

I found her in bed, complaining of headache. I took her temperature, and found it 100 deg. Fahr.; in the evening it was 99 deg.; the next morning it was again 100 deg., and in the evening had again fallen. I now felt sure that some amount of encephalitis was present, and told the mother so, giving her careful directions as to her attendance on the child. On visiting the child the next day, the mother informed me that "as her daughter got no better, she had called in Mr. —, who said that there was nothing the matter with the brain, and that the symptoms were only hysterical". The child belonged to a sick society, and it was necessary for me to sign her weekly certificates. The first was brought to me for my signature, and had been filled up by the surgeon attending, and the disease was marked "hysteria". Before the end of the second week her death-certificate was brought to me, and the cause of death was given as "meningitis". I believe that in this case I should not have had so early a conviction that meningitis was commencing, had it not been for the high morning temperature; and since then, in similar cases, I make it a rule to observe both the morning and evening temperature before eliminating from my mind the possibility of the advent of encephalitis.

The following is the record of the morning and evening temperatures in a case of encephalitis in a boy aged 15.

	Morning.	Evening.
1st day ... ..	102.6 Fahr. ....	101.8 Fahr.
2nd „ ... ..	102.9 „ ..... 100 „	
3rd „ ... ..	98.8 „ ..... 98.4 „	
4th „ ... ..	99.2 „ ..... 99 „	
5th „ ... ..	99.7 „ ..... 99 „	
6th „ ... ..	99.8 „ ..... 99.4 „	
7th „ ... ..	100.6 „ ..... 99.1 „	
8th „ ... ..	100.4 „ ..... 99.5 „	
9th „ ... ..	100.1 „ ..... 99.2 „	
10th „ ... ..	99.8 „ ..... 99.8 „	
11th „ ... ..	99.5 „ ..... 99 „	

From this period convalescence established.

I lay these slight notes before the profession, hoping that they may be confirmed by more numerous observations, and believing that the rule of temperature which I have so far found to exist in these cases will prove a valuable aid to diagnosis.

## THE RELATION BETWEEN ARTERIAL TENSION AND ALBUMINURIA.

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I DO not desire to enter into a controversy with Dr. Johnson on the much vexed question of "capillary power"; for, although he naturally adheres to his theory of the stopcock action of the arterioles, still I am not singular in believing in the existence of such a power; and Dr. Johnson must surely allow that the transudation of hæmoglobine, which is associated with high arterial tension, but absent in venous congestion, together with other facts brought forward in my paper and previous article, are strong proofs of its presence. They at least prove the occurrence of high pressure in the arterial capillaries; and he does not attempt to explain them otherwise, or to contradict them.

Dr. Johnson, however, not only mistakes my theory of the pathology of Bright's disease, but he lays down a rule and a challenge, to which I gladly respond. He says that "a true theory must be consistent with all ascertained facts"; and he then instances the high arterial pressure existing in cases of contracted kidney, and the small amount of albumen discoverable in the urine, as a condition that cannot be explained by my theory. Not only have I found my views to be "consistent with all ascertained facts", but I have solved by their assistance many problems which, when regarded from Dr. Johnson's point of view, are difficult of comprehension; such, for instance, as intermittent albuminuria, rupture of capillaries and appearance of blood in the urine, the immediate relief afforded by purgatives, the production of dropsy, and the transudation of hæmoglobine.

With regard to the slight degree of albuminuria accompanying contracted kidney, it may be explained as follows. The changes which precede and accompany cirrhosis of the kidney are peculiarly insidious in their commencement, gradual in their development, and prolonged in their existence. There is, therefore, at first a slight but gradually increasing want of relation between blood and tissue; this produces a correspondingly slight, but also gradually increasing, condition of high tension in the arterial system. This increase of tension is too slight and too gradual to produce an albuminuria; but it nevertheless, if allowed

to continue and to increase, produces a thickening of the arterial walls, a hypertrophy of their muscular coats, and a hypertrophy of the heart. I believe it is this condition (when accompanied by a particularly active excretion by the skin or bowels, affording relief to the kidneys) that gives rise to those changes described by Sir William Gull and Dr. Sutton as "arterio-capillary fibrosis", and stated by them occasionally to exist without marked change in the structure of the kidney. When at last this very gradual increase of pressure, which may require years for its development, reaches that degree which, in an acute case with healthy vessels, would give rise to a copious transudation of albumen, the walls of the capillaries and arterioles are so thickened that it is far more difficult to force through the albumen. For the same reason, the hæmoglobine is absent from the urine of these patients, in whom the high arterial tension is invariably well marked, but whose urine may be free from albumen. In such cases, slight constipation or chill (by arrested excretion) will produce that temporary existence or increase of albuminuria which is so characteristic of cirrhosis; or a more severe constipation or chill will immediately give rise to the symptoms of the acute form of Bright's disease.

Again, Dr. Johnson states that he has been unable to discover any hypertrophy in the muscular walls of the arterioles of the lung, produced by the existence of high pressure from the passive engorgement of these organs; but, on the other hand, I have constantly seen thickening and endarteritis of the pulmonary vessels, demonstrated by Dr. Moxon as the result of "strain" from high pressure in these vessels, due to this cause.

Dr. Johnson considers a similar "unity of action" to exist between the heart and muscular arterioles, as between the detrusor urinae and the sphincter vesicae, or between the muscular walls of the rectum and the sphincter ani. I, however, am unable to perceive the resemblance, for while, in the former case, according to Dr. Johnson, the heart and arteries are simultaneously contracting and acting in opposition, in the two latter instances, while the expulsors contract, the sphincters are relaxed, and *vice versa*.

Finally, Dr. Johnson has entirely misapprehended my theory of the production of the pathological changes; he has omitted the most important step in the process. Given the predisposing condition of blood-poison and high tension, from capillary obstruction, I do not believe that any acute kidney-change would be produced without the occurrence of another most important condition—namely, arrested action of one of the excretory organs, either bowels or skin. This throws increased work on (and produces, therefore, congestion of) the kidney, and subsequent cell-proliferation—a somewhat different sequence of events from that described by Dr. Johnson as entailed by my theory.

## THERAPEUTIC MEMORANDA.

### ON VALERIANATE OF AMYL AND ON AMYL ETHER.

A SHORT time ago, I mentioned in this JOURNAL the merits of a new preparation of valerian, viz., a spirit of valerianic ether. Notwithstanding that this preparation is less disagreeable to patients, as testified by those who have taken both, than the ordinary valerianic preparations, it has its drawbacks, the principal one being the diffusion of the smell through the apartment or ward when a dose is being taken, or through a shop when it is being dispensed.

It occurred to me that possibly, the valerianates of amyl or methyl might be more elegant than the ethyl preparation. Messrs. Southall undertook to prepare them for me. The methyl one is not much superior to its predecessor, but the amyl one is incomparably more agreeable. A spirit of the valerianate of amyl, to which a few drops of a spirit of acetate of amyl (known in commerce as essence of Jargonnelle pear), have been added, is, without any qualification or exception, not merely the most elegant known preparation of valerianic acid, but intrinsically an agreeable drug. All these spirits improve in odour by a little keeping. I find that, to some stomachs, ethers are repugnant, unless well diluted; and six or eight drops of the compound spirit are, generally speaking, sufficient for an ounce of water, and also an adequate dose. I think that the effects of the amyl valerian are quite as good as those of the ethyl preparation. This latter will be called, in future, valerianate of ethyl, instead of valerianic ether. The new preparation will be called compound spirit of valerianate of amyl (sp. amyl valer. co.), and will consist of one part of ether to nineteen of spirit. To two ounces of this are added twenty minims of a spirit of acetate of amyl, made in like proportion, viz., one in twenty. The simple spirit will also be procurable.

I take this opportunity of saying that, by my direction, the oxide of