citement returned, and she would then pass the day in repeating the same expressions. This state continued for about a month, during which time she frequently refused to take food, declaring that she was unable to swallow; so that she was reduced to a state of great emaciation. Then she became quiet and comparatively calm, but still very uneasy and distressed in mind. For the first time, she complained of pain, all over her body. Her tongue was parched and dark brown; her pulse rapid and extremely feeble. After remaining for about a week in this condition, she gradually and quietly sank.

Post Mortem Examination Forty Hours after Death.—April 15th, Weather not warm. Along each side of the falx cerebri, the membranes were closely adherent to each other, and firmly attached to the side of the falx and to the skull a little beyond it. The entire brain was very small and particularly globular in shape. The veins on its surface were full of dark-coloured blood, and several large clots were found in the longitudinal sinus. Beneath the arachnoid, a quantity of fluid was found in the sulci between the convolutions, which in some places were extremely atrophied. At the vertex, over a somewhat circular area of about an inch and a half in diameter, on each side of the longitudinal fissure, the convolutions were so atrophied, that the spaces between them were in some instances three quarters of an inch in breadth. It was over this space that the membranes were adherent to each other, as well as to the falx and skull. The quantity of fluid which ran out from beneath the arachnoid was about two ounces and a half. The substance of the hemispheres was not at all soft. When sliced, the grey portion presented its normal colour and consistence, and was not unusually vascular. The lateral ventricle contained a small quantity of fluid. The right optic thalamus was decidedly smaller than the left, and somewhat soft. The corpus striatum on the left side was decidedly small, soft, and altered in shape, being bent, as it were, upon itself. The pons and medulla oblongata were very small.

REPORT ON THE SUPPOSED PRESENCE OF LEAD IN AERATED WATER FROM SYPHON-BOTTLES.

By JOHN ATTFIELD, F.C.S.,

Professor of Practical Chemistry to the Pharmaceutical Society of Great Britain, etc.

IN a recent number of the BRITISH MEDICAL JOURNAL there appeared a letter, dated February 9th, 1874, and signed "M.D., F.R.S.," drawing attention to an examination of eight bottles of aerated water, each of which was stated to contain lead. The writer thought that the results of the examination should be made known to the medical profession and the public, he regarding such aerated water as a source of poisoning by lead. The Editor of the JOURNAL, commenting on the letter, suggested that, although the evidence as regards the presence of lead was strong, the metal was probably not present in all syphon-bottles of aerated water; thereupon the manufacturers of the metal fittings of such syphon-bottles (Messrs. Hayward, Tyler, and Co.) desired me to make an extended investigation of the subject, the results of which investigation I now describe.

Several specimens of aerated water taken out of syphon-bottles, of which some had been filled in London and others in the country, were first tested for metals generally—that is, not only for poisonous metals such as lead, but for iron and tin, metals of which cooking utensils and other vessels are made or lined, and minute amounts of which find their way occasionally into articles of food or drink, without, as is well known, doing the slightest harm to persons partaking of such food or drink. This preliminary testing showed, when the experiments were very carefully performed, that most of the specimens did contain a trace of a metal of some kind, but in such extremely minute quantity, that, whether harmful or harmless, could not be ascertained without further operations on large quantities of the water. About half a gallon of aerated soda-water was therefore made the subject of a single experiment. The still extremely slight amount of metal thus obtained was *not lead*; and further tedious experiments had to be conducted before its real nature could be definitively decided: *it was tin*.

While the experiments just mentioned were progressing, the water with which some of the specimens of aerated liquid were made was tested for lead or tin. Not a trace of either was present. The small metal ferrule in the syphon-bottles, with which ferrule alone is the aerated water in contact while in the bottles, was also analysed. It proved to be pure tin. The metal tap or valve forming the head of syphon-bottles was next subjected to examination. The aerated water is only in contact with this tap when the liquid is allowed to flow out into a drinking-glass, and therefore would not be likely to impart metallic impurity to the water; still, it is conceivable that by a culpable act of carelessness a tap might be so placed as to become corroded, and afterwards, without

being cleaned, be screwed on to a syphon-bottle. Neither of the taps that came under my notice gave any indication of ever having been so corroded. One tap, which had evidently been much used, was next carefully divided by a fine saw, and the whole interior thus exposed. Thorough scrutiny, by aid of a microscope, detected minute portions of a white powder adhering to the inner walls of the tap. Analysis of this incrustation showed it to be ordinary calcareous matter, such as commonly occurs in kettles and kitchen-boilers, and which is a common constituent of water used for drinking purposes. The tap itself proved to be pewter, the material of which metal drinking-vessels, such as publicans' pots, are made.

1. Some tin ferrules were placed in pure distilled water, in water containing the aerating gas, and in aerated water containing the carbonated alkali, generally present in aerated alkaline waters. Each of these liquids dissolved a trace of tin from the ferrules.

2. Into each of the three liquids named a tap was placed. Only in the distilled water was any metal from the pewter detected, showing that the aerated water of syphon-bottles cannot dissolve any of the pewter of the tap through which it flows when the vessel is in use.

3. A pewter tap, coated with silver inside and out, did not yield a trace of metal to either of the three liquids.

4. The three metals mentioned were exposed in the three liquids mentioned for one day and then tested, for two days and then tested, and for three days and finally tested. The results were as already indicated; namely, that only the harmless metal, tin, is slightly, very slightly, attacked by aerated waters—a trace, an insignificant trace, of the tin going into solution. A man would have to drink many glasses of such aerated water before he would have taken even a medicinal dose of a soluble compound of tin.

Conclusions.—1. The aerated waters of either corked bottles or syphon-bottles cannot, except by accident, contain lead. 2. Aerated waters in corked bottles may possibly contain in solution a trace of tin, derived from the manufacturer's mixing vessels—an amount smaller than that frequently found in food cooked in tinned saucepans. 3. Aerated water kept in syphon-bottles is likely to contain a trace of tin. 4. Makers of aerated waters may exclude even a trace of tin from the waters, not only by using manufacturing vessels silvered inside—a plan already extensively adopted—but also by silvering the tin ferrules of their syphon-bottles.

TEMPERATURE AS AN ASSISTANCE TO DIAGNOSIS IN SOME CASES OF ENCEPHALITIS.

By E. CHARLTON FOX, M.D., Redditch.

ON account of the difficulty of always accurately distinguishing between inflammation of the substance of the brain and inflammation of its membranes, and also because I believe that few cases of meningitis occur in which the brain itself is not, to some extent, implicated, I have, in these notes, used the term "encephalitis" in preference to "meningitis". Two years ago, while attending, in a large manufacturing town, a case of encephalitis (Mrs. B., aged 28), in which I took carefully the morning and evening temperature, I observed that the morning temperature was always higher than that in the evening. The following is the record of the case.

	Morning.			Evening.		
1st day	...	...	...	99.4 Fahr.	99.4	Fahr.
2nd "	...	...	99.8 Fahr.	99.0	99.0	"
3rd "	...	...	99.4	99.0	99.0	"
4th "	...	...	100	99.4	99.4	"
5th "	...	...	100.4	99.5	99.5	"
6th "	...	...	99.9	99.1	99.1	"
7th "	...	...	99.9	99.2	99.2	"
8th "	...	...	100.2	99.5	99.5	"
9th "	...	...	99.5	99.8	99.8	"
10th "	...	...	99.7	—	—	"
11th "	...	...	—	99.0	99.0	"
12th "	...	...	99.4	—	—	"
13th "	...	...	98.5	—	—	"

Convalescence was now established.

It struck me that if this rule were constant, it would afford a valuable assistance in the diagnosis of the early stages of encephalitis, in which, as the following case shows, a combination of circumstances and symptoms may lead to erroneous conclusions.

Shortly after the occurrence of the case of Mrs. B., I was called to see a little girl, aged 13, "in a fit". Her mother said that a few weeks before she had had a slight one. From her age and appearance I imagined that the fits were hysterical, and caused by uterine disturbance; but the appropriate remedies for such a case did no good, and the cerebral disturbance grew worse. One morning, calling to see her,

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.

By F. A. MAHOMED, Esq.,
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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 urinæ and the sphincter vesicæ, 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