

ECLAMPSIA.

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IN bringing the subject of eclampsia under notice, it is not my intention to treat of it in all its branches, or occupy the valuable time of the Section by recapitulating facts concerning which the leading text-books of to-day fully inform us. Neither shall I enumerate the many theories advanced as to its causation, but shall rather endeavour to point out what I conceive to be the sound principles of treatment, based on the present facts known in connection with this complaint.

Almost all are agreed that the disease arises from an accumulation in the system of a toxin poison—a normal resultant of tissue waste—which the kidneys, acting in an abnormal manner, fail to eliminate.

As to the *modus operandi* of this poison I shall say nothing, believing that such speculative inquiry is outside the province of practical obstetricians, and I look forward to the day when our text-books shall cease to be padded with fairy tales of physiology.

Whatever we may call this toxin, I believe it to be similar to that which gives rise to the convulsions known as uræmic, and the arguments adduced to prove the dissimilarity of the fits are for the most part worthless. It is urged, in the first place, that the convulsive attacks differ in character, but I have not been able by personal observation to detect any difference; and certainly those mentioned by authorities,

such as a different range of temperature, &c., are entirely untrue.

Again, it is said that eclampsia is not often associated with chronic forms of kidney disease, but with an acute variety that has earned for itself the name of the "kidney of pregnancy;" while, on the other hand, uræmic convulsions are equally as rare in the acute variety of Bright's disease. Before answering these objections, let me for a moment refer to this "kidney of pregnancy," for I fail totally to see on what grounds it came specially to be described as such. So far as I can discover, it differs in no way from the ordinary degeneration kidney seen under many other circumstances.

In pregnancy it is accompanied by a certain amount of parenchymatous nephritis, and arises in most instances as a consequence of that disease. Neither is it by any means constantly seen as a kidney lesion in eclampsia.

To give an answer to the question why acute nephritis is so often associated with eclampsia, we must remember that the centres of nervous control are in a highly unstable condition throughout pregnancy, their functions being often suspended by comparatively trivial causes. It is therefore to be supposed that a smaller dose of poison will exert a graver influence in the pregnant than in the non-pregnant condition. In addition to this there is an absolute increase of toxin manufactured during pregnancy, for not only are metabolic processes more active in the woman herself, but likewise her blood receives the effete material formed during the growth of the foetus.

This latter factor appears to largely account for the onset of eclampsia, for we find that not only do multiple births predispose to the attacks, but that the death of the foetus enormously improves the prognosis of the condition.

Again, the rarity of eclampsia in connection with chronic Bright's disease is no cause for wonder, for this latter disease

is rare in pregnancy. It leads, as a rule, to endometritis, which in its turn leads to sterility or early abortion; while of those cases of pregnancy which persist in spite of chronic Bright's disease, as large a number (if not a larger) develop convulsions than would do so in the unimpregnated condition.

There is apparently a strong argument urged against the theory which supposes a urinary toxin in connection with this disease—namely, that eclampsia sometimes occurs without any apparent kidney disease, and no diminution in the quantity of urine secreted. In connection with this point, the observations of Dr. Ethridge, published in the June number of the *American Gynecological Journal*, are very valuable.

He finds that many women, under certain depressing influences, secrete for long periods at a time an enormously diminished quantity of urinary solids, the kidneys at the same time being in an apparently normal condition, and that these patients exhibit symptoms similar to those known as premonitory in eclampsia.

Thus we see it is not necessary to pre-suppose disease of the kidneys when accounting for a self-intoxication sufficiently potent to excite fits under conditions favourable for their onset.

It is impossible to doubt that the kidneys are the chief organs concerned in the elimination of the uræmic poison. That the skin and intestinal mucous membrane get rid of it to any appreciable amount is greatly to be doubted. Nature appears undesirous of relegating the functions of the kidneys to any other set of organs, and for my part I do not much regret that she has withheld from us the luxury of urinating through our sweat glands. Why do those who believe that the skin can act as a third kidney not settle the question, by injecting the sweat of a uræmic patient into some small animal, and thus contrast its poisonous effect with that

of normal perspiration? Experiments such as this have already been undertaken to test the toxicity of the urine at different periods of the eclamptic attack, with the result that it has now been fully shown that the urine is poisonous to its highest degree during the convalescing period of the disease. I am told of patients who have been seen covered by a fine flour-like deposit after a severe sweating; this phenomenon must be very rare, and possibly may be due, when it does occur, to a diseased condition of the sweat glands.

In dealing with the treatment of eclampsia in the absence of any specific remedy, our efforts should be directed to counteract as many of the gravest conditions tending towards death as possible. Thus the introduction of cleanliness in midwifery has enormously lessened the death-rate in eclampsia by removing sepsis from the list of those conditions. If we except this latter as a cause of death, our present statistics bear very unfavourable comparison with those of former years, and I think the proofs are convincing that we are not at present moving along rational paths.

Chloroform may be said to be now in general use in the treatment of eclampsia, and to this the administration of chloral and of bromide of potassium is generally added. All these drugs act as powerfully depressing agents to the heart, and tend to kill in precisely a similar manner to that of eclamptic poison; for all are agreed that heart failure, followed by oedema of the lungs, is amongst the most constant precursors of death. Furthermore, chloroform as usually recommended is applied only on the occurrence of the first signs of an approaching fit. Given in such a manner, it has but slight effect in lessening the severity of the attack; and by limiting the amount of oxygen that would otherwise pass into the lungs, it in some cases actually increases the severity of the convulsions. Much reliance, also, is placed on getting the skin to act vigorously, the

supposition being that toxins are thus eliminated. It cannot be claimed for this plan that it is at all times easy in its accomplishment, very certain in its good effects, or even safe in its application—always some depression, and occasionally alarming collapse, follow from it.

I do not believe that a more fatal drug could be found for uræmic patients than pilocarpin. It is a heart sedative of the first order, and it has the additional power of promoting free salivation and bronchial secretion—very dangerous complications of the eclamptic state. But in the face of so much experience to the contrary, I am slow to think that at least a temporary improvement in the patient's condition does not follow this treatment, and an explanation of its action has occurred to me.

Convulsions result *not* from the presence of toxins in the blood, but rather from their deposition in the more solid structures of the body, especially in the nerve centres. Now, to deplete the blood by sweating would be to create a current proceeding from these structures into the blood to make good its deficiency in water; in this way the convulsive centres will get rid of some of the poison, the blood at the same time becoming richer in toxins. Whether you accept this explanation or not, I do not think that severe sweating is a justifiable proceeding, and hope to see it soon more generally condemned. Efforts also at giving drugs, stimulants, or nourishment by mouth, must be emphatically condemned. As a rule, the power to swallow is completely in abeyance, and the stertorous breathing is evidence of this fact. Thus croton oil, and even the mouth secretion, find a much easier entrance to the lungs than to the stomach. Let whoever doubts this statement pass a nose-tube on an eclamptic patient, and he will find himself astonished at the ease with which the instrument finds its way into the larynx—an impossible accident under ordinary

circumstances. To ascertain the situation of the nose-tube, all you have to do is to note at what period of respiration air rushes through it. If this occurs during expiration, then the tube is in the trachea; but if during inspiration, the tube is in the stomach, and the air is forced out by the descending diaphragm compressing that organ. It is astounding that this fact has received (so far as I am aware) no notice, for I cannot doubt that the so-called œdema of the lungs, seen with such frequency in fatal cases, is largely to be attributed to fluids which have reached them from the mouth. If this be true, it follows that position must have an important bearing on the proper treatment of these cases. In giving anæsthetics, if fluids form in the mouth, we turn our patients on the side; and so we ought to act in eclampsia, and either prevent our patient at any time lying in the dorsal position, or else employ some suction appliance to keep the mouth emptied of its contents. The gag, too, as usually recommended, and especially in the hands of a nurse, is an instrument fruitful for harm. Physiology teaches us that the act of swallowing cannot be accomplished with the teeth and lips apart. So by inserting this instrument between the teeth any remote chance of the mouth fluids reaching the stomach is entirely removed.

Let me now turn to the surgical treatment for eclampsia—namely, the induction of premature labour, and see to what extent it commends itself as a justifiable proceeding. Its claims to popularity are based on the observed fact that the death of the foetus and emptying of the uterus, as a rule, materially improve the condition of the patient. It has also recently received commendations from the highest authorities as a prophylactic against the onset of convulsions. As regards its employment as a prophylactic agent, nothing could be more unjustifiable, for the reason that no man can say what case is, and what case is not, going to develop

eclampsia—and when I add that not six per cent. of patients showing premonitory symptoms finally develop the disease, the truth of this statement will be evident. It is an unfortunate fact that the surgical treatment is growing in favour day by day, and this is not surprising, for not only has it the sanction of great names, but it also affords the medical attendant much satisfaction, substituting as it does an active mode of treatment for inactivity with apparent impotence. I cannot but think, however, that it is unsound in theory and disastrous in practice.

I remember Dr. W. Smyly pointing out to me what he believed to be a mistake in the text-books—viz., “that the earlier in pregnancy the fits occurred, the more fatal the disease proved.” He rather believes that one of the most serious complications of eclampsia is the onset of labour; and as there is less liability of labour ensuing in the early than the late months, these early cases would usually end in recovery if not interfered with by active treatment. I think a careful study of statistics will fully confirm these views. Certainly, from a theoretical point of view, we should pause before inducing labour, knowing as we do that the danger of the disease is enormously increased by the multiplicity of the fits, and that the fits are easily induced by reflex stimulation. Thus a vaginal examination, abdominal palpation, massage of the kidneys, cold blasts of air, but worst of all, and above all, the dilatation of the cervix, have been observed over and over again to increase the frequency and severity of the attacks. And yet we have to expose our patient to almost all these conditions in order that a very uncertain benefit may follow the emptying of the uterus. Those who support such unscientific methods ought at all events to be able to point to superior results in practice as a justification for them; but these, so far as I know, are not forthcoming. Thus, with your permission, I shall give some figures from Dr.

Green's Report of the Boston Lying-in Hospital, which are excellently compiled and at the same time comparatively recent. They deal with all cases treated in the hospital between the years 1886 and 1892—thirty-six in all—and will be found fully published in the *American Journal of Obstetrics* for June, 1893. The number of cases is, unfortunately, not many, but will be found exceedingly suggestive.

Dr. Green divides his cases into five tables; the fifth deals with fits occurring after labour, and therefore does not concern the present subject. Table I. shows three non-viable cases in which there was no obstetrical interference: all three recovered. Table II. shows six non-viable cases in which labour was induced, with a maternal mortality of 50 per cent. Table III. shows four viable cases in which labour was induced: maternal mortality 75 per cent. Table IV. shows eight interpartum cases with operative interference, upon the usefulness of which there is no disagreement: maternal mortality 25 per cent.

Thus we see that six of the women died on whom labour was induced, and that the mortality actually rose to 75 per cent. when this induction took place with a viable foetus; while the mortality is *nil* in those cases in which labour did not set in, and was only 25 per cent. in those who were allowed to fall into labour naturally. As far as these figures go, I believe they fully prove the harmfulness of operative proceedings; and what appears to me absolutely astounding is that Dr. Green draws from them arguments in favour of surgical interference.

Dr. Green's statistics have been selected because they are not only recent but complete, as far as they go, and I believe the experience of the Rotunda Hospital will be found to fully agree with my reading of them.

Let me now turn to the method of treating eclampsia that has been pursued in that institution for upwards of

three years, and which I had the honour of carrying out under the present Master. I allude to the hypodermic injection of large doses of morphin. Beginning with the injection of $\frac{1}{2}$ grain, this is followed in two hours by $\frac{1}{4}$ grain, and so on gradually, until either the symptoms are alleviated or until 2 grains have been given in twenty-four hours. If, in spite of this treatment, labour sets in, forceps are applied to hasten delivery so soon as the os will safely admit their application; but it is held that manual dilatation of the cervix, which in reality means a bursting of that structure, is not a justifiable proceeding. So far as my own personal experience goes, it has been entirely favourable to this treatment, nor is it (notwithstanding the fact that opium is believed to be injurious in most renal diseases) theoretically unsound. The best authorities agree in believing that opium has but little action on either the heart or kidneys, Brunton stating that the secretions from the latter organs at times are increased by its employment. Again, we know of no other drug that will lessen metabolic changes in the tissues to so great an extent, and will thus limit toxin formation. It is a nervous sedative of the first order, diminishing the intensity of reflex impressions, powerfully controlling convulsions and uterine contractions, and, lastly, drying up salivary and bronchial secretions. It is obvious, therefore, that in this drug we possess a means of *counteracting to a great extent* those conditions which tend to kill in eclampsia.

It has been objected that this is a dangerous remedy, viewing it from the standpoint of safety to the child. But this argument can hardly be taken seriously when we remember the very high mortality to the fœtus that obtains, no matter what the treatment may be, and also the bettering of the patient's condition by its death. But, apart from these considerations, I do not believe that morphin can exercise any harmful influence on the child when administered as

above. I have calculated that the child's body at no time can contain a dose of morphin larger than what would be equivalent to four minims of the liq. morph., and when we remember that the inhibitory action of morphin on the respiratory centre of the foetus does not imperil its existence, it at once becomes evident that a foetus before birth is much less susceptible to the influence of morphin than a nursing infant would be. All practical experience goes to prove the difficulty of influencing the foetus before birth by this drug.

Let me now say a few words in praise of blood-letting. Dr. Atthill, in Dublin, has always been a warm advocate for this procedure; and lately Dr. Williams, of London, has urged its employment strongly. It was in general use when the results in the treatment of eclampsia were better than they are now; provided we eliminate the septic cases from the old statistics, and viewing it merely from a theoretical standpoint, there is much to be said in its favour. By abstracting blood we relieve the kidneys, we dry up the bronchial and salivary secretions, diminish venous congestion, and remove an absolute amount of toxin from the body.

If the amount of blood required was abstracted by two operations, with not more than an interval of a couple of hours between them, then the second blood drawn off would likely be richer in toxin, due to the before-named current becoming established in the direction of the blood.

I hold that nourishment in no instance should be given throughout the attack. If hot water stimulants or medicine be called for, let them be administered through a nose-tube, or, better still, per rectum. Purgatives are absolutely necessary, and of other drugs the nitrites or digitalis, in connection with spartein, may, perhaps, be found useful. Hot water may be necessary when the disease has lasted for many hours, and after the second bleeding has been under-

taken, or the convulsions subsided. But if given in the earlier stage of the disease it will do harm by counteracting the necessity of the blood to make good its deficiency in water by an indrawing from the tissues.

In conclusion, I suggest that it is possibly the attention to small details not considered worthy of recording that enables some authorities to show results immeasurably superior to others, though both may apparently pursue a similar line of treatment.

DR. HORNE said, notwithstanding Dr. Tweedy's contribution, he was still of the opinion that as regards the pathology of the disease they were as much in the dark as hitherto. In attributing the disease to toxins, they were, he believed, begging the question, for the presence of those toxins had not been demonstrated. As regards blood-letting, he could not understand how it was a treatment applicable to all cases—for instance, to one patient who was plethoric, and to another who was anæmic. He spoke favourably of the treatment of eclampsia by $\frac{1}{2}$ -gr. doses of morphin, or corresponding doses of opium. He also expressed himself in favour of croton oil—a drop being placed on the back of the tongue. He had experience of pilocarpin in one case, and, although in that case he himself did not administer the drug, under its influence the woman rapidly developed œdema of the larynx. He did not assent to Dr. Tweedy's theory as to the elimination of ptomaines by blood-letting.

The PRESIDENT said that the danger of the convulsions is infinitely greater when they occur in the early stages than when they occur in the later stages of labour. He had induced premature labour successfully in two or three cases. Under certain conditions he would be prepared to adopt the same line of treatment again. However, he regarded such a procedure as a very serious one.

DR. ALFRED SMITH pointed out the fact that some German investigators were inclined to believe that acetones in the blood was the cause of eclampsia. The recognised treatment of eclampsia was by large doses of morphin. He followed the practice that obtained at the Rotunda, when he was Assistant Master to that institution—chloroform, purgation, bromide of potassium, &c.—in two cases,

which had occurred in his private practice, and with satisfactory results. They should take their instruction from the large institutions, and when the statistics of such institutions demonstrated the value of opium, the opium line of treatment would be adopted. He would be satisfied with the treatment adopted by Dr. Macan until some better method was discovered.

DR. SMYLY observed that the difference of opinion on the question of the treatment of eclampsia arose chiefly from the habit of forming conclusions on the experience gained of one or two or a dozen cases. No matter what the treatment they had recourse to, sometimes they would get a run of successful cases, and sometimes the reverse. To his mind the question of inducing premature labour or not, was by no means a practical one; for the induction of labour occupies considerable time, and causes great reflex action. He believed chloroform increased the tendency to death. If the patient's death was inevitable, he did not think it was a matter of great consequence whether she died in convulsions or not.

DR. McWEENEY did not think there was anything special about the eclamptic kidney or anything special about the toxæmic condition of the urine. He held with Bouchard that eclampsia was an auto-intoxication. He had not, when examining for Dr. Horne a specimen of eclamptic urine, the means of demonstrating toxins otherwise than by experiments on animals. He said that in eclamptic urine albumen in a greater or lesser degree was always present. A microscopic examination of that fluid invariably revealed hyaline tube casts. Bouchard proved that toxins could be eliminated by acting on the bowels; but the question of the administration of purgatives should be determined by the condition of the patient; and mentioned that small vessels had been found plugged with a tissue, structurally identical with the chorion.

DR. LANE dwelt on the necessity for prophylactic treatment—dietetic treatment. He thought morphin inferior to pure opium. Dr. Lane made a passing reference to serum treatment.

DR. PARSONS could not consider the treatment by chloroform a rational method, since it was well known that that drug depresses the higher cortical centres. By giving opium they were likewise introducing into the system a substance which exercises a depressing effect on the heart. Opium, however, was a less dangerous drug than chloroform. He failed to understand the advantage to be derived from sweating the patient, in face of the fact that Dr.

Purser had assured him that there was more urea in one drop of urine than in as much sweat as would cover the body from head to foot. He regarded pilocarpin as simply a poison in this disease, since it paralyses the sensorium, already too depressed.

DR. MACAN ridiculed the theory that attributed eclampsia to plugging of small cerebral vessels.

DR. TWEEDY said that success in the treatment of eclampsia largely depended on attention to details. He mentioned many of those details—as, for instance, turning the patient on her side during administration of chloroform.