the stomach, which even ice fails to allay, and I have seldom seen it vomited after the first dose or two. I have met with one or two instances in which it was found impossible to obtain blood from the veins, but where, shortly after a few doses of the medicine had been taken, bleeding was practised with success.

In the cases of simple diarrhoea without tenesmus now prevailing, I find the most immediate effect produced by a grain or two of quinine given with half a drachm of the muriated tincture of iron, in a wineglassful of water. With respect to the cramps and spasms, I have been in the habit of looking upon them as mere symptoms, only to be obviated by removing the cause; it would, however, appear from the observation of others, that, apart from the more fatal symptoms of suppressed arterial circulation evinced in convulsions; the cramps in the legs, &c., are closely connected with the contracted condition of the colon, and I should therefore feel inclined to try the effect of assafoetida injections.

I have now, gentlemen, only to thank you for the attention with which you have listened to my remarks. The treatment which I chanced to adopt on purely empirical grounds appeared to me more than usually successful, and I was led to enquire whether there was anything in the nature of the disease, or the remedies, that was explicable on known and acknowledged principles of physiology and therapeutics; the result was, that there existed a deficiency of facts by which to determine the question, and, till these were settled, it was impossible that all should agree on the treatment or deductions, at the same time it was evident that there were facts which, if generally admitted, would necessitate universal concurrence in a broad principle of treatment, to the exclusion of a vast amount of erratic practice, that might be termed a blind groping for facts on which to reason.

In my endeavour to ascertain what were the established facts, it appeared that beyond a rehearsal of symptoms there was not one universally admitted, save the constant existence of venous congestion,—contagion, the effect on the blood, the cause of death, the influence of particular treatment, were all subjects of hot contention, and the medical profession was divided into as many parties, and more, than there were sections in the materia medica. Under such circumstances it appeared that the weight of any single evidence could not possibly turn the scale of opinion, and that the only chance of arriving at a useful conclusion was to examine the disease in all its forms, as they relate to its cause, its course, its terminations, and its connection with other diseases. The result of this I have endeavoured to lay before you, in the reduction of the essential facts to six, not one of which is generally admitted, but if they be proved to be facts, the deductions from them are too evident to require comment. These which have previously been stated at length are shortly as follow:—

1st. That cholera is the effect of impression on the sympathetic system,—that its cause does not primarily affect the blood, is incapable of reproduction in the body, and, therefore, incommunicable.

2nd. That its effects begin by impaired vitality and disordered function of the capillary circulation.

3rd. That congestion of the thoracic and abdominal veins is the consequence of the above condition, and the cause of other symptoms.

4th. That exudation of the watery particles of the blood, whether through the mucous, or into the serous tissues, constitutes the natural mode of relief to this congestion.

5th. That cholera goes through certain diurnal stages, of which capillary disturbance is the first, venous congestion the second, more or less intermission or cessation of capillary disturbance the third.

6th. That the natural tendency of cholera is to go through these successive changes for three days, and then cease, or to run into consecutive or remittent congestive fever.

It appears to me that if these are facts (the establishment of which will depend on the observation and reading of every practical man,) a principle of treatment is inevitably established along with them, because they are facts on which the whole pathology of the disease may rest, and the whole course of treatment depend. If they prove otherwise, I have wasted your time and my own, in a feeble attempt to recommend a combination of quinine with iron, as only one of the numerous medicines said to be beneficial in cholera, but the action of which will then be totally inexplicable; and I shall owe you all much apology for inviting you to listen to the arguments by which I have attempted to substantiate as facts, what another process of reasoning may, perhaps, demonstrate as fallacies.

[At the conclusion of the lecture the thanks of the meeting were voted to Dr. Bell, on the motion of Dr. Bardsley, senior, seconded by Dr. Lyon.—ED.]

BRONCHITIS AND DEATH

IN CONSEQUENCE OF TWO TEETH BEING DRIVEN DOWN THE LARYNX.

By S. B. DENTON, M.D., Hornsea.

I was called to a Miss Fawcitt, from Sheffield, September 14th, 1848, who had been ill three days. The history she gave me was this:—Feeling tolerably well she went to the sea-side; while there she had an accidental fall; and found on getting-up that two front teeth had disappeared, (they were false, tied together in a gold socket,) they were not to be found about her person, or amongst the shingle. A cough almost immediately came on, which had continued to harass her much up to the time I saw her.

I found the poor sufferer labouring under an acute attack of bronchitis, and although her attenuated frame could ill spare one drop of blood, I durst not omit the application of a few leeches, with two or three emetic draughts of Antimonii Potassæ Tartras.

It was rather late at night when I saw her, and contrary to my request, she left at eight o'clock the next morning for her home, at Sheffield, I did not, therefore, see her again, but was told that she was much relieved.

I wrote to Dr. Corden Thompson, of Sheffield, (Miss Fawcitt's physician,) giving him the above account, and at the same time stating that I had no doubt in my own mind that the teeth went down the windpipe, which was the *mechanical* cause of the inflammation and suffering, and I feared would, (if it had not already done so,) terminate in death, and on a *post-mortem* being obtained, doubted not but the teeth would be found in some of the air-passages. I had not an opportunity of exploring the chest by auscultation, and therefore could not decide upon the exact spot where the teeth were lodged.

Dr. Thompson very kindly sent me the dissection report of this interesting case, which is as follows:—"From the previous history of the case, and the existing symptoms, I was led to conclude that the teeth lay in the right bronchus, near its issue from the trachea. The patient gradually sank. I do not dwell on the symptoms particularly; they continued of the same character as you witnessed; there were also evidences of hepatic disease, and she had a heart affection, of some standing. The teeth were found precisely where it was anticipated: they lay firmly impacted near the mouth of the right bronchus. I need hardly say there were marks of severe inflammation in the tube; some also in the left bronchus, but not extending in the trachea to the larynx; the left lung pretty natural in appearance; the pleura costalis recently inflamed, but without effusion; on the right side the lower portion of the lung was somewhat consolidated; there was inflammation, too, both of the pleura pulmonalis and costalis, with slight adhesions posteriorly, and a little effusion; the upper portion of the lung had a pretty natural aspect without, but did not collapse; it appeared to have kept distended by the impeded egress of the air having, no doubt, been fully dilated by the inspiratory act which forcibly drew in the teeth. The parietes of the left ventricle of the heart were thick and firm; the mitral valve was also much thickened and indurated. The liver at its fine edge was verging to scirrhus, with traces of recent peritonitis of its surface. The other viscera were sound. The head was not allowed to be opened."

I believe Mr. Wood attended the patient with Dr. Thompson, and took the same pathological view of the case.

POISONING BY THE BERRIES OF THE TAXUS BACCATA.

TO THE EDITOR OF THE PROVINCIAL MEDICAL AND
SURGICAL JOURNAL.

SIR,

There are very few cases recorded of poisoning by the berries of the *Taxus baccata*; indeed, many persons disbelieve in its poisonous nature altogether. Gray, in his treatise on "Pharmacology," says, "the berries have been thought to be poisonous, but they may be eaten." I believe they are not mentioned by Dr. Christison, in his valuable work on "Poisons." Drs. Copland, Guy, and Taylor, in their respective works, bear testimony to the extremely poisonous nature of the *Taxus baccata*. Mr. Lloyd's experiments and case, recorded in the last Journal, afford additional proof of the poisonous nature of the tree and its fruit; like

him, during many years practice, I have only met with one case, which I subjoin for insertion, if you deem it worthy, in the Journal.

I am, Sir, yours faithfully,

JAMES TAYLOR, M.D.

Castle Cary, Dec. 12, 1848.

Sunday, October 28th, 1838. Mary Baker, a fine healthy child, between five and six years of age, ate freely of yew berries just before going into church. About an hour after, during divine service, she fell from her seat and was instantly removed, in an insensible state, to her home. I saw her immediately; the surface of the body was cold; the countenance pale; breathing laborious and frequent; pupils very dilated; pulse feeble; convulsions, and vomiting. Having carefully examined the head, and finding it was not injured by the fall, I gave an emetic, and from what was ejected, it was evident she had eaten a considerable quantity of the berries; not the mucous part only, but the seeds, wherein I believe is the most active principle of the berry, for the mucous or fleshy part of the berry has been frequently eaten with impunity. As soon as it appeared the stomach had been freed of its contents, a purgative was given, and had the desired effect, but the child never rallied from the first. She continued in a comatose state, and died in *four hours* after eating the berries. An inquest was held, but no *post-mortem* examination allowed. I stated, in my evidence, I considered yew berries poisonous, and that the child's death had been occasioned by them, but I remember several on the jury were very sceptical on the point.

PROVINCIAL Medical & Surgical Journal.

WEDNESDAY, DECEMBER 27, 1848.

As the present number forms the concluding one of the volume for the year, and a considerable portion of its columns is necessarily occupied with the Index, we feel ourselves obliged to confine our observations within a very limited compass. Perhaps we cannot do better on this occasion than congratulate the members of the Association on the value and importance of the many original communications which have been published during the twelve months now approaching to a termination.

We have no wish to take merit to ourselves for this manifest improvement, but we cannot but consider it as a mark of confidence and approbation on the part of the members, and a testimony of the favour with which they are disposed to regard the Journal. At the same time we have the gratification of knowing, from many quarters, that while the communications