On the other hand, statements most definitely denying any such connections are not wanting. Dr. Wood, in his large American work on the practice of medicine, states—"It is generally admitted that ordinary unhealthy hygienic influences, such as confined and vitiated air, and all the accompaniments of poverty and privation, have no effect in favouring the development or propagation of the disease, which seems to be as prevalent and destructive in healthy situations as in the most unhealthy." Sir William Jenner, in his recent monograph on the subject, is most definite: he states "that the infection element in diphtheria does not require for its development any of the ordinary considered antihygienic conditions," and "that it is very doubtful if any of those antihygienic conditions favour its development or give it a more untoward course when it occurs."

For my own part, I must say that, in several outbreaks of diphtheria which I have investigated, I do not think that the presence of that disease in conjunction with prominent sanitary deficiencies could be fairly explained as a coincidence.

To aid in settling this important question, I venture to propose to the members of this society two questions.

1. Does diphtheria ever arise in consequence of sanitary deficiencies?
2. Are sporadic cases of diphtheria often met with?

In the discussion which followed the reading of this paper—

Mr. GILL (Wem) was decidedly of opinion that bad hygienic conditions could produce diphtheria, and stated that a few weeks ago he had investigated an outbreak of true diphtheria in his district, in which there was evident connection with various nuisances; and, from inquiry and personal knowledge, he was in a position to state positively that there had not been for a length of time any other cases in the neighbourhood.

Mr. S. WOOD (Shrewsbury) said that some years ago a remarkable series of cases came under his notice. The epidemic extended to several hamlets in one district, and in each locality he ascertained that the disease had first attacked the children attending one national school, and commenced immediately after the holidays. On investigation, he found that, during the holidays, the contents of a ditch receiving the overflow from some privies had been mixed with ashes and spread over the playground; on the reassembling of the school, this formed a ready material for making ovens and dirt-pies, and, as a matter of fact, the children had so amused themselves. It was impossible not to connect this circumstance with the outbreak of diphtheria.

Mr. MATHEWS (Nantwich) referred to an epidemic in which the cases were almost entirely confined to farm-houses, to which a quantity of night-soil had recently been removed from a neighbouring town.

The opinion of the meeting seemed decidedly to favour the theory of a definite connection between diphtheria and bad hygienic local conditions.

THE OBSTETRIC BINDER IN *POST PARTUM* HÆMORRHAGE.

By A. B. STEELE, L.K.Q.C.P.

Lecturer on Midwifery in the Liverpool Royal Infirmary School of Medicine.

My views as to the true value of the obstetric binder differ so materially from those of Dr. Boulton, that I feel bound to state them during the present discussion on the important question of the means of anticipating flooding.

I find it difficult to believe that the use of the binder in the way suggested, or, indeed, under any circumstances, can have much to do with the prevention of flooding, because I have never used it with that object myself; and my experience includes the observation of a very large number of cases of unassisted labour, and those attended by other practitioners and also by midwives, in all or nearly all of which the practice has been not to apply the binder until *after the uterus has contracted*. Its proper use is, in my opinion, to give moderate support to the relaxed condition of the abdominal parietes following labour. As a means of compressing the uterus, in anticipation of and during the presence of flooding, it is vastly inferior to the hand, and less trustworthy. In natural labour, if the last stage be either uninterfered with by "meddlesome midwifery", or judiciously managed, the uterus contracts and no flooding occurs, even although the patient may be subjected to reasonable movement. Such, at least, is my experience; and I regard flooding as an extremely rare accident, and, when it does occur, as due to a constitutional cause—to inertia from protracted labour; to errors in the management of labour, a common one being too great haste in emptying the uterus and removing the placenta; and in no case that I have met with could it be attributed to the want of

attention to the binder. If it were otherwise, surely those who, like myself, do not trust to it, and especially those, and they are not a few, who never use it, must have discovered their mistake long ago. Nothing, I imagined, had been more generally understood and agreed upon by experienced obstetricians, than that the essential point in the management of the third stage, where interference of any kind was called for (and this, I suspect, is more seldom than is often supposed), was to compress and follow down with the hand the uterus during the expulsion of the child, and to maintain such pressure until the uterus has firmly contracted. This manœuvre, I conceive, would be in no way assisted, but, on the contrary, somewhat embarrassed, by the application of a binder. I am aware that, in the Dublin school of obstetrics, great importance is attached to the value of the binder and compress as a means of sustaining contraction; but I have always preferred sustained hand-pressure until firm contraction is secured; and then, so far as my experience goes, the danger is over. In those extreme exceptional cases which I have ventured to call recurring hæmorrhage, in which, in spite of due compression, the uterus, after having once contracted, melts away under the hand again and again, I know no more effectual remedy than the intrauterine injection of perchloride of iron, as recommended by Dr. Barnes.

ELECTROLYSIS OF EXOSTOSIS OF THE EAR.

By THOMAS E. CLARK, M.D., C.M., Surgeon to the Bristol Royal Infirmary.

THE following case of exostosis, blocking up the meatus of the right ear, causing total deafness of that side, and its removal and cure by the continuous current, may be of interest. The patient was M. D., a girl aged 15. In the early part of 1869, a discharge from the right ear was noticed; some simple remedies were used, but without any good effect. In April, I saw her; and, upon examination, there appeared a mucous polypus filling up the meatus, and bathed in a thin purulent discharge. Tannic acid was ordered; and, under this treatment, the mucous polypus disappeared, leaving a hard substance, which proved to be an exostosis.

January 1871. I examined the ear, and found a bony growth nearly filling up the cavity of the meatus, allowing only the end of a director to be passed with difficulty and pain between it and the anterior wall. An application of tincture of iodine and liquor belladonnæ was locally used, and iodide of potassium and citrate of iron internally; and this was continued for some weeks without any beneficial result. I again examined the ear in July, when a director could not be introduced, and only a very small fissure existed between the exostosis and the anterior wall, the largest parts of the fissure being above and below the centre. The hearing was very indistinct and uncertain, the slightest moisture causing entire deafness.

I suggested the application of the continuous current to the exostosis, to promote its absorption and arrest its growth by blocking up the vessels of the periosteum. This was found too painful, and was given up, except under the influence of chloroform.

August 29th. The whole meatus was entirely blocked up by the exostosis; its surface was pinkish, and it felt of stony hardness. No probe could be passed in any direction by the side of the growth. She was totally deaf in this ear. She was placed under chloroform. I introduced three needles, two at the base, close to its attachment to the posterior wall, and one at its anterior edge. The former were attached to the negative pole, and the latter to the positive; and the application was continued for three minutes. This produced a large coagulum at the point of entrance of the needles, and rendered the whole tumour white. There was no subsequent irritation or pain produced. The battery used was Dr. E. Stöhrer's (six pairs of plates).

September 12th. Two needles were introduced while she was under chloroform; and in introducing them the resistance was very slight, whereas at the first application one of the needle-points broke on account of the great hardness of the growth. The connections were similar to the last, but were continued for five minutes. No ill effect followed.

September 26th. There appeared some diminution in the size of the growth.

October 4th. She was again placed under chloroform; and, upon examining the exostosis, it gave the idea of being loose; and with a closed scissors I pressed against its base, and it came away entire. Only a few drops of blood followed. There was no subsequent pain.

On examination of the growth, the front, or part facing the anterior wall of the meatus, was seen to be convex, with slight depressions above and below, covered with periosteum. The posterior part, or its

attached surface, was hollowed out corresponding to where the needles had passed or absorbed to a mere point; and thus it was that it broke away so readily.

October 9th. I syringed the ear. The tympanum was perfect. Hearing was also perfect.

THE MECHANISM OF THE ACCOMMODATION OF THE EYE.

By PRIESTLEY SMITH, Esq.,

Resident Surgeon to the Birmingham and Midland Eye Hospital.

In spite of much elaborate investigation, the mechanism of accommodation in the eye still remains, to some extent, a matter of speculation. At the recent annual meeting of the British Medical Association, a novel and ingenious theory was brought forward by Mr. A. T. Norton of St. Mary's Hospital, and to test the truth of this theory the experiment described below has been undertaken. I shall venture, however, before discussing the uncertain points, to state briefly the facts which are already established in connection with the subject.

1. Parallel rays of light proceeding from a distant point and striking the eye can be brought to a focus upon the retina. Divergent rays of light proceeding from a point only four inches distant from the eye can also be focussed upon the retina. The eye, then, has the power of changing its refraction as it views objects at different distances.

2. This power of accommodation depends solely upon changes of form in the crystalline lens. This fact is proved by the catoptrical examination of an eye during accommodation. A flame is placed towards one side of the observed eye, at a certain angle with the axis of the cornea; the observer's eye is placed towards the other side, at a similar angle. Three reflections or images of the flame are seen; the first being reflected from the cornea, the second from the anterior lens-surface, the third from the posterior lens-surface, or, more strictly, from the anterior surface of the vitreous body. If these images be viewed through a microscope, and if the person whose eye is under observation be directed to look first at a distant object, and then at a nearer object placed in the same line of vision, the second image is seen to move towards the first image, and also to become smaller. The movement indicates an advance of the anterior lens-surface; and the diminution of size indicates, in accordance with optical laws, an increased convexity of this surface.

3. Accommodation for near objects is produced and maintained by voluntary effort.

4. Accommodation for distant objects is a purely passive condition; and to this condition the eye always returns when the voluntary effort above-mentioned ceases.

5. The mechanism by which accommodation for near objects is produced is actuated through the third nerve. So far, no room for doubt remains: all is definitely proved. The next statement is almost as certainly true—viz.:

6. The chief agent in the mechanism is the ciliary muscle.

Now, in what manner does contraction of the ciliary muscle effect a change in the form of the lens? Several different explanations of its action are given by the best authorities; but it is proposed here to examine only the ingenious theory brought forward by Mr. Norton. It is briefly this. The veins through which the blood returns from the ciliary processes pass through the substance of the ciliary muscle; hence, when the muscle contracts, these venous trunks are compressed, and the highly vascular ciliary processes are thrown into a state of turgescence and enlargement, producing an increase of pressure around the periphery of the lens, and a consequent change of form in that body. In direct opposition to this view is Mr. Bader's description of these parts. At page 215 of his work on *The Human Eye*, he says: "The arrangement of the blood-vessels of the ciliary region renders it probable that, during contraction of the ciliary muscle, the influx of blood into the ciliary processes is impeded, and that they contain less blood. The efflux of venous blood from the processes is supposed not to be impeded, since these veins do not pass through the muscle."

While such opposite descriptions are given of the minute anatomy of the region, it appeared to me that the turgescence theory could be conclusively tested only by ascertaining whether the circulation of blood in the vessels is necessary to the production of accommodation in the lens. To ascertain this, a galvanic current was passed through the enucleated eye of a rabbit, while, by means of a phacoidoscope, the effect produced upon the image of a flame reflected from the lens-surface was observed. In overcoming the difficulties of this experiment, I received much valuable help from Mr. Lloyd Owen, surgeon to this hospital, and from Mr.

Frederick Maberley; and, before attempting to carry it into execution, I was careful to make myself familiar, by numerous observations with the phacoidoscope, with the accommodation-movements of the flame image in the living human eye.

The phacoidoscope consisted simply of a black wooden stage, upon which a quadrant was figured; a microscope of simple construction, magnifying eight diameters, and having a micrometer eye-piece; a gas jet travelling in a groove, and capable of being placed at any desired point of the quadrant; and a point of attachment at the centre of the quadrant for the apparatus containing the rabbit's eye. This latter apparatus (Fig. 1) consisted of two small brass cups, each soldered to an

Fig. 1.

upright support, and of such size that, when containing the eye, their rims did not quite lie in contact. One of the uprights being made of watch-spring, the cup could be drawn back when the eye was introduced. The wires from a Gaiffe's battery, producing a gentle interrupted current, were connected, one with each upright, so that the circuit was completed by the passage of the current through the eyeball. A rabbit, weighing 29 ounces, was stupefied with chloroform, and then received a hypodermic injection of 20 grains of hydrate of chloral. During profound anaesthesia, the left eyeball was excised, and at once dipped in olive oil heated to 100 degs., before being placed in the cups. This point is important, for, if it be neglected, evaporation quickly changes the tension of the globe and dims the cornea. By means of a heated brick, the temperature of the air around the holding apparatus was maintained above 90 degs. Fahr.

When the microscope and the flame were placed, each at an angle of 35 degs. with the axis of the cornea, the reflexions from the cornea and anterior lens-surface were admirably seen. To prove that these were the images required, and that neither was produced by the posterior lens-surface, it was only necessary to observe that both travelled in the same direction when the flame was moved. During the first minute or two after the removal of the globe, the galvanic current produced no movement either in pupil or image. Doubtless, the division of the ciliary nerves, during the operation, had caused a spasmodic contraction both of iris and ciliary muscle, for the pupil was small, and the lens-image occupied that position which it afterwards assumed only during galvanic irritation. The pupil recovered its mobility a little while, perhaps a quarter of a minute, before the image did so. Very soon, however, both became responsive to the current. Figs. 2 and 3 show the

Fig. 2.

positions assumed by pupil and images during non-stimulation and during stimulation. The reflection from the cornea is fixed, and brighter than that from the lens-surface. The vertical lines in the diagram show