appears to be distinctly connected with the absence of the destructive effects which are observed under opposite conditions, it may be fairly inferred that these processes are protective—that is, that although they are themselves constituents of a morbid process, they conduce to the destruction or elimination of the *materies morbi*; but in the facts before us there is nothing to justify the conclusion that the incorporation of microphytes in cells is the chief or only way in which the human or animal organism exercises its protective influence.

THE STREPTOCOCCUS.

It must be borne in mind that there are other organisms which stand in the same or in a similar relation to the process of inflammation. I have referred to streptococcus only, as regards which it has been shown by a large body of evidence, clinical and experimental, that its action resembles staphylococcus in most respects. The question of the difference between them has been lately satisfactorily dealt with by MM. Lannelongue and Achard in the research to which I have already referred. The results to which they have come are these. The visceral lesions produced by the chain-coccus differ only in degree—that is, in their being less intense—from those due to the cluster coccus. Each is capable of producing osteomyelitis experimentally, and may be the cause of it clinically.

That two microphytes which are so easily distinguished in the laboratory should be almost indistinguishable in their action is a fact which evidently bears on the view to be taken of the relation between a microphyte and its morbid action. This we shall have the opportunity of considering in relation to another phlogogenic microphyte—namely, the lung coccus, which plays so important a part in several infective inflammations.

PUERPERAL ECLAMPSIA.

By ROBERT BARNES, M.D.

I HAVE read with much interest the discussion on puerperal eclampsia started by Dr. Galabin at the recent meeting of the British Medical Association at Bournemouth. As I was not able to assist at the meeting I trust that I may be allowed to take part in the discussion in the *BRITISH MEDICAL JOURNAL*. I think I am not exceeding the bounds of just criticism in saying that almost every point brought forward by the speakers at the discussion is fairly set forth in the *System of Obstetric Medicine and Surgery* by myself and Fancourt Barnes. Reviewing the principal points expounded by the speakers, the following seem most directly to challenge observation.

1. It seems to be taken for granted, by Dr. Galabin especially, that nephritis and the attendant albuminuria and uræmia cause the convulsions. He states indeed the "form of nephritis associated with eclampsia appears to be especially severe in its effect of diminishing the excretion of solids, and that in the puerperal woman reflex excitability is naturally increased in preparation for the process of labour." Where is the proof that the kidney is inflamed? Because there is sufficient evidence of inflammation in acute and chronic Bright's disease, one credential of which is the presence of albumen in the urine, does it follow that whenever there is albumen in the urine the kidney is inflamed? If it be urged that there is further evidence of inflammation in the state of the kidneys revealed by necropsy, it may be objected that there is a wide difference between the conditions of cases that end fatally and cases that recover. Not to insist too much upon this distinction, it is not unreasonable to submit that some of the cases ending fatally do so greatly because the kidneys are too completely disabled by inflammation or other change; whilst the numerous cases that recover do so because the kidneys are only functionally affected. A strong argument in support of the latter proposition is this: the rapidity, even suddenness, of the recovery of the kidney power when the cause of the convulsions—the pregnancy—is removed. Is there any analogous example of acute inflammation of an organ passing away in a few hours

or even minutes? More than this, disease of the kidney is not constantly found, even in fatal cases. Depaul (1854) stated that in the necropsies he had made, the kidneys were perfectly healthy or simply congested. It is further certain that in some of the cases in which disease was found, this was of chronic character antecedent in origin to the pregnancy.

2. Let us now examine the relations of albuminuria to eclampsia. Dr. Byers puts the case as follows:—"Does the kidney affection cause the eclampsia, or does the eclampsia give rise to the abnormal state of the kidney; or, finally, is there some primary cause occasioning the convulsions, and at the same time giving rise to the kidney lesion?" Dr. Braxton Hicks has advanced the view that the convulsion may cause the albuminuria, but this theory has met with little support." Dr. Byers thinks Dr. Hicks's view is disproved by the fact that no albumen has been found in the urine of many patients after an epileptic attack. The argument is altogether strained.

It is enough to reflect that epilepsy is not puerperal eclampsia. The cases are essentially distinct. Dr. Hicks shows clearly the fallacy of Dr. Byers's argument in the *BRITISH MEDICAL JOURNAL* of October 3rd. In the discussion under comment Dr. Byers adverted especially to the researches of Leyden. This pathologist found that when the kidneys in three fatal cases of eclampsia with albuminuria had remained for a time in spirit the fat in great part disappeared, and then the organs on microscopic examination appeared to be normal, and accordingly inferred "that this fatty condition is not a degeneration but an infiltration." That is to say that, even in some fatal cases, the appearance taken to show inflammation really indicate an exaggerated physiological condition similar to that which I have described in the mucous membranes of the pelvic organs.

The believers in the nephritic theory think their case is confirmed by the observation of granular casts and epithelial scales along with albumen in the urine. But this is a fallacy not difficult to explode. Under the dominant vascular tension of pregnancy (and this is open to direct observation in the living subject) the mucous membrane of the pelvic organs undergoes an intense degree of hyperæmia which may be called congestion, but which it would be absurd to call inflammation. This hyperæmia in the cervix uteri and vagina is marked by deep red and purple coloration, by tissue engorgement, by the profuse shedding of epithelial scales and exudation of muco-albuminous fluid. These appearances, indeed, constitute one of the most obvious objective signs of pregnancy. It is quite legitimate to infer that a similar condition exists in the kidney which is within the sphere of similar influences. And, be it noted, we find these conditions present in cases free from excess of albumen in which the pregnancy and labour run a healthy course. This is described in the work referred to, but there is the confirmation of strong analogy in the researches of Tarnier, De Sinéty, Leyden, and others as to the changes wrought in the liver in pregnancy. These changes I have myself observed, and, working with me, the late Dr. Sutton at the London Hospital and Dr. Ewart at St. George's Hospital have made similar observations.

3. What, then, are the essential factors of eclampsia? It is clearly not necessary to invoke disease, inflammatory or other, of the kidney, since clinical observation proves that eclampsia may break out in subjects whose kidneys are sound; it is equally clear that albuminuria is not the cause, since eclampsia breaks out where no albumen is detected in the kidney. The theory that the immediate cause of the convulsion is some poison circulating in the blood rests upon strong physiological and clinical evidence. True, what that poison is has not been shown. Urea is found in the blood and is wanting in the urine. Gegenbauer and George Harley were among the first to attest it. In one case in which I bled the patient, Dr. Bernays found urea and uric acid in the blood. This does not prove that urea is the active poison. All that is known positively is that the kidneys, and the liver often in similar cases, are checked in their function of elimination. What the kidneys ought to excrete is retained in the blood. The noxious elements have certainly not been clearly identified. For this reason I have proposed the word "urinæmia" as not committing to an absolute dogma; and even this term

is perhaps too narrow. The intoxication theory is very happily expressed by Dr. Auvard when he says, "Eclampsia is the result of a 'strike' on the part of the organs of elimination." The liver is also on "strike."

4. But admitting the poison factor, the problem is not entirely solved. There is another and an essential factor which was very imperfectly recognised in the discussion: that is, the exalted nervous and vascular tension special to pregnancy. The increased nervous tension was, indeed, adverted to; but the attendant high arterial tension was not even mentioned. Now this high arterial tension is the immediate efficient cause of the kidney struggle and "strike." It is not a little remarkable that the brilliant researches of Dr. Mahomed demonstrating this arterial tension as giving rise to the pressure on the circulation in the kidney should have been overlooked. These researches explain the occasional occurrence of convulsion before the appearance of albuminuria; they prove that there is a prealbuminuric stage, the first stage of the kidney trouble. His observations, followed up by those of Fancourt Barnes with the sphygmograph, throw a new light upon the genesis of convulsion.

It is associated with a condition not adverted to in the discussion, namely, the normal hypertrophy of the heart evoked by pregnancy. This was first described by Larcher in 1857. Ducrest, Blot, Joulin confirmed this physiological fact; and I, having had opportunities of assisting at some of the observations made in Paris, and of making others in England, have almost invariably found the weight of the heart of women dying soon after labour to be an ounce or more in excess of the standard of eight or nine ounces. The observations of Marey, Mahomed, Macdonald, Fancourt Barnes and others demonstrate this hypertrophy of the heart and consequent high tension. Now this state begins in early pregnancy; it continues throughout while the child is alive; and under the mutual reactions with exalted nervous tension may so overpower the natural balance of the functions that convulsions may burst out before the appearance of albumen in the urine. The convulsion does not restore the balance; the underlying factors of nervous and arterial tension remain, and presently albuminuria may appear. In many cases the first call on the physician is to see the woman in convulsion. He, of course, examines the urine, and in all probability finds albumen. It does not follow that the convulsion was the cause of the albuminuria, or that the albuminuria was the cause of the convulsion. Both phenomena are due to the exaggerated nervous and arterial tension. The altered constitution of the blood certainly is a third factor, and it is probable that advancing toxæmia may act as the immediate cause of the explosion. The increased proportion of water and the diminution of red globules are established facts. It is not so generally known that the blood "cups" when drawn into a basin. This shows excess of fibrin. This has been taken to prove that there was fever. This, I—who am one of those who have bled, and seen pregnant women bled by others—am in a position to affirm that the blood of gravid women may "cup" when there is no fever. This characteristic must be weighed in connection with the hypertrophy of the heart and the other physiological phenomena of pregnancy.

These and other associated facts, adverted to in the work already cited, throw instructive light upon the genesis of eclampsia in puerperæ. They deserve the serious attention of those specialists who devote themselves to the study of kidney, heart, and nervous disorders, specially neglecting the luminous illustrations supplied by the great physiological experiment of pregnancy, and its analogue, menstruation. They illustrate in a striking manner the history of nephritis, Bright's disease, the capillary arterial thrombosis of Gull and Sutton, and many other pathological problems.¹

The above phenomena are physiological. Organs strained to the utmost become unequal to their functions. Overstrained physiology is, indeed, perilously near to pathology. Disease may be induced if the strain is kept up, or, on the other hand, a critical catclysm, such as abortion, or labour, or a copious hæmorrhage, may take off the strain, and physiology resumes her sway. Or the overwrought nervous system may give way to convulsion, which may eventuate in recovery, protracted disease, or death.

The principles of treatment flow logically from the obvious etiology of the affection. In the whole range of medicine, there is probably no case in which the disease so clearly dictates the treatment. In the first place, we know the main cause of the disease, the pregnancy. Some specialists, ignoring this primary factor, have enunciated the catching aphorism, "Treat the convulsion or the albuminuria, and let the pregnancy take care of itself." It would be difficult to imagine a sentence composed of two more absurdly contradictory propositions. It amounts to saying: "Treat the effect, and let the cause go on acting." I have not seldom had to contend against this theory. Wherever we find marked albuminuria, with or without convulsion, in a pregnant woman, the first and most pressing indication is to bring the gestation to an end, to reduce the complication to its simplest expression, and then to treat what remains of the effect. In many cases nothing more may be required. *Sublatâ causâ tollitur effectus*. Frequently Nature takes the right course, and frees herself from impending danger by abortion or premature labour. This is one example of what may be called "conservative abortion." Timely effected, it may save two lives; the opportunity neglected, two lives may be lost; or if the mother is saved, it is possibly at the expense of her future health.

But we cannot always depend upon delivery in time. The next indication is to moderate the high nervous and vascular tension. Here again Nature sometimes guides the physician. A copious flooding, which may or may not be the precursor of abortion, sets in, and the fit and the albuminuria may be at once relieved. This suggests venesection—a disputed point. It is not wise to make venesection a rule in practice, but the empirical evidence in its favour in appropriate cases is incontestable; nothing so quickly lowers the excessive action of the heart.

The next indication is to calm the nervous irritability and reduce nervous tension. This indication is of extreme importance. A golden rule I have always long enforced is "in any case in which a fit is probable, to make no examination, not to pass the catheter, to force no food or medicine until the patient is under the influence of chloroform." The slightest disturbance, especially touching the genitals, may provoke a fit.

In the prealbuminuric stage, salines, calomel, podophyllin are eminently serviceable. In the eclamptic stage chloroform comes first. It will frequently avert or shorten the fit. Chloral also acts well. I have seen inhalation of nitrite of amyl act excellently. I advised recourse to this agent in my Lumleian Lectures in 1873. Of pilocarpin I am not in a position to speak with confidence. Antipyrin is worthy of trial, but I doubt if it will act as well as chloroform or nitrite of amyl.

The main motive of this communication is to invite more extended inquiry into the relations of albuminuria and eclampsia; to show that there is no necessary or constant association of the two conditions; that we cannot regard either as the cause of the other; and that therefore a primary cause of both exists in the constitutional conditions evolved by pregnancy. All rational treatment flows from the recognition of this elementary fact.

THE DIAGNOSIS OF SPURIOUS AND DOUBTFUL PREGNANCY.

Read in the Section of Obstetric Medicine and Gynecology at the Annual Meeting of the British Medical Association held in Bournemouth, July, 1891.

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SPEAKING generally spurious pregnancy is most commonly met with in elderly women about their climacteric, or in young women unduly anxious to become pregnant, or guiltily conscious of the possibility of pregnancy.

Following Pajot, errors of diagnosis regarding these cases may be divided into three classes:—1. Affirmation of pregnancy which does not exist, pseudocyesis. 2. Denial of

¹ We have other evidence of high arterial pressure in the cerebral pulmonary and intestinal hæmorrhages of pregnancy.