cision. Five cases died; two without any other measures being adopted, and three after subsequent incision. It is important to add, that of the five deaths two were cases of *serous* effusion at first. In all the cases, therefore, aspiration failed to cure. Aspiration is exceedingly useful in some cases as a means of temporary relief; in others, as a means of permanent benefit by allowing of contraction of the abscess sac and expansion of the lung; in all cases, for purposes of exploration; but I have seen no case of cure through its agency alone. At the same time, one cannot ignore the fact that others have obtained much success and cure by means of it. This has notably been the case with Dr. Bowditch of Boston. Notwithstanding, I wish to say that aspiration is not so successful, or simple, or harmless, as some would have us imagine; at least, this is so in its application to children. The pus is often very thick and will not run; at others, there are loculi formed by adhesions and the chest is not drained; at others, undue suction is exercised upon the lung, and air is drawn into the pleura through the weakened pulmonary wall. It is possible that in the latter way, the operation has helped to convert a serous into a purulent effusion: a matter of no slight importance.

Simple paracentesis was practised in eleven cases. Eight required subsequent drainage by other means; in another, a fistula formed; and one left the hospital with fluid still in the pleura. Four of the cases died; one of tuberculosis, one of diarrhoea (amyloid disease), one of pneumonia, and the cause of death in one is not stated. This is an operation which ranks with aspiration in its action, and some think highly of it as a curative agent, but I should say the same with regard to it as to the other: it is but seldom that a cure results. Now this is one of the points to which I would especially direct the attention of the meeting, and I make this somewhat dogmatic statement because I wish to bring out, if there be such, figures which tend in any way to show that simple tapping cures empyema sufficiently often to allow its use in the face of certain risks. Some say that though not likely to be followed by a cure in adults, simple paracentesis and aspiration are successful in childhood. That statement was made in one of the medical journals the other day. Will any one give us some facts on this point? I must confess to being biassed on this question by a former surgical experience. It has always seemed to me that, putting aside the special case of empyema, a very unfavourable one for a cure, and taking that of acute or chronic abscesses in the extremities or external parts, there is but a small amount of evidence in favour of abscesses disappearing after tapping alone; they require subsequent incision. If this be so with superficial abscesses, where all is in favour of the apposition of the abscess-walls, and many other points can be attended to, *à fortiori* must we expect that the same treatment will fail in the case of empyema.

The aspirator or trocar is also, I believe, useful in coaxing the pus to point spontaneously. Cases which point thus without interference seem to run less risk than others in which it is necessary. Eleven cases are found in our seventy-seven, and no death. It is true that only one cure was effected, a fistula remaining in the others. Still, a certain proportion of these recover after a time, and, any way, a fistula is better than death. It is not easy to say why cases which point of their own accord are less fatal than others, but I believe it is because the pus finds a free exit without admitting air to the pleural cavity. In most cases, treated surgically, the pus becomes offensive sooner or later, and in so doing, adds immensely to the risk. This is a point which is strongly suggestive.

Lastly, we have to examine three methods of treatment which profess to drain the chest completely. These are subaqueous or syphon drainage; drainage without any precautions for excluding air; and free incision of the chest.

The advantages of the *syphon* plan are obvious. It is simple in performance; needs no cutting; the soft India-rubber tube is so well tolerated by the skin and soft tissues that, under favourable conditions inside the chest, pus can be drained away for weeks without any decomposition; while, if it be thought advisable to apply any local stimulant to the pleura or to wash out the chest, either can be done most readily. Then what do figures say upon it?

The number of cases treated was	31
Cases drained thoroughly	7
The drainage failed more or less in	18
A fistula formed subsequently in	2
Deaths from complications shortly after the operation	3
Doubtful	1

The eighteen cases of imperfect drainage include several which ultimately did well; though, as far as can be seen, the result cannot be attributed to the method of syphon drainage. Thirteen died; but of these, three may be deducted as succumbing to causes independent of the present inquiry. Thus there is a mortality of ten in a total of twenty-eight, or 35.7 per cent. This death-rate is not higher than

that given by others as the rate for all cases of empyema taken together. But since the operation in question is not successful in the performance of drainage, it might reasonably be hoped that any plan working better would lead to more favourable results. The reasons of its failure are chiefly those which tell against aspiration, paracentesis, or even incision, viz., that the pus is often curdy and will not run, and that adhesions form and subdivide the cavity to be drained. Under these circumstances, it is difficult to drain the chest by any method.

Incision embraces twenty-four cases; seven recoveries, six with a fistula remaining, three with chronic lung-disease, and eight deaths. This gives a mortality of about 33 per cent. This is no great improvement upon the syphon plan, notwithstanding that there would seem to be more likelihood of its accomplishing complete drainage. That it has not produced better results is, I believe, in large measure due to a want of care in making the aperture in the chest as low as possible, and then, too, one is apt to assume, when the incision has been made, that the aperture is necessarily free enough. The approximation of the ribs, however, reduces even a large external opening to quite small dimensions, and it is generally necessary to have some short drainage-tube placed between the ribs. Then there is the atmospheric difficulty, the barrel requiring a vent-peg before the fluid will flow from the tap. It was to avoid this trouble that the original drainage plan of Dr. Morgan was adopted, that of an opening and counter-opening, a tube being passed in at one point and out at the other. This is still looked upon by many as a good method of treatment, and it is, I believe, from what I have seen, a really effective mode of drainage. Our records give no numerical results sufficient to enable one to say much upon its value. It has only been practised five times, with three recoveries and two deaths; but I think it an objectionable feature of this plan that there must be a tube inside the chest for a long time, and quite as free drainage may be ensured by one incision and the insertion of a short tube. This is the plan that I should now usually adopt; but occasionally no plan seems to be successful, and it becomes necessary, as some foreign writers and my colleagues Dr. Frederick Taylor, Mr. Howse, and Mr. Bryant, have found, to saw across and remove a piece of the rib to secure a sufficient outlet for the pus. In all cases of incision, it is better to operate and to dress the wound antiseptically.

Incision of the chest has one great drawback when practised in large effusions, viz., that it somewhat suddenly disturbs the intrathoracic tension by the withdrawal of the fluid, and may lead to urgent and alarming dyspnoea. This has happened in two or three, not more, of our cases. To obviate this, it will perhaps be better to do as Dr. Frederick Taylor advises, viz., remove the pus gradually by the syphon drainage for a few days, and then incise the chest at the end of that time at the seat of the puncture. In the case of a small or localised empyema, incision, if necessary at all, may be practised at once.

ON THE SKIN-AFFECTION LATELY DESCRIBED AS "DYSIDROSIS".

By TILBURY FOX, M.D., F.R.C.P.,

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DR. THIN contributes to the BRITISH MEDICAL JOURNAL of December 1st a paper on "Dysidrosis"; and as in that paper he seeks, by the aid of certain statements recently published by Dr. Robinson of New York in the American *Archives of Dermatology*, to show that there is no disease exactly such as I have described under the term dysidrosis, I should be glad to point out very briefly wherein both he and Dr. Robinson are in error.

Firstly, my difference with Mr. Hutchinson—the resuscitation of which I much regret—was not so much upon the question of mere priority in observation, or even of publication, as of a complete ignoring by Mr. Hutchinson, in his first publication upon the subject of Dysidrosis, of what I had written *three years previously*; and this notwithstanding the fact that I had been in correspondence with Mr. Hutchinson on the subject some time before the appearance of his first publication about the disease, and that his attention had been otherwise called to my original papers. Mr. Hutchinson in 1876 ignored what I had written in 1873; and he has candidly admitted to me that he never read my papers, though he intended to do so. Besides, if Mr. Hutchinson's account of the disease be true—viz., that it is a pemphigus—there is really no novelty about the matter, for pemphigus of the fingers has been known and described for any number of years past. The novelty consists in my assertion as to

the existence of a special form of disease having its primary anatomical seat in the sweat-follicles.

Secondly: to Dr. Robinson's paper I have replied at length in a communication which is now in the hands of the editor of the *American Archives of Dermatology*; and I will, therefore, only say here that I am not prepared to recognise Dr. Robinson's case, upon which so much reliance is placed, as one of dysidrosis—at all events, as a typical instance of the disease; nor that recorded by Dr. Thin last week in the least degree as typical of the early phases of the affection. Neither gentleman seems inclined to make the least allowance for the fact that there are several instances of vesicular conditions (*ex.* sudaminous, pemphigoid, etc.) of the hand which are quite distinct from dysidrosis, but which are very liable to be mistaken for it.

Thirdly: that there is such a disease as I have described, and which can be proved to begin in the sweat-follicles, the fluid present having the characters and reaction of sweat at the outset of the disease, but subsequently becoming altered by the admixture of serosity, is unquestionable. Only a couple of weeks since, in the cold weather, a case presented itself at University College Hospital of dysidrosis in its earlier stages, in which to a score of persons present the origin of the disease in the sweat-follicles was conclusively evident. Just at the tips of the fingers, on their palmar surface, the skin was somewhat swollen and reddened; the ridges of papillæ and the openings of the sweat-follicles much more distinct than usual; and from the latter, fluid oozed out, which had a distinctly *acid* reaction. When the fluid was wiped off, a fresh supply oozed out, and was *acid* in reaction. With a lens, the fluid was actually observed to come from the distended sweat-follicles, and not the papillary layer, which was not vesiculated at all, and had its cuticle intact upon it. Lower down on the fingers and about the palm of the same hand, the parts were more inflamed and swollen, and the secretion was *alkaline*. Yes, it is a fact that there were two distinct reactions in adjoining parts of the same hand. A number of us carefully examined the case with good lenses, and satisfied ourselves that the disease began as a sweat-flux; that the parts around the sweat-glands and the papillary layer soon became secondarily involved in inflammatory action, which spread farther and wider from the original essential anatomical seat of the disease, the sweat-glands; and that the fluid originally effused was sweat, which became mixed with serum derived from other parts to which the inflammation had spread. This is the account I have consistently held about the disease from the first: that it is a disease of the sweat-apparatus, with secondary results. A condition analogous to ordinary vesiculation does *necessarily* occur *secondarily* in dysidrosis; but this is not the *primary* and *essential* condition. Both Dr. Thin and Dr. Robinson have signally transgressed one of the elementary rules of dermatology; *viz.*, not to confound secondary with primary phenomena.

Dr. Thin makes the strange statement, that "Dr. Fox has produced no evidence which could affect our judgment one way or the other" as to the seat of the original vesicles. My account of the disease was based upon nothing short of the actual observation of a large number of cases; and in my *Atlas*, Plate 51, I have actually figured the ducts of the sweat-apparatus distended with sweat. Dr. Thin's remarks would lead the reader to suppose that I made a "guess" about the disease, and did not pursue a long series of observations before I wrote upon it.

Fourthly: Dr. Robinson's argument that, because the fluid in dysidrosis is usually found to be alkaline, therefore the disease can have no connection with the sweat-glands, or that the fluid secreted does not come in any part from the latter, is a most fallacious and valueless one. For how long a time has it not been well known that altered sweat is alkaline? Are Dr. Thin and Dr. Robinson really ignorant of this fact? and, if not, why ignore it? Besides, I have shown that the secretion at the outset of the disease is really acid and nothing less than sweat, though it speedily becomes alkaline. At a later stage, the fluid is *necessarily serous* in chief part. In the face of all that has been said, and upon very incorrect observation, I can only reassert that there is such a disease as I have described under the term dysidrosis, and that time will show that I am quite correct.

Fifthly: There are many others besides myself who can offer abundant testimony to the fact of the commencement of the disease in the sweat-apparatus; and I commend to notice the statements made by one of the most careful observers in America, Dr. Dühring, who in his recent work, p. 237, says: "In the several instances in which I have observed this affection, these symptoms (referring to Dr. Fox's description) were all present in a striking degree. The disorder manifestly has its seat about the sweat-glands, and consists in an undue distension of the sweat-duct throughout its entire course, followed by a collection of fluid within the skin," etc.

THE PATHOLOGY OF CHOREA.

(Being an Abstract of an Address delivered at the Annual Meeting of the Staffordshire Branch.)

By JOHN T. ARLIDGE, M.D., F.R.C.P.,

Physician to the North Staffordshire Infirmary; President of the Branch; etc.

CHOREA is very prevalent among the patients of the North Staffordshire Infirmary, and would appear to be more so among persons engaged in the pottery manufacture than among other inhabitants of the district.

Under the term chorea, many different nervous disorders and lesions have been comprehended; and it remains to be determined what group of symptoms shall be represented by that term.

Notes of one hundred and fifty-six cases, the majority of them occurring in the practice of the North Staffordshire Infirmary, were analysed. The results, as to age and sex, were confirmatory of the general statements that chorea is especially a disease of girls under puberty. There is a rapid rise in the numbers affected after the eighth year is attained, and this reaches its maximum in the course of the tenth year; it then falls as rapidly until the sixteenth, when an increase again, of smaller amount, is met with, and is maintained almost to the same extent in the following year; after which, cases are few.

In connection with these facts, it is noteworthy that the cases among girls of fourteen and upwards comprise a very considerable proportion of relapses, and not a few of them are examples of evident nerve-lesions, accompanied with heart-disease; or else are of reflex origin, from uterine disorders or pregnancy.

The tendency to relapse is a special feature of the disease, and obtains in about one-fourth of the number of cases. The malady, in half the number under observation, affected both sides of the body; of the remainder, one side was affected more seriously than the other in two-thirds; and in the rest was sufficiently one-sided to be called hemichorea. In more than one-half of the cases, the muscles of the face were involved. In one-sixth, the speech was affected. The ophthalmoscope was used in a few instances, but revealed nothing save some pallor of the disc.

No positively general rule exists regarding the urine. It is of higher specific gravity usually when the movements are most energetic; but the variations in this respect are great in different patients, and at different times in the same patient.

Choreic patients are nearly always in low health, very often anæmic, and generally of emotional or excitable nervous temperament, with feeble nerve-power. Hereditary predisposition is very rare. In about one-seventh of the number examined, rheumatism or rheumatic pains, or heart-disease, has been found to have affected one or both parents; consumption also appears to have been as frequently a family inheritance; and the same may be said of nervous disorders among immediate relatives.

Of the number of patients in whose cases the fact was ascertained, rheumatism had occurred previously to the chorea in about one-fifth; but it would seem, as far as inquiry was made, that scarlatina was about as frequent an antecedent.

Distinct evidence of cardiac lesion was found in rather more than one-fourth of ninety-two cases in which it was sought for and noted. In thirty-seven of these, which were the most recently collected, it was discovered only in one-fifth.

As to assigned causes, fright stands as causing one-fifth of the number of cases; and previous rheumatic and choreic attacks, severally, as productive of a like proportion. Of the remaining two-fifths, reflex irritation, antecedent epileptic fits, and injuries to the head, were made accountable for a considerable proportion; the cause being unknown or unassigned in the rest.

A first group of cases comprises such as are met with in children up to the age of puberty, and in most of which the disorder exhibits itself in its simplest and highly curable form. In this simple form, rheumatism, cardiac and cerebral lesion, do not constitute an element; but there are exhaustion, anæmia, a disturbed nervous condition, and defective nutrition. This form best deserves the special name: chorea.

A second group is connected with the occurrence of rheumatism, and with cardiac lesions traceable thereto: not that the chorea is due directly to embola in the great ganglionic centres in the cranium, although it is probably due indirectly to them, and to their paralyzing and inhibitory action upon the controlling power of the cerebral lobes.

Although further investigation be first necessary to establish the relation and its frequency, there appear grounds for believing that there may be a group of scarlatinal cases of chorea, parallel with the rheu-