

diffuse, and more wide-reaching in its effects, than is apparent on laryngoscopic examination.

The value to be attached to microscopical examination of pieces of the neoplasm is somewhat equivocal. If it present distinctly affirmative characters, the question may be regarded as definitely settled; if negative, and more particularly if it show simple inflammatory or granulation tissue, the diagnosis still remains doubtful. I have already shown¹ that pieces of a growth, which is really of a highly malignant character, may present a most innocent appearance under the microscope, especially in the earlier stages of the disease. How is this to be accounted for? Mr Lennox Browne, whose utterances always merit the highest respect, is inclined to believe that the irritation caused by the frequent introduction of laryngeal instruments favours malignant degeneration in a previously benign neoplasm. It is interesting, however, to note that in a case such as the present, which has been freely irritated, off and on, for about three years, the growth still apparently remains benign, whilst in an example of malignant disease which was lately under my care, the early microscopical appearances were of a benign character (inflammatory tissue), although not a single instrument had been introduced into the larynx. Fragments of the neoplasm sufficient for microscopical examination were in this case spontaneously detached and expectorated. This fallacy appears to me most likely to occur when there is much concomitant laryngeal inflammation.

Another point in this history deserving attention is the exceptional inefficacy of cocaine, in strong solution, as a local anæsthetic.

III.—EXPERIMENTAL DIETETICS IN LUNACY PRACTICE.

A RECORD OF INVESTIGATION AND RESULTS.

By A. CAMPBELL CLARK, M.D. Edin., Medical Superintendent, Glasgow District Asylum, Bothwell.

(Continued from page 403.)

I. DIGESTION.

(a.) *Salivary*.—Least important though salivary digestion is, it is in man of some significance as contributory to the sum total of digestion, and as the period of its operation is brief, the measure of it depends largely on the rapidity of its performance. A study of the present investigation seems to show that it may probably, in mental cases at least, be taken as an indication of the rapidity and efficacy of digestion in the individual as a whole, and in adynamic states such as we have just considered, its action is materially retarded and defective. Nor is quantity and consistence always a

¹ *A Practical Treatise on the Sputum*, 1886, pp. 50-52.

criterion of quality, for it was limped and superabundant in Cases II. and V., with very different digestive results. Inference may be drawn respecting secretory reflexes from a study of the cutaneous and muscular reflexes; and, though the inference is not absolutely perfect, the general statement is certainly true, that where the cutaneous and muscular reflexes were impaired the rate of salivary digestion was correspondingly slow.

(b.) *Gastric*.—The evidence of the tongue, gastric washings, percussion, and palpation abundantly testifies that in the stomach digestion was slow and very considerably retarded. Liquid absorption, which in health is rapid, was in the more sluggish cases of the first selection (Cases I. to IV.) very much delayed, and in this way the digestive mass must have been very much diluted. The circulation in these cases was at low ebb, the absorbing surface presented a small quantity of blood, and, owing to muscular inactivity, the outside pressure was at a minimum. The degree of organic impurity, as indicated by Condy injections, was well marked at the outset of the investigation, although no food had been taken for twelve hours. At the end of the second experiment (four and a half hours after custard diet) it was still more marked, and throughout, when four or four and a half hours were allowed to lapse after food, gastric oxidation was in excess. It is a notable fact, however, that lavage in the early morning found the stomach after the first experiment remarkably pure. The result of washings suggests the following conclusions—(1), That apart from the effects of each digestive prescription organic impurities were at first in excess, and were materially reduced by repeated lavings; (2), That the excessive amount of Condy decolourized at mid-day, compared with that decolourized in the early morning, confirms the preceding statement, and is itself chiefly accounted for by retained and delayed products of digestion; (3), That Condy disinfection did not promote appetite, but appeared to invigorate digestion. It is possible that it was an antidote to egg poisoning, if there was any risk of such a mishap, as possibly there might be where the functions were so inert. The residuum in the stomach at the end of four or four and a half hours' digestion had decreased considerably towards the end of the experiments, showing a more rapid digestion, a result of improved dietary and more speedy absorption. The Condy injections probably paved the way for these. Even at the last, it must be admitted, gastric digestion was slow, and the stomach was too long of being emptied.

(c.) *Biliary, Pancreatic, and Intestinal*.—The excess of fat in the stools is the best evidence we have of the inefficacy of biliary and pancreatic digestion. When starchy food was moderately abundant, excess of it was seen in the stools, thus demonstrating the inefficacy of pancreatic and intestinal digestion. It may correctly be assumed that the great excess of fatty diet made unreasonable demands on the digestive capacities of such patients as were selected

for experiment, and that, when there was so little vital activity and so much risk of hepatic obstruction from absorbed products of imperfect digestion, all the secretions, especially the biliary, must necessarily be scanty.

II. EXERCISE.

(1.) *In Relation to Appetite.*—No immediate connexion could be traced; but the dietetic programme was not of a kind to give exercise as an appetizing agent free play, and it might have been decidedly nauseating but for the exercise.

(2.) *In Relation to Digestion.*—The bilious tendency of milk and egg diet was probably counteracted by exercise, and the circulation of the digestive organs was periodically roused by exercise. It is nevertheless true that in anergic states the circulation only makes a spurt during exercise, and quickly thereafter resumes its sluggish pace.

(3.) *In Relation to Sleep.*—There was here unmistakable proof of its genial influence, and this is *a priori* what might be looked for.

(4.) *In Relation to Weight.*—This is most difficult to determine. I have carefully studied and compared the tables in pursuit of some conclusion, and am disposed to believe that the exercise was excessive in some of the experiments, and may in a measure account for loss of weight, not so much by excessive tissue metamorphosis, as by inducing a degree of fatigue inimical to active and thorough digestion. The question is still further complicated by the fact that we have no precise data of circulatory response to exercise in mental disease. The reaction of the circulation, its amount and duration, requires for itself alone an extensive investigation of sphygmographic tracings in different mental conditions, and under the stimulus of the same and different amounts of exercise or work. It is also manifest that we have so many overlapping factors to take into account in tracing cause and effect, that more dogmatic statements are here inadmissible.

III. FOOD.

(1.) *Its Effects on Digestion.*—If appetite is any criterion of the effect of the prescribed foods on digestion, we are driven to the conclusion that digestion was in no way influenced by any change of diet. Happily, appetite is not an accurate indicator of digestion, especially in the treatment of mental disease. Nor can we learn enough from our meagre study of salivary digestion, and the use of medicated injections obscures the issue in respect of gastric digestion. Frequent examination of the stomach contents revealed a gradually decreasing weight of them as the experiments proceed; but there was no corresponding decrease of organic impurities. That digestion, at first slow, became gradually more rapid, is clearly established. Whenever light custard diet had barely commenced, the stomach contents were heavier at the end of $4\frac{1}{2}$ hours than a fort-

night later when twelve eggs had been taken daily for six days; and during the fifteenth experiment (with heavy egg diet), after a lapse of four hours the decrease in weight was still more marked, the weights for these three periods being on the average 50, 25, and 5 grammes. It has also to be remembered that albuminates (which are a special charge on gastric digestion) were conversely increased; but in this fact we may find a clue to what otherwise seems paradoxical, that with a gradual increase of egg diet the gastric residuum of each digestion at a given hour became gradually less. The increase of egg albumen alone will not account for it, as the egg fat was correspondingly increased; but albumen derived from other sources, notably milk, was increased in the later experiments, and albuminates led off in considerable excess. The stimulus of beef-tea, brandy, and rum may also have contributed to the increased speed and to the thoroughness of gastric digestion. Arrange these facts in order of merit as we may, we are confronted by other factors outside the question of diet, and these add to the complexity of the question. We have to judge how far this manifest increase in the rate and volume of gastric digestion may be due to exercise, Condy injections, dietary, or accessories. Whether the relative and absolute increase of albuminates is in itself a prime factor, I cannot say; but putting that aside as an open question, I am disposed to attach special value to Condy injections, accessories (notably stimulants and stimulating diet), and lastly, to exercise in those cases which exhibited any reaction at all to it.

Biliary digestion was promoted by the cautious exhibition of calomel, and zymine told effectually as an adjunct to pancreatic digestion. Egg diet explained largely the organic impurities which decolourized the Condy; for in the morning washings, after a long digestive pause, the Condy returned comparatively pure. The effect on biliary secretion was undoubtedly serious.

A study of the table of excreta in relation to diet establishes the fact that liquid diet with a less injection of water than is contained in mixed and more solid diet is attended with a greater flow of urine; but with 50 per cent. of egg diet added, the water of the urine considerably decreases. Liquid diet therefore favours and increases diuresis, and relative excess of egg diet diminishes diuresis. Beef-tea diets were attended with the largest flow of urine, and large milk ingesta came next in the scale of diuretics.

(2.) *Its Effect on Weight.*—The net weight result of all the experiments is summed up as follows:—Case I. *lost* 7 lbs., Case II. *gained* 9½ lbs., Case III. *gained* 1½ lbs., Case IV. *lost* 3¾ lbs., Case V. *gained* 1¾ lbs., and Case VI. *gained* 1¾ lbs.

The following facts are established by these experiments:—(1.) That all the patients lost weight at first, or remained stationary. A mental effect at first produced by the experiments may account for this. (2.) That eggs did not effect an increase of

weight in proportion to their quantity. (3.) That when milk was not correspondingly increased a loss of weight ensued. (4.) That a disproportionate combination of milk and eggs is more or less rectified by the addition of a cholagogue such as calomel; but the exhibition must be carefully minimized, especially if acid nit. mur. dil. and tr. nucis vomica are added to intensify its action. The success of the first exhibition of calomel, despite the comparatively large doses, can be accounted for on the assumption that hepatic obstruction was being induced by the relative excess of eggs previously ingested. (5.) That beef-tea, acting as a stimulant, diuretic, and mild laxative, is a valuable addition to milk and egg diet, but has little intrinsic value as a weight producer, except when infused in cold or tepid water. (6.) That rum or brandy, but especially rum, is a suitable stimulant of milk and egg digestion, and averts the evil effects of a disproportion of milk and eggs. In this way they aid materially in weight production. (7.) That zymine contrasted favourably with calomel as a digestive aid, but that the calomel had been exhibited in excess. (8.) That the beef refuse of beef-tea served up with rice in addition to beef-tea was not followed by gain in weight. The duration of these last experiments was brief, and they require to be repeated and the fact confirmed.

The weight curve is therefore influenced by many factors, and the effect on it of milk and egg diet depends on relative proportions as well as absolute quantities. A maximum custard diet is baneful if concentrated, and is fattening only when milk reduces the consistence to meet the capacity of fatty digestion. The happiest combination effected in my experiments was an egg ratio of 20 per cent., or 1 in 5—that is, three hen's eggs to one pint (20 oz.) of milk. Beef-tea was made according to various prescriptions—some being recommended because of their flavour, others on account of their richness: the former, where flavour and taste were recommended, were thin and poor samples, the latter thick and substantial. In proportion to thickness and substantiality was there a taste of blood, the result of extraction in cold or tepid water previous to heating. The percentage of extract in three samples of different degrees of extraction was 14, 17, and 25 per cent. respectively.

IV. EXCRETA.

A. *The Stools*—(1.) *Their Frequency*.—Defæcation was infrequent in all the cases excepting Case II. The dejections in Case I. numbered 16 in 49 days, in Case II. they numbered 27 in 69 days, in Case III. 22 in 69 days, in Case IV. 15 in 69 days, in Case V. 5 in 20 days, and in Case VI. 7 in 20 days. A laxative was administered during the first experiment in Case I. and Case III.; and an enema once during second, third, and fifteenth experiment to Case IV. in the first instance only, without effect. It is difficult to say what influence the respective diets had on the frequency of

the stools, as some patients were irregular in the action of the bowels. The most natural case throughout in this respect was Case II., and the average of the three most regular of the other five corresponds very nearly to the number of dejections recorded in her case under each change of diet. Mixed diet without tea gave the greatest frequency—7 stools in 11 days; and next in order came custard diet with beef-tea, 3 in 5 days; mixed diet with tea, 4 in 8 days; custards with excess of milk and beef-tea, 4 in 8 days; custard diet with calomel, 2 in 4 days; the same with acid nit. mur. dil. and tr. nucis vom.; custard diet with minimum of milk, 3 in 8 days; and lowest of all, custard diet with rum or brandy, 1 in 3 days.

(2.) *Their Quantity.*—Table III. gives the answer to this question at a glance, the highest percentage being induced by the combined action of calomel, acid, and nux. vomica. Pure custard diet with eggs in excess gives the next percentage in the scale, excepting in Experiment I., where a liberal discount must be allowed, owing to the use of laxatives in two cases. With these exceptions, the percentage will be regarded as low throughout; and while in Case II. the average is uniformly higher, it is lower always than in the other cases as regards the urine solids.

(3.) *Their Colour* was a well-marked feature under changes of diet, and a yellow fatty appearance was most noticeable during the most of the experiments. Such additions as beef-tea, zymine, etc., did not affect the fatty appearance much where weight was lost.

(4.) *Their Composition* has already been fully described. They were proved to be fatty in large amount under plethoric custard diet, and the absence of starch, which at first was evident in small amount, is fully accounted for by the poverty of starchy food. The other elements noted in their composition are of no abnormal significance, nor are they of interest to the present inquiry.

B. *The Urine.*—(a.) *Its Quantity.*—The subnormal urinary outflow in Case II. has been already mentioned, and the balance is not restored by the intestinal dejections, but lachrymal and salivary excess. The urinary outflow in the other cases, notably the hysterical cases, is above normal in many instances. (1st and 2nd.) The nocturnal outflow is larger and of less specific gravity in most of the records than the diurnal. As regards quantity—not specific gravity—the nocturnal and diurnal outflows are the opposite of what we find in health; for, as Thudicum observes, “Nothing could better demonstrate the influence of activity and rest upon the quantity of urine than the differences between the secretion during day and night.” This observation is so far borne out by the records in this investigation; for Cases II. and III. were mentally and physically more awake in the daytime than the others; and we find that in the first four and the last six experiments Case III. evinced greater renal activity in the daytime, and Case II. in

Experiments V. to X. Accepting all this *pro tanto*, we cannot mistake the fact that, with quiet and repose at night, and a minimum of conscious activity by day, the night urine collection of ten hours exceeded the day collection of fourteen hours. The total solids by night is found to exceed the solids by day in three patients having more or less of the hysterical element. In the remaining cases, especially in the last twelve experiments, the solids by day were in excess of the solids by night. (3rd.) The quantity is increased by relative excess of milk in custard diet, and to an even larger extent by beef-tea. Relative excess of eggs diminishes diuresis. Mixed diet with tea diminishes diuresis more than without tea. The very small diuresis during Experiments VI. and VII. may be accounted for by the repetition of calomel with and without acid and nux vomica; but this combination was not attended with a like result in Experiment XIII., where, however, beef-tea was a counteracting force. The lowest percentage of urinary solids appears under a mixed diet with tea. A mixed diet without tea, and mixed diet with custard, or light custard diet comes next. The solids percentage increases with heavier custards, and calomel with acid and nux vomica still further increases the percentage, which is still more influenced in this direction by the addition of beef-tea.

(b.) *Its Quality.*—(1.) *Phosphoric Acid.*—The striking relation of ingesta to egesta of phosphoric acid—while leaving room for other sources of production—is a well-established physiological fact. This fact is confirmed by the charts in these experiments, but their course is erratic enough to imply other underlying causes. A universal and accentuated rise is registered in Experiment X. during the administration of brandy. The exhibition of calomel seems to have had the same effect in a less degree, and the same applies, with two exceptions, to the use of zymine. The excretion during the first five experiments is very low; in few after instances is it large, and only in Case III., the most rational subject of all, does it approach a normal uniformity. The night urines of Cases I. and IV. were frequently more highly charged with phosphoric acid than the day urines, and the water of the urine was also in these cases increased. They were, it will be remembered, hysterical cases.

(2.) *The excretion of chlorides* was also named on the charts, and the effect of custard diet is unmistakable. The first experiment was attended with a maximum excretion, which is not equalled or approached through the succeeding nine, ten, or eleven experiments. The introduction of the custard element is the signal for a steady, continuous fall, which, with occasional slight fluctuations, is maintained through the course of several experiments. Beef-tea appears to increase the excretion, as might be expected, and variations of the amount of mineral ingesta is roughly reflected by the course pursued on the charts. With pure custard diet the ingestion of

chloride of sodium was very slight. This, I believe, can best be corrected by the addition of the deficiency to beef-tea.

(3.) *The elimination of urea* is known to be largely dependent on the amount of nitrogenous ingesta, and according to Dr Noel Paton (*British Medical Journal*, July 31st, 1886) it bears a direct relationship to the secretion of bile. The charts and tables of the experiments give no conclusions of any certain character on the subject. Considering the nature of the cases experimented on, the excretion of urea in the later experiments is startling until it is viewed in the light which Table II. sheds upon it. There we find that albuminates had increased in the diet by steady gradations to 209 grammes, while the equilibrium standard is 100 grammes. By tracing out the weight curve and urea curve we come on some interesting results, which, though the subject of weight-production is already disposed of, may be more appropriately introduced here. This increase of weight accompanies the rise of the urea curve twenty times, the fall twelve times, and twice only was the tracing stationary. The minima daily outputs of urea with a rise of weight are given in the order of the cases as follows:—26, 18, 24, 23, 32 grammes, and the maxima, 36, 29, 46, 44, 43, 42 grammes. The weight curve is subject to fewer fluctuations in proportion as a higher urea altitude is reached. With a daily out-put of less than 20 grammes the weight rise occurs once for seven records. With twenty records of 20 to 25 grammes a rise of weight is registered ten times; with eighteen records of 25 to 30 grammes, three times; with fourteen records of 30 to 35 grammes, ten times; with six records of 35 to 40 grammes, five times; and 5 records of 40 to 46 grammes, five times. During the experiments which were expected to induce an increased flow of bile, the readings of urea fell, except where mercurialization supervened; but these readings are not sufficient to point an exception to the results of Dr Paton's experiments.

In conclusion, let me occupy the limited space which remains to me in further allusion to fatty accumulation. The human economy is supplied with fat, probably from three alimentary sources, viz., albuminates, hydro-carbons, and carbo-hydrates. The nitrogenous source is believed to be morally certain though not chemically demonstrated. This much at least is certain, that hydro-carbons result from the breaking up of nitrogenous matter, and are available for force production. The great source of fat is, however, fatty food and carbo-hydrates. The fact that may be derived from any of the three sources mentioned favours the conclusion that fat is the most indispensable essential for human life. Given alternative sources, the supply can only fail by absolute starvation; but while each of these sources may be recognised as having a special value in emergencies, it is well to consider that no one is in itself physiological, but that each supplies its quota. To overcharge any alimentary principle for the sake of fatty production, is to exact

from one digestive and assimilative process more than it is capable of, and to allow another to fall into abeyance. When fat is largely ingested out of proportion to the other alimentary principles, the power of digestion is unduly taxed, and a vicious circle is produced through the portal circulation. The blood becomes vitiated, fatty digestion is still further minimized, and emaciation results.

Experience and experiments furnish abundant evidence of the fact, that fatty accumulation is at its maximum when the three sources are drawn from. Thus, for example, as quoted by Pavy, there is no fatty accumulation on a diet of potatoes and rice, for the process requires the co-operation of nitrogenous and saline matter. Experiments on ducks with rice are also quoted by Pavy, to show that no gain was registered, and that on the addition of butter a remarkable gain was soon apparent. Lastly, Boussingault fed a duck on butter only, and at the end of three weeks it died of starvation, butter exuding from all parts of the body. These and many other experiments, together with a study of dietetic habits generally, go to prove that while in cold climates carbo-hydrates may be largely excluded without injury to health, in the temperate zone a mixed diet is the best, and the exclusion of any alimentary principle is there inimical to perfect digestion and complete assimilation.

The statement of results and deductions is condensed as much as possible, for the investigation has opened up many more questions, and been pursued over a wide field. The data presented are still open to many interpretations which I have not recorded, and the outlets of further investigation are well-nigh inexhaustible. I am reluctantly compelled to forego the pleasure of a further consideration of the subject for the present; but it will give me satisfaction to see other investigations of the kind pursued elsewhere, and few are of so great importance to medicine generally, and especially to psychological medicine.¹

(The bulk of this paper was read in the Psychological Section of the British Medical Association at Brighton in August 1886.)

IV.—A CONTRIBUTION TO THE ETIOLOGY OF NEURALGIA.

By JAMES R. WHITWELL, M.B. Edin.

UNDER the term Neuralgia, it has long been customary to class a certain set of diseases which have been defined by Prof. Erb as "disorders of the sensory nervous apparatus, the chief and most important symptom of which is pain." Now there are many conditions classed under this heading which one must consider. There is the pure form of neuralgia, with no apparent and definite

¹ *Erratum.*—On page 397, line 19, for "a custard diet of 12 eggs and 2 quarts milk," read "a custard diet of 12 eggs and 2 pints milk."