Surgical Treatment. In recent years surgical treatment has been advocated, especially by Edebohls and Ferguson, in the chronic types of nephritis. Briefly stated, the surgical intervention consists in nephrotomy or decapsulation. This procedure leads to an increased elimination by drainage of the accumulated products. As the operation is done in the primary stage of anesthesia, the untoward effect of the anesthetic is very much lessened. The whole operation consumes only three to four minutes, and, judging from case reports, is often very beneficial. Much may be expected of this form of treatment, as it is surely rational in some cases and is successful in many.

In cases such as those described by Brewer, nephrectomy seems to have yielded cures. It may be that, in these forms also, decapsulation with drainage will be useful; it will lessen the danger of operative procedure.

THE USE OF THE X-RAYS IN UNRESOLVED PNEUMONIA.

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That exposure to the *x*-rays might be useful in the treatment of unresolved pneumonia was suggested by one of us[1] in discussing the effects of the *x*-rays on metabolism in leukemia. It was then stated that the remarkable effect that this agent usually exerts upon metabolism is most readily and most satisfactorily explained by considering it to be due to action upon ferment processes normally resident in the tissues; the action in this instance apparently being to accelerate autolytic processes inherent in the tissues. The reasons then evident for adhering to this view were given at that time, and continued studies of the effect of *x*-rays upon metabolism in various diseases—studies now amounting to twelve—have emphasized our opinion that this is the most satisfactory conclusion to be reached. Further consideration will be given this point in a more extended discussion of the accumulated results in the various conditions studied, and it is, therefore, unnecessary to burden the present communication with it.

Granting the correctness of the view mentioned concerning the

[1] Musser and Edsall. Trans. Assoc. Amer. Phys., 1905; Univ. Penn. Bull., September, 1905.

action of the *x*-rays, the idea at once presents itself that treatment with this agent may be beneficial in many conditions in which metabolism is not sufficiently active; but there is great difficulty in determining this point, on account of our very limited knowledge of most conditions in which such metabolic inactivity is suspected of being present. As was said, however, in the paper first referred to, our present knowledge of the process of resolution of a pneumonic exudate indicates that unresolved pneumonia is the most evidently suitable condition in which to try the effect of the *x*-rays from this point of view. We know that ferment processes cause the digestion and resolution of the exudate in the ordinary pneumonias that pursue a satisfactory course; and lack of resolution, while sometimes really a continued inflammation, appears from Flexner's observations to be often due to insufficiency or absence of digestion of the exudate. In the latter case evidently the rational therapy is, if possible, to excite or accelerate the action of the ferments that should digest the exudate.

The possible suitability of the *x*-rays as a therapeutic measure in this condition led to its being used, in April, 1905, in a case that has already been mentioned.[2] Since that time we have kept under careful observation the cases of delayed resolution of pneumonic exudate that have come to our notice, with a view to choosing from them, for this treatment and for a study of metabolism while under treatment, those cases that seemed fairly certain not to undergo resolution under ordinary treatment, or to resolve so slowly and imperfectly as to lead to permanent crippling of the lung. We limited our choice also to cases in which noteworthy fever and all other evident signs of continued active pneumonitis were absent.

Of the five such cases that became available—all fever and evidences of distinct toxemia having disappeared, and the signs of solidification having been stationary for at least ten days or a fortnight—three patients grew weary of the waiting that we had imposed upon them and insisted upon leaving the hospitals in which they had been kept under observation, so that our further studies of this question have been confined to two cases. The conditions of metabolism observed in these cases while under treatment with *x*-rays were, however, so striking and the clinical effects in them and in the case previously mentioned appeared so uniformly good, that it seems proper to describe the results in them, in order to encourage a more extensive test of the treatment. In such tests, however, a few considerations need to be kept in mind, in order that the conclusions may be just.

In the first place no results can be expected unless the duration of the condition has been reasonably short—a few weeks at most—

[2] Musser and Edsall, loc. cit.

for, if organization of the exudate has occurred, a satisfactory result of treatment is almost inconceivable.

Secondly, the condition should be chiefly a real lack of resolution and not a continued inflammation of the lung. We are unable to state what effect the *x*-rays may have upon chronic pneumonia, but we are not dealing with this point; and in testing the effect upon delayed resolution as such, active continued inflammation should be excluded as far as possible. To exclude even slight grades of chronic inflammation would be exceedingly difficult, for such slight grades produce no symptoms that are distinguishable from those due to the exudate itself; and, indeed, a low-grade inflammation is probably always present, even comparatively early, and certainly when organization begins. Such a condition we could not reasonably desire to exclude, but we should exclude those cases in which decided fever and other evident symptoms of persistent active inflammation and toxemia exist; for, when these are the chief abnormalities present, resolution of the exudate may be impossible, and may also be of little or no value for the time being. In the low-grade inflammation, however, that appears to be due largely, and perhaps entirely, to the persistence of the exudate, resolution of the latter would probably be accompanied by cessation of the inflammation.

Thirdly, it is exceedingly important to determine, as far as possible, that one is actually dealing with unresolved pneumonia and not with tuberculosis. Many cases that are hastily thought to be unresolved pneumonia are, of course, unquestionably tuberculous; and judgment as to the value of a treatment of unresolved pneumonia cannot be based upon them. If cases fail to resolve under the influence of the *x*-rays, it should be determined before using them, in reaching a decision, whether they are or are not instances of tuberculosis.

Finally, we would earnestly recommend that the use of the *x*-rays in the treatment of cases of this or any other kind be undertaken with a proper realization of the fact that there are dangers associated with it; and the treatment should, therefore, be begun tentatively, and with brief exposures and small doses, until it is determined in the individual case that serious results are not likely to ensue. This is an additional reason for excluding those cases in which there is marked fever and in which other evidences of toxemia are present. For reasons that we shall point out in another paper, we are convinced that persons that are already the subjects of a noteworthy degree of toxemia are more likely than others to be dangerously affected by exposures to the *x*-rays; and until our knowledge of the contraindications to the use of the *x*-rays becomes better defined, we believe that with all such persons the use of this agent should be withheld unless there are urgent reasons for administering it.

We would also at this point, and for the reason just given

advise that the *x*-rays be not tried, at least until their effects are better understood, in the treatment of pneumonia during the course of this disease. Such treatment has already been used purely empirically, and if we are correct in thinking that resolution may be hastened in unresolved pneumonia by the use of the *x*-rays, a very natural corollary of this idea would be the thought that exposures to the *x*-rays during the course of pneumonia might hasten resolution and shorten or favorably modify the course of the disease. We do not, however, subscribe freely to such a notion, the chief objection to it being that the most important factor in the course of most pneumonias is not the presence of a mass of exudate in the lung, but the presence of a dangerous toxemia. Resolution of the exudate might, if it could be accomplished, be associated with control and reduction of the toxemia, but it is equally possible that it might not influence the toxemia to an important degree; and the point we would make at present is that whatever might be the result upon the local mass, there is actually serious danger that, if resolution could be accomplished, the sudden digestion of the exudate and absorption of its products might, if occurring during the course of a severe toxemia, be productive of much more harm than good.

The main clinical features of our cases will first be given, including the case that has already been briefly reported in the paper referred to.

CASE I.—A man, aged fifty-six years, was admitted to the Episcopal Hospital in the service of Dr. A. A. Stevens, on March 13, 1905. He had been taken acutely and severely ill three days before with high fever, dyspnea, and pain in the right side; and presented the usual signs of a pneumonia involving the whole right upper lobe. His disease ran a moderately severe course, and crisis occurred on March 20. The fever and the signs of toxemia disappeared at this time, but the consolidation persisted, and was still present and but little changed when seen by Dr. Edsall on April 1. There was no noteworthy change, except a very moderate degree of lessening in the intensity of the signs, up to April 21, on which date it was ordered that he be given over the affected area five-minute exposures to the *x*-rays. These were continued for four days.

During Dr. Edsall's temporary absence from town, Dr. George W. Norris kindly watched the course of the case during the next week. There was no special change in the man's general condition, which was already satisfactory, but on April 23 it was noted that there were many rales in the affected area of the lung and that the breathing was somewhat cavernous in type in the upper portion of the lobe, below this bronchovesicular. Three days later he was noted still to have a moderate number of rales in this lobe, while the breathing had become bronchovesicular everywhere. The dulness had greatly decreased. When seen by Dr. Edsall on April

27 the rales had disappeared; there was very slight dulness over the upper half of the lobe, with no dulness elsewhere; the breathing was a little weak over the whole lobe, expiration being slightly prolonged. A week later, when discharged, there was, except for slight weakness of the breath sounds over this lobe, practically no abnormality left. The man's general condition was entirely satisfactory.

CASE II.—A Norwegian sailor, aged twenty-eight years, was admitted as a British Consul case to the University Hospital under the care of Dr. Hollingsworth Siter, who kindly gave us the opportunity to study the patient. It was difficult to secure a detailed history from him; but it appeared that while at sea he had been taken acutely ill with fever, dyspnœa, and pain in the left side. He had been ill for eight days, when he had had an apparent crisis; and the day following, when he arrived in port, he had been sent at once to the University Hospital. When admitted his temperature and pulse were normal and his general condition was entirely good, but he still had the signs of marked solidification in the left lower lobe, involving the whole of the upper part of the lobe and reaching down to the level of the ninth rib.

For three days there was no change in these signs; but throughout the next five days there appeared to be a slight improvement, the dulness becoming less intense and the breath sounds less harshly bronchial. From this time on, however, for two weeks the signs remained stationary; and when he was first exposed to the *x*-rays he had, over the whole upper part of the left lobe down to the level of the ninth rib, a moderate degree of dulness, with moderate bronchial breathing and increased fremitus. Rales were absent. An *x*-ray picture was taken at the first exposure, and this showed a moderately intense shadow in the area corresponding to the signs described.

He was exposed daily for seven days. Two days after the first exposure there was no striking change, although it was noted that the dulness was perhaps less marked and that there were a few fine rales in the affected area. Two days after this it was noticed that the dulness had receded upward for about an inch and a half and that even above this point the dulness was less marked; the breathing was much less harsh, and was bronchovesicular; it could not be determined that the fremitus was increased, and there were many fine rales over the upper part of the lobe. Three days later, at the end of the experiment, no abnormalities could be discovered, except slightly weak breathing over the diseased lobe and a small area of dulness, about an inch in diameter, at the upper extremity of the lobe. An *x*-ray picture taken at this time showed nothing abnormal except a small, indistinct shadow at the area in which a trace of dulness was still found. The man appeared to be entirely well in other ways.

CASE III.—A Cuban, aged thirty-four years, was admitted to the Pennsylvania Hospital, in the service of Dr. Arthur V. Meigs, who kindly gave us the opportunity to study him. Dr. Longcope also made frequent notes of his case, of which we shall give an abstract, and was likewise kind enough to watch carefully over the details of the weighing of his food in the metabolism experiment, the securing of the twenty-four hours' urine, etc.

The patient was admitted on the fourth day of an illness that had come on with the usual symptoms of an acute pneumonia of the right side. Examination of the right lung showed dulness over the whole lower lobe, with bronchial breathing and increased fremitus and vocal resonance, and with fine crepitant rales over this area. The upper portion of this lung was negative, as was the left lung. The examination, otherwise, is of no consequence. On the 5th, his condition was worse, the pneumonia having extended throughout the lung. The signs over the lower lobe, however, had decreased somewhat in intensity. On the 8th, he had the usual signs of crisis, but the lung remained solidified. Three days later the lower lobe appeared to be slowly clearing up, but the uppper lobe remained solidified. On the 21st, thirteen days after the crisis, the lower lobe had cleared up with practical completeness, but the upper lobe remained unchanged and was entirely solidified. Tubercle bacilli were absent from the sputum.

On this date the first *x*-ray exposure was given him, a picture being then taken which showed a shadow corresponding with the signs of solidification in the upper lobe. Two days later it was noted that the breathing was less harsh and the dulness less marked, but the signs were not strikingly changed. On the 26th the signs were decidedly less marked, dulness and bronchial breathing being much less pronounced. On the 28th there was still some dulness, but it was decreased. The breathing was blowing, but not distinctly bronchial. On March 1 the dulness reached only to the third rib, and was slight, the breathing still being somewhat blowing. On the 3d the signs were about as last noted. On the 6th there was still slight dulness to the third rib anteriorly. Posteriorly, the resonance above the spine of the scapula was skodaic. The breath sounds were somewhat harsh over the dull area, but not tubular. Rales were absent. The patient was discharged two days later, at his own request, in apparently normal condition, except for the signs mentioned. The *x*-ray exposures subsequent to the 21st occurred on the 24th, 26th, 27th, and 28th of February; and the 1st, 3d, 4th, 5th, 6th, and 7th of March.

Metabolism experiments were carried out in the two cases last described. As previous studies had shown no noteworthy influence of the *x*-ray exposures upon absorption from the alimentary tract the feces were not studied. The patients were given a diet composed of milk, bread, butter, eggs, rice, potatoes, and sugar. The food was from

the same source each day and was accurately weighed or measured. The amount of nitrogenous intake was calculated from the weight records of the food, using figures that we had previously obtained from numerous determinations of the same food. The salt also was kept at the same amount each day, in order that variations should not influence the chloride determinations; and it should be noted that the diet was purin-free and hence that variations in the uric acid were due solely to changes in metabolism, and not to changes in the food. Before studies of metabolism were begun, the patients were put for three days on the diet described. For several days control studies were then made, and after this the x-ray exposures were commenced. Each "x" in the second column indicates an exposure; it is to be noted that the effect is shown in the figures of the following day.

TABLE I. CASE II.—URINE.

Date.	x-ray exposures.	Nitrogen ingested Grams.	Nitrogen.	Chlorides.	Uric acid.	Phosphates.
Jan. 12		6.54	8.971	12.600	0.2293	1.900
" 13		6.54	8.262	8.262	0.1944	1.296
" 14		13.08	14.840	11.125	0.3022	2.710
" 15		13.08	9.908	13.360	0.3424	1.996
" 16	x	13.08	8.153	12.350	0.2427	1.716
" 17	x	13.08	17.575	20.900	0.3248	2.257
" 18	x	13.08	Lost			
" 19	x	13.08	10.819	14.630	0.3190	2.296
" 20	x	13.08	13.464	14.490	0.3522	2.250
" 21	x	13.08	9.222	14.170	0.2764	1.861
" 22	x	13.08	8.119	16.250	0.2917	2.168
" 23	x	13.08	9.863	17.250	0.3346	1.905
" 24		13.08	11.752	16.980	0.3720	2.080
" 25		13.08	7.207	14.560	0.2388	1.565

TABLE II. CASE III.—URINE.

Date.	x-ray exposures	Nitrogen ingested. Grams.	Nitrogen.	Chlorides.	Uric acid.	Phosphates.
Feb. 19		9.25	4.872	2.400	0.1260	0.720
" 20		"	9.015	8.920	0.2835	1.169
" 21	x	"	8.288	4.800	0.3000	1.170
" 22	x	"	17.096	8.400	0.4662	2.390
" 23	x	"	12.776	7.475	0.4215	1.794
" 24	x	"	5.264	3.200	0.1380	0.795
" 25	x	"	5.895	5.330	0.2007	1.404
" 26	x	"	6.428	2.780	0.2120	0.848
" 27	x	"	9.872	5.800	0.3767	1.430
" 28	x	"	7.915	5.060	0.2268	1.408
Mar. 1	x	"	12.958	8.937	0.7187	2.614
" 2	x	"	10.668	8.300	0.2962	1.800
" 3	x	"	4.868	2.240	0.1440	0.552
" 4	x	"	11.220	4.042	0.2756	1.764
" 5	x	"	4.211	2.880	0.0740	0.920

Clinically then in these three cases the result was in each instance a rather rapid clearing up of most—in two cases practically all—of the signs of solidification that had previously persisted in the three cases, respectively, for one month, two weeks, and thirteen days,

without improvement. The relation of the improvement to the *x*-ray treatment may have been merely a coincidence, for these exudates often, without any evident reason, clear up after more or less delay. Three cases do not suffice to decide this point, but that the *x*-rays played a lively part in causing the improvement appears to be at least probable, when one considers the influence that the *x*-ray exposures had upon metabolism.

In Case II, as a result of the first exposure (which was *only eight seconds long*, but was actually a fairly intense exposure, since it was one intended to produce a negative), the nitrogen increased to more than double what it had been before, and the chlorides showed an almost equally striking increase; the phosphates were somewhat less decidedly increased, while the uric acid showed relatively little change. The increase persisted in less-marked degree, gradually decreasing coincidently with the decrease of the solidification, and when the latter had disappeared, excretion had reached about its former level.

In Case III, the relation of the excretion to the *x*-ray exposures is even more striking. The first exposure was followed by a decided increase. Owing to a misunderstanding, the next exposure was not given for three days, and in the interval the excretion dropped decidedly; after the second exposure there was a moderate rise and then there was again an interval of two days between the exposures, and the second increase of excretion was also followed by a decided drop. The subsequent daily exposures were accompanied by an increase that was somewhat irregular, but became extremely remarkable on March 1. Again following the omission of an exposure on March 2, the urine of the 3d shows a striking drop, and finally, in this case also, as the solidification largely disappeared, the excretion appeared to be dropping pretty well to the original point, in spite of continued exposures, though, as the patient was soon to leave the hospital we could not carry the observations far enough to determine the latter point positively. At the end of the experiments in the two cases the first patient was retaining a great deal of nitrogen and the second patient was showing a strong tendency to retention; while in the periods during which they showed a marked increase in the excretion as the result of the *x*-ray exposures both were showing a very decided loss of nitrogen.

The figures that we have given show that the effect upon metabolism was indeed a most remarkable one. We have seen similarly profound effects only in one of the cases of leukemia that we have previously described—in which there was an extremely rapid improvement in general condition, splenic tumor, and leukocytosis—and in the two cases of general toxic reaction that we shall describe in another paper, though it should be noted that in the two latter cases the effect, while profound, differed strikingly from that

in the case of successfully treated leukemia and in these cases of unresolved pneumonia.

Since such a remarkable effect was exerted upon metabolism, and since this was coincident with rapid improvement in the pulmonary signs, it would appear highly probable that the *x*-rays caused the improvement. The conditions observed are, in fact, extremely similar to those that occur at the time of an ordinary crisis in a favorably progressing pneumonia; that is, the exudate grows moist and clears up within a short time and, coincidently with this, there is a large increase in the metabolic output through the urine.

Whether the *x*-rays will prove to be of great importance in the treatment of unresolved pneumonia is, therefore, a question that is worth settling, but it is one for the future to decide. While, however, this decision is of importance, it seems to us that there is a broader interest in the question whether the theory that was the basis of these observations—the theory that the *x*-rays act upon metabolism chiefly as a ferment accelerator—is correct or not. When, for example, one administers hydrochloric acid with pepsin or alkalies with pancreatin, one consciously or unconsciously uses, to be sure, the same principle; that is, one attempts to provide conditions that are known to be especially favorable to the rapid action of these ferments; but in the manner in which these measures can now be used artificially, the results of such treatment do not at best distantly approach those accomplished in natural circumstances. Except in a few such simple and comparatively ineffectual instances, this principle of using measures that provide conditions that directly accelerate special ferment processes has not, to our knowledge, been deliberately used previously in the treatment of pathological states. In case our view is correct this study furnishes the first example of the deliberate use of this principle in the treatment of disease that lies beyond the digestive tract, and more especially the first instance of the conscious use of this principle with results that are so striking in their effect as to approximate those that the human organism itself accomplishes when successfully reacting to disease.

If, therefore, we are correct this is of considerable interest when one reflects upon the importance that ferment processes have assumed in the last few years in our conceptions of physiology and pathology. There is at present a widespread and somewhat excited tendency to give ferment processes a much more overwhelmingly important place than has been demonstrated to be their due; but the fact that they play an important part in the body economy is perfectly evident, and that methods that would clinically influence them in definite and controllable ways would be of great value scarcely needs to be stated.

Numerous attempts have been made to isolate and administer ferments themselves, of various kinds, in the treatment of diseases

in which they are supposed to be absent; but such attempts have certainly been, in almost all instances, largely or entirely failures. This is scarcely to be wondered at, for in the first place knowledge of these ferments and of their relation to disease is in most instances at best very hazy, and, furthermore, attempts at isolating them are built upon most incomplete and insecure methods; and it is likely to remain most questionable whether tissue ferments can ever be obtained in active form, and then guided to the proper spot still in active form. It is equally questionable whether, should they arrive there, the disordered medium in which they would often find themselves would be such as to permit them to act. Indeed, it is exceedingly doubtful whether the whole idea of administering ferments is not as a rule wholly unphilosophical, both for the reasons given and because in many of those instances in which they appear to be absent they are probably not entirely absent, perhaps not even reduced in amount, but are merely unable to act properly owing to unfavorable chemical or physical conditions. It is possible that we may ultimately learn to stimulate the production of ferments in different organs with some accuracy, and this may prove to be occasionally a successful measure; but it is at present in the distance, and for the reason mentioned in the last sentence, it would frequently have only ineffectual results, for it is wholly rational to conceive of most varied chemical and physical conditions that would more or less completely interfere with the action of ferments in the tissues, and many such conditions, both simple and complex, are already known. If such conditions were present, increasing the amount of ferment would, of itself, be of no avail; and the essential matter would be to substitute for existing substances or conditions others that would inaugurate or accelerate the action of the ferment that is present. This is probably done already in using various therapeutic measures that have been evolved empirically; many drugs, for instance, are known to have a very marked effect upon a number of catalytic processes, and observations yet unpublished, that Dr. Casper Miller and Dr. Edsall have made, indicate that the striking lesions produced by toxic doses of mercury, and probably the remarkable effect that mercury exerts on syphilitic tissues, are, partly at least, the result of increased autolytic activities. The conditions under which a ferment acts are probably more frequently important in producing pathological states than is the amount of ferment present, for ferments are exceedingly sensitive to the conditions under which they act, in both a positive and a negative way, and the conditions influencing them are exceedingly numerous and varied. The almost inconceivably energetic effect of conditions that have a very marked accelerator action is shown, for example, by the fact that the intestinal wall appears to be able in the brief period that is necessary for their passage through it, to rebuild into the native protein of the individual the fragments of protein broken down in digestion, while the best conditions

that experimenters can now provide furnish, even after long periods of action, at most, only doubtful evidence of slight artificial imitation of such a process; and, on the other hand, as is well known, mere traces of certain substances, such as hydrocyanic acid, will greatly retard or wholly interfere with the action of relatively enormous quantities of some catalyzers.

This casual mention of some of the most evident conditions that are known to influence the action of ferments suffices to make it reasonably clear that if a study of ferments is to become of value clinically it is likely to be chiefly through determining the conditions that influence the activity of individual ferments under normal and pathological conditions and through making use of measures, clinically applicable, that can be shown to provide, of themselves and directly, favorable conditions for the action of these ferments, or that overcome unfavorable conditions.

Whether our view regarding the action of the *x*-rays and their effect in unresolved pneumonia is correct or not the future will probably see the principle upon which we were working used many times, as knowledge of ferments and of their accelerator and anti-accelerator conditions increases. That which has just been said must not, however, be misinterpreted. It is intended largely as a protest against the blind but common belief in a mysterious value of ferments as therapeutic agents, a faith that gives opportunity to manufacturers to foist upon many of the profession preparations that are even supposed to contain, isolated and in active form, ferments that are by no means definitely known to exist; and still more would we protest against the excited enthusiasm of those investigators whose work has been such as to lend them the respectful attention of the profession, and who now seem occasionally to be heralding the approach of a therapeutic millenium that is to be distinguished chiefly by the administration of ferments. We would not be thought to believe, however, that there is, instead of this, a nearer-by millenium, in which knowledge of the conditions in which ferments act will lead to inevitable success in the treatment of disease. We would merely point out that in contrast with the dubious plan of treating disease by administering ferments, those who think conservatively in regard to this matter can scarcely believe that much will ever be accomplished in a large proportion of cases in which ferment action is at fault except by modifying the conditions under which ferments act. Through happy chance it will not improbably occur that occasionally the knowledge that accumulates will be made of service in successful therapeutic practises, if this manner of viewing the relation between ferments and therapeutics is held in mind; but any such success will almost certainly, for a long time to come, be largely the result of happy chances that are pointed out by a modicum of suggestive investigation. It will surely be a wearily long period before slow, pains-

taking, and complex investigations make it possible to apply any such principles in a considerable number of cases with a just expectation of success. The exciting work on ferments that has occurred in recent years has greatly transformed the point of view assumed in attacking problems in pathology, but has not in other ways profoundly simplified these problems. The difficulties and complexities have not been essentially lessened; their nature has rather been made more evident.

A BRIEF STUDY OF A DIPHTHERIA EPIDEMIC AT THE ADIRONDACK COTTAGE SANITARIUM (FOR INCIPIENT PULMONARY TUBERCULOSIS).

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Diphtheria epidemics in institutions for the tuberculous are of unusual occurrence, for which reason, as well as on account of the many problems that arise under such conditions, it has been deemed of sufficient interest to report in more or less detail a recent epidemic of diphtheria at the Adirondack Cottage Sanitarium.

The origin of the first case has never been clearly determined. We present the facts as they occurred. Early in October, 1905, one of our orderlies, after a visit to his home, developed a membranous tonsillitis, which on a single culture showed streptococci and no diphtheria bacilli. He was allowed accordingly, after a few days' isolation, to resume his duties. Several cultures taken from his throat at intervals later on during the epidemic, as well as those from his room-mate, were always negative. In the latter part of the month, one of the maids, who had but recently begun work in the laundry and who was with the orderly quite frequently, reported with a well-developed tonsillar diphtheria. She was promptly isolated in her room with her room-mate, her sister, and a strict—as we thought—quarantine established. Later we learned that, standing in her doorway, she would hold conversation with the other maids. After most rigid precautions, including negative cultures, both sisters were discharged, the latter on Saturday evening December 30, 1905. The same day, before the first maid was discharged from quarantine, a patient and one of the maids had a slight soreness of the throat, and two days later when they reported that they felt ill, a second patient went to bed. The second maid, who was employed in the dining-room and linen-room, was at work