

site is a small cylinder with rounded ends, resembling a segment of a ruler.

These parasites are rather sparing in number, and very difficult to stain. For some time I was attracted by what I thought were vacuoles or grease globules; these were the parasites which did not take the stain.

I stain with warm carbol-fuchsin for twenty minutes, then soak in weak spirit and water.

The bodies I describe, though I do not claim them to be the causal parasite of the disease, are of sufficient interest to warrant a description. I take them to belong to the sporozoa, members of which group are credited with pathogenic characters.

Rixford and Gilchrist describe in detail two cases of Protozoon infection of the skin (John Hopkin's Hospital Report, Vol. I., page 209, 1896), and the bodies seen in this chronic cattarrhal pneumonia may be the cause of the lesions just described.

In conclusion, I may state that up to the present I have failed completely to convey the disease from sick to healthy animals by cohabitation, feeding, and all varieties of inoculation methods, both with blood and affected tissues.

This disease I think is mainly worth note because of its insidious progress and incurability, and because it shows no tendency for the affected parts of the lung to undergo degeneration, caseation, or the formation of abscess.

## DESCRIPTION OF PLATE VII.

### *African Coast Fever.*

FIG. 1. Exact reproduction of a microscopic field in a blood film from an advanced case of African Coast fever. Stain azure II. and eosin. Zeiss  $\frac{1}{12}$  oil imm., ocular 4 (*see* p. 217).

### *Jagzickte.*

FIG. 2. Section of badly affected lung, showing complete blocking of the alveoli with proliferated epithelium.

FIG. 3. Section showing junction of badly affected and comparatively healthy lung tissue.

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## IMMUNITY.<sup>1</sup>

By WILLIAM BULLOCH, M.D., Bacteriologist to the London Hospital, E.

THE advent of the bacteriological era in medicine twenty-five years ago revived the old question whether resistance to the invasion of infectious virus is brought about by the body humours or the body cells, and just as the old humoral doctrines preceded Virchow and his cellular pathology, so here the humoral doctrines are chronologically the first. In dealing with the question of the relation of the cells and fluids to the immunity problem—a problem which for interest to the

<sup>1</sup> Reprinted from the "British Medical Journal," 10th September 1904.

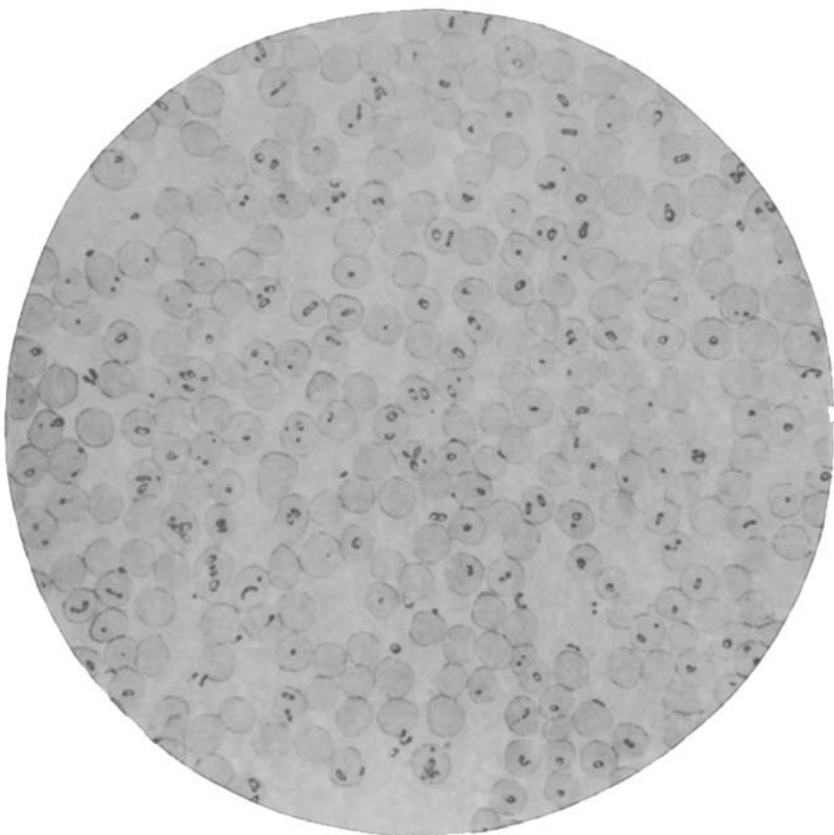


Fig. 1.

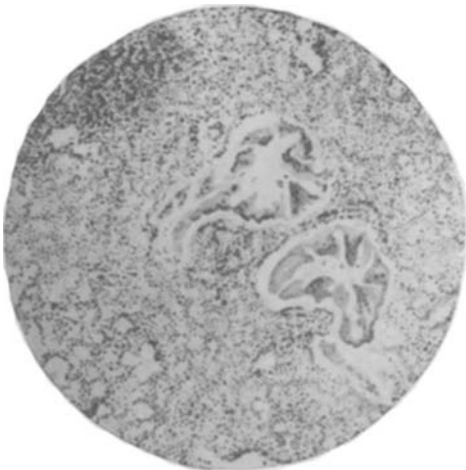


Fig. 2.



Fig. 3.

medical scientist is without its rival at the present time—it seems to me necessary to pause and consider the foundations of our beliefs as progress towards the final goal—the construction of a rational system of therapy based on the etiological curative principle—can only be obtained by following the great currents and by avoiding the eddies and backwaters which lead nowhere. In the present instance I have attempted by a study of the extensive literature and by independent observation to determine what is fact and what is fiction in regard to the question of the cellular pathology and immunity. Too often even the youngest of us have seen beliefs taught to us as facts swept away because their foundations were insecure, and isolated experiments on animals of lowly organisation have been utilised without further criticism for the construction of hypotheses applicable to problems of vast complexity in the higher mammalia. The Pathological Committee of this Association has, I think, done wisely to discuss at the present time the doctrine of immunity in its various aspects, so that by observation, criticism, and study we may direct our course into paths likely to be heuristic, and that we may distinguish incontrovertible facts from what is doubtful, and from what perhaps may better be referred to the domain of myths.

That there is a condition of immunity which can be artificially produced is certain, and we owe the basis of this knowledge to the classical work of Pasteur on *Cholera des Poules*, Pasteur and Thuillier on *Rouget*, and Pasteur, Roux, and Chamberland on *Anthrax*. Not satisfied with the mere existence of immunity, attempts were then made to solve the nature of the immunity and its mode of action. Certain facts were observed, and then speculations arose as to how these facts are to be interpreted, and from this time dates the difficulties which have arisen in the immunity question. From actual observations in flasks of broth with cultivations of the chicken cholera bacillus, Pasteur formulated his *theorie d'épuisement*, or exhaustion theory, to explain what takes place in the body of the fowl immunised by the hand of man. Now, it often happens that a theory, although it contains a great element of truth, may be subverted, because it cannot at the time satisfactorily explain particular observations. Such has been the fate of Pasteur's theory because it could not explain the fact that immunity can be produced by the soluble products of microbes, and because the tissues of animals, immune or otherwise, suffice perfectly as a medium for the growth of microbes. (Bitter, Griffin, Metchnikoff.) Although, therefore, Pasteur's theory cannot be held to explain the whole question of immunity, it is still a question whether certain forms of natural immunity may not owe their existence to an absence of suitable nutritive pabulum—a view revived in recent years by Ehrlich in the conception of the absence of suitable receptors.

Chauveau on the analogy of the noxious substances produced by microbes in artificial media, thought that in the living body substances might be retained, these acting detrimentally on the microbe, and this was the foundation of his doctrine or *theorie de la substance ajoutée*. In his belief he was supported by the fact that the lambs of sheep vaccinated against anthrax may be born immune; and secondly, that it is possible to immunise by means of soluble products of bacteria.

Both these old theories neglect what is now known to be a funda-

mental factor, namely, the host infected and the means employed by that host to resist microbial infection. This was first clearly set forth by Metchnikoff, whose writings have played an immense rôle in the elucidation of this whole problem. As is well known, Metchnikoff's theory of immunity is that the cells bring about the resistance to infection, and this is based upon a number of observations, notably the discovery by Haeckel (1862) that amœbæ englobe foreign bodies, the discovery of Langhans (1870) that emigrated leucocytes in hæmorrhagic foci became charged with red blood corpuscles. Birch-Hirschfeld (1872) had found that micrococci injected into the blood are taken up by leucocytes, and Panum had suggested that possibly this was a method by which microbes might be destroyed.

Röser expressed himself much more definitely that the immunity is due to the ability of contractile cells to seize the infectious virus.

Long before this Metchnikoff (1865) had himself shown that the intestinal protoplasm of Geodinus, a land planarian, is capable of englobing foreign bodies, and from about 1878 onwards he has in a long series of researches laid the foundations of his so-called phagocytic theory. These foundations cover a wide zoological field, ranging from the lowly rhizopoda to the highest mammals (Metchnikoff). Briefly, the theory held by Metchnikoff was that the virus was destroyed in the interior of certain mesodermic cells by a process of digestion. In recent years Metchnikoff has had to shift his standpoint to a certain extent, although in the main the cardinal doctrine is the intracellular digestion in leucocytes and other cells of mesodermic or even epiblastic origin.

While Metchnikoff was collecting his data and building up his hypothesis another theory was gradually being evolved—the humoral theory defended chiefly by Buchner, and at first this was directly antagonistic to the views promulgated by Metchnikoff.

The humoral theory was based on certain experiments by Traube and Geschleiden, Wyssokowitch, and Fodor, who observed that bacteria injected into animals cannot be recovered again although the same bacteria did not appear to have made their exit from the body.

From 1888 dates a long series of researches inaugurated by the important memoir of Nuttall, showing that destruction of bacteria takes place in cell-free serum outside the living body altogether, and that the bacterium-destroying property in the serum can be inhibited if the serum be heated to 56° C. This fundamental experiment has been confirmed by numerous workers, among whom one may mention Behring, Nissen, Lubarsch, Stern, Denys and Kaisin, de Giæxa and Guarnieri, Leclef, Denys, Buchner. In particular Buchner has elaborated this branch of work in a long series of elaborate memoirs, culminating ultimately in his alexic theory of immunity. In spite of various objections raised by Christmas, Székely and Szana, Baumgarten and his pupils, it is now accepted as proved that in the serum there are substances—alexins—which destroy certain bacteria *in vitro*. Buchner has drawn special attention to the characters of the alexins—their thermolability, their absorption by bacteria, and their behaviour on dialysis.

Leaving the question of the existence of alexins, we find ourselves

on very uncertain ground when we come to the problem of the site and the mode in which Buchner's alexin is manufactured in the body.

Hankin was the first to refer the alexins to a leucocytic source, but erred in regarding the eosinophilic cells as the alexocytes (Mesnil). There is no doubt, however, that his work led to other researches, which culminated in a partial fusion of the rival humoral and cellular theories. These researches are closely connected with the names of Denys and his collaborators in the Louvain School, and were accepted by Buchner in his address at the Congress for Hygiene and Demography in Buda-Pesth in 1894. The opinions of Denys were further strengthened by the observed correlation between hyperleucocytosis and alexic action (Sanarelli, Everard, Massart and Demoor, and Bordet).

Before dealing with this important question of the connection of the leucocytes to the amount of alexin, it will be remembered that in point of time the discovery of antitoxin had insinuated itself into the immunity problem, and the same humoral and cellular difficulties again arose. Is this remarkable substance antitoxin merely in the humours or is it a vital cell product? That the latter view is correct is apparent from the researches of Roux and Vaillard, Salamonsen and Madsen, Knorr, and constitute a support for Metchnikoff's views of the cellular origin of antibodies, the whole question being, Are we, or are we not, in a position to locate this cell factory? Is it limited to one type of tissue, as affirmed by Metchnikoff, or not? In all his publications Metchnikoff has definitely committed himself, and has asserted the pre-eminent rôle of the leucocyte, and most of the researches emanating from the Pasteur Institute have been dominated by Metchnikoff's ideas.

We have already referred to the possible origin of alexins from leucocytes, and we must now consider carefully the grounds on which Buchner accepted this new view as expressed by him at Buda-Pesth in 1894, and why he departed from the purely humoral conception, for he definitely stated that the leucocytes possess important functions as defensive organs, and especially from their ability to secrete alexins. Phagocytosis he regarded as a secondary phenomenon.

Denys and Havet found that dog's serum is less bactericidal than dog's blood, and special attention was directed to the factors to which this could be due. They tried blood which had been filtered through paper, and serum with and without the addition of leucocytes, and using as test objects bacterium coli, bacillus subtilis, and staphylococcus, they came to the conclusion that by filtration dog's blood loses practically all its bactericidal properties, and as the leucocytes were kept back by the act of filtration, they assumed that they must in some way be connected with the formation of alexin. The blood of man, pigeon, or fowl did not, however, yield similar results. The microscopic examination permitted the observation of all stages of phagocytosis, and although they thought that the serum itself plays a very small part in bactericidal action, they stated in their conclusions that neither the phagocytic nor the humoral theories taken separately are sufficient to explain immunity, but the cells and fluids act together in a manner varying with the species of animal, and probably also with the nature of the virus.

Havet observed an increase of bactericidal power following a leucocytosis produced by injections of staphylococcus cultures.

By means of exudates artificially produced by aleuron emulsions, Buchner found that the leucocytic fluids were more bactericidal on bacillus coli than serum or blood. The question of phagocytosis was put out of court by killing the leucocytes by freezing and thawing. Denys also believed that the alexins were secretory products of the leucocytes. The experiments of Buchner were repeated with variations, who came to similar conclusions. In order to obtain leucocytes as isolated as possible, he introduced into the peritoneal cavity small pledgets of sterile cotton-wool, and after a number of hours these were removed and squeezed, and the fluid frozen and thawed, and then tested. The results were not quite harmonious, for although the bactericidal action of the leucocytic fluid for bacillus typhi and staphylococcus was greater than that of the serum, the contrary was found to be the case for vibrio cholerae. From Hahn's calculations it is not apparent that the bactericidal action on staphylococcus was very pronounced, or even beyond the limits of experimental error. Schattenfroh made an extensive series of observations with leucocytes more isolated than had been the case in the experiments of Denys, Buchner, Hahn. The leucocytes were thoroughly washed in saline solution, and then centrifugalized. They were then added to the inactivated serum, and the bactericidal action was observed.

Schattenfroh also found that if isolated leucocytes were frozen and then mixed with inactive exudate and macerated for one to two days in the cold they acted bactericidally on certain bacteria.

From this period onwards a very large number of researches have been published to determine the question of the exact relationship of the leucocytes to the alexins, and as time has gone on it has become apparent that the question is a complicated one. This is the outcome of investigations by Bordet, Ehrlich and Morgenroth on hæmolytic serums and from the fact that Buchner's alexin is a complex of two distinct substances, namely, a complement (alexin proper) and an immune body (amboceptor. Ehrlich).

As is now well known, the injection of alien erythrocytes leads to the development of a hæmolytic serum, the hæmolysis being due to the co-operation of a thermolabile complement—also called alexin and thermostable immune body, otherwise amboceptor.

Clinging fast to his cellular theories, Metchnikoff believes that both these bodies are contained in certain cells or at any rate are produced by them, and that in the main cells like erythrocytes are destroyed by certain ferments—cytases—present in the macrophages. The existence of these cytases is based on experiments with protozoa and actinia. Experiments with neutral red, led Mouton to the belief that amoebæ possess a ferment—amibodia' stase. Similar ferment, actinodiastase, was extracted from actinia by Mesnil. Arguing from this Metchnikoff supposes that the leucocytes are similar and that they also possess endocellular cytases.

For a number of years Metchnikoff has distinguished two great types of phagocytes, namely, macrophages and microphages, the former possessing no granulations, the latter including the neutrophile and eosinophile leucocytes. The exact histological characters

of the macrophage are to my mind not apparent from Metchnikoff's writings. He sometimes describes them as large lymphocytes and speaks of their chief site as being the lymph glands, epiploon, and spleen. At other times (p. 84) he says that the mesoblastic phagocytes are divisible into fixed phagocytes or macrophages of the spleen, endothelium, connective tissue, neuroglia, muscular fibres, and into free phagocytes. He also tries to show that the macrophages and microphages act differently, in so far that the macrocytase destroys red corpuscles and cells, the microcytase being chiefly concerned with microbicidal action, and he parallels this conception with the existence of other leucocytic ferments, such as fibrin ferment, amylase, lipase, and oxidase.

Proceeding still further, Metchnikoff regards the identity of the cytases with Buchner's alexin as demonstrated, and that the alexic action is of the nature of a digestive process, although this is entirely negated by the experiments of Nolf, Gruber, and others. He also assumes that any macrocytase which is present in the serum has come there by the phagolysis of the macrophages. The discovery of the complex character of cytolytic serums no doubt was a puzzle to Metchnikoff at first, but he rose once more to the occasion by affirming that the amboceptor, or, as he prefers to call it, the fixator, is also phagocytic in origin, and is produced in abundance during intracellular digestion. When it is produced in excess it escapes from the macrophages, and makes its way into the serum, in which it can be demonstrated in large quantity.

We have in these statements the most concrete and most extreme views yet published on the relation of cells to immunity, and it now behoves us to consider whether these views have met with the support of other investigators.

I have already dealt with the views of Denys, Havet, Hahn, and Schattenfroh on the alexic secretory action of leucocytes, and we have to discuss whether there are real grounds for this belief.

In the first place, it may appear strange that the simple question whether the alexin is free in the plasma or not has been the subject of numerous researches of contradictory character.

Metchnikoff, basing his conclusions on the work of Gengou, affirms that the plasma is alexin-free. Gengou examined the blood of rabbits, dogs, and rats in paraffined vessels to prevent coagulation and failed to demonstrate bactericidal action in the plasma for *bacillus typhi*, *bacillus anthracis*, *bacillus coli*, and *V. Metchnikovi*, a result confirmed by Levaditi; Petterson, on the other hand, using oxalate and citrate plasma, got entirely different results in the case of *bacillus typhi* and *bacillus coli*. Von Dungern found that the plasma of *scyllium canicula* was quite as hæmolytic for rabbit's corpuscles as the serum. Hahn could find no difference between the serum and histon blood. Similar results led Sinnitzky and Lambotte to the belief that alexin or complement circulates free in the plasma.

The Munich school, influenced by the work of Buchner, Hahn, and Schattenfroh, and regarding the alexin as a secretory product of the leucocytes, have attempted by a variety of methods to extract alexin from these cells. Van de Velde used distilled water as an extractive. Bail used leucocidin, Löwit ground up the leucocytes with glass powder. As a result of numerous experiments Schattenfroh came to

the conclusion that the substances which can be extracted from the leucocytes are not identical with the serum alexins. Shibayama examined the effects of extracts of guinea-pig's organs on dog's blood and found that hæmolysis was produced by splenic and lymphatic gland extracts, but not by bone marrow or other organs. Tarassewitch examined the hæmolytic action of extracts of organs of guinea-pigs, rabbits, and dogs, and found the so-called macrocytase in the lymph glands and other macrophagic organs, a conclusion also supported by the experiments of Levaditi. Korschun and Morgenroth found that certain organ extracts were hæmolytic, but that the hæmolytic substances present were quite different from alexins, as they were very heat-resistant, soluble in alcohol, and incapable of forming antihæmolysins. Similar results were obtained by Donath and Landsteiner and Doemeny.

Petrie was unable to find any bactericidal substance for bacillus typhi in any of the leucocytes either of the normal or the immune rabbit. Nor was he able to reactivate an inactivated serum by the addition of leucocytes or leucocytic extracts. Similar negative results were obtained by Ascher in the case of *vibrio cholerae*. In a study of the source of hæmolytic complement and amboceptor, Bulloch, by simultaneously measuring the amount of the hæmolytic constituents on the one hand and the mononuclear and polynuclear leucocytes on the other, found that the appearance of the amboceptor in the serum is coincident with a rise in the number of mononuclear leucocytes, and the latter follow the fluctuations of the former with regularity.

By producing an artificial polynuclear pseudo-eosinophilia there is a coincident rise in the amount of the complement, but no rise in the quantity of immune body. Ainley Walker also believes that complement is a leucocytic product appearing in the blood plasma mainly as a result of disintegration of the leucocytes. Studying the serum complement in disease, Longcope observed a rise in the quantity of complement associated with hyperleucocytosis.

In the case of immune serums, Pfeiffer and Marx, Wassermann, and Deutsch found the main site of formation of antibodies to be the spleen, bone marrow, and lymph glands.

Taking advantage of the fact that a certain time—usually four or five days—elapses between the injection of ox blood and the development of the specific hæmolysis in the blood of rabbits, I attempted, by making extracts from the different organs, to determine where the hæmolysins were formed; but the technique was so difficult and the results so uncertain that the research was not continued, but so far as it went there was no evidence that more hæmolysin was formed in the spleen and bone marrow than in other organs.

On the other hand, Wauters, experimenting with staphylococcus and hay bacillus in the case of pigeons and rabbits, and making extractions of the organs with inactivated serum, found the most marked bactericidal properties in bone-marrow extracts. Briscoe and Moxter could find no immediate relation between the microphages and the presence of bactericidal complement.

Other researchers are not only satisfied that this relationship exists, but they have sought to demonstrate the manner in which the leucocytes part with their alexin. Laschtschenko used as a leucocytic extractive a serum heated to 55° C., and not in itself bactericidal, and



he assumed that in such a serum the leucocytes were living, and that the increased amount of alexin must necessarily be due to a secretory activity on the part of the leucocytes. Trommsdorf, repeating this work, estimated the vitality of the leucocytes by their amœboid movement and by their staining reactions with Nakanishi's method, and he found that the majority of the leucocytes were living. Most recently Lazar, doubtful as to whether these estimates of vitality were sufficient, took as his test the ability of the leucocytes to exert a phagocytic action, and he ultimately came to the conclusion that bactericidal substances get into the serum only when a certain number of the leucocytes are destroyed.

From this mass of conflicting details it would appear difficult to draw accurate conclusions in regard to the origin of protective substances. In the first place, the existence of alexins is undoubted, but nothing certain is known of their origin. Even the question of their existence in the plasma is not definitely settled, although the balance of opinion is in favour of their concurrence outside the blood cells. It is not certain that cytases exist in Metchnikoff's sense. It is very improbable that alexin acts like a digestive ferment. It is not proved that leucocytes secrete alexin, although it is probable that they do.

It may be asked why there is this extraordinary diversity of opinion, and the answer must be sought in the complexity of the subject and the defects in the technique employed, for there is no doubt that the experimental error in determining the number of bacteria destroyed, especially when this estimation is carried out by the older plate method, is a very great one. This is clearly seen when one comes to an organism like staphylococcus—an organism much in favour in this class of work. According to the best bactericidal methods of Wright, it is impossible by the aid of a serum, normal or immune, to kill a staphylococcus by bacteriolysis, using the term in its technical sense.

The result of all this investigation has been the development of a great harmony between the views of the rival schools of Buchner, Ehrlich, and Metchnikoff, and it would seem at the present time that the differences are more those of words than of actualities. Perhaps the main point still is that the French school attribute to the leucocytes a more important rôle than that admitted by the Germans, and there is a great amount of evidence that the leucocytes are of importance, although the exact manner of their action is still unsolved. We have spoken of antitoxic immunity and bactericidal immunity, and we may ask ourselves whether the host has exhausted its resources against the virus or the poison by these two methods. In other words, is immunity always antitoxic or bactericidal? and I use the word "bactericidal" in a specific sense and not in its etymological sense. According to Nuttal, Stern, Neisser, and Wright the serum of an animal is not bactericidal towards staphylococcus. I have found the same in the case of *B. pyocyaneus*. No doubt staphylococcus and *B. pyocyaneus* are destroyed in the living body, but not by the specific method we know as bacteriolysis. Now it is a remarkable thing that for the majority of infective diseases the immunity is neither an antitoxic nor a bactericidal, and it is chiefly in the case of these diseases that Metchnikoff's phagocytic theory was established, and he has always clung to the belief that the serum

plays a subordinate rôle compared with that of the cells. By a study of the serum and the phagocytes separately Wright and Douglas, in two epoch-making papers, have shown that in so-called phagocytosis a most important, if not a cardinal, rôle is played by substances in the serum, which substances have nothing to do with Buchner's alexins. Their method has been to mix serum or plasma with bacterial emulsions and washed leucocytes, and after contact of these for fifteen minutes films are prepared and stained by Leishman's dye, and the number of bacteria ingested by the leucocytes is taken as the index of the phagocytosis. This method is a modification of that originally brought forward by Major Leishman in his studies on phagocytosis.

Wright and Douglas found that the phagocytic property is arrested by heating the serum to  $60^{\circ}$ - $65^{\circ}$  C. for ten minutes, although the leucocytes are, of course, normal; that is to say, the leucocytes are practically unable to pick up microbes by themselves. If the unheated serum is mixed with bacteria at  $37^{\circ}$  C. for fifteen minutes and the mixture is then heated to  $60^{\circ}$  C. for fifteen minutes, phagocytosis can still take place, thus demonstrating that the serum acts in some way on the bacteria, rendering them suitable prey for the phagocytes. This thermolabile serum feast preparer is called by Wright and Douglas opsonine (*ὀψωνιαζω*, to furnish with provisions).

They have also shown that during the process of active immunisation the opsonic value of the serum is increased, and they have succeeded in demonstrating this opsonic immunity for a number of infections, such as staphylococcus, Malta fever, pneumococcus, tubercle. In conjunction with E. E. Atkin I have devoted considerable time during the last six months to the question of opsonic immunity, and we have been able to confirm the work of Wright and Douglas, and have studied in other directions the mode of formation and action of opsonic serum. We have found it impossible to increase indefinitely the amount of opsonine in the serum as a result of inoculation. A limit is apparently reached very soon, so that if the serum is diluted the opsonic effect is not manifest. The effect of injections of microbes on the opsonic content of the serum is similar to the effect on the antitoxic or bactericidal content. There is, as is well known, a negative phase, a positive phase, and a higher base level. The reason why the opsonic value cannot be indefinitely increased by inoculation is difficult to understand, especially as we have found that the opsonine is a body built on the type of agglutinin, and analogous to Ehrlich's receptor of the second class. Opsonic serum which has been heated cannot be activated again, although this inactive opsonin is seized by bacteria and fixed to them, so that when mixed with fresh active opsonin no opsonic effect is apparent. We must admit, therefore, the existence of an opsonophoric and a haptophoric group. Further, we have demonstrated that the serum contains a number of opsonines, each of which is specific for given bacteria. By saturation experiments we can remove the one opsonine after the other, our results being in harmony with the pleurimistic doctrines of Ehrlich. From the researches of Wright and Douglas, Bulloch and Atkin, it would appear that in addition to antitoxic and bactericidal immunity there is a third type, namely, opsonic immunity, which is a common phenomenon, and is brought about by the coalition both of humours

and cells, and it corresponds largely to what Metchnikoff has preached for years as phagocytosis. The actual part played by the leucocyte in the drama seems less clear than ever, as evidently the bacteria are altered in some way before being ingested. This alteration is not, however, a fatal one to the microbe, and no doubt plenty has to be done by the leucocyte before the microbes are disintegrated.

Thousands of facts point to the conclusion that our leucocytic-forming tissues are our great defensive organs against parasitic invasions. The mystery is how the microbes are destroyed, and in this connection we seem pretty much in the same state as John Hunter over a century ago, when he wrote of pus: "The final intention of this secretion of matter is, I believe, not yet understood, although almost everyone thinks himself able to assign one, and various are the uses attributed to it."

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## EDITORIAL ARTICLES.

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### THE TREATMENT AND PREVENTION OF SHEEP-SCAB.

IN the month of April 1903 the late Mr Hanbury, then President of the Board of Agriculture, appointed a Committee "to investigate experimentally and to enquire into and report upon:—

"(1) The composition and essential constituents of efficient dips and other preparations for the treatment and dressing of sheep, and their effect upon the animal treated or dressed and upon the parasites and other organisms for the destruction of which they are used ;

"(2) The methods in which such dips and other preparations should be employed, and the appliances and facilities requisite for the purpose ;

"(3) The times and intervals at which sheep should be treated or dressed, regard being had (*a*) to the life-history and characteristics of the sheep-scab acarus and of the other parasites and organisms of sheep which require external treatment ; and (*b*) to the practical conditions under which sheep-farming is carried on in various parts of the United Kingdom."

The Committee's Report and the Minutes of Evidence taken by them have recently been published, and the principal recommendations contained in the former are given at a later part of this number. Although the Minutes under which the Committee was appointed were so drawn as to cover the treatment necessary to combat all the common external parasites of the sheep, the enquiry was mainly