In answer to the second question, there can be no doubt in reasonable minds, it seems to me, that in following old methods of merely sending patients away from home haphazard, without special supervision, the chances of permanent cure are much greater if the patient selects a favorable climate, for instance, like Colorado, New Mexico or California, than if he stays without supervision in our changeable and harsh New England climate.

Sunshine and a dry, porous soil are known to be among the most important factors for health, and in the Western regions just mentioned these conditions are found in a remarkable degree, to say nothing of the altitude which doubtless has its own favorable effect for various reasons, some of which can be obtained only by artificial means at a lower level.

Taking these facts into consideration, then, and although greatly impressed by results which I have seen accomplished in most unfavorable climates, especially in New England, I am at present strongly of the belief that if sanitarium methods are adopted and thoroughly carried out in what are considered the most healthful climates, we shall accomplish even better results than in less favorable regions.

In regard to the third question, it would seem to me impossible, with our present knowledge, to answer it with any degree of precision, and we can only give our impressions gained from our experience in general.

In a paper read before this society two years ago, I gave you the subsequent histories of cases of arrested phthisis,² which had been at the Sharon Sanitarium. Some of you may remember that the percentage of those who had remained well after a number of years was gratifyingly large, even when in some cases they had again taken up occupations not of the most hygienic nature.

It is too early yet to give satisfactory data upon this point at the Rutland Sanatorium, for not enough time has elapsed since the first arrested cases left the institution to make their subsequent histories of much value as yet. In our last annual report (1900), however, the following statement was made: "Of the 35 cases 'arrested' at time of discharge during the year 1898-1899, all are alive, and, with the exception of 3 cases, have not relapsed. Two of these relapsed cases have been readmitted. The recurrence of disease in these cases appears to be wholly due to lack of means and consequent inability to live under proper conditions. A large number, however, of the 'arrested' cases, although advised to the contrary, returned to the former environments, in which the disease originated, and have, contrary to our expectations, almost uniformly remained (to this date, October, 1900) free from abnormal symptoms or disease. A few with care have become even stronger and heavier than when they left the institution."

²Subsequent Histories of Arrested Cases of Phthisis Treated at the Sharon Sanitarium; *Boston Medical and Surgical Journal*, June 22, 1899.

This is, to say the least, gratifying as far as it goes. The future alone must decide the matter when similar conditions prevail in the East and West as far as the erection of sanatoria is concerned. Here again my belief at present is that the latter will show superior results.

I trust that the foregoing remarks may not be so misconstrued that I shall be thought to have abated in the least in my desire to have sanatoria for consumptives erected wherever that disease may prevail. Such would be an entire misapprehension of my position. In every community there are many who for various reasons cannot go to distant resorts, and it is for these people especially that we need large numbers of properly equipped sanatoria throughout the country.

In a few words, then, my position in this discussion is as follows:

(1) From my own experience in New England I am strongly in favor of the erection of sanatoria for the treatment and cure of consumption wherever that disease prevails.

(2) As to the relative results to be obtained by this method in favorable or unfavorable climates, I do not think we are in a position to judge fairly at present.

(3) I believe that at present better results can in all probability be obtained by this form of treatment in the Western sections of our country, like Colorado, New Mexico, Southern California and similar regions, than in the Eastern sections.

EOSINOPHILES AS CONSTITUENTS OF PUS.

BY EDWARD T. WILLIAMS, M.D., BOSTON.

THE study of the leucocytes forms the most interesting, and undoubtedly the most complicated problem of modern hæmatology. This interest dates from the remarkable investigations of Professor Ehrlich on their staining properties about the year 1880. He discovered a marked difference in their behavior with the various dyes, and identified five distinct species, which he designated by the first five letters of the Greek alphabet. At the head of the list he placed the Alpha cell, now universally known as the eosinophile. Professor Ehrlich is an incorrigible Hellenist.

Since that time a vast deal of speculation has been going on as to the nature and origin of all these cells. It is safe to say that a great deal of misleading nonsense has been written about them.

It is tolerably well ascertained that the acid uninuclears (lymphocytes) take their origin from the lymph glands and lymph spaces. The lymph spaces, as is well known, are in open communication with the terminal lymphatics, and form a vast and continuous cavity extending all over the body through the meshes of the connective tissue. These spaces are filled with innumerable myriads of free cells, the so-called wandering cells, which exactly resemble the lymph cells, and are, without doubt, identical with them.

The supposed origin of other leucocytes from the bone marrow, the spleen and the ductless

glands is more or less problematical. It has not yet been scientifically proved. The present tendency is to ascribe a myelogenous origin to many very different cells. Hence the doubtful applicability of such terms as myelocytes. The myelocytes are simply uninuclear leucocytes with a neutral or alkaline cell-body. How they originate is as yet uncertain. They are very probably degenerated lymphocytes.

The same uncertainty exists as to the origin of the multinuclears. The prevalent opinion is that they are developed out of the uninuclears by division of the nucleus, and that the change takes place in the blood itself, where all the materials for their growth are ready to hand. I incline to this opinion myself.

The eosinophiles are the greatest problem of all. They have been much discussed. Their striking and beautiful appearance when properly colored has always attracted particular notice. They are, in brief, multinuclear leucocytes with alkaline bodies. Ehrlich evidently regarded them as a distinct species *sui generis*. Ewing informs us that they are coelomic cells, derived solely from the eosinophile cells of the bone marrow, finding their natural habitat in the tissues and not in the blood, and constituting a distinct, self-perpetuating series. An overwhelming assumption.

In all this confusion it seems strange that no one has yet thought to make a systematic study of the leucocytes in pus. The pus-cell is simply a leucocyte at large, melted out of the tissues by the liquefying action of the pus-germs. Yet I believe no complete investigation of their staining properties has been attempted. Neisser's gonococcus stain of eosin and methylene-blue, I fancy, first made us aware of the remarkable affinity of the pus-corpuscle for eosin. The body of the corpuscle stains red, the nuclei blue, and the diplococci a still deeper blue, almost a blue-black. A very pretty picture of this can be found in Warren's Surgical Pathology (1st edition) Fig. 12. It has been observed that pus from other sources stains equally well with eosin. This is imperfectly shown in Warren's Fig. 29, but here the bodies of the pus-cells are too much broken up and lack definition. Still they show perfectly well their affinity for eosin. I have employed these stains many times myself. It is the most perfect method in the world for the demonstration of micrococci, far preferable to the old Gram process, yet I never thought to notice until recently whether or not these eosin-staining pus corpuscles showed the true granular structure of the eosinophile leucocyte. And yet eosins have been found in pus. Bettmann, quoted by Ewing, found them common in gonorrhœa. Ewing admits that they occur in cutaneous pustules, while Bettmann and others demonstrated them in herpes, pemphigus, eczema and other diseases of the skin.

To test the question I have lately been searching for myself. I profess no skill in such matters, except that of the average practitioner. I made simple smears on cover-glass, stained them with eosin, and counterstained with methylene-blue.

The great danger is in overstaining with the blue. Neisser's method of staining 15 seconds with concentrated alcoholic solution of methylene-blue completely drowns out the eosin. The blue solution should contain from 1 to 3% of dyestuff. The pus should be fresh, alkaline pus from an acute abscess. The power not less than 1,000 diameters. The examination should be made in salt water, 4 grains to the ounce, since plain water swells and distends the corpuscles. They usually show best on the second day, when they are completely dry, but yet not dry enough to shrivel them up.

To state it briefly, I found eosins in every specimen examined. They form a large proportion of the cells present. Many had 2 or 3 nuclei, others only 1. The protoplasm of these eosins was coarsely granular, as in the blood eosins. These granules, as I judged, averaged from 1 to 2 micra in diameter, 2 or 3 times as large as the accompanying staphylococci, and a tenth or more of the diameter of the containing corpuscles. They are readily distinguished from the cocci by their color, size, and relation to the pus-corpuscles; the granules being intracellular, the cocci extracellular. Occasionally we find a few granules escaping from a crushed or broken cell, yet they are always easily distinguished. They average, I should say, from 30 to 40 granules to the cell, yet it is not easy to count them. The reason for this is plain. The eosinophile in nature is quite a different object from the same body in a color plate. It is, in fact, impossible to picture them on a plane surface. The pus-cell is a globe about 10 micra in diameter. The granules lie at different depths within these globes, hence they are not all seen at a single glance. It is necessary to focus carefully up and down to bring them all into view. Begin at the top of the cell and first focus the superficial layer. Then screw down a little and bring out the middle layer; the upper ones will become less distinct. Then go down a little farther and bring out the lower layer, which may put the upper ones entirely out of view. They cannot be satisfactorily demonstrated in any other way.

You will sometimes notice an apparent clumping of the granules. Three or four granules seem to be run together, and look like larger bodies than the granules, and of quite irregular shapes. But this is usually an optical illusion. It occurs when two or more granules happen to lie nearly in the same line of vision, but not all in focus. By focussing closely up and down one generally succeeds in resolving the clumps into separate granules. Still I am not prepared to deny but there may be a genuine coalescence of granules. At any rate the clumps are not always resolvable. But this is a fine point.

A more important question is as to what the granules really are. Heidenhain, Gulland and Ewing contend that they are integral parts of the cytotreticulum. They appear to me like precipitates of the cellular protoplasm. My reasons for this view are twofold. (1) The granules are sometimes seen extruding from a crushed or broken cell,

looking like coins dropping out of an open purse. If they were integral parts of the reticulum they would not run out in this manner. (2) They shrink upon drying. If you let your specimens dry in the air for a few days you will find the granules shrunken and distorted, and around and among them innumerable vacuoles or air spaces. If you attempt to preserve them in Canada balsam you will find them completely mummified and distorted beyond all recognition. If they were parts of the reticulum they ought to keep perfectly well in balsam. Therefore I conclude that they are precipitates, and probably of a plastic consistency, as we might expect protoplasmic precipitates to be.

If the foregoing observations are correct, it is clear that the eosinophile can no longer be regarded as a specific cell. It is a purely pathological product, resulting from a natural, alkaline decomposition of the body of a common leucocyte. It is a necrotic cell.

The decision of the case must be left to the discerning judgment of the profession.

Reports of Societies.

SURGICAL SECTION OF THE SUFFOLK DISTRICT MEDICAL SOCIETY.

F. G. BALCH, M.D., SECRETARY.

REGULAR meeting Jan. 2, DR. F. S. WATSON in the chair.

DR. FRANCIS S. WATSON read a paper on

MOVABLE KIDNEY.¹

DR. C. B. PORTER: With regard to the new ideas which Dr. Watson has advanced, I have nothing to say. As I thought over this subject, it occurred to me that a copy of the report of the first case of movable kidney I recollect operating upon might be interesting. It was in 1889. A number of the points in connection with it are so characteristic of floating kidney that I thought I would have a copy made, which I will read.

May 13, 1889. Mrs. R., 21 years, Montreal. Thinks she was perfectly well till 9 years ago, when in a coasting accident her left patella was fractured, and she received a general shaking up; since this accident she has never been perfectly well. For several years treated by a doctor for consumption. The symptoms then were too frequent menstruation, fainting spells, pain in back and general debility. Symptoms were improved a little, but after a year recurred. The only time since then when she has felt entirely well was while carrying her baby, who is now 3 years old and her only child. One month after the birth menstruation began and appeared ever since at periods of about 18 days. Has been troubled much for a long time with indigestion and flatulence, occasionally vomiting.

¹ See page 318 of the Journal.

For about a year past has had "stitches" in right side, especially when sitting in a forward position. Pain in small of back and right thigh, at times very severe; frequent micturition, sometimes painful. During past 3 months has noticed at intervals a "lump" in right iliac and lumbar regions; would appear for a time, and then suddenly disappear to reappear without apparent cause at intervals of a varying number of days. Tumor very tender.

Diagnosis, movable kidney.

Operation, May 15, by Dr. Porter, under ether. Patient was placed on left side with pillow under flank. Incision about 3 inches long made in usual line of lumbar incision; dissected carefully through the various layers of muscles, several bleeding points being tied with catgut. Finally a fibrous layer with fat-filled meshes was exposed; upon incising this the kidney was developed and was seen to move up and down freely with each respiration. The incised capsule was then held up by forceps, and 6 silk stitches were placed in its free edges. With kidney held firmly in place by manual pressure from in front, the 6 stitches were passed through the muscle layers at edges of wound and tied tight. Respiratory movement of kidney now ceased. The wound was thoroughly irrigated with corrosive, and closed with deep silk sutures without drainage. Antiseptic dressing.

Convalescence uneventful; discharged well June 12.

I had examined this lady in private before she went to the hospital and readily felt a floating kidney. The history of this case showed many of the symptoms of movable kidney, and followed trauma; was relieved for a time by the support of the pregnant uterus and restored to health by nephrorrhaphy.

Dr. Watson did not speak specially of the surgical treatment other than the last. So far as my experience goes the symptoms vary very much in different cases, and that the detecting of movable kidney needs an examination in the dorsal position and also in the upright position. In the dorsal position sometimes forced expiration will not dislocate the kidney and bring it down where it can be felt between the two hands, but when the patient is erect or leaning over to relax the abdominal walls, the kidney can frequently be felt making large excursions downwards. When a kidney is quite movable, it can be pushed out past the middle line or down into the pelvis. The mobility does not seem to be the element which enters into the production of the severity of the symptoms in all cases. A very mobile kidney sometimes does not give any severe symptoms except when pressed upon, and a slightly movable kidney may give a great deal of pain. To what that is due I cannot say, unless when the pedicle is short the weight of the kidney drags more than when it is long. The method of operating which I used on this case, and have practically used since, was by exposing the kidney and sewing the kidney in place, attaching it to the muscle and the deeper fascia, not bringing the