the sac of which a fresh loop of intestine had passed and become strangulated. Here there were two conditions. First, the old gradually increasing rupture, of which the contents had contracted adhesions to the sac; and, secondly, the suddenly occurring second extrusion, which rendered immediate operation necessary. No doubt at all but Mr. Lawson Tait's procedure would liberate this latest coil more easily and readily than by opening the sac itself, but, this having been done, it would be impossible in this way to separate and reduce the whole previous mass; and it would be in the last degree unwise to leave that in *statu quo*, for thereby a repetition of the danger would be directly solicited. A combination of both operations, more or less modified to suit existing conditions, would best fulfil all indications. Mr. Bishop failed to see the great danger of a fresh incision in the abdominal wall, and nothing had surprised him so much as the utter absence in the debate of reference to a procedure introduced, he believed, by Mr. Greig Smith, and based upon Mr. Lawson Tait's method of perineorrhaphy, that of splitting longitudinally any tendinous edge or ring and uniting as a flat connective-tissue surface what before was a mere tendinous edge. Such reticence on the part of Mr. Greig Smith and Mr. Tait could only be due to that modesty which characterised all great surgeons. This procedure had been found by him to be eminently satisfactory in its results.

XI.—THOMAS H. MANLEY, M.D.,

Visiting Surgeon Harlem Hospital, New York.

DR. MANLEY said that without question, when in that class of cases described by Mr. Tait in women on whom operations were performed for the removal of ovarian tumours which were associated with inguinal extrusion of the bowel, when the peritoneal cavity was opened it was then a timely procedure to draw the bowel up, insert a few sutures through the columns of the ring, and there and then effect a cure of a reducible hernia. But it must be borne in mind that the analogy between this case and the ordinary variety of hernia was very slight; for in this instance, by displacing the ovarian cyst and thereby relieving intra-abdominal pressure, they removed the cause, which tended to originate and perpetuate the rupture; and no doubt in many of those cases reduction by taxis after ovariectomy with rest would often in itself effect a cure. In a general way he (Dr. Manley) said that ordinarily every hernia had a sac, for hernia as a pathological entity must have an investment, and this commonly was a fold of serous membrane; besides, this pouch almost invariably had intimate adhesions. Now, Mr. Tait's plan left the sac undisturbed; but he approximated the walls of its neck and brought the tendinous pillars of the ring into close apposition by sutures inserted from within the peritoneal cavity, a procedure which, as all knew, when applied in the usual way, was as a rule quickly followed by relapse. He (the speaker) maintained that if this plan of operating had been recommended for the treatment of strangulated hernia by a less eminent authority, it would scarcely seem to merit the serious attention of experienced surgeons, for in this class of cases it certainly could have no application whatever, except under very rare circumstances. In conclusion, he wished to be permitted to cite a case which recently occurred in his hospital service in New York, which would go to prove that had the method under consideration been adopted, success would have been impracticable; besides, he wished to say a word on the inestimable value of local cocaine anaesthesia in those cases of strangulated hernia where the patient was in profound shock when presented for operation, and pulmonary anaesthesia was badly borne. The patient was 25 years old, had obstruction of the bowels for nine days, and stercoraceous vomiting, with marked collapse. Strangulated femoral hernia was diagnosed. An operation was performed under cocaine, a 1 per cent. solution being administered hypodermically. Immediately before the cutaneous incision was made the surface was doused with a jet of an ice-cold siphon of Vichy water. He came down on to a mass of omentum with old, tough adhesions in every direction. The extension had no peritoneal investment. The stricture was divided, and the compressed intestine relieved. The patient made a rapid recovery. Now he (Dr. Manley) asked how efforts at reduction by tension from within might have terminated in this case with the protruded mass by chronic inflammatory

exudation bound to everything, besides the additional gravity attendant on the median abdominal incision?

XII.—JOHN WARD COUSINS, M.D., F.R.C.S.,

Senior Surgeon Royal Portsmouth Hospital; President of the Section.

THE PRESIDENT expressed the thanks of the Section to Mr. Tait. He remarked that the original communication they had heard referred only to some kinds of hernia, as clearly stated by the author. All surgeons would admit that in many complicated cases the method must prove invaluable for diagnostic purposes. The introduction of two fingers into the abdominal cavity would also be extremely useful in many manipulations, and some would occasionally be impossible without it. He considered that Mr. Tait had clearly proved the great advantages of his incision in many umbilical hernias. Of course, in a large number of cases of single strangulation it would be unnecessary, and in complex cases of inguinal hernia with adhesions within the sac traction from the abdominal side would be dangerous, and if a necrotic state of the intestine was present it would certainly end in rupture. Every case had to be dealt with on its own merits. The subject was vast, the varieties and complications were many, and with them would rest the responsibility of utilising Mr. Tait's original suggestions.

Mr. LAWSON TAIT'S REPLY.

MR. LAWSON TAIT, in reply, said that he had secured a distinct success in so far that he had the support of such good authorities as Mr. Greig Smith and Professor Bennett May in his view that any increase in the tendinous aperture was a thing to be deprecated. He could not help Mr. Keetley's scepticism about the difficulty in getting the fibrous aperture to close. The cases occurred and did exist, and could be exhibited, in which the tendinous aperture had been closed for years after the operation of ovariectomy, without any recurrence of the hernial protrusion. Whatever the theory might be, the facts remained the same. He was somewhat shocked at Mr. Jordan Lloyd's conclusion that he had made a proposal that the two proceedings—direct operation and the operation of median section—should be antithetical. It was very unlikely that either one or other should be applicable to all cases. Such was not his view. As to the alleged difficulty of withdrawing protruding intestines through the ring, all that Mr. Tait had to say was that the objection was purely theoretical; in practice it could be done perfectly well. So also the theoretical difficulty in dealing with adhesions, raised by Mr. Jordan Lloyd and Mr. Barling, was one which in practice did not exist, for the adhesions were purely omental and they could be torn with a little roughness but great readiness. He thanked them for their patient listening to his paper, and particularly was he grateful to the President for his gracious words of praise. Not a word in the discussion had occurred to which he could take any objection, and that in his experience through life had not been a uniform one. There was evidently a spirit of conservatism in this as in other matters, but he had no doubt that on fuller consideration the proposal would receive a substantial and unprejudiced trial.

ON THE TREATMENT OF PNEUMONIA.

Read in the Section of Therapeutics at the Annual Meeting of the British Medical Association, held in Bournemouth, July, 1891.

By SIDNEY COUPLAND, M.D., F.R.C.P.,

Physician to the Middlesex Hospital.

It seems presumptuous on my part to venture to discuss before a gathering like this the treatment of an affection so familiar as acute pneumonia, more especially as it is a question upon which there is comparatively little to be said that is either novel or open to debate. It is, however, mainly in the hope that my brief exposition of the subject may elicit the experience of others that I feel encouraged to deal with so trite a topic, and I trust that my lack of originality may be overlooked in pursuance of this laudable object.

In considering the treatment of any acute disease, and especially of a fever like pneumonia, it is impossible not to be struck with the change that has been wrought in cur

methods consequent on that which has taken place in our views of the essential nature of such affections. It would seem as if, with clearer insight on this point, the more have our therapeutics become restricted, and the more humble we have become in our opinions as to the advantages of any particular treatment. This applies in special degree to the limitations imposed in respect to drugs, since in addition to truer conceptions of morbid processes, we are met by the increasing knowledge of the action of medicinal agents. Hence we are fast emerging from an empiricism which, guided in some degree by theoretical fancies, and to no small extent by traditional usage, paid little or no regard to the individual circumstances of the case, and less still to the pathological changes that accompany the progress of a disease. The subject of the treatment of pneumonia from its historical side affords ample illustration of this. It has long stood as a test example of the powers of the healing art, and to this day we hear from time to time of the paramount virtue of certain remedies to influence its course. It was around this disease that the battle which ended in the greatest revolution in medicine that the world has seen chiefly raged; and which led also to the introduction of the theory that the type of disease was altered in apparent justification for the abolition of a heroic therapeutics. It is, however, far from my intention to revive the memory of the great blood-letting controversy, and I am willing to admit that the reaction against the practice went beyond the point of what may be considered justifiable. I will merely content myself with the remark, which will I presume be universally accepted, that the routine practice of repeated and copious venesections in the treatment of acute fevers and inflammatory diseases has no rational basis.

It is well to remember, before acknowledging the superiority of any particular line of treatment in acute diseases, that there are several grounds for fallacy in our conclusions thereupon, and it is largely owing to these being disregarded that so many different remedies have been vaunted as being most efficacious in controlling or arresting the natural progress of the affection. I may briefly refer to a few of these considerations which should always be borne in mind in dealing with pneumonia.

1. Although the pulmonary inflammation is the outward and visible sign of its morbid process, I would place as the most essential characteristic of pneumonia the state of fever which runs a fairly definite course, but is very variable in intensity and duration, which mostly ends as abruptly as it begins, and the issue of which is far more favourable in the young. It can hardly be otherwise than that any inferences derived from the observation of such a disease under particular lines of treatment may be very misleading. We know that under the most varied methods the crisis still occurs in a capricious and often unexpected manner, and we have yet to meet with means whereby we can interfere to hasten its occurrence. No one can predict in any given case that this abrupt termination to the fever will occur as early as the third day or as late as the tenth, or be still further delayed; whilst we are sometimes baffled by a recrudescence with signs of fresh involvement of lung, or by a prolongation of the febrile state from the implication of other parts or organs in the inflammatory process. Theoretically it might appear feasible that we should be able from our resources to influence the course of the disease—to prevent, for instance, the stage of pulmonary engorgement from passing into that of hepatisation—but for my own part I do not hesitate to say that no sufficient proof has yet been afforded of any such power to arrest or abort the pneumonic process; and that when such arrest seems to have followed our interference we are wrong in attributing it to the latter. As illustrations of methods which have for their avowed object the subduing of the intensity of the lung inflammation, mostly by their sedative and depressant action on the circulation, may be cited the use of such drugs as anti-mony, aconite, and veratrum. These have been, and some are still being, strongly advocated; but it is surely not determined that any one of them has produced such an effect on the general course of the disease as to justify the routine employment of remedies that are powerful for evil as well as good. I have no right to criticise their employment, since I have never felt justified in prescribing them, at least in doses sufficient to produce any marked effect; but I may say that as

much good can be effected by treatment directed from another standpoint and, may I add, perhaps less harm. To afford satisfactory proof of the efficacy of such measures would require not only a large number of instances, but the inclusion of cases presenting every type of the affection, and without any age limitation. To do this thoroughly would entail on the part of the practitioner such a degree of confidence in the benefits of his treatment as to amount to rashness, for the prescription of such depressants to the aged and enfeebled would be hazardous in the extreme, whilst to limit their administration to the young and robust would prove little beyond the natural powers of resistance to lowering measures.

2. Another fact in the history of acute pneumonia may be mentioned as likely to vitiate the statistical results of any line of treatment, especially as regards mortality. This is the undoubted variability in the intensity of the disease at different periods. Here, too, pneumonia exhibits its kinship with other specific fevers, and it is particularly marked when it occurs in epidemic form. We have witnessed during the past few months in connection with influenza a more malignant type of the disease than is commonly met with; and I believe that if material existed for comparison between the relative mortality from pneumonia during this year's outbreak as compared with that of last year, it would be found that there was a great difference between them in this respect. Such variations can hardly be explained except on the hypothesis that at one time the pneumonic virus is more potent than at another, for it is not easy to discover any other factor which might be supposed to account for them. Moreover, it is well known from hospital experience that the mortality from pneumonia varies from year to year, sometimes far exceeding, at other times falling far below, the average. Many an erroneous conclusion as to treatment has been founded on the imperfect data derived from a too restricted period of observation.

3. This brings me to the third objection to the adoption of a narrow therapeutics in acute diseases, namely, the impossibility of continuing the experimental method for a sufficient length of time and under varied circumstances, so as to arrive at a reliable result. In pneumonia the tests of efficacy are mainly these two: Does the treatment materially shorten the duration of the disease, and does it diminish its mortality? Of these, the first is perhaps the more important to determine, since the second is governed by so vast a number of special factors beyond those inherent to the disease itself. It would, however, require a large accumulation of careful observations to determine the first point, and, judging from the records of the past, it may be doubted if it be possible to collect such a body of facts as will carry conviction.

4. Lastly, the greatest obstacle to the conduct of a therapeutical experiment on a large scale is the fact that it must necessarily entail a neglect of the individual case. This objection applies to all routine measures, and is one which cannot be disregarded. For, although we may lay down general principles to guide us in the treatment of disease, we must always reserve to ourselves full liberty of action to vary its lines in presence of circumstances which depart from the ordinary conditions. Systems of treatment therefore become less and less binding, for it is not only the disease that has to be treated, it is the individual organism whose functions have become deranged by the action of what we term morbid agents.

I feel that in saying this much I have been only uttering truisms, which I might well have left aside were it not for an ingrained belief in the efficacy of certain methods which tend to become stereotyped amongst us. Nor in what I have now to say in support of a line of treatment which seems to deserve the name of rational do I wish to imply that there is no other line of practice open to us. I would only say that the pursuance of these principles seems to me to obviate the recourse to measures involving the administration of powerful drugs without commensurate advantages. It may be that there has been no very marked lowering of the general mortality, but at least it may I think be claimed for the "restorative" method introduced by Professor Hughes Bennett that it was more in accordance with our present knowledge of fever and that it affords a further proof that in the treatment of pneumonia we must have regard to the pyrexial state quite as much as to the lung inflammation. Indeed, in every case

of acute pneumonia there are these two main indications to be met by treatment—the systemic effects of the virus and the local pulmonary lesion. That pneumonia is something more than inflammation of the lung and that the degree of constitutional disturbance present in any case is not proportionate to the extent of the local lesion are propositions in support of which many facts might be cited, but which are doubtless within the experience of all. The doctrine of “pneumonic fever” advanced, among the first, by the late Dr. Austin Flint necessarily requires the postulation of a specific virus with all that follows therefrom, and this doctrine has been materially strengthened by the researches of Friedländer, Fraenkel, Weichselbaum, and many others on the micro-organisms of pneumonia and the association of many other inflammatory affections with this of the lung. Both clinically and pathologically, then, it seems to be true that pneumonia is to be ranked among the specific infective fevers, and, this being so, it remains to be seen how far its treatment must be modified by this view of its nature.

Now, failing the employment of any specific remedy which can neutralise or destroy the virus, our treatment of every acute febrile disease must be largely limited to the support of the natural vital resistance of the body to enable it to withstand the effects of the poison, and, in addition, to diminish so far as may be the lesions incidental to the pyrexial state itself. In pneumonia, as a rule, the pyrexia, although high, is mostly comparatively brief, so that it may be questioned whether the nervous symptoms and the more formidable cardiac asthenia which constitute the greatest dangers of the affection may not be partly, or perhaps mainly, due to the action of the virus. On the other hand, the remarkable change from a state of imminent danger to one of comparative safety that ensues on the crisis would point to the pyrexia itself being intimately associated with these grave symptoms, and justify us in treating the fever as such in the hope that some of these effects may be prevented. If the fever *per se* is not to be regarded as a source of danger, its occurrence affords at least an index of the continued operation of the virus, and the case is often most grave just before the febrile process comes to a stop; nay, even the perturbation of the crisis is itself sufficient to quench the last spark of vitality in the patient. Recognising this, it is plain that one main line of treatment must be that which affords nutriment in a form easily assimilable, and given with a sufficient frequency to maintain strength whilst the febrile process lasts; nor is there any need at the present day to dwell on this, which is the cardinal point in the treatment of all fevers, and has been so since the time of Graves; but linked with this comes the important question of the administration of alcohol. The value of alcohol in pneumonia depends, I believe, as much on its utility as a temporary food as on its action as a cardiac stimulant, but its prescription must be guided by the same rules as govern its administration in all fevers, and have reference to the signs of exhaustion and cardiac failure that may be present. Remembering that we have to deal with a fever of limited duration, and knowing the importance of maintaining the strength beyond the crisis it may be sometimes necessary to give very considerable doses—as much, for instance, as 12 ounces of brandy in the twenty-four hours—but one would reserve it until such indications arise, and not prescribe it as a routine, merely because the patient has pneumonia. There are many cases which neither require alcohol at any period nor are benefited by it; but to discard it altogether on this ground would be as rational as to accuse it of causing the mortality from the disease, because the fatal cases are just those which have probably had the largest quantities of alcohol administered to them. I confess I do not know what we should do without this drug in some cases of pneumonia. The more diffusible stimulants, as ether and ammonia, are not equivalent to it, although they have their place as adjuvants in circumstances of marked cardiac failure. In such conditions, too, the prescription of digitalis, or of caffeine, may be of benefit, and cases are recorded where the timely hypodermic injection of such drugs has apparently maintained the heart's action over the critical period.

In addition to these restorative and stimulant measures, the object of which is self-evident, it becomes a question how far it is advisable to employ means to mitigate the intensity of the fever itself. For, although as I have just said, it is

by no means certain that the cardiac enfeeblement is solely due to the febrile state, there can, I think, be little question that it is materially influenced thereby. In a long continued fever like typhoid, there is every reason to believe that the cardiac degeneration, of which there is clinical and pathological evidence, is directly related to the pyrexial condition. Still that the briefer fever of pneumonia is adequate to induce profound muscular changes is shown in the fact that one of the best marked instances of Zenker's degeneration of the recti abdominis muscles that I have seen occurred in a subject of pneumonia, who, in leaping out of bed in a fit of delirium, ruptured both the degenerated muscles. I am bound to state, however, that in many cases of fatal pneumonia the heart muscle has shown no manifest signs of degeneration. At any rate, it is plain that where the heart is already enfeebled, as in the alcoholic or aged, the subjection to a week's high fever is likely to still further impair its functional capacity.

It must, I think, be admitted that the antipyretic treatment which has proved of such signal service in typhoid fever is also of distinct advantage in pneumonia, quite apart from cases of hyperpyrexia, about which there can be no doubt as to its merits.

The choice of means for carrying out this treatment is considerable. For my own part, I incline rather in favour of the application of cold as against the administration of drugs. The pneumonic patient should be very lightly covered, and the value of free exposure to air was pointed out by the late Dr. Handfield Jones. He may be exposed to the air beneath a cradle covered by a sheet—a plan which will be likely to ensure a slight but appreciable reduction of the temperature of the body. Of all methods I have myself most experience of that of cold compresses, which were recommended by the late Professor Niemeyer, at a time when the use of poultices was considered almost indispensable in every acute inflammation of the respiratory organs. Such compresses are mostly quite as well borne as poultices, and are by some considered to have a direct influence on the inflamed organ. This is, I think, an open question, and it may not be absolutely necessary to apply them to the affected side of the chest, since their main effect is no doubt that of mitigating the pyrexia. One rather grave objection to their use is the necessity for their frequent renewal, which disturbs the patient, and for this reason, if for no other, the plan introduced by Dr. David Lees of applying ice bags over the affected lung is to be preferred. Dr. Lees claims for the ice bag in some cases a very considerable control over the extension of the inflammatory process, but here again I would rather attribute the somewhat striking results that he records to the general antipyretic action of the application.

Amongst many other methods of applying cold the most efficacious is undoubtedly the immersion of the whole body for ten minutes in a bath at a temperature of 85° F. This may be safely carried out in pneumonia as in typhoid fever, but it is, except in cases where the range of temperature is hyperpyrexial, seldom necessary to have recourse to a measure, the practical difficulties of which are considerable; nor, so far as I can judge, is the effect of the bath so lasting in pneumonia as in typhoid. It may, however, be taken as certain that this subjection to the influence of cold does not in the least increase the inflammation, or expose the patient to the risks of bronchitis.

Medicinally, we possess in quinine, salicin, antipyrin, acetanilide, phenacetin, and similar drugs agents whose antipyretic properties have led to their free prescription in fever. That they have remarkable power in reducing temperature is undoubted; but respecting their employment in acute pneumonia it is necessary to speak in guarded terms. Of all fevers, that of pneumonia would seem the most refractory to their action, and we may thus be led to administer very large amounts, which are not without risk to the cardiac power, before any appreciable effect can be observed. Quinine, which in small doses has but slight effect in this direction, is at the same time the safest to employ, and it alone may be necessary if the cold applications are well borne. As to the others, I will content myself with merely expressing my preference for the treatment by cold to that by antipyretic drugs.

In thus placing in the forefront the treatment of pneumonia

as a fever I would not be thought to ignore the treatment of the lung condition, which may be so extensive as by itself to threaten life. I have already expressed my preference for cold compresses over poultices as the local application to the chest, and subsequently, when the physical signs indicate the persistence of pleurisy, counter-irritation is of service. For the early stitch, which is probably pleuritic in origin, nothing is more serviceable than the application of three or four leeches, and I can hardly recall a case where they have failed to give relief. But as an alternative the local hypodermic injection of morphine may be employed. The cough of pneumonia in cases uncomplicated by bronchitis is seldom very troublesome, but if it is then it may be allayed by medicated inhalations—for example, of eucalyptus—rather than by opiates. When there is great involvement of lung, and the dyspnoea and cyanosis, together with the physical signs of such wide implication and of a labouring right heart, then venesection is distinctly indicated, and may be freely performed with much benefit. Indeed, it is only in such circumstances that venesection, once the routine measure for the treatment of pneumonia from the onset of its symptoms, can rationally be had recourse to. In such circumstances, also, the inhalation of oxygen has been successful in prolonging life until the resolution of the inflammation has set in. Mostly, I fear, the case in which one feels impelled to resort to such a measure is in desperate straits, and little aid can be rendered by it. Expectorants are not needed unless the case be much complicated with bronchitis, when, too, one must abandon the cold compress in favour of the poultice.

To enter into full detail concerning the treatment of all eventualities, or even of the various types of the disease, would occupy too much of your time, with little profit. It would involve the consideration of many a disease in which pneumonia occurs as a complication, as well as of such of its direct sequelae as empyema and pulmonary gangrene. I hope, however, that my reticence in this matter will not prevent our hearing the experiences of others on any point that may occur to them. Especially should I like to invite an expression of opinion upon the best measures to adopt in dealing with pneumonia complicated by delirium tremens, and as to the propriety of giving opiates in these circumstances. The gravity of the prognosis in cases having this conjunction makes it all the more incumbent on us to agree as to the safest mode of dealing with it. If there be no disease of the kidneys, nor any marked bronchitis, I should myself employ morphine in preference to chloral or the bromides, and perhaps hyoscine is to be preferred to morphine, if it were not that its effect in delirium tremens is sometimes disappointing.

In conclusion, I feel that I have but touched upon the fringe of a wide subject, in presenting in merest outline the principles which it seems to me should guide us in dealing with a disease like acute pneumonia. It may be said that after all this is nothing but "expectancy," and the criticism may be a just one. Still I would ask whether, in attempting anything more heroic or striking we do not deceive ourselves, and whether it is not true that in acute disease our rôle is really that of one who, aware of the possible eventualities and watchful of the indications of failing powers, strives to render assistance to the natural forces and to bring their efforts to a successful issue in the combat with morbid agencies. To claim more than this for our art is to magnify our office, and lay us open to receive unmerited credit or discredit according to the issue of the case. To claim less would be to abrogate our function entirely and leave all in the hands of Nature.

Dr. SANSOM concurred with the author in his view as to the comparative inefficiency of drugs in the treatment of pneumonia. On the other hand his experience of the treatment of pneumonia by means of cold applications was exceedingly encouraging. He insisted, calling attention to the analogy with the cold treatment of typhoid fever and the important statistical information recorded in regard to this disease, on the importance of the early routine practice of cold, preferably ice applications, and hoped that members of the Section would carefully try the plan and record their results.—Dr. BARRS referred to the treatment of insomnia falling short of

actual delirium, and expressed his preference for sulphonal in the management of this symptom. He also expressed his strong belief in the antipyretic treatment by means of exposure, sponging, icepacking, etc., in preference to drugs.—Dr. HUGH WOODS said, with regard to the drug treatment of pneumonia, he could not altogether agree with Dr. Coupland as to the uselessness of drugs and especially as regards digitalis, which he had found of very great value in pneumonia if used early enough. This view was founded not so much on the results of a very long series of cases as on the observation of individual cases, in which he had found the blood disappearing very soon from the sputa under the influence of digitalis, and reappearing forthwith if the administration of the drug was prematurely desisted from.

MEMORANDA: MEDICAL, SURGICAL, OBSTETRICAL, THERAPEUTICAL, PATHOLOGICAL, Etc.

STERTOR AND POSITION.

THE following case is such a striking illustration of the really practical value of Dr. Bowles's views on the treatment of unconscious conditions by position, as recently published in his work on *Stertor*, that I have thought it worth recording.

A. C., aged 2½, a well-nourished child, but with a heavy hydrocephalic head, was seized with convulsions early in the morning. On my arrival, half an hour later, he was cyanosed, frothing at the mouth, had violent twitching of the facial muscles and of the limbs. The pulse was full and laboured; the respirations could best be described as jerky attempts at breathing accompanied by pharyngeal stertorous sounds. The child was sitting in a semi-upright position on its mother's knee, with the chin bent down on the sternum. On being placed on his side on a bed, the stertorous sounds became palatine in character, and ceased entirely as the chin was raised from the sternum; at the same time a large quantity of mucus and saliva commenced to dribble from the corner of the mouth, and the breathing in a few minutes became free and easy. On auscultation of the chest at the moment of being placed on his side loud sonorous and mucous râles were heard all over both sides. At the end of twenty minutes the child had the appearance of one sleeping peacefully; there were no stertorous sounds, the pulse was soft and natural, the breathing free and easy, the usual healthy colour of the child had returned, and all the abnormal sounds had vanished from the upper lung, though some were still heard in the lower. Once the chin fell down again on to the sternum, when all the symptoms of suffocation recurred and the child nearly died before it was rectified; but the urgent symptoms again disappeared on placing the child in the proper position. He slept for two hours, then became conscious, took food, and finally made a good recovery.

Folkestone.

PERCY G. LEWIS, M.D.

HYOSCINE HYDROBROMATE IN CHRONIC MANIA.

Mrs. F., a widow, aged 62, who has two uncles and an aunt insane, recently came under my care. She was then in a very noisy, restless, and excited condition, constantly chattering or shouting, and complaining of various vague pains, the exact position of which she was unable to locate definitely, though she most frequently referred them to her head. She declared that she could not remain quiet, and was perpetually either pulling at the bedclothes, moving about in bed, or making faces. If left by herself for a few moments she got out of bed and wandered about the room in an aimless manner, chattering to herself. Only with difficulty could she be induced to allow herself to be dressed, and when dressed she would attempt to take her clothes off again. She slept but little and at irregular intervals.

The history given was that she had been very much in this condition for the previous five years, but that of late she had become worse and her strength less. She informed me that medicines never had any effect on her, and her relations stated that she had taken a large quantity of medicine from various sources without deriving benefit.