facts seem quite incompatible with the hypothesis of "luxus-consumption"; and tend to prove that, when the body is in equilibrium, the nitrogenous tissues are nourished according to that standard.

The conversion of albumen into urea is not merely a process of oxidation, but is connected with the vital actions of the body. But with which of these is it so connected?

The muscles perform a most active part in the bodily functions; and in composition are most allied to the albuminous compounds. It would at first appear that we must look to their action for an explanation of the elimination of nitrogen; but it is not so. The quantity and kind of food being the same, there is no appreciable variation in the amount of nitrogen excreted under varying circumstances of rest and of exercise. This was first pointed out by Edward Smith and by Voit; and in tetanus, no increase of the amount of nitrogen eliminated has been found. From his own experiments, Dr. Parkes has been led to the conclusion that muscular exercise produces little change in the amount of nitrogen eliminated; but he cannot agree with those who hold that it produces none. The increase in the elimination of nitrogen produced by exercise is, however, very moderate. Moreover, it must be remembered that urea has never been found as a constituent of muscle; on the other hand, kreatin is found in muscular tissue.

Do the changes in the nervous tissue account for the elimination of nitrogen? There is no proof that mental labour produces any difference in the amount of nitrogen excreted; and, at any rate, the effect produced must be so small that we may say that there is no relation between the amount of mental work and the excretion of nitrogen. Such tissues as bone and cartilage, not being concerned in any active operations, cannot, of course, be concerned in the question.

We have now to consider the part performed by the glandular and cellular structures. Albuminous fluid is constantly passing to and from gland-cells; and thus a constant interchange is kept up between their contents and the blood. Here we may look for the conversion of albuminous matters into urea. Voit has drawn a distinction between the albumen fixed in muscles and other organs and that circulating in the blood. The first, or "organ-albumen", changes slowly; the other is unstable, and changes readily into urea in passing through the body.

We must look to the gland-cells for the passage of albumen and the formation of urea. It is known, too, that urea and uric acid are formed in these cells. Nearly thirty years ago, Dr. Parkes noticed in India, in cases of hepatitis and hepatic abscess, that the amount of urea in the urine was lessened when there was much suppuration; while, on the other hand, when the liver was greatly congested, the amount of urea and uric acid was increased. He has lately had an opportunity of making observations on a case of extensive hepatic abscess. Three and a half months before the patient's death, he found that in six days, during which the man must have taken about 1,152 grains of nitrogen in his food, only 792 grains were passed in the urine. Allowing that 150 grains were, during this time, excreted in the fæces, there were still 210 grains retained; either because the pus-cells appropriated the nitrogen, or because the formation of urea was impeded by the pressure on the hepatic cells. The latter explanation Dr. Parkes thought the more probable one. In cases of extensive cancer of the liver, the amount of urea excreted is generally very small. In acute yellow atrophy of the liver, urea sometimes disappears from the urine, its place being taken by leucin and tyrosin. Meissner hence argued that destruction of the liver-tissue interrupted the formation of urea.

There is no doubt that urea can be proved to exist largely in the liver. This has been stated by Meissner and others; and Cyon has proved that urea is formed in this organ—a marked increase being present in the blood of the hepatic vein over that in the blood of the vena portæ. Popp has also lately asserted that urea is a normal constituent of bile.

According to the experiments of Cloetta, Scherer, Meissner, and others, in birds, uric acid is also formed in the liver. This acid and its allies are also found to be increased in congestive hepatic affections.

It is not impossible, also, that urea may be formed in the blood-corpuscles and in the enlarged spleen in cases of leucocythæmia.

The fact of the influence of cells in the absorption of albumen and production of urea must be of importance in regard to the dietetic treatment of disease. The albumen of the food is chiefly concerned in growth, and the non-nitrogenous materials in the evolution of energy. As, then, growth both in plants and in animals is influenced by nitrogen, cannot we regulate this at will by means of the food?

The nutrition of the voluntary muscles and nerves is to a great extent regulated by the will; growth and expenditure are here correlated with exercise. Some parts, again, such as the glandular organs, are removed from the influence of the will, though still under the control of the nerves.

In the next place, there are certain tissues less under the influence of the nerves, but which have an independent power of nourishing themselves in proportion to the flow of blood and the supply of food. The well-known experiments of Hunter on transplantation prove this; and it is further proved by the recent observations made on the transplantation of skin to ulcers, where the transplanted part appropriates pabulum, after the same manner as a plant does by its roots. Are not the liver and other glandular organs fed in the same way? A man was fed for four days on starchy food, and at the same time caused to take much exercise; as far as his voluntary muscles were concerned, energy was well maintained; but the action of the heart was greatly reduced in power. May we not, then, starve an over-growing part by reducing the supply of blood, through acting on the heart?

Dr. Parkes asked further whether it is not possible to influence the growth of carcinomatous tumours by means of diet. At present, no means of causing the disappearance of these growths is known; but can they not be starved? Many must have seen patients fed on nourishing food, but still wasting in every part of the body, except perhaps a cancerous liver or a cancerous cardia of the stomach. In such cases, the nutritious food supplied with the intention of prolonging life may have only served to increase the disease. It is a significant fact, that the most rapidly growing malignant growths occur in later life, when the activity of the normal nutritive processes is diminished, and when, therefore, it may be supposed that the morbid cell-growth will more readily appropriate nutriment to itself.

Are we right, then, in our course of supplying highly nutritive food to persons suffering from such diseases? May we not give them starch and fat, withholding the nitrogen? This diet may be well borne for four or five days; and the state of the heart will indicate how long it can be continued safely. We may not be able to arrest the morbid growth altogether; but we may retard it. Experience alone, however, can show whether we have such a power.

The deprivation of nitrogen appears not to arrest, but to diminish, the formation of uric acid; which probably has an origin independent of urea.

TREATMENT OF CONVULSIONS BY BLOOD-LETTING AND CHLOROFORM.*

By JAMES BOYD, L.R.C.P. Ed., Newcastle-upon-Tyne.

MRS. J., aged 39, a stout plethoric woman, of the bilious temperament, and within three weeks of her expected time of confinement, and in her sixth pregnancy, was suddenly seized with an attack of puerperal epileptic convulsions on July 4th, 1869. The proximate cause of the fits was my having necessarily informed her in the early part of the day that her child, aged 8, was about to die. About half-past five o'clock the same evening, I was sent for in all haste. On my arrival, I found her labouring under the most violent paroxysms. I found the os uteri undilated, rigid, and oedematous. Labour had not commenced. I without delay bled to the amount of two and a half pints, and administered chloroform; after which, the os gradually became more soft and pliable. With two fingers of my right hand I commenced mechanical dilatation; and, as soon as I succeeded in this to the amount which I considered necessary and sufficient, I ruptured the membranes, and endeavoured to apply the long forceps to the head, which then presented at the brim in the first position. Having failed in this attempt, and seeing the dangerous state in which the patient was, as the fits continued unabated, I perforated the head. By this time the labour set in, and the head de-scended into the cavity of the pelvis: by means of the crotchet, I secured the cranium, and quickly delivered. She was kept under the influence of chloroform eight hours afterwards. The bandage and pad which had been applied to the arm after bleeding having accidentally become loose, she, unobserved, bled about a pint afterwards, and awoke to consciousness the next morning about four o'clock, calling the attention of the husband to her bleeding arm, which, on being sent for, I secured.

There are two or three things which might be worthy of our notice in reference to this case. 1. The large quantity of blood that was withdrawn from the body without producing any dangerous and injurious effects, notwithstanding that blood-letting or bleeding in the present day has now become almost obsolete. 2. The long continuance of the convulsions unabated in their paroxysms. 3. The length of time during which she was under the influence of chloroform.

* Read before the Midwifery Section at the Annual Meeting of the British Medical Association, in Newcastle-upon-Tyne, August 1870.