mal animal, and the increase is not absent in fever. Further experiments are to be desired, but of the mere fact of fluctuation in heat-production there seems no doubt. Some of the figures, moreover, show that, as the course of the temperature varies, the rate of heat-production may actually be highest when the temperature is lowest. An excessive rate of heat-loss may overbear and so disguise a simultaneous excessive rate of heat-production; and conversely, the time when the temperature is high may coincide with a time when the heat-production is low. The inference from this is very important, namely, that the height of the temperature in fever is in great measure dependent on the momentary relation of the two processes of heat-production and heat-loss. And inasmuch as these two processes tend to vary irregularly, with but a weak *nisus* towards a daily rhythm, the fluctuations of the temperature do not afford an accurate measure of the changes in either process. The daily and hourly variations in febrile temperature are wider and more irregular than those in health; febrile temperature is inconstant. Moreover, trifling changes in the surroundings of the patient or in his functions readily send his temperature up or down; febrile temperature is unstable. The two co-operant factors of the temperature are acting more or less independently; thermotaxis is enfeebled or overpowered.

The lecturer then proceeded to criticise Liebermeister's theory (adopted by Dr. Hilton Fagge) that pyrexia consists in a change in the normal function of the heat-regulation by which the production and loss of heat are so balanced as to maintain an abnormally high temperature. He objected to this that febrile temperature, even when it is at its height and oscillating with fair steadiness about a mean level, is marked by its instability. A trifle that in health would have but a vanishing effect will in fever produce a large and enduring variation. A whiff of cold air, a little food, a passing excitement, a feeble muscular effort will send up or send down the temperature. The thermostatic character of the healthy normal is conspicuously absent in the mean high level of fever. The imperfect daily rhythm of temperature observed in some continued fevers is no evidence of true regulation. We have seen that even the fluctuating heat-production of a fevered animal showed signs of a periodicity. In health this rhythm is deep-seated and singularly persistent, in spite of the most various disturbances. That it should to some extent "show through" the irregularities of the fever heat is not wonderful; it merely proves that not all the thermal processes are utterly perverted in pyrexia; some that are normally rhythmic may be rhythmic still. But regulation has to do with balancing the integral sum of all the processes, and it is that balance which is overthrown, or at least rendered tottering, in fever. The risk of laying too much stress on the high temperature of the body, and of regarding it *per se* as an index of the heat-production, was exemplified in a recent address by Dr. Ord to the Medical Society. In this address (see JOURNAL October 24th, 1885, p. 783) Dr. Ord had criticised the theory that the increased heat of the body in fever was due simply to increase of combustion, and had pointed out that the increased exhalation of carbonic acid and excretion of urea were not found on calculation to represent a source of heat sufficient to cause the increased temperature of the body. The lecturer considered that the difficulty was an unreal one. There is, he continued, high temperature in fever, and there is increased production, but the one is not the measure of the other. There is increased heat-production in fever as compared with the heat-production of health on the same diet, but not necessarily as compared with that on full diet. Somewhere between these two propositions comes Dr. Ord's, and I am afraid that between them it comes to the ground. I shall be glad if a re-examination of the premisses convinces him that he need not lose faith in his first belief. Dr. Sanderson's memoir, taken in conjunction with Professor Wood's, which supplements and corrects it, when properly weighed, will show him that his first belief is true, though perhaps not the whole truth. And as a final satisfaction I may add that, even though the preamble of his address is unproved, though the discontent with the existing, which acted as a stimulus to his research, was not well founded, the happy suggestion to which it led him appears to me to be true notwithstanding, in a certain guarded and limited sense.

When we say that more heat is produced by a patient in fever than by a patient in health, we must add the proviso—in like circumstances. One of the most important of these circumstances is diet. I being in health can, by eating to excess, raise my rate of heat-discharge far above my usual rate, and I can lower the rate by fasting, and still remain in health. If I become fevered I eat less and less, but my rate of heat-discharge does not fall accordingly. In the report already mentioned, Dr. Sanderson, from the data before him, finds that the heat-production of a fevered patient might be about

50 per cent. higher than in the same patient in health on a low diet. And if that be the case, some "increased combustion explanation" is not only satisfying but inevitable. Admitting increased heat-discharge, increased exhalation of carbonic acid, increased excretion of urea, we must admit a source whence they are derived. They cannot come from the food consumed, for that is notably reduced in quantity; they can only come from the body-substance itself; there must be increased combustion—the tissues must be "consumed by an unseen fire." But, to go further, there are not wanting grounds for believing that fever-heat is often greater than normal heat even on a full diet. The Philadelphia experiments, made since Dr. Sanderson's paper was written, enable us to follow the thermal history of an animal for several successive days under varying conditions of diet and in varying degrees of pyrexia. The outcome is that in dogs and rabbits at least the fundamental part of the febrile process is an increase in heat-production "by chemical movements in the accumulated material of the organism." And though, to use Professor Wood's expression, this increase was usually insufficient "to overplus the loss of production from abstinence from food," in some cases it was more than sufficient. Moreover, reasons are given—valid reasons, as I think—for surmising that what is occasional in the lower animals is probably common in man, in whom febrile movement is more pronounced and severe. In other words, it is probable that in the fevered man there is sometimes an absolute, and not merely a relative, increase of heat production.

ILLUSTRATIONS OF EXCEPTIONAL SYMPTOMS AND EXAMPLES OF RARE FORMS OF DISEASE.

By JONATHAN HUTCHINSON, F.R.C.S., F.R.S., LL.D.,
Emeritus Professor of Surgery at the London Hospital, Vice-President of the
Royal College of Surgeons.

[Continued from page 1212, JOURNAL, June, 1886.]

XIV.—ON INABILITY TO KNOW WHEN THE BLADDER IS FULL.
A GENTLEMAN once told me that for nearly two years he had never known the feeling of wishing to make water. He went to relieve his bladder at stated times, and if he did not do this, overflow might occur, but he had never had any sensation whatever which warned him that the bladder was full. Incontinence had occurred several times in the night. He had never had retention. I found, as expected, that he was the subject of ataxy. His patellar reflex was quite lost and so also his sexual desire and aptitude. His pupils were large and sluggish, if not motionless, on exposure, but acted somewhat in accommodation. I have known this form of vesical anæsthesia, in some degree, in many other cases of ataxy, but never to so marked a degree. It may be of interest to note that in this instance the patient retained the appearance of good health, and had perfect use of all his voluntary muscles with good muscular sense, being a distinguished lawn tennis player. He was single, only 30 years of age, and had suffered from syphilis seven years before I saw him. He had suffered much from the characteristic pains of ataxy in his lower limbs, and also from pelvic aching. Although moderate exercise at tennis did him good, any unusual fatigue made his pains worse. He had, when I saw him, been already greatly benefited by a year's specific treatment of mercury and iodide, but the hebetude of his bladder remained as it was.

XV.—ON PAINLESS RETENTION OF URINE.

In close alliance with the symptom just mentioned, and often in the same patients, we have a form of retention of urine which is unattended by pain. In cases of absolute paralysis, whether cerebral or spinal, we are of course quite accustomed to meet with painless retention. I am, however, wishing now to speak of cases in which no obvious paralysis is present, and the patient is walking about and believing himself in his usual health. Under such circumstances, painless retention occurs, I think chiefly in association with two conditions, senility and locomotor ataxy. It is in connection with ataxy that the best marked and most absolutely painless cases are met. In old persons with enlargement of the prostate, the bladder often becomes gradually accustomed to retain habitually a certain quantity without inconvenience, and even without cognisance, but if the retention should become complete, the distension pain is always experienced, and may become considerable. It must be borne in mind in this connection that old age and ataxy have many symptoms in common; that an obtunding of sensibility, and especially of certain special forms of it, is a very frequent condition in the aged. In the painless retention which we encounter in connection with ataxy,

however, the symptom is a most definite and extraordinary one. The bladder, not being the seat of any previous hypertrophy, is capable of great dilatation, and it may fill until it reaches the umbilicus, without any overflow occurring, and without the slightest pain. Sometimes a sense of mechanical fullness is present, but there is nothing approaching to pain. I have several times known ataxic patients retain their urine for more than forty-eight hours without any suffering. Whenever the bladder shows unusual tolerance of distension, the symptoms of ataxy should be searched for. In the retention which so often follows operations for piles and the like, there is probably some loss of sensation, as well as of motor power, but neither is complete, and the latter probably preponderates.

[To be continued.]

SURGICAL MEMORANDA.

CASE OF SWALLOWING ARTIFICIAL TEETH, WITH RAPID EXPULSION BY THE RECTUM.

On Wednesday, January 19th, 1887, between the hours of one and two in the afternoon, I received an urgent request to see Helen M., aged 35, residing about a mile and a-half from this town. I was informed that she had swallowed her false teeth, and that the plate had stuck in her throat.

On my arrival shortly afterwards I found the woman in a semi-erect posture, afraid to move, in case, as she said, the plate might assume a more dangerous position. Pain in the throat and difficulty in swallowing were complained of, but there was no interference with the respiration. I made a rapid, but minute, examination of the pharynx, carrying my right forefinger well downwards and backwards, but was unable to detect the presence of any foreign body. I now directed her to take a draught of water, which she swallowed without any difficulty. To make doubly sure, I then passed a large-sized probang, but was sensible of no obstruction of any kind, the instrument finding its way easily and smoothly into the stomach. I now assured her—not a comfortable assurance certainly—that there was every probability that her artificial teeth had entered the stomach. A laxative diet was ordered, and instructions were left to send for me at once if any urgent symptoms developed. Next morning I received a message that the plate and teeth had been passed at stool at 6 A.M. I learned, moreover, in the course of the day that, with the exception of slight nausea and retching about an hour after my visit, and some occasional pain, slight in character, in the right iliac region for two hours previous to the action of the bowels, nothing unusual had occurred.

The absence of any serious symptom and the rapidity with which the denture was passed by the rectum are points of interest in this case, and were accounted for, to some extent at any rate, by the form and position of the hooks. These, as will be seen from the subjoined sketch (exact size) taken after the occurrence, are well turned in, and not projecting, in which case it is just possible a much more serious result might have taken place.

Several somewhat similar cases have been recorded in the JOURNAL during the last six years, and I find that in most of them the time stated to have elapsed from the entrance to the exit of the foreign body varied from two to six days. Seventeen hours was the time occupied in this case—an unusually short period of time.

Elgin.

WILLIAM GALLETTY, M.D.

OBSTETRIC MEMORANDA.

CASE OF OVARIAN FETATION.

OVARIAN fetation being undoubtedly the rarest form of extra-uterine pregnancy, the following case will, no doubt, be interesting to your readers. Velpau and Allen Thomson do not believe in the existence of the ovarian variety. As "seeing is believing," I do. M. S., aged 20, a black, unmarried woman, apparently in robust health, pregnant

for the first time, was last seen alive at 6 A.M. on January 29th, when she complained of pain in the bowels. Three hours afterwards, she was found lying on the floor dead. I made a *post-mortem* examination seven hours later. A fluctuating tumour in the abdomen directed my attention at once to that cavity. On dissection, I found a large quantity of fluid and coagulated blood effused into the peritoneal cavity. In this blood I found a male five months' foetus enclosed in its membranes. The umbilical cord was attached to a thin placenta, embedded in a ruptured cystic ovary (right side). Rapid and profuse hæmorrhage was the cause of death. The fertilised ovum had clearly attached itself here, and the cyst, no longer able to contain its burden, had given way. The Fallopian tubes and the rest of the uterine appendages were healthy. The uterus was of natural size and shape. St. Andrew, Jamaica. JASPER CARGILL, M.D.

REPORTS

HOSPITAL AND SURGICAL PRACTICE IN THE HOSPITALS AND ASYLUMS OF GREAT BRITAIN, IRELAND, AND THE COLONIES.

ST. LUKE'S HOSPITAL FOR CHINESE, SHANGHAI.

A CASE OF EXPLORATORY LAPAROTOMY, WITH A QUESTION IN SURGICAL ETHICS.

(Under the care of R. A. JAMIESON, M.A., M.D., Consulting Surgeon to the Imperial Maritime Customs in China.)

KU SIAO-YUN, a fat, muscular shopkeeper, aged 42, was admitted on September 2nd, 1886. His story was that on August 30th he was occupying a few moments of leisure by harpooning bugs on the frame of his bedstead with a shoemaker's awl, when he was suddenly called. He stuck the instrument into the woodwork by his side, and forgot all about it until night, when, preparing for bed, he sat down exactly on the handle of the awl which, tearing through thin silk drawers that he was wearing, entered the anus. Springing up, he dragged the awl with him, and a friend whom he called to his assistance professed to see the point of it slightly protruding from the bowel. On endeavouring to pull it out, however, the friend succeeded only in pushing it altogether out of sight. Next morning the patient had an ordinary stool, but since then he had eaten nothing, he had felt no desire to evacuate the bowels, apprehension had prevented him from sleeping, and he had severe and continuous pain in two places corresponding to the ascending and descending colon. He brought the drawers with him that he had worn at the moment of the alleged accident. The seat was irregularly torn, and there was one small bloodstain, about a quarter of an inch in diameter, on the edge of the rent.

The patient was flushed on admission, extremely restless, the temperature under the tongue was 102° F., the skin was very dry, with occasional fits of sweating, the tongue was clean, the urine was passed in normal quantity, but was muddy and deep red, clearing on the application of heat. It contained no blood. There was no wound or scar at or near the anus. The entire hand inserted into the bowel found the rectum empty. Palpation of the fat-laden abdominal wall, which was impossible without an anæsthetic on account of great sensitiveness, was not decisive even after administration of chloroform. Two irregular masses could, however, be made out in the region to which pain was referred. A grain of opium every four hours was ordered. The night was passed quietly. Next morning (September 3rd) about 60 ounces of warm water was slowly injected into the bowel. This produced several hard lumps of fæces, and some relief was acknowledged. A repetition of the enema next morning (September 4th) brought nothing away. Meanwhile, the patient refused to eat, his temperature did not fall below 101° F., he continued sleepless in spite of the opium, and he would listen to no suggestions as to the probability of his having been mistaken. He announced his intention of leaving the hospital, of cutting open his abdomen, or of hanging himself. Next day (September 5th) he was almost maniacal. Under the pretext of examining him again an enema of 60 grains of chloral was administered. After a few hours' sleep he woke quiet and rational, but urgently demanding operation, at the same time promising to be satisfied that he was under a delusion as to the presence of the awl, if nothing was found. Chloroform having been administered, the mass which had previously been made out on the left side was found to have disappeared, but there was still considerable resistance to deep pressure over the ascending colon. Dr. Jamieson there-