entered the circulation; yet the pancreas did not pick it out of the blood and secrete it in the juice.

In a second set of experiments, pancreatic juice from milk-fed dogs, and therefore containing lactase, was injected subcutaneously into a biscuit dog, and on the following day the juice of the latter was collected and examined; in such experiments, the pancreatic juice of the injected dog contained no lactase.

From the results of these experiments it may be concluded that the lactase of the pancreas is not derived from the lactase of the intestinal mucous membrane, but is formed by the pancreas itself in response to a milk diet. On a priori grounds one would naturally expect that lactase was the particular constituent of milk, which actually evoked the production of lactase by the pancreas. This has been shown to be the case by Weinland, who fed a dog for some days on biscuits and lactose only, and found that the pancreas contained lactase. Weinland suggested the possibility that the production of lactase might be due not directly to the milk sugar, but to the products of the conversion of milk sugar. It is obvious that dextrose has no influence on the formation of lactase, and Weinland showed by experiment that galactose given by the mouth was quite ineffective. I have confirmed his observation as regards galactose.

There is therefore no doubt that lactase itself is the particular constituent of milk which leads to the production of lactase by the pancreas; but a further point of great importance must be discussed. Weinland raised the question as to whether lactase was absorbed as such from the intestinal canal into the circulation, and exerted a direct influence on the pancreas, or whether it only acted indirectly. In order to decide this point he injected lactase subcutaneously into a dog for several days, keeping the animal during that period on a diet free from milk; he found that the extract of the pancreas contained no lactase. I have confirmed this observation, which demonstrates very clearly that lactase, when introduced into the circulation, has no direct action on the pancreas, but is only effective in producing lactase when given by the mouth. Such a result is entirely in harmony with the facts that lactase introduced into the circulation is rapidly eliminated by the kidneys, and that it is not normally present in appreciable quantity in the blood.

Consequently, the formation of lactase by the pancreas is a specific reaction to a diet containing lactose, a reaction which takes place only when lactose is given by the mouth.

III.—The Influence of Secretin.

The important part played by the intestinal mucous membrane in the adaptation of the pancreas to food was recognized by Vassiliew and Walther, and was still more strongly emphasized by Weinland, to whose experiments reference has just been made. All these observers, however, considered that the mechanism of adaptation was purely nervous, and, apart from one or two experiments by Weinland, the possibility of a chemical mechanism was not even raised.

My first observations as to the mechanism of adaptation were made on the assumption that secretin was not a perfectly definite chemical substance, but that secretins made from animals kept on different diets might differ to some extent as regards the character of the pancreatic juice to which they gave rise. It seemed possible that variations in diet modified the kind of secretin produced, and thereby altered both the nature of the chemical stimulus reaching the pancreas and the characters of the juice secreted.

Secretin made in the ordinary way from the duodenal mucous membrane of milk-fed dogs was injected into biscuit dogs, and the pancreatic juice of the latter was collected and examined for lactase. The results, however, were entirely negative. Negative results were also obtained when the mucous membrane was incubated with solutions of lactose, or partially converted lactose, for some hours before making the secretin. Secretin from a milk dog was injected subcutaneously into a biscuit dog on two successive days, and the pancreatic juice of the latter collected; it also contained no lactase.

Finally, it was found that when secretin from biscuit-fed dogs was injected into milk-fed dogs, the juice of the latter contained lactase as usual. There is, therefore, absolutely no evidence that secretin takes any part in the adaptation of the pancreas to food. On the contrary, the facts show that the action of secretin is always the same: it stimulates the pancreas to secrete a juice containing all the enzymes present in the gland at that moment. The amount and nature of the ferments, which are present in and secreted by the pancreas at any moment depend not on the action of secretin, but on the previous diet of the animal and are quite independent of secretin. This view is fully in accordance with the conclusions arrived at by Bayliss and Starling as to the chemical nature of secretin.

IV.—The Mechanism by which Adaptation is Effected.

An attempt was next made to determine whether unboiled extracts of the intestinal mucous membrane had any influence on the production of lactase. Extracts of the small intestine were made from milk-fed dogs and from biscuit dogs. In both cases some lactose was thoroughly mixed with the extract, the mixture allowed to stand for twenty-four to thirty-six hours in the cold, and then filtered through muslin. Such an extract was injected intravenously into a biscuit dog, and after waiting for an hour or two ordinary secretin was injected and pancreatic juice collected. These experiments also yielded negative results.

It then occurred to me that even if the formation of lactase were due to a chemical stimulus arising in the intestinal mucous membrane, the methods of experiment just described did not allow sufficient time for the pancreas to manufacture an enzyme which was not normally present in the juice of adult dogs. Consequently I made chloroform-water extracts of the entire mucous membrane of the small intestine of several kittens fed only on milk, and injected such extracts subcutaneously for two days in succession into a biscuit dog. The pancreatic juice of the latter was then collected and examined for lactase. In subsequent experiments the intestine of milk-fed dogs was used for making the extracts; with two exceptions, the pancreatic juice of the injected dog invariably contained lactase. As a rule, two injections only were given on two successive days, the dog being anaesthetized and the juice collected on the third day. The extracts were made with as little water as possible, and the bulk of the extract could be injected in each experiment. In spite of the inevitable presence of micro-organisms in such extracts, the chloroform seemed to inhibit their growth, and the dogs showed no signs of infection or illness except in two cases in which injections were made on five days in succession. In those two cases the dogs were unwell, although the local inflammatory reaction was not severe; and these were the only experiments in which the results were negative.

In several experiments Pavy's method was controlled by the fermentation and osazone methods, which gave confirmatory results.

TABLE III.—Pancreatic Juice of Biscuit-fed Dogs Injected with Extracts of the Intestinal Mucous Membrane of Milk-fed Dogs. (The figures represent the number of c.cm. of lactose solution which reduced 50 c.cm. Pavy's solution.)

Experiment.	Solution of Lactose.	Lactose + Pancreatic Juice Boiled.	Lactose + Pancreatic Juice.	Degree of Conversion.
I.	7.40 c.cm.	—	6.80 c.cm.	Per Cent.
II.	8.20 c.cm.	8.2 c.cm.	7.60 c.cm.	18.1
VII.	4.10 c.cm.	—	3.75 c.cm.	16.5
VIII.	9.25 c.cm.	—	8.20 c.cm.	20.8
IX.	4.60 c.cm.	—	4.60 c.cm.	25.9
X.	4.30 c.cm.	—	4.30 c.cm.	0

The last two experiments were those in which the dogs re-

ceived four and five injections respectively, and were not in a normal condition.

Although the degree of inversion produced in these experiments is not large, yet in some of them it is as large as that obtained with the juice of milk-fed dogs. Moreover, the constancy of the positive results gives them an additional reliability, particularly as this was the only means (apart from a milk diet) by which the appearance of lactase in pancreatic juice could be produced; the other methods used yielded uniformly negative results. In addition, the difficulty of obtaining sterile extracts of the mucous membrane limits the number of injections which can be made, and consequently, it is impossible to obtain a marked cumulative effect on the pancreas. It is very probable that the most satisfactory mode of treating the mucous membrane has yet to be found, but beyond the fact that the action of the extract is destroyed by boiling and by standing for some days, nothing has been ascertained in regard to it. Moreover, the actual amount of ferment formed is of little importance, since the appearance of enzyme even in small amount is quite sufficient evidence that adaptation has taken place.

The experiments described in an earlier section seem to me to negative the possibility that the lactase secreted by the pancreas is in any way derived from the lactase present in the extracts used for injection. It must therefore be concluded that extracts of the intestinal mucous membrane of milk-fed dogs contain some substance which stimulates the pancreas to produce lactase. This substance, which is only formed in the mucous membrane as the result of a diet containing lactose, is normally carried in the circulation to the pancreas, upon which it exerts a specific action, giving rise to the formation of lactase by the latter. The nature of this substance is as yet absolutely unknown. The process presents an obvious analogy to the action of secretin, but it is more complex in that it evokes the production of a new ferment, whereas secretin merely causes the secretion of pre-existing ferments.

The production of lactase by the pancreas as a result of the subcutaneous injection of intestinal extracts made from milk-fed dogs conclusively shows that adaptation is not invariably dependent on a nervous reflex originating in the duodenal mucosa, since the adaptation may be produced in a dog to which no lactose at all has been given.

Moreover, if lactose is injected into the duodenum of a dog during life and left there for some hours, no lactase is present in the pancreatic juice subsequently collected from that dog. I made several such experiments, all of which yielded negative results.

On the ground of these experiments, I hold that the adaptation of the pancreas to a diet containing lactose is brought about by a chemical mechanism; by the action of lactose on the intestinal mucous membrane some substance is produced which acts as a specific chemical stimulus to the pancreas, causing it to form lactase; the reaction is produced slowly, and reaches its maximum gradually.

The existence of a chemical mechanism for the adaptation of the pancreas is merely a natural corollary to the chemical mechanism of normal pancreatic secretion; since it was the discovery of secretin which provided the means of carrying out these experiments, and indicated the lines along which investigations should be made. But the mechanism, though in each case chemical, is not of exactly the same nature; the stimulus of secretin calls forth an immediate response on the part of the pancreas, whereas the specific stimulus of the food stuff has a slower, but more prolonged effect.

Hence, secretin evokes the secretion by the pancreas of all the different enzymes present in the pancreas at the time; and the actual composition of the juice as regards its ferments for any given meal depends mainly on the previous diet of the animal, and little if at all on the nature of that particular meal, except in so far as the nature of the food determines the amount of hydrochloric acid secreted by the stomach.

The influence of diet in modifying the nature and amount of the enzymes secreted by the pancreas has some bearing on the time of appearance of the ferments after birth. Apparently lactase is not present in the pancreas at birth, although it makes its appearance certainly within ten days, and in all probability earlier. I have made one experiment on the pancreas of a newborn child which had received no milk, and also on three puppies taken away from the mother and killed almost immediately after birth; in both cases extracts of the pancreas contained no lactase, although it was present in the intestinal mucous membrane of the puppies. Here, again, the secretion of lactase by the pancreas occurs in response to

the milk diet, and the presumption is that the adaptation takes place in the same way as in adult dogs. If this proves to be the case, it is possible that the same process may occur as regards the amylolytic ferment, and that the absence of this enzyme in early life is due to the lack of an adequate chemical stimulus for its secretion.

BIBLIOGRAPHY.

Bayliss and Starling, *Journal of Physiology*, 1902, vol. xxviii, p. 325; 1903, vol. xxx, p. 61. Croft Hill, *Journ. Chem. Soc.*, 1898, p. 634. Jablousky, *Arch. des Soc. Biologiques (St. Petersburg)*, 1896. Vassiliw, *ibid.*, 1893. Walther, *ibid.*, 1890. Pawlow, *The Work of the Digestive Glands* (Thompson, 1902). Weinland, *Zeitschrift f. Biologie*, 1899, vol. xxxviii; 1900, vol. xl.

MEMORANDA:

MEDICAL, SURGICAL, OBSTETRICAL, THERAPEUTICAL, PATHOLOGICAL, ETC.

A CASE OF ADIPOSIS DOLOROSA (DERCUM'S DISEASE) IN A MALE.

THIS case, which I exhibited before the Huddersfield Medical Society on January 5th, affords many points of interest. The patient, aged 75 years, is a gentleman of independent means. The body back and front, the arms to the wrists, and the legs to the knees only, are one mass of typical lipomata. The disease began at the age of 40 years. Before each growth appeared he had an aching sensation at the spot which lasted until a shotty papule made its appearance. Some of the tumours are as big as a child's head. In fact there are representations of all stages of development. He has no pain on manipulation of the growths nor on either active or passive movement of the underlying muscles. There is not a single growth on the hands, feet, or face; this is, I believe, characteristic of this disease. There have never been any hæmorrhages in connexion with the tumours. His father had the disease, and died a hale and hearty old man; one of his living sisters has the disease. Two of his sisters and one brother have been subject to fits ever since they were 10 years old. Here there seems to be a marked hereditary tendency, a point which I have not seen previously raised with regard to this disease. This gentleman is otherwise perfectly healthy. There is no sign of asthenia.

At a meeting of the Clinical Society of London, held last October (BRITISH MEDICAL JOURNAL, October 31st, 1903) Dr. Leonard Williams exhibited a case in a male, and made note of the fact that there was only one other recorded case in a male. If this is correct then the case now reported makes the third. In Professor Osler's *Medicine* he states that the disease is characterized by irregular symmetrical deposits of fatty masses in various portions of the body, preceded by or attended with pain, and that it is an affection of women, occurring at the middle period of life. In my patient the aching only preceded the appearance of each growth. Professor Osler also says that in a case of Burr's and in one of Dercum's the thyroid gland showed atrophic changes. There is evidence of no such changes in my case.

The patient has a very large scrotal hernia, and for which I was consulted in the first instance, but owing to the asymmetrical condition of the buttocks and the friction caused by the growths I cannot get a truss to act.

Slaithwaite, Huddersfield.

MARTIN J. CHEVERS.

THE TENDON REFLEXES IN URAEMIA.

DR. MITCHELL STEVENS'S note on the condition of the deep reflexes in some cases of acute uraemia emphasizes a point which is well worthy of clinical recognition. The remarkable exaggeration of reflexes which he has noted is not limited to acute cases, but may also occasionally occur in cases of chronic uraemia also. The differential diagnosis is then peculiarly difficult.

My attention has been directed to these alterations in the reflexes since 1892, and my connexion with St. Thomas's Hospital has afforded me many opportunities of observing the phenomenon. In the *St. Thomas's Hospital Reports* for 1896 I published a short paper dealing with the subject, and my later experience has only served to corroborate the conclusions there formulated.

I cannot, however, attach much weight to the value of the symptoms in the differential diagnosis of such cases as Dr. Stevens mentions. Increase of the reflexes in the earlier stages of certain cases of cerebral hæmorrhage, and also in other convulsive affections, is not very rare. In the earlier

stages of cerebral embolism it is remarkably common. In all these conditions it may be bilateral.

The note also suggests another point—that of the use of morphine in uraemic states. I am inclined to think that the dangers of the use of morphine in convulsive and maniacal uraemia are exaggerated. I should say that, judiciously used, it is of almost equal value with venesection. I am certain, however, that its danger in comatose uraemia is very real.

London, W.

CHARLES R. BOX, M.D.

ADRENALIN IN PULMONARY HAEMOPTYSIS.

A REPORT of the successful administration of the above preparation in hæmoptysis by Dr. Arthur C. Bird, of Sidmouth, in September last, which appeared in the BRITISH MEDICAL JOURNAL of January 23rd, reminds me of a like success which followed immediately the administration of two suprarenal tabloids (Burroughs, Wellcome, and Co.), in a case under my observation in March, 1902.

In the early part of that month a young man, whose father had some time previously succumbed to phthisis, suddenly suffered from a somewhat alarming attack of hæmoptysis. The usual methods were adopted with a view to control the bleeding, such as absolute rest, the application of ice over the apex, the internal treatment by gallic acid, ergot, hamamelis, turpentine, and small doses of morphine to allay the cough, but all with not much success, as fresh bleeding seemed to recur, when it occurred to me to try the suprarenal extract. I gave four of the tabloids, directing that one was to be crushed, placed on the tongue, and washed down by means of a little water, a second to be taken likewise in four hours. After the second tabloid the bleeding entirely ceased. In a few weeks the patient had sufficiently recovered to resume his out-of-door employment, and on only one occasion since has there been any return of the hæmoptysis, for which he immediately took, as before, a couple of the tabloids, which he asked me to let him have in his possession.

He always now keeps a couple of the tabloids in case of emergency, being convinced of their great efficiency. I have also found the tabloids most effective in a persistent case of epistaxis which baffled usual treatment.

Liverpool.

O. BOWEN, M.R.C.S., etc.

RECURRENT HERPES ZOSTER.

As I have seen it stated lately that herpes zoster recurs in the same patient more frequently than is generally supposed, it may be worth while to place on record my personal experience. I have had three attacks in seven years—November, 1896, November, 1898, and June, 1903, all in South China.

The first attack was on the right side, affecting the cutaneous lateral branch of the last dorsal nerve. There were three erythematous patches, with vesicles on them later, two just below the crest of the ileum, which appeared first and were also the largest, the third on the abdomen just above the groin. The inguinal glands were swollen and painful. There was the usual smarting pain, and the scars of the vesicles were visible for many months afterwards.

The second attack, in November, 1898, showed itself by an erythematous patch about an inch above the left elbow-joint on the ulnar side; there were ten or twelve papules, of which two first, and afterwards all, became vesicular. In the axilla was a smaller patch. Next day a large patch was observed on the back, midway between the spine and axilla, and just under the spine of the scapula—three patches in all. The pain was slight. The lesion was connected with the second dorsal nerve. I had been taking small doses of arsenic and nux vomica for a week. On the appearance of the herpes arsenic was discontinued. The third attack in May, 1903, was of the terminal branches of the first and second divisions of the trigeminal nerve on the right side, and occasioned some alarm on its appearance. On May 29th I felt a little burning pain at the outer canthus of the right eye; next day it was more painful, and the eyelid was swollen, while red streaks appeared on the forehead above the eyebrow, below the eye, in front of the tragus, and near the nose. On the third and fourth days the eye was nearly closed with moderate swelling; the surrounding skin was hot, swollen, and extremely painful if touched. A few vesicles then appearing, made the diagnosis certain. Fortunately the eyeball itself was not affected, there being only slight conjunctivitis. The vesicles were all small and aborted quickly, but in November the scars left were visible, and for several days after the scabs had dried up the right eyelid still felt stiff.