

form and arsenic are indicated. The anemia is best met by the use of arsenic; iron and strychnia are valuable adjuvants. Other tonics, especially the mineral acids combined with nuxvomica and pepsin, or the malt preparations with tonics, are also valuable. In adolescent female cases iron and manganese make an excellent combination.

As convalescence progresses, some active exercise may be encouraged, such as walking, riding and light gymnastics. When convalescence is established, and not until then, travel may be suggested. Traveling with a convalescent post-febrile case is not safe, and suicides occur among such patients.

PRACTICAL PHYSIOLOGY OF THE DIGESTIVE ORGANS.*

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In this paper it is the intention to allude to some well-known physiologic principles and to describe certain details not so well recognized, all with reference to their application in digestive practice. It is obvious that such a consideration, to be even fairly brief, must be disjointed and incomplete.

1. *The Mouth and Salivary Glands.*—The importance of mastication and insalivation is an old story. While admitting it, I am skeptical as to the chemical value of salivary digestion. While, by very elaborate mastication of dry cereals, J. H. Kellogg has been able to accomplish the conversion of considerable quantities of starch into sugar, as ordinarily carried out, even by careful eaters, only the small proportion of soluble starch of the very light test-meal is digested. Some animals are quite lacking in ptyalin, and I believe the principal function of the mouth and salivary glands to be mechanical. It may be of interest to know that ptyalin will convert small quantities of cane-sugar into a sugar that reduces an alkaline solution of CuSO_4 . Several investigators have conducted elaborate experiments to ascertain just how much HCl is required to inhibit carbohydrate digestion by ptyalin. In general, these investigations may be summed up as follows: Ptyalin digestion normally continues during the first hour of gastric digestion, or, in fact, till HCl secretion reaches its normal maximum.

To what extent is it necessary to administer artificial diastase in failure of starch digestion in the stomach? In view of the fact that, normally, only small quantities of starch are digested before the intestine is reached, it would seem rational to conclude that the mere failure of ptyalin digestion is not an indication for diastase. If the starch simply does not digest, as in hyperchlorhydria, let it alone till the pancreatic ferment strikes it. If it ferments, treat the bacteria and fungous activity in the stomach, preferably by administering HCl. If there is a well-marked indication for superalimentation, then there exists the only possible indication for artificial diastase, unless the amylopsin is lacking, and this condition I have diagnosed only once, in a case of Addison's disease.

Regarding sugars, there is a physiologic puzzle that awaits a solution. Starch is digested only into maltose either by ptyalin or amylopsin. Maltose is a double hexose— $\text{C}_{12}\text{H}_{22}\text{O}_{11}$ —like cane-sugar, and lactose, both of which are foods of considerable quantitative import-

ance. All of them are converted into dextrose and levulose by the inverting ferment of the intestinal glands. Certainly some of these double hexoses must be absorbed before the intestine is reached. Do they exist as such in the portal blood? Are they changed in passing the gastric epithelium? Is there a hepatic or hemic ferment to convert them into glycogen? Do they pass through the liver into the general circulation?

The activity of the salivary glands in excreting such substances as the iodids has been adapted to the test of gastric absorption. After administering a capsule of KI, the drug is normally detected in the saliva within fifteen minutes. A delayed appearance has been considered proof of delayed gastric absorption. In one of my patients, in whom it appeared only after an hour, I found that the capsule would not dissolve in water, but required digestion, so that the iodid test was really one of proteolysis. In some very serious cases, I have found that the iodine went the round of the circulation and was eliminated in the saliva with normal promptness. On the whole, the test is so artificial and depends on so many unknown factors that it has little practical value.

2. *The Esophagus.*—Some years ago, while listening for deglutition murmurs at the cardia, it occurred to me that the time of the passage of water through the esophagus, between the elevation of the hyoid and the sound at the cardia, might afford an indication of the rate of peristalsis in general. With a little experience, the average time will be learned—it is about 8 seconds—and a marked delay is almost invariably associated with gastric atony, dilatation, ptosis or intestinal sluggishness. However, I have recently examined a case in which the esophageal peristalsis was normal, but in which there exists marked constipation.

The esophageal mucus resembles that of the cervix uteri, in being tenacious and stringy. It is a most excellent lubricant, and it is entirely unnecessary to lubricate the stomach-tube, though it is well to moisten it. The appearance of esophageal mucus in considerable quantity sometimes indicates that the tube has kinked, and explains the failure of lavage. In a recent case, the gastrodiaephane, having a rigid tip two inches long, turned in a roomy esophagus, from which 250 c.c. of slime was regurgitated during the attempt to pass the instrument through the cardia. The normal esophagus will just about admit a No. 11 tube, although some will allow the passage of food, or at least a breaking of the vacuum beside the tube. Patients with roomy esophagi have usually been in the habit of bolting their food half masticated. A practical question in this connection is as to how large an object a baby can swallow. He has been seen playing with some article which has disappeared; has it gone into the stomach? Even infants may take an adult-size stomach-tube, and I am inclined to think that they may swallow smooth and approximately spherical objects of a diameter up to one-half inch. If a child or adult swallows a quantity of solid objects—for example, bits of glass, corn or vegetables having indigestible seed-coats, the pits of fruits, etc.—when may we expect the appearance of the foreign body at the anus? From some slight experience and much study, we may lay down the rule that, there being no diarrhea, the first of the foreign matter will appear in twenty-four hours, the last at the next day's passage.

The esophageal murmurs are commonly described as first and second, only one being present. A clearer and simpler way of stating the thing is that the sound made by liquids passing through the cardia varies from a sharp squirting to a low gurgle, according to the force

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of peristalsis and the degree of contraction of the cardia. The sound is of slight diagnostic value.

3. *The Stomach.*—Probably every one who has given much attention to this organ will agree that if we could know but one fact regarding it, we should choose to be informed as to its acidity. There is no longer the least doubt that the normal secretion is of hydrochloric acid, along with pepsin and rennin. A long list of series of experiments with various meals allows the general statement that, after an ordinary meal, there is a period of from one to two hours, according to the size and nature of the meal, before the capacity of the proteids is reached by the acid secretion. From this time on there should be a small excess of HCl, and, provided the excess is not sufficient to irritate the stomach, the exact proportion is not a matter of importance. While HCl is undoubtedly an antiseptic, it is not very markedly so in a proportion so small as not to irritate the mucosa, and the fermentation in achlorhydria is rather due to lack of digestion of proteids and to the usual concomitant failure of peristalsis than to direct lack of antiseptic. After a light meal, however, such as the test-breakfast, there should be found, after an hour's preliminary digestion, more than a trace of HCl, not because more is needed for that particular meal, but because the stimulus of food should call forth something like the normal quantity of HCl secretion. Just what this excess should be is a matter of controversy. Gastric juice contains about 20:10000 of HCl, and this may be taken as the maximum of free hydrochloric acidity which should be found in the chyme. Is it correct, following what might be termed the Boardman-Reed school, to call hyperchlorhydria every case in which the chyme after the test-meal contains more than about 10:10000 of free HCl? If so, we shall find hyperchlorhydria a comparatively common neurosis, and we shall find an excess of HCl in connection with quite a considerable organic acidity, in numerous instances. The opposite view, to which the writer subscribes, is that a case should not be called hyperchlorhydria unless there is at least the amount of free HCl found in normal undiluted gastric juice, or unless there is an excessive dilution of the food with gastric juice of considerable hydrochloric acidity and unless the HCl is clearly the cause of symptoms of irritation. According to this view, hyperchlorhydria can not coexist with any appreciable degree of organic acidity, except occasionally on account of the introduction of food already fermenting or very prone to fermentation. The difference of definition is not entirely a matter of terminology, as, according to the conception of the case, one might administer HCl, another alkalies, and both can not be right. The real question is whether, in these cases of gastric irritability, the HCl is the irritating factor or a conservative one. Löwenthal, quoted by Alois Pick, of Vienna, in a pamphlet on gastric hyperesthesia, introduced 250 c.c. of 3 to 10 per cent. solutions of HCl and lactic acid respectively, into the stomach, through a tube, and allowed the solutions to remain from one to several minutes. These solutions, varying from fifteen to fifty times the strength of normal gastric juice, produced no sense of burning or pain, although the stomachs were hyperesthetic to substances produced in the course of digestion. Hence, it seems conclusive that the HCl is not the irritating factor, unless organic fermentation can be excluded, and that we should not use the term "hyperchlorhydria" nor oppose the hyperchloric secretion, unless organic acidity is nearly absent and hydrochloric acidity surpasses the standard of gastric juice, i. e., 20:10000.

Text-books are surprisingly non-committal regarding the amounts of free or combined HCl, and of total acidity to be expected under normal conditions. Using dimethylamido-azobenzol as an indicator for free HCl, alizarin as an intermediate indicator and phenolphthalein for the total acidity, I should place the degrees of acidity, meaning the ratio of decinormal solution of monobasic alkali to the quantity of chyme used, about as follows: Total acidity, 50 to 100; combined, 20 to 30; free HCl, 20 to 50. Free HCl should be about half the total acidity.

When, a few years ago, Boas announced that the formation—not the presence or introduction—of lactic acid in the stomach was pathognomonic of carcinoma, most Americans accepted the statement as gospel truth. Some few of us were skeptical enough to exclaim: "How can lactic acid, which can be produced from almost every food except pure fats and oils, by any one of six or a dozen different bacteria that may be present in the stomach, be pathognomonic of cancer or of anything else?" But it required much tedious labor to disprove the assertion. Like every fallacy, this statement does contain a germ of truth. Lactic acid is the most conspicuous product of fermentation in the stomach. While practically always present in traces, the disease which most frequently and uniformly results in marked gastric fermentation is carcinoma and especially that of the pylorus, with secondary dilatation and stagnation of gastric contents. But there may be carcinoma without lactic-acid excess, or vice versa.

A very practical suggestion for the busy physician is whether stomach contents change materially after withdrawal, so that the results of examination would be vitiated by delay. From numerous experiments, comparing titration tests immediately after withdrawal and at the expiration of several days, I am able to state that, if cooled immediately to 40 F., very slight changes of any kind occur within two or three days, though there is a gradual absorption of free HCl by proteids, with corresponding increase of acid albumin and, possibly, of albumose.

While HCl fluctuates according to trivial exaltations or depressions of gastric function, pepsin and rennin are always present in gastric juice in sufficient quantity, except in the gravest gastric conditions, carcinoma, anadenia and achylia due to severe constitutional states, such as Addison's disease, according to a personal observation, and probably high grades of fever and the antemortem state. It is usually said that, if rennin is present, pepsin is also, and thus the simple experiment of curdling milk may be substituted for the more tedious one of digesting albumin. I have observed a case of carcinoma in which the rennin test was normal, but albumin was not digested, even after adding HCl. Ptyalin, by the way, is practically never lacking, according to 1000 experiments by Ewald, and I can add my mite in corroboration. The practical deduction is that ferments are very rarely indicated as aids to digestion. Certainly, they should not be given unless their lack has been actually demonstrated, and, in such exceptional instances, the chances are nine out of ten that the stomach and bowel are in no condition to act as digesting cavities, even with artificial aid, and that predigested foods should be given instead of ferments.

It was formerly believed that the stomach emptied into the intestine, all at once, several hours after a meal, but we have long known that the emptying takes place in waves, beginning within a few minutes after a meal and continuing at intervals of a few minutes throughout

gastric digestion. The portions allowed to pass the pylorus are semi-liquid and quite completely digested, according to the standard of thoroughness of the stomach, which is not very high. Personally, I am inclined to believe that further observations will show that there is exceedingly little absorption through the glandular membrane of the stomach, and that the potassium-iodid test really marks the first peristaltic wave into the intestine, provided that the gelatin capsule is rapidly soluble. The salol test for gastric motility usually results positively three-quarters of an hour after taking the powder, and this, I have shown, is the case whether the powder is given immediately at the conclusion of a meal or at some other time. Some years ago I made a thorough study of this test, including not only personal observations, but the work of others, with the following conclusions: The test can not possibly represent an emptying of the stomach, though it may theoretically correspond in time to the average motility of this organ; the very conditions which are prone to accompany delayed motility, diminished hydrochloric acidity and catarrh, allow the absorption of salol from the stomach (Cornil); the test does not agree with the other more reliable clinical observations; no reliance can be placed on it unless in such extreme instances that the existence of stagnation is already evident. Ewald, to disprove these views, ligated the pylorus of dogs and found that absorption of salol did not take place. These experiments, however, are fallacious, because: 1, the animals were presumably healthy, there being neither diminution of HCl secretion nor mucous excess to favor the absorption of salol from the stomach; 2, the shock of such an operation would naturally prevent absorption, according to all surgical experience.

A very practical question regarding gastric peristalsis is as to its excitability by electricity. Experiments on this point, by Einhorn, Meltser and others, are somewhat contradictory. All, however, agree to a very practical extent. Contractions can be readily, though slowly, produced, if one or both poles are applied to the peritoneal coat of the stomach, and they either do not occur at all or only infrequently if one pole is within the stomach and the other on the skin. On account of clinical experience I abandoned this method seven or eight years ago. More recently, having a patient who insisted on its use, I connected a stomach-tube with a bubble manometer, which, although simple and not adapted to quantitative estimates, is very sensitive. After several sances, we were convinced that, while respiration affected the endogastric pressure considerably, the faradic current had not caused a single peristaltic wave, though a few had occurred during the sances. In vivisections, I have frequently been able to confirm the belief that gastric peristalsis, due to faradism, even with both poles on the serous coat, is tardy, slow and slight and by no means equal to that produced by mechanical irritation.

An ingenious but fallacious method of judging of absorption, etc., depends on quantitative examinations applied to chyme, after the introduction of known quantities of certain substances. For instance, to prove that soluble solids, but not water, are absorbed from the stomach, a sugar solution of known percentage is introduced. After an hour, the stomach is thoroughly emptied and the liquid quantitated. The invariable result is a diminution of percentage, after allowing for the additional amount of water introduced in thoroughly cleansing the stomach. But this diminution is inevitable from the fact that some sugar is lost by a passage

through the pylorus, while water is added by secretion of gastric juice. For instance, in a case of hyperchlorhydria, I found 500 c.c. of chyme an hour after a test-meal amounting altogether to only 250 c.c. Similar, but less striking results are often noted. Huber's oil test, consisting in the estimation of oil remaining after an hour and intended to afford information as to gastric peristalsis, lacks this theoretical error, but it is extremely difficult to remove every particle of oil by lavage, nor is it possible to deduce information as to how the stomach will deal with ordinary food from observations as to how it responds to a stimulant of peristalsis, like oil.

INFANT-FEEDING.*

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There is no more important subject in the domain of preventive medicine than that of infant-feeding. While the melancholy fact remains that a third of all infants born, largely because of improper nutrition, die before the completion of their third year, the abundance of literature on this subject constitutes no sufficient reason why it should not again and again be taken up. On the other hand, the infant mortality records plead piteously that the subject be not laid aside as threadbare. It is not merely on a par with the most important subject in preventive medicine, but is itself the most weighty in this branch of the sanitary sciences. Not merely the life and well-being of the infant and child, but those of the adult and generations yet to be born are most vitally influenced by proper feeding of the infant. That real progress has been made in our understanding of the subject no one endeavors to say. That we are better able to cope with the needs of so great a problem than they of the former generation many are fondly hoping. Yet when the more or less completely theoretical is brushed aside and the practical weighed in the business balances, the progress made in life-saving appears alarmingly visionary. Our clear conception of the chemistry of human milk in all its minutiae has failed to give us a synthetic breast milk capable of producing precisely the same physiological effects as the natural product; and it is essential that this fact be more generally recognized and made the basis of action.

Mother's milk for mother's baby affords human food for a human being, and will go farther to solve the vexing problem of a persistently high mortality among infants than all the chemical erudition in the world. A recent German writer has well said that the natural food of infants is not vegetable, but animal, and that proper digestion and nutrition depend more on the biology than on the chemistry of the food taken.

The purpose of this paper is to plead for a closer adherence to, and a stronger advocacy of, Nature's method of feeding on the part of our profession. Two forces are to be combated in this connection, if we would make genuine advances. The first is the present alarming disinclination on the part of mothers to breast-nurse their offspring, and the second is the clamor for commercial supremacy in the sale of artificial foods. These are mutually independent, each taking advantage of the other. Young married women wish no offspring, and finding themselves mothers, they shirk the vitally important duty of committing themselves to the highest

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