silver in the tube connecting the ozone generator and the manometer.

Mr. W. M. Conway exhibited by optical projection some photographic lantern pictures of the scenery of the Baltoro glacier in the Karakoram Mountains, Kashmir, in 1892. Some of them were taken from the summit of the Pioneer Peak, 22,500 feet high, which he believed to be the greatest altitude yet climbed anywhere.

Sir David Salomons, Bart., and Mr. L. Pyke exhibited some high-tension electrical experiments, in which discharges were sent through a number of vacuum tubes of different degrees of exhaustion. When the tubes were coupled in parallel, varied degrees of brilliancy were obtained, and when the same tubes were connected in series, the degrees of exhaustion which in the first case gave most brilliancy, in the latter mode of connection gave least. They also exhibited a small closed magnetic circuit transformer lighting three tubes with bunch electrodes, specially constructed for heavy discharge; the coupling was by their parallel inductive method.

The president of the Royal Society, Lord Kelvin, exhibited some physical illustrations of the molecular tactics of a crystal.

Mr. Edward Whymper exhibited the Corry "protected" aneroid, newly and specially designed for use in mountain travel, or for aeronauts. This form of mountain aneroid is designed to avoid the inaccuracies which result from continued exposure to low atmospheric pressure. It is inclosed in a perfectly air-tight outer case, and the internal atmosphere is kept at about a normal pressure at all times, except when an observation is to be taken, and then the cock is opened and communication with the external atmosphere is established. After taking a reading, the pressure is restored to the normal by means of a small force pump. The conditions thus correspond to those which originally obtained, when the aneroid was graduated under the air-pump receiver.

The next item of interest to which we think it well to draw attention, is one which is always on view at the Royal Society, consequently perhaps little noticed by the press, namely, one of the earliest reflecting telescopes, and made by Newton's own hands. He presented it to the Royal Society about the end of 1671, and it attracted the attention and approval of Charles II. The telescope is represented in Fig. 4. Its little metal-

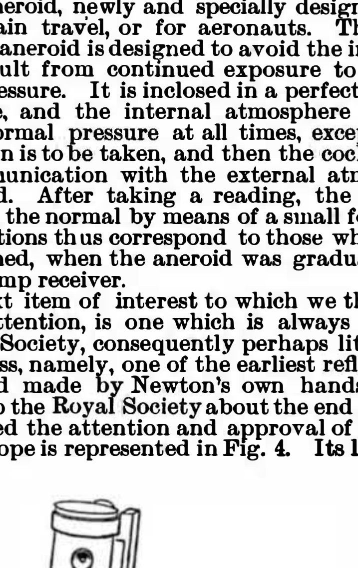

Fig. 4

lic speculum has a radius of 12⅜ or 13 inches, consequently its focal length is about 6¼ inches; in the tube is a little plane mirror at an angle of 45°, and the telescope has a plano-convex eyepiece; it magnifies 38 times. Upon it is the following inscription: "The first reflecting telescope invented by Sir Isaac Newton, and made with his own hands."

As a matter of fact, Mr. James Gregory, of Aberdeen, made the first reflecting telescope, and described it in his *Optica Promota*, printed in 1663. Newton's first reflecting telescope was smaller than the one possessed by the Royal Society; the first one was made in 1668; it was 6 inches long, and had a speculum with an aperture of rather more than 1 inch. With it Newton saw Jupiter distinctly round, his four satellites were visible, and the "moon-like phase of Venus" was also to be seen by its aid. Martin Mersenne, a French mathematician, seems to have first had the idea of a reflecting telescope, and suggested it in some letters to Des Cartes a little before 1640.

One of the most popular exhibits at the conversazione consisted of specimens of living Canadian walking-stick insects, *Diapheromera fermorata*, exhibited by the Zoological Society of London. They were hatched from eggs laid in the insect house of the society. They are about 3 inches long, and average, perhaps, $\frac{1}{16}$ or $\frac{1}{10}$ inch in diameter, and they walk on long, slender, hair-like legs. They have been reared in the gardens on hazel leaves, but have not fattened thereupon.

Mr. Edwin Edser exhibited some apparatus to illustrate Professor Michelson's method of producing interference bands, which method is likely to be of great value in scientific research. His method was described in these pages shortly after he had read his paper thereupon at the last meeting of the British Association. In Mr. Edser's modification light is allowed to fall on a mirror thinly silvered, so that about half of the light is reflected and half transmitted. The two rays pursue paths which are mutually perpendicular, are reflected back by two ordinary mirrors, and on meeting, interfere. The interference bands can be projected on a screen, and this fact, together with the simplicity of the arrangements, makes the method useful for lecture illustration.

Professor Henrici exhibited a harmonic analyzer, constructed by Mr. G. Coradi, of Zurich, according to instructions from Professor Henrici and Mr. Sharpe. The instrument gives, on going once over a curve, the first five terms of the expansion in Fourier's series, and on going twice more over the curve, it gives five additional terms. The constant term is not given.

Mr. C. J. Woodward exhibited some apparatus to indicate the phenomenon of the interference of waves of sound. The Karakoram Mountain Survey Expedition exhibited some water-color drawings of the scenery of the Karakoram Mountains, Kashmir, by Mr. A. D. McCormick. These drawings were made at altitudes of from 15,000 to 20,000 feet during the expedition in 1892. Professor Osborne Reynolds exhibited a textile fabric contrivance giving motions analogous to vortex ring in fluids. Mr. A. A. C. Swinton exhibited some more of his high frequency electric experiments. Professor Thorpe exhibited some autotype enlargements from photographs illustrative of the recent African eclipse expedition. The Rev. F. J. Smith exhibited some more of his inductoscripts. The Egypt Exploration Fund Archæological Survey exhibited some water-color drawings executed by its artists. Professor Norman Lockyer exhibited photographic spectra of some of the brighter stars, and Major P. A. McMahon exhibited a new method of obtaining designs for tesselated pavements.

Captain McEvoy exhibited his hydrophone. A heavy iron vessel containing the transmitting part of the apparatus may be sunk from one to five miles out at sea, and it is connected by means of an electric cable with the receiving station on shore. When a torpedo or other boat driven by steam gets within half a mile of the transmitting apparatus in the submerged case, signals of various kinds are transmitted to the land, and by means of the telephone the beats of the paddles or of the screw of a boat can be heard. Some main features of the apparatus were not made public. A very light pendulum seemed to be the body taking up the vibrations inside the case.

THE CHOLERA EPIDEMIC.

"MIXED INFECTION" IN CHOLERA.

THE *Vratch* of January 7 contains a report by Professor M. Nencki on the work done in St. Petersburg Imperial Institute of Experimental Medicine during the cholera epidemic of last year. At the very beginning of the outbreak the institute was enabled, by the kindness of Prince Alexander of Oldenburg, to establish two temporary branches for practical work at Baku and at Astrakhan, and to furnish them with the necessary apparatus and instruments for bacteriological investigation, as well as with a large supply of old and new remedies for the disease. As the result of these investigations, Dr. Blachstein and Dr. Shebenko came to the conclusion that the infection of Asiatic cholera is due, not to the action of the comma bacillus alone, but to its co-operation with other microbes. They found in the stools of a large number of patients suffering from typhoid cholera, and sometimes also in cases of typical cholera, three kinds of short bacilli, which they named bacterium Caspicum *a*, B1, and B2. The bacterium Caspicum *a* does not liquefy gelatine. It is very difficult to distinguish this bacterium from the bacterium coli commune and from the bacillus of typhoid. The bacterium Caspicum B1 is chiefly found in cases of typhoid cholera, but now and then also in typical cholera it liquefies gelatine. The bacterium Caspicum B2 was obtained from the contents of the small intestine of a person who died from typical cholera. It acts on gelatine like the foregoing, from which it differs very little, and it is possible that both the kinds last mentioned are identical. Dr. Blachstein states that, after subcutaneous injections of bouillon inoculated with the discharges of a cholera patient, mice and rabbits died in 24 to 36 hours. On the other hand, injections of a pure culture of comma bacilli or of pure cultures of the three species of Caspic bacilli, taken separately, did not cause death. This observation is in accordance with that previously made by Bouchard. Dr. Blachstein prepared artificially pathogenic mixtures in which the comma bacillus and some of the other kinds of intestinal bacilli were present. These mixed cultures were obtained either by inoculating a 24 hours old culture of one of the other microbes with the comma bacillus, or by placing one of the other bacilli in the culture of comma bacillus, and it is observed that mixtures obtained in the former way were more virulent than those prepared in the latter. Experiments on mice always gave uniform and positive results, illustrating the fact that the comma bacillus, in combination with some of a large number of other bacteria, is able to kill mice, while in the same animals pure cultures of any single kind give negative results.

The Caspic bacillus *a plus* comma bacillus kills rabbits and pigeons; bacillus Caspicus B2 *plus* comma bacillus kills only mice and guinea pigs. The mixed cultures were generally injected in the quantity of 0·1 cubic centimeter into the mice; the rabbits were inoculated with 2 cubic centimeters. Death quickly ensued, generally within 24 hours.

Dr. Blachstein, in collaboration with Dr. Zunft, succeeded in obtaining from the water supply in St. Petersburg a bacterium which by itself was quite innocuous, but which, mixed with the comma bacillus, caused the death of animals. A mixture of comma bacillus with the bacterium coli commune obtained from the contents of the intestines of a cow killed pigeons. The comma bacillus grows very well in a pure culture of bacterium coli, and in growing displaces the latter so effectually that within a week no trace whatever of it remains.

With regard to the other mixed cultures, it was found that the comma bacillus entirely disappears in a culture of bacterium Caspicum, and within 36 to 48 hours; on the other hand, transferred into a culture of bacterium, B1, it first develops abundantly, but soon stops in its development, and after two or three days disappears, and leaves the place to its companion. A growth of the inoculated comma bacillus in the bodies of animals was not observed by them; on one occasion only Blachstein and Zunft succeeded in recovering the cholera bacillus from the place of inoculation.

In Professor Nencki's opinion, neither Koch nor his followers have given sufficient attention to the importance of bacteria other than the comma bacillus in the infection of cholera. He thinks it clear that other bacteria in some way or other increase the virulence of the comma bacillus. Nencki believes that if Pettenkofer and Emmerich, instead of drinking a pure culture of comma bacilli, had taken one of the mixtures mentioned above, they would not have escaped with so light a form of cholera. For the treatment of cholera, Nencki recommends B-naphthol-bismuth, especially in the first stages of the disease. He also found pine tar an excellent disinfectant.

DUODENAL SECRETION AND DIGESTION.

By Dr. G. ARCHIE STOCKWELL, F.Z.S.

WHILE for some time it has been generally thought that the functions of digestion and assimilation were thoroughly mastered both from chemical and physiological points of view, it has, nevertheless, been understood by working physiologists that a missing link existed—that there were certain phenomena continually manifesting themselves which did not tally with generally accepted ideas; notably, that the complex substance termed pancreatin (made up of *trypsin*, *amylopsin* and *steapsin*), which in consonance with all teaching should be destroyed in the stomach, on the contrary, on investigation frequently gave results that did not uphold this view; and also that when a mixture of pancreatin, pepsin, and hydrochloric and lactic acids is ingested by the mouth, therapeutic effects are induced that do not obtain in the least degree when the first named substance is eradicated. Consequently, it is evident there is a serious error in physiological teaching, and particularly in connection with the function of digestion in so far as it takes place in the intestinal tract of mammals.

Recently the statement appeared in a new edition of a volume on therapeutics, that the action of mercurous chloride (calomel) depended solely upon its transformation into a black oxide by contact with the alkaline juices of the duodenum—that the activity, therefore, in fact, was not due to the calomel *per se*, but to the oxide.

Such teaching cannot but be pernicious, since it has no better basis than mere assumption, being derived neither from physiological theory nor fact; and a very little knowledge of the mercurial salts would have saved the author from an accusation of unpardonable ignorance. Again, it may be added, the action and transformation of this particular salt of mercury (the mild chloride) is by no means definitely understood; indeed, for many years it has been a question of dispute. It has generally been believed, however, that the transformation of this medicament takes place in the larger intestines instead of the duodenum, and consequently, after its physiological fact has been manifested, it is changed into the black or sub-sulphuret (Æthops mineral),* but in this connection it must be remembered that both the black oxide and sub-sulphuret of mercury are, at best, admitted to be possessed of but very little therapeutic activity; so little, in fact, that both are commonly believed to be wholly inert, for which reason they have long since been dropped from all civilized pharmacopœias.

The general teaching of schools and text-books is that all secretions of the *prima viæ* below the pyloric orifice, and especially of the duodenum or "second stomach," are invariably alkaline. Experimenters who have made pancreatic fistulæ, whereby the pancreatic fluid is made to flow through a cannula, outside the body and into a specially prepared receptacle, have certainly noticed this peculiarity, but they have not, however, taken into consideration the possible changes that may follow as the result of contact of the secretion with the oxygen of the outer atmosphere. Continually, too, is reiterated the statement that while pepsin requires for its proteolytic activity a medium most decidedly acid in character, the complex substance denominated pancreatin can only act in the presence of alkaline media—that the pancreatin is necessarily destroyed when brought into the presence of an acid.

I am now prepared to prove the function of digestion within the duodenum, far from being an alkaline process, is distinctly an acid one; that, moreover, the pancreatic fluid in its normal condition is *never* alkaline, but reacts in greater or less degree, according to circumstances, to blue litmus paper.

Dalton, Flint, Foster, and others are responsible for the statement that the pancreatic juice is alkaline, apparently unaware of the fact that it is *normally* acid and only becomes neutral and then alkaline on contact with oxygen, as upon exposure to atmosphere. Another source of error is the supposed relationship between the pancreatic fluid and the secretion of the salivary gland, as judged by the action of both upon starch, especially within the test tube of the laboratory.

Time and space will not permit, at this juncture, of going into the details of the experiments that have led to the discovery above outlined, viz., that the secretion of the duodenum, in state of health, or when normally functionating, is invariably acid; nor do I pretend to say when all function is in abeyance, the alkaline reaction is not sometimes possible, since it has frequently been found so, especially when the secretion of the gall duct preponderates; just so when perfectly quiescent the gastric secretion proper is likewise often found alkaline; and the reasons for alkalinity, in both instances, are most obvious; I will, therefore, in conclusion, merely confine myself to outline.

For some years it has been my good fortune to be retained as the confidential medical adviser of a well known pharmaceutical house. Some three years since, this house endeavored to secure a pill coating which would be impervious to the action of gastric fluid, and, at the same time, readily soluble in the duodenum or beyond. Already experiments had been made in this direction in Germany and France, with a product obtained by exhausting horny matters, such as bristles, nails, horns, and epidermal tissues, successively with alcohol, water, and dilute acids, the result being an albuminous substance known commercially as keratin. These experiments, however, have never proved satisfactory, and never fully filled the above requirements owing to several causes—chiefly that a large number of medicaments were decomposed by contact with the fluids which were necessarily employed to render the coating insoluble; that a pill mass containing water caused keratin to shrink and crack into fissures; that the process of coating was one which did not permit the pill to be fixed on the point of a needle, since it left an opening in the coating that could never be completely closed; finally, in a large majority of instances, it was found keratin-coated pills passed through the entire length of the bowel undissolved.

A chemist in the employ of the house before referred to, undertook to supply the desired pill coating from the

* *Vide* National Dispensatory.

standpoint of his profession,and finally submitted a number of spherules enveloped in a thin varnish of shellac and tolu, his theory being based upon the supposition that the gastric fluid must, under all circumstances and all conditions, both in health and disease, be necessarily acid; and that likewise, under like circumstances and conditions, the secretion of the duodenum is necessarily alkaline. In proof the utility of his discovery, he exhibited a series of test tubes, some containing artificial gastric juice, others a 1 per cent. solution of sodium bicarbonate, in the former of which the pills covered with the tolu-shellac coating remained intact, while in the latter they speedily disintegrated.

The general manager of the house before referred to, on several occasions, had opportunities to observe in his own person, especially in connection with the employment of digestive ferments, that the chemistry of the test tube and laboratory by no means produced results identical with the chemistry of digestion as manifested within the human economy. He, therefore, was somewhat skeptical as to the report of the chemist, and requested further investigation, and from a purely physiological standpoint, at my hands.

On general grounds, I had always believed the secretions of the intestines to be acid, or perhaps nearly neutral, but had forgotten whether any positive teaching had ever been promulgated on this point. To be sure, the medical press frequently reiterated as a well-known fact the alkalinity of the intestinal secretion, but as these eriodicals, in the main, are established for other than scientific purposes—conducted in the interests of advertisers solely, and neither their editors nor contributors, as a rule, representatives of the better medical talent of the country, or men to whom original investigation is familiar—I gave the matter little thought; and, moreover, these claims, I was well aware, were for the most part advanced in the interests of manufacturers of digestive ferments.

Delving into text books in search of evidence of the alkalinity of the intestinal and duodenal secretions, or tracing back, I was surprised to find the theory originally expressed as a *possibility* only—as a bit of guesswork with no better foundation than the "three black crows," and that had grown by constant repetition; that later physiologists, while accepting and boldly advancing the idea, had been at no pains to seek its verification, except as such might present itself as a side issue, as, for instance, through pancreatic fistulæ. The only experiments that I could unearth were of the character of those of the chemist before mentioned, and consequently bore no reference to digestive chemistry as it obtains within the animal economy.

Instituting a number of experiments upon dogs, it was found the shellac-tolu coating did not dissolve within the stomach. *Neither did it dissolve in the intestines.* Tartar emetic pills of from one-quarter to ten grains each, and administered in various degrees of dosage, had no effect upon dogs of eight pounds weight and upward; pills of morphine, and likewise a sulphide of calcium with like coating, were equally negative in results. Subsequent and duplicate experiments upon a series of individuals by means of like coated spherules of tartar emetic, podophyllin, etc., in varying doses, were no more satisfactory. Further, as a means of control in each instance, an equal number of men or dogs were selected who received pills without the coating. The negatives in one experiment, as a rule, became the positives in the next, and *vice versa.*

Next was undertaken vivisections during all stages of digestion of food and ingestion of pills, and these revealed the intestinal secretion, including that of the duodenum, to be unvaryingly—though sometimes but faintly—*acid*, depending more or less upon the stage of the function; that when the digestive function was wholly idle, and the intestines and stomach entirely empty, likewise the pancreatic secretion either totally inhibited or in abeyance while that of the liver persisted, there might, sometimes, be temporary but slight alkalinity that rarely extended further than the lower part of the jejunum. Incision of the pancreas *in situ* at the origin of the ducts gave an acid reaction of secretion; pancreatic fluid almost instantaneously became first neutral, then alkaline, on contact with air, and such, gathered by means of fistulæ, presented the latter reaction most markedly after a few seconds' exposure.

Experiments were made upon dogs at a time when their lacteals were congested and distended as the result of the absorption of digested milk, and while abundance of this fluid still remained in the duodenum to the exclusion of all other fluids—this being the time when the majority of writers insisted upon an alkaline condition of the duodenum—yet an acid reaction was found to invariably obtain throughout the intestinal tract.

Again, on various occasions, alkalinized milk was digested with the so-called pancreatin (duly rendered alkaline) of commerce, yet the result was invariably the same—the end product was acid!

Like experiments made with various other substances, especially beef peptone, not only in condition of alkalinity, but sometimes neutral, only induced like results. In the same way, milk and beef peptone, when brought into the presence of pancreatic secretions obtained by means of pancreatic fistula, differed in no way, save in the time necessary to produce the acid end product. The most marked evidence was obtained by making a pancreatic fistula and introducing the free end of the tube into a bottle of milk.

These experiments have been repeated again and again during a period of three years, with unvarying results.

Recently, in conversation with Dr. O. W. Owen, late Professor of Physiology in the Detroit Medical College, I was pleased to learn that he also had been engaged in experiments of like nature, and had obtained precisely parallel results. On one occasion, accepting the statement of the alkalinity of the intestinal and duodenal fluids, he endeavored to demonstrate the same by means of a vivisection before his class, when, to his surprise, an acid instead of an alkaline condition was revealed. Conceiving this to be an error, the result of accident perhaps—that possibly the knife which opened the duodenum, and before had been employed to incise the stomach, was at fault, or that possibly accidental pressure upon the stomach might have forced a portion of the gastric juice into the bowel immediately contiguous—he introduced to the table another dog, and with a new and perfectly clean instrument, opened the abdomen from below upward, in order to avoid any possible passage of fluid from the stomach, then segregated the duodenum just below the pyloric orifice by means of a ligature, and then only incised. The result was as before, distinctly acid, a condition which persisted until the lower rectum was reached, where, owing to the existence of special glands peculiar to canines, an alkaline condition obtained—and invariably obtains—a condition that is not found in man.

This experiment was repeated on many occasions. He also experimented regarding digestion on the same lines with myself. The results in each and every case were identical and confirmative, and no amount of control in any way contravened.

As the experiments of Dr. Owen and myself extended far into the hundreds, and in each instance, where possible, were carefully guarded by control, and the results unvarying, there can be no doubt of the correctness thereof and of the conclusion that the normal secretion of the intestine in mammals (*Carnivoræ* and *Omnivoræ* at least), including that of the duodenum, is acid; that duodenal digestion is an acid and not an alkaline function, also that the accepted views regarding the utility of pancreatin (so called) are based upon false premises resulting from imperfect knowledge and experimentation.

I may here say that since these experiments were performed, I have made a very exhaustive, thorough, and careful research of modern literature, and, as before remarked, can find no evidence that has ever been offered as to the alkalinity of the intestinal tract, except mere supposition, which by repetition has been accepted as fact; I may also add that in one instance it was found Claude Bernard, who had made extensive investigation in this direction, declared the intestinal tract was invariably acid throughout its length. Therefore, neither Dr. Owen nor myself can claim an absolute discovery, but our researches must be of value as a reaffirmation of a fact that has been forgotten or escaped notice.

I may here remark that this evidence regarding the acidity of the intestinal secretion placed the matter of a pill coating that will dissolve only after the medicament has passed the pyloric orifice, upon an entirely different basis. Subsequent study and experimentation, however, secured the desired result in this direction, but this it is not necessary to explain, since it is a matter that pertains solely to the firm in whose interest it was evolved; further, I have not thought it of sufficient moment in this connection to have solicited their permission to make public the details of this process, though I have no doubt such would have been granted, since it has always been their rule never to surround with secrecy, or secure monopoly by copyright, patent, etc., their formulæ or undertakings.

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NOURISHMENT IN ACUTE DISEASE.*

By FRANCIS H. WILLIAMS, M.D.

THERE are few questions in the treatment of disease which have to be decided so often during the daily routine of practice as those which concern the proper support and nourishment of the patient, and further, there are not many things connected with the care of patients which are so difficult.

It is not easy to find a person competent to prepare suitable food; it is, therefore, the more incumbent upon the physician to be able to give proper directions for its preparation.

The chief thing is to take pains, and those who can do this are rare people, whether physicians, relatives, or nurses. This is why less is accomplished than there should be in the support of the sick. If we wish to succeed in avoiding nausea, vomiting, and loss of strength, and even loss of life, we must learn to offer nourishment to our patients in a suitable form.

It will be my endeavor to call to your minds a few of the principles to be kept in iew in feeding patients acutely ill.

Food may be classified as follows:

(1) Water, (2) salts, (3) fats, (4) sugars (fruit), (5) starches, (6) albuminoids.

The classes of food known as starches and albuminoids are the ones which require the most care to offer to a patient in a proper form.

Water is of prime importance. Consider for a moment the composition of the body of a man weighing 154 pounds, as illustrated by these blocks. He is 108 pounds water, or about two-thirds.

It does not follow from this that we need to give every patient several pints of water a day; by no means; but it is fair to infer that water of a suitable temperature should not be denied the sick, and that patients too young, too delirious, or too ill to ask, should not be neglected in this regard. The physician should see to it that water is offered the thirsty economy in all cases, nature demanding it, though the patient makes no request.

Salts are present in small proportion in most foods, and are essential constituents of our foods.

Fats as a rule are not tolerated by patients acutely ill, and their use should be limited to such forms as are finely divided, as in milk or yelk of egg (and even in milk it may be necessary to reduce the amount of fat by skimming off the cream).

Common sugar is rapidly and perfectly changed into grape sugar, and into maltose, before it is assimilated.

Grape sugar and maltose are very soluble, and for this reason seem a very desirable form in which to give nourishment. Horlick's and Mellin's foods are examples of preparations made up chiefly of saccharine substances.

Fruits are valuable to give variety to the diet, and to contribute water, which they contain in large proportion. Most fruits contain eighty-five to ninety-odd per cent. of water, some sugar, and the citrates, malates and tartrates of potassium. Other fruits, such as grapes and bananas, contain sugar in considerable proportion, to twenty per cent., and their value as foods is not to be despised. Among dried fruits, dates and figs contain sixty per cent. of sugar, and six per cent. of albuminoids.

The value of certain fruits for persons who are predisposed to uric acid, gravel, and concretions in the bladder, I shall not discuss here, though it is well worthy of attention.

Though we take foods into the stomach in solid form, it is necessary that they be made soluble before they can be assimilated. The classes of foods which we have thus far considered are readily absorbed, namely, water, salts, fats, and sugars; they are all liquid or readily soluble substances. In the remaining classes, starches and albuminoids, we have foods with which there are several steps to be gone through before they can be taken up by the system.

With all starchy foods, like grains, potatoes, and rice, it is necessary to break the starch granules by heating, or some other simple process, and before the starch can be absorbed, it must be converted into a soluble substance, such as dextrine, which is the same in composition as starch. Starches, therefore, are not absorbed as such, but must first be rendered soluble.

Uncooked starches vary very much in the rapidity with which they can be converted into sugar by the action of the saliva. The starch of Indian corn is converted in three minutes, whereas wheat starch takes forty minutes, and potato starch three hours. After thorough cooking, all starches require nearly the same time. It is, therefore, important to have starchy food well cooked before it is given to patients.

Arrowroot forms, by cooking with water, a mucilaginous liquid, not a pasty mass; it is, therefore, not apt to irritate the stomach and intestines. It should, however, not be used alone for more than a few days together, as it contains too little of the other constituents of a proper diet. In this it differs from rice or potato bread, which contain gluten, salts, and fats in addition to starch. Rice is better steamed than boiled, as it loses to boiling water much that is valuable. Boiled potatoes are for the same reason less nutritious than baked ones.

As regards *albuminoids.* Wholly without albuminoids, unless the disease is of short duration, the patient cannot exist. Since they are imperatively needed they should not be omitted from the diet, even where digestion fails almost completely.

Albuminoids are complex in composition and decompose readily, and in their preparation more care is required than with any other kind of food. To avoid decomposition they should always be fresh; and to prevent losing the albuminoids by coagulation, they should not be heated to too high a point.

To prepare meat foods properly two things must be borne in mind: (1) Albuminoids, as a rule, coagulate when heated to boiling. (The casein of milk is an exception to this.) (2) To obtain a good meat flavor, the meat must be subjected to a temperature much above the coagulating point of albumen. It is, therefore, necessary to resort to two procedures, one which has for its object to extract the flavor, the other to extract the albuminoids without coagulating them.

If we treat meat with boiling water, we get beef tea, which contains only a small percentage of solids and almost no albuminoids. This applies to all clear beef tea. Beef tea is of service in two ways, its taste and odor are agreeable, and, together with the heat of hot water, which acts as a stimulant, it makes a valuable article for use in the sick room—not as food, but as a flavoring.

Some of the extracts of meat made with hot water may be used instead of the ordinary beef tea, thus saving much time. Liebig's, and probably similar extracts of beef, contain no fats, gelatine, or albumen. It is desirable to use only a small amount of extract, say one-third of a teaspoonful to a teacupful of hot water, as too much gives an unpleasant flavor.

Now as regards the juices of meat which contain albuminoids in solution.

From raw meat one cannot obtain as much juice as is easily expressed from the same amount of meat which has been previously heated. The reason is this: The envelope of the muscular tissue is a substance similar to gelatine, which swells and dissolves when heated, and thus after broiling the liquid portions of a steak flow out more readily. A steak when well broiled swells; if it is cooked too long, the albuminoids coagulate, it loses moisture, shrinks and becomes tough. A slightly broiled steak may be cut into square pieces, and pressed or squeezed or twisted in a piece of cotton cloth to extract the juice.

In administering beef juice great care should be exercised to avoid heating it to 136° F., at which point its albumen coagulates in flakes. Beef juice, though fourteen times as rich in albuminoids as beef tea, is so raw in flavor that it is rejected by many palates. To overcome this objection, it is only necessary to add a proper quantity of any extract of beef to make it delicious—about the size of an almond to an ounce of beef juice. Thus by a union of two bodies, one rich in albuminoids and the other rich in flavor, we get something that is superior to either. Beef juice is an excellent article of diet where solid food cannot be given, but is somewhat troublesome to prepare. A pound of meat yields about four ounces of juice; it therefore costs about five cents an ounce.

Soluble albumen, such as is contained in expressed meat juice, is absorbed in the rectum to nearly the same extent as complete peptones. Albuminoids in solution are not precipitated in the stomach, and afterward dissolved, except in the instance of casein milk, which, as already said, is first coagulated and then dissolved.

Being accustomed to prescribe meat juice, I was much pleased to find a preparation of it manufactured by a well known firm. I hoped in this to realize all the advantages of beef juice without its inconveniences. An analysis of this preparation, which was made for me, was disappointing, as it was found to contain only one-third of one per cent. of albuminoids, compared with seven per cent. of beef juice; it had also more salt than is desirable—twelve and one half per cent. This is mentioned to illustrate the advantage of using foods which are prepared at home in preference to those made by manufacturers, of which the composition is unknown. This preparation costs thirty-five cents per ounce, though it is only one-twentieth as rich in albuminoids as beef juice costing about five cents.

If one cannot conveniently get albuminoids from meat, a very nutritious broth may be made by means of hot water into which an egg has been stirred. Here we may heat three ounces of water to not above

* Abstract of a paper read before the Massachusetts Medical Society.