

NOTES ON ECLAMPSIA.

T H E S I S

--- presented to ---

THE FACULTY OF MEDICINE.

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No.	Para.	Ante or Post.	Mother.	Child.	Fits.	Time.	Placenta.	TREATMENT.		After History.	Remarks.
								Medical.	Obstetrical.		
1	Pr.	P	R	L	1	-	-	Eliminative	Natural Delivery	?	
2	2	A	R	S	10	40 hrs	-	Nitroglycerin, Chloral Eliminative	Natural Delivery	Permanently damaged	Fits ceased on death of child
3	3	P	R	L	6	:	-	Nitroglycerin, Chloral CHCL.3 Elimination	Natural Delivery	Good	Each fit preceded by afterpain
4	1	A	R	D	5	6 hrs	-	Nitroglycerin, Chloral CHCL.3 Elimination	Manual Dilatation Forceps	Good	
5	6	P	R	L	2	-	-	Elimination	Natural Delivery	Good	Fits followed Pituitruin
6	Pr.	A	R	L	3	9 hrs	Infarcts	Veratrone, Chloral CHCL.3 Elimination	Natural Delivery	Good	Child had fits immed- iately after delivery
7	Pr.	A	R	S	6	12 dys	Infarcts	Nitroglycerin, Morphia Elimination	Natural Delivery	Permanently damaged	Fits ceased on death of child
8	5	P	D	L	1	-	Apparently healthy	Elimination	Assisted Delivery		Oedema before delivery No albumen till after fit.
9	Pr.	A	R	L	10	7 hrs	Infarcts	Nitroglycerin, Morphia CHCL.3 Elimination	Manual Dilatation and Forceps	Good	
10	2	A	R	S	4	8 dys	Infarcts	Veratrone, Morphia Elimination	Induction	Permanently damaged	Fits ceased on death of child on Induction
11	2	A	D	S	20	24 hrs	-	Chloral CHCL ₃ Elimination	Manual Dilatation Forceps		
12	Pr.	A	R	L	4	4 hrs	-	Morphia Elimination	Manual Dilatation Forceps	Good	
13	Pr.	A	R	S	3	6 hrs	-	Elimination Morphia	Manual Dilatation	Good	Fits ceased on death of child
14	Pr.	A	R	S	3	? dys	-	Elimination	Natural Delivery	?	Fits ceased on death of child
15	Pr.	A & P	R	S	4	5 hrs	Infarcts	Elimination	Assisted Delivery	Good	

Note 1. In 5 out of 7 cases where child still-born, fits ceased on death of child. 2,7,10,13,14.

Note 2. In 5 out of 8 cases where child lived, or died after birth, either persistence of fits led to obstetrical treatment, or there were post partum fits. 1,3,4,5,8,9,12.

Note 3. In 4 cases where delay of 24 hours or over there was Death 1. Permanent damage ~~2~~ ³ Slow Recovery 1. 2,7,10,11.

Note 4. In 2 cases fits ceased but child continued to live and there was recurrence of fits after an interval of days. 7,10.

ANALYSIS OF REPORTS OF 137 CASES OF ECLAMPSIA
NOTIFIED IN NEW ZEALAND DURING 1927 and 1928.

Reported,	137
Mortality: Mothers	...	23 (16.8%)
Infants	...	53 (38.7%)
Incidence:	2.44 per 1000 births.

Age incidence with percentage of all parturients in corresponding age-groups over the same period for comparison.

	<u>Under 20.</u>	<u>20 to 30.</u>	<u>30 to 40.</u>	<u>Over 40.</u>
Eclampsias.	10.9%	64.2%	20.4%	4.5%
All parturients.	4.5%	51.9%	37.5%	6.1%

Note: Of all primiparae in New Zealand, about 80%, are under 30.

Relative incidence in primiparae and multiparae.

Primiparae: 70%. Multiparae: 30%

Note: Of all parturients in New Zealand, primiparae form about 30%, multiparae 70%.

Incidence in relation to stage of Pregnancy.

<u>Under 6 mo.</u>	<u>7th mo.</u>	<u>8th mo.</u>	<u>9th mo.</u>	<u>Term or over.</u>	<u>Not stated.</u>
4	11	12	33	56	21
(2.9%)	(8%)	(8.8%)	(24.1%)	(40.8%)	(15.4%)

Incidence in relation to Plural Births.

137 cases: Twins 10 times: 7.3%

Note: In all births in New Zealand, Plural Births occur in about 1.1%

Relation of Antenatal Care to Mortality.

	Care	103 cases	No care	34 cases.
Mortality:	Mothers	13 (12.6%)	10	(29.4%)
	Infants	32 (31.0%)	15	(44.0%)

Time of appearance of Albuminuria.

<u>Unknown.</u>	<u>Over 1 mo. to 1 mo.</u>	<u>1 wk. to 1 wk.</u>	<u>1 wk. to 1 day.</u>	<u>Day of attack.</u>	<u>After.</u>	<u>Never.</u>
22	19	23	24	36	6	7
<u>Unknown.</u>	<u>Ascertained prior to day of attack.</u>		<u>Same day.</u>		<u>Not prior to attack.</u>	
16%	48.2%		26.3%		9.5%	

Note: Of those in whom Albumen was first found on day of attack, 22 cases had been previously examined.

Number of Fits, and relation of Mortality thereto.

<u>Fits.</u>	<u>Cases.</u>	<u>Mortality:</u>	<u>Mothers.</u>	<u>Infants.</u>
1	26		14%	23%
1 to 5	52		9.6%	25%
5 to 10	59		13.8%	34.5%
10 to 20	14		34.4%	50%
20 to 30	6		33.3%	66%
over 30	1		Nil	100%
Not stated	9		-	-

Distribution of Fits in relation to Labour and to Mortality.

		Mortality:	Mothers	Infants,
Antepartum.	53.3%		16.4%	45.2%
Post partum	26.3%		16.6%	19.4%
Ante and post partum	19.7%		19.4%	37%
Coma without Fits	.7%		Nil.	100%

Treatment and Mortality in relation thereto.

<u>Method.</u>	<u>Treatment.</u>		<u>Mortality.</u>	
	<u>Cases.</u>		<u>Mothers.</u>	<u>Infants.</u>
Expectant	72 (52.5%)		8 (11.1%)	28 (38.8%)
Obstetric	30 (21.9%)		1 (3.3%)	12 (40.0%)
Induction	16 (11.7%)		5 (31.2%)	4 (25.0%)
Acch. Force	6 (4.4%)		3 (50.0%)	3 (50.0%)
Caesarean	11 (8.0%)		5 (45.4%)	5 (45.4%)
No Treatment	2 (1.5%)		1 (50.0%)	1 (50.0%)

Inter-relation of Mortality of Mothers and Infants.

Both lived.	85 (57.5%)
Mother lived, Infant died.	36 (24.3%)
Infant lived, Mother died.	11 (7.4%)
Both died.	16 (10.8%)

Relation of death of Mother to life of Infant.

Mothers died.	23 (100%)
Infants born alive.	12 (52.2%)
Still born 8, unborn 3.	11 (47.8%)

Relation of Still birth to life of Mother.

Still births.	40 (100%)
Mothers lived.	32 (80%)
Mothers died.	8 (20%)

Recurrences:-

Of 41 Multiparae 5 (12.2%) had Eclampsia in previous pregnancy.

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My personal experience covers 18 cases in which there has been a culmination of the disease in convulsions; and a number of cases of pre-eclamptic toxæmia of varying degrees of severity. I find I have preserved meagre notes of 22 such, but that is only a fraction of the number actually seen. Perfection of recording in general practice is not difficult an ideal to attain.

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N O T E S O N E C L A M P S I A .
----- associated

I. INTRODUCTORY.

The following is a record of the cases of Eclampsia which I have attended over a period of twenty years, together with a suggestion as to the mode of origin of the disease, some observations on treatment, and a commentary on my analysis of 137 cases recently reported in New Zealand.

The series of Eclampsias that occurs in a private practice not largely of an obstetrical character is naturally a small one; and it may reasonably be objected that no general conclusions should be made from such limited experience. Nevertheless, the actual experience of a small number is extraordinarily instructive, and suggestive information can be gained by careful study of the details of a few cases perhaps more easily than from large statistics in which detail is obscured by the mass of material, and personal contact is lacking.

My personal experience covers 15 cases in which there has been a culmination of the disease in convulsions, and a number of cases of pre-eclamptic toxæmia of varying degrees of severity. I find I have preserved meagre notes of 22 such, but that is only a fraction of the number actually seen. Perfection of recording in general practice is too difficult an ideal to attain.

It is opportune here to remark that a definition of Eclampsia is not really easy. It is too closely associated in our minds with fits, and we regard fits as being a necessary feature. I am convinced that this is irrational and wrong. Convulsions are merely the terminal phase of a toxæmia occurring in pregnant women, which presents many other symptoms quite as essential and characteristic. It would, I think, be just as reasonable to regard coma as a disease entity, apart from the diabetes, or whatever else is the cause; or to think of septic peritonitis as a disease, apart from the perforation, or appendicitis producing it.

Further, the syndrome of Eclampsia is very varied. Almost each individual common symptom of it may be absent in a given case. The common symptoms are albuminuria, oedema, headache, sensory disturbances, especially of sight, raised blood pressure, and fits, occurring in pregnancy. But on reference to my own cases, and to other recorded cases, I find Eclampsia without albuminuria, Eclampsia without oedema, Eclampsia without headache, Eclampsia without sensory disturbance, and even Eclampsia without fits. There remain only two constant features - pregnancy, and a blood pressure

which seems to have been found invariably raised whenever it has been taken.

II. REVIEW OF MATERIAL IN RECORD OF PERSONAL EXPERIENCE.

The incidence of Eclampsia in my experience has been extraordinarily high in proportion to the number of cases of maternity attended, being 1 in every 79 cases up to the present. This is due, I think, to the widely-scattered, rural nature of the practice in which I met most of these cases, where there was practically no possibility of antenatal supervision. As a rule, the first one knew of a case was that a message would come in from a country store that a man had just ridden up to say a woman was taking fits fifteen or twenty miles along a mud road. Usually it was dark and raining, and no trained nurse available.

Considering the circumstances, the maternal mortality was reasonably low - 2 deaths in 15 cases - which works out at 13.3%; but these details are really only of personal interest, for in such a short series the element of chance may enter in too greatly.

There are, however, four points in my record to which I wish to direct attention, because I think we may reasonably take certain suggestions from them. These points are the infant mortality; the relation of the survival of the child to the condition of the mother; the condition of the placenta; and the relation between the duration from the onset of convulsions to delivery and the after-history of the mother.

(1) The infant mortality was much greater than

the maternal, 8 infants being dead, ordying, as against 2 mothers. This appears to be a constant thing in most statistics of Eclampsia, and in about that proportion. There can be little doubt that the reason is that the child absorbs, and is acted on by the toxine just as the mother is, and its smaller size makes it generally more vulberable. As I will endeavour to show, it probably absorbs the poison more directly; and doubtless birth accidents and pre-maturity also increase the mortality.

It is appropriate here to point out that the child is eclamptic as well as the mother. Post mortem examination shows exactly the same pathological lesions in the dead infants as in the mothers dying. As a clinical proof of the condition of mother and child being similar, note that in Case 6. the child had typical Eclamptic convulsions a few minutes after birth. This is said to be not uncommon; but it occurred only once in this series.

(2) The relation of the survival of the child to the condition of the mother is extremely interesting, and, to my mind, of first-rate importance in regard to prognosis, and to choice of methods of treatment. In 5 out of 7 instances in which the child was still-born (Cases 2, 7, 10, 13, 14), the fits ceased following the death of the child; while in 7 out of 8 cases in which the child lived, or died only after delivery (Cases 1, 3, 4, 5, 8, 9, 12) there was either persistence of fits necessitating delivery, or post partum fits occurred; and (Cases 7. and 10.) where there was an apparent cessation of fits, but the child continued to live for a time,

there was recurrence of fits after an interval of days, with subsequent death of the child and cessation of fits.

Thus we have 5 cases of intra-uterine death associated with cessation of fits, and 9 instances of intra-uterine life associated with persistence of fits in 15 cases. In only one case, and that a protracted and fatal one, (Case 11) did fits persist after death of the child, and due no doubt to extensive involvement of the liver. The inference from this is clearly that intrauterine death of the infant is a good prognostic for the mother, and that continued intrauterine life is a menace to the mother. In view of this it appears to me inadvisable, where the infant continues to live, to wait too long on nature, unless improvement in the pulse and urine, and natural fall of blood pressure indicate a waning toxæmia.

It has for long been recognised that convulsions will generally cease following intra-uterine death; but it does not appear that any practical conclusion has been drawn from the fact. The reciprocal fact - if it be a fact, and not merely a coincidence in this small series - that continued intrauterine life of the child perpetuates fits, has received, so far as I can discover, no attention whatever. Yet I cannot think the point can have escaped notice.

Attention having been arrested by those twin facts in studying the details of my series, I could not escape the suggestion that the living child in utero may be often an important agent in the distribution of toxine to the mother. The mechanism by which the distribution may be effected I will discuss presently in relation to the

theory of causation.

(3) The condition of the placenta.

In my earlier cases I paid no special heed to the placenta; but in Case 6. the placenta was so extremely diseased that in subsequent cases I noted it, and in every instance, with one exception, it was found to be infarcted. In that one instance an infarct was probably really present, for at that time I did not know how to look for it.

Referring now to the cases not exhibiting convulsions, the pleasing feature of pre-eclamptic cases is that, as a general rule, they are very amenable to treatment. The well-established practice of "eliminative" treatment has such excellent results that we are apt to neglect to note particularly details of the confinement. Hence my record of 22 cases in which any detail at all noted is most unsatisfactory. Still, I find amongst those 5 cases of diseased placentas, and 5 still-born children. Probably had I known to be on the watch, I should have found many more infarcted placentas; and probably the five still-born infants by dying saved the mothers from actual Eclampsia.

This association of placental disease with the Eclamptic conditions has long been known. Most writers refer to the frequency with which placental disease is met in toxæmia; but hitherto the observation has either been disregarded, or the placental condition has been deemed an accompaniment of, or attributable to, the toxæmia.

This latter view is open to strong criticism. When we have a toxæmia of a kind occurring only when there is

placental structure present, and a pathological condition is found in the placenta, surely it is a most reasonable inference that the placenta may be the source of the toxæmia, rather than that the placental disease is a result of a toxæmia arising somewhere else - and where else cannot be suggested. Further, if the placental involvement be due to toxins originating somewhere else, in cases where the child dies in utero, there is no reason why the toxæmia in the mother should abate, as it so frequently does. In that case the toxins would continue to act on the mother just as potently as before, and the death of the child would be an evil omen, instead of the favourable one it actually is.

(4) Influence of the lapse of time between the onset of convulsions and delivery upon the after history of the mother.

In my record it is to be noted that in four cases, - 2, 7, 10, 11, where 24 hours or more elapsed there were, death, 1; permanent damage, in the shape of persistent renal inefficiency, cardio-vascular changes or visual defect, 3. Compare this with cases 4, 9, 12, 13, 15, in each of which labour being reasonably expedited, and delivery effected within 24 hours, the after histories were quite satisfactory. We would therefore like to know more of the after histories of cases recovering after protracted conservative treatment on a larger scale, as it is quite reasonable to suppose that greater systemic damage will follow long exposure to the Eclamptic poisons.

I strongly suspect that in recovered cases in which subsequent pregnancies are marked by albuminuria, miscarriage or recurrence of Eclamptic fits, the cause is to be sought in permanent damage sustained by protracted, severe toxaemia in the first Eclampsia, and that "follow up" enquiry would reveal this association.

In addition to my own record, I am permitted by a colleague, Dr. D. C. Low, to refer to two cases in which I assisted him. Neither case had fits; both were early in the 8th. month; both had pronounced oedema, much albumen, with tube casts and blood, and were almost anuric. Eliminative measures failed to reduce pulse or blood pressure or to improve the urine, and the lungs were becoming oedematous. The infants were alive. In both the cervix was firm, and there was no sign of labour. Both cases were terminated by Caesarean section under spinal anaesthesia. The infants were both delivered alive. One died a few hours later, but the other survived and is well. Both mothers were saved and made rapid recovery. Both have since been pregnant, with happy results.

In these cases persistence in medical treatment would certainly have been fatal. Induction was out of the question, for there was neither time, nor strength in the women to carry through induced labour. General anaesthesia would have been hazardous.

These cases illustrate three points:--

1. That all the essentials of Eclampsia may be present without convulsions.
2. The persistence of toxaemia with continuing intra-uterine life of the child.

3. That in certain cases surgical treatment is the proper course. I am certain that in these the correct procedure was accurately chosen.

III. THEORY OF ORIGIN OF ECLAMPTIC TOXAEMIA.

Turning to the intriguing question of causation, whatever be the source of the disease, and its exact mode of production, we can stand fast on one certain, basal, indisputable fact - that it is a disease of pregnancy, and of pregnancy only. No man, and no nulliparous woman has ever suffered from it. (It is necessary to approach the subject on this very elementary basis in order to show clearly how the conclusion, to which I am coming, is reached).

We may safely conclude, therefore, that it is due, directly or indirectly, to the ovum; and as it does not appear until the placental structure has been formed, and not until the placenta has become a large, well-developed organ, we are justified in a careful scrutiny of the placenta in relation to the disease.

Further, from the pathological evidence, there can be no doubt that in Eclampsia there is a virulent auto-intoxication which produces the lesions in the liver, kidneys and other parts of the body. Since the disease is one of pregnancy only, and affecting both mother and child, it is natural to look first to the essential bond of union between mother and child - the placenta - as a probable source of the toxine.

Following this obvious line of reasoning, work on Eclampsia during the early years of this century was to a

great extent concentrated on the placenta; so that in 1909, Eardley Holland, in a survey of the work to that time, came to the conclusion that "The primary cause of Eclampsia is to be sought in the placenta";⁽¹⁾ and that "In the light of present knowledge, the most probable theory of the cause of Eclampsia is an autointoxication of the body by the passage of ferments and autolytic products from the placenta into the circulation."⁽¹⁾ But Holland did not regard placental infarcts as the origin of those autolytic products.

In 1914, Young of Edinburgh submitted to the Royal Society of Medicine his theory that the toxins of Eclampsia were derived from placental infarcts, being the autolytic products of necrosing areas of placenta, that these products were absorbed by the mother through the surrounding placenta, and set up the systemic poisoning. His proofs were that he had found massive recent infarct in every Eclamptic placenta in his research; that he had produced typical eclamptic symptoms in laboratory animals by injecting autolysed placenta; and that he had demonstrated the pathological lesions typical of Eclampsia in those animals.⁽²⁾

In subsequent years Young confirmed and extended his research, and wrote further on it. Still, it does not appear to have gained the general acceptance it merits. I am here advancing Young's theory, together with an amplification, and indicating some of the argument, because I really think it the best explanation yet heard of the aetiology of Eclampsia.

The amplification which I suggest is that while absorption from placental infarct will take place through

surrounding healthy placenta, an important factor in the absorption and dissemination of the toxins is the living foetus, which absorbs the poison from its side of the infarct, and distributes it through the healthy portion of the placenta, poisoning the mother and often killing itself in the process.

Now, how does the theory fit the facts? It is well known that when a portion of living tissue is cut off from its blood supply in situ it undergoes molecular changes, termed autolysis, quite distinct from the putrefactive changes due to organisms which occur when those have access.

"Every cell has intra-cellular ferments or enzymes. These exercise ^{an} ~~all~~ all-important function in the metabolism of the cell, which depends on them for the assimilation of food-stuffs brought by the lymph and blood. Normally they exist in a very firm combination with the protoplasm and have no action on the cell itself; their action is limited entirely to the fluids circulating round it. Some cells have more ferments, and stronger ones, than others: of this the liver and placenta are examples. In the normal state of affairs these ferments all work in harmony for the good of the cell. It is quite otherwise when the integrity of the cell is impaired. They then turn upon the cell and attack it, breaking down the protoplasm. This process is known as autolysis. During life, the result is the passage into the system of toxic substances, products of protein disintegration." (1) (Holland).

This toxic effect of autolytic products, where

they have easy access to the blood stream, is well known. It is frequently seen after burns, two or three days following the injury; and it was noted in the high-explosive wounds in war time, when areas of tissue quite beyond an infected wound underwent this autolytic change. (Young).

The placenta is unique in the facilities it offers for this absorption; for necrosing villi actually hang in the blood spaces until clotting shuts them off from the blood stream; and there is also an active agent in the live child for the distribution of the poison taken up on its own side.

As already mentioned, the frequent association of placental infarct with eclamptic toxæmia is well recognised; but its invariability has only been shown by Young's work. There is a very important point here, which requires some elaboration.

Different types of infarct are often observed, and frequently in quite normal cases. In my own mind I had infarcts classified in three groups. (1) Sometimes, not often, one sees a dark, purplish nodule or mass in the placenta. (2) Quite frequently rounded, whitish, cheesy lumps of varying size, and often multiple, are noted. Occasionally there are thinner fibrous patches. These are really different stages of the same condition. The purple one is a recent infarct; the white one an old infarct, and the fibrous one an infarct in the final stage. But there is yet another, the new infarct, and that is the one we do not recognise, and cannot recognise unless we know how to

look for it. The new infarct looks and feels so much like the rest of the placenta that it cannot be recognised with certainty until the placenta has been gently washed, and immersed for a few days in preservative solution. The reason for this, as pointed out by Young, is that the uncongealed blood in the normal placenta lakes out on immersion, since it does not clot in the healthy blood spaces; but the congealed blood in the infarcted portion remains in situ, and shows up distinctly against the paler structure of the exsanguinated placenta. Whereas in the fresh state the whole placenta looked alike.

It is the early proteolytic products of infarct that are most toxic. The new infarct that we do not see, and the recent or purple infarct, are the deadly ones. The older, white and fibrous infarcts have passed into a harmless stage.

Hence, in fulminating cases of convulsions, new infarcts are to be looked for: in cases in which there is an increasing toxæmia culminating in convulsions, purple infarcts are present: in preeclamptic cases that become normal, old infarcts are found: and many normal cases may show small old infarcts which have been insufficient to produce symptoms. The observation of all workers of the frequent association of placental disease with the Eclamptic condition must have some meaning; and this, I believe, is its true significance.

As to the cause of infarction, the placenta is only intended to live for nine months, and some cases may be

(3)
 due to pure senility. Then, the known increased coagulability of the blood in later pregnancy may be a frequent cause. Others are probably due to injury, and in this regard, the violence of the child within the uterus should not be forgotten.

This theory thus seems to me to offer a workable explanation of many facts:--

1. It explains the essential association of Eclampsia with pregnancy.
2. It gives a probable origin for the toxins, hitherto obscure.
3. It explains the slow, threatening aspect of some cases, and the fulminating character without previous symptoms of others - in the one instance the patient is steadily absorbing poison from an infarct of moderate size, or from a series of infarcts; in the other there is a large, sudden absorption from a massive infarct.
4. It shows a rationale for the success of "eliminative" treatment in pre-eclamptic toxæmia, where the amount of infarction is not too great; and why that same treatment is not effective in the aggressively advancing and fulminating cases.
5. It indicates how antenatal care, which consists of the maintenance of a high standard of health, and efficient action of the excretory functions, lessens the incidence of pre-eclamptic symptoms.
6. It shows a reason for the frequency with which the death of the child is followed by cessation of fits; and for the association of the persistence of the life of the child with the persistence of fits.
7. It accounts for the greater vulnerability of the child.
8. It clears up the strange association of Eclampsia with plural pregnancy, there being, in such cases, a larger area of placenta, and duplicate pumping plants to distribute toxins.

Now, what of the objections? I have thought of many, and have found all that have occurred to me susceptible to explanations that fit in with the theory. Probably many have escaped me. There are, however, three obvious, and seemingly serious difficulties which are so patent that they must immediately occur to any reader. These I will anticipate.

1. Why, since infarct is so common, are there so many cases that show no symptoms, and why do not more women have Eclampsia?

The answer to this is much the same as the answer to the question, why do we not all get pneumonia, since the pneumococcus is to be found in nearly every throat? The dose is not usually sufficiently great, and the natural defences of the body are too good. The body can deal with the products of the small infarcts of varied age generally seen; but the infarct of convulsions is always massive. If there is time, the infarcted area becomes shut off by thrombosis, absorption diminishes, and recovery can take place. With small infarcted areas this is possible without dangerous symptoms, or with none at all; but with massive new infarcts, the toxic process overtakes the eliminative powers.

2. Why does not the death of the child always terminate fits, instead of only frequently?

There is a two-fold answer to this, the first part of which I will give now; but the second part comes also as the answer to the next objection. Although the

child die, there may still be sufficient absorption directly through the placenta surrounding the infarct to keep up the toxæmia. The structure of the placenta has to be borne in mind. It is made up of separate lobules or cotyledons, and there is no, or very little, direct blood-vessel communication between these. If infarct involves a portion of one or more cotyledons, there can still be absorption if the child dies. If the entire cotyledon be infarcted, there will be little or no absorption excepting indirectly, through the child; and consequently, death of the child cuts off absorption. For further explanation note the answer to the next difficulty.

3. How are fits continuing post partum, and initial puerperal fits to be accounted for?

To both of these, if there are but one or two convulsions after delivery, there is the reasonable explanation that enough toxine had already been absorbed to cause those, though the source of toxæmia had been recently removed. To explain prolonged, severe or fatal post partum or puerperal cases we have to remember the pathology of the toxæmia. The outstanding feature is focal necrosis of the liver, though thrombosis and necrosing foci may occur in all parts. When in such a large organ as the liver, there are scattered about innumerable necrotic foci in association with a venous vascularity second only to that of the placenta, there is ample opportunity for further absorption of proteolytic poison. A

pathological chain has been established. The placental infarct produces toxic bodies which cause thrombosis and necrosis in the liver lobules, from which further toxins are absorbed which act similarly elsewhere, so that a cumulative series of infarct and toxine, infarct and toxine, occurs, which leads to disaster. You can break the first link in that chain by the timely removal of the infarcted placenta; but you cannot break the second link by removing the infarcted liver. This is the reason why the post partum cases have such an evil reputation and bad prognosis; and this is the reason for my argument, shortly to be put forward, against permitting the mother to be indefinitely exposed to the multiplying action of the toxins, and for watching the child as a prognostic for the mother.

I have purposely withheld reference to those post-partum cases to the present juncture, because I conceive that the answer to this question becomes one of the strong arguments in favour of the theory.

A further likely objection is that the fact that Eclampsia has occurred in association with vesicular mole must at once dispose of both the placental infarct theory of its origin, and the suggestion that the survival of the foetus plays any part in the production of symptoms.

Consideration of the pathology of Vesicular mole as presented by Marchand shows the theory able to withstand this criticism also. "The essential feature of Vesicular mole is that both the syncytium and the cells of Langhan's layer undergo profuse and irregular proliferation, pene-

"trating Nitabuch's fibrin layer, and making their way into the depths of the decidua, and not infrequently into the uterine musculature as well. At the same time the blood vessels of the terminal villi disappear, the stroma degenerates, and the cells present a necrotic appearance." ⁴

In other words, we have in Vesicular mole an abnormal type of villous structure deeply embedded in, and closely enveloped by uterine structure, and undergoing autolytic changes. Such conditions, in the opportunities offered for absorption, are so extremely suitable for the production of Eclampsia in terms of Young's theory, that one would almost expect Eclampsia to be relatively more frequent in Vesicular mole than in ordinary pregnancy.

As regards the effect on this criticism on the part the living foetus plays in Eclampsia, it is not suggested that the child is an invariably essential link in the chain of absorption. What is claimed is that, while the child is alive, it is an important factor in the distribution of the toxins, and a factor which frequently turns the scale against maternal recovery.

IV. TREATMENT.

If we are to reduce Eclamptic mortality, the first step is to prevent Eclampsia, if possible. That much can be done by prophylaxis cannot be doubted. The extraordinary frequency of cases in the earlier part of my own obstetric experience in the back-blocks, where local

circumstances were such that ante-natal care was impossible, is in itself evidence of the great extent to which the disease is controlled by the simple hygienic management which is available in average circumstances. As ante-natal supervision becomes more thorough and more universal, a decrease can confidently be expected, owing to better standard of general health, and better maintenance of excretory function. No prophylaxis, however, will ever completely remove the menace of the fulminating case without previous symptoms. The overwhelming toxæmia of a sudden, large, unforeseeable infarct cannot be guarded against. I have myself seen and examined a patient in the most perfect health and exuberant spirits, only to find her fourteen hours later in violent convulsion.

As regards the treatment of the pre-Eclamptic condition, that is so well based, and generally so successful, that it need not be referred to, excepting to remark that the profession stumbled on the valuable "eliminative" method by a misapprehension. This treatment was introduced at a time when it was believed that Eclampsia was primarily a form of renal inadequacy producing a condition akin to uræmia, and due to the kidneys being incapable of undertaking the extra burden of excretion imposed by pregnancy; and the eliminative method was based on the principles of treatment of uræmia. The real reason for ^{the success} the success of the method, however, is that the liver is not on the direct line of absorption from the uterus. If the uterus were on the portal system, Eclampsia would be untreatable;

for the liver is the organ most susceptible to the action of the proteolytic toxins, and it would be overwhelmed so rapidly. As it is, the liver receives only its quota of toxins through the hepatic artery; and if "Elimination" can be promptly and efficiently established, toxins are excreted so rapidly through the skin, kidneys and alimentary tract that the force of their attack on the liver is to a great extent broken. Thus, on the perfection of excretory function both prophylaxis and pre-Eclamptic treatment mainly depend.

In the treatment of the convulsive stage for two generations there have been two schools, and practice has swung between them, now inclining towards the side of active interference to put an end, as speedily as possible, to the source of toxæmia; now tending toward the conservative method of the least possible interference with natural delivery, and that of the gentlest, meanwhile stimulating the eliminative functions to the utmost, and restraining convulsions by narcosis. The present tendency is very definitely toward the conservative method which has been advocated by Fothergill and others since the closing years of last century, and has been followed consistently in Dublin for many years.

The Report of the Special Committee to the Liverpool Congress of Gynaecology and Obstetrics in 1922 has had a marked effect in strengthening the conservative school by showing the widely varying mortality rate, differing from 10% in Dublin, to 25% in Edinburgh, where active inter-

ference has been more freely practised. All are now, fortunately, agreed that that harsh, shock-producing, and dangerous procedure, *Accouchement Forcé* is a bad method; and the weight of statistical evidence clearly shows that the conservative method is, in general, the one to be followed.

Yet it is apparently a method not universally applicable. In a city, and under conditions in which oversight can be well exercised, and treatment promptly begun, and in the hands of its best exponents, it can magnificently surpass previous records; but it still shows a mortality of 10% (some of which, of course, is an inevitable mortality). So it can be said, generally, that it is a method not suited to approximately one in ten cases; and the question arises whether anything else can be done for those. They will not benefit by more conservatism, for they evidently die under it.

Improvement in results is most likely to be obtained by earlier discrimination in the adaptation of method to the needs of the individual case, based on better knowledge of the aetiology, and on clearer visualisation of the stage at which the pathological process has arrived in the particular instance.

In view of the prior observations in this paper, I suggest that there are four types of case, which can be recognised, that are not suited to the full regime of conservative treatment; and I believe that the early adoption of appropriate surgical or obstetrical treatment in these

would further diminish mortality.

1. Fulminant cases without previous symptoms, who are not in labour. In these cases there is a large new placental infarct in the early stages of its lethal programme. To allow it to remain for the probable 24 or 36 hours before nature has completed labour is to permit the cumulative sequence of toxine formation to devastate the patient. "You can break the first link in the chain by timely removal of the infarcted placenta; but you cannot break the second link by removing the infarcted liver." The path of greatest safety in these cases would be prompt Caesarean section.
2. Cases in which medical treatment has failed to improve the pulse, urine, and blood pressure, and the child is alive and vigorous. Here the infant is an active distributor of poison from a large recent infarct; and continued medical treatment entails continued toxination until the death or expulsion of the child. Active obstetrical or surgical treatment is here in-

measures are appropriate indicated according to the obstetrical position at the moment.

3. Primiparae any considerable period prior to term - 6th. to 9th. months. Here labour is likely to be so tardy that to wait upon it is dangerous. Caesarean section would usually be the best method, though induction might be suitable if convulsions had ceased; or, in cases early in pregnancy, vaginal Caesarean section.

4. Multiparae in whom labour has begun, and the os is dilated or dilatable easily, but labour flagging. Unless the child is dead, or the toxaemia definitely abating, delivery should be expedited by the gentlest obstetric methods, as such must be less dangerous than overexposure to continued toxaemia.

I am conscious that the surgical and obstetric measures which I am here advocating for special purposes are generally in disrepute at the present time. But that disrepute is chiefly due to their untimely employment in the past. I am advocating their use at a period when they can be of avail. It can be of no avail to empty the uterus either surgically or obstetrically, as a last resort, after the liver has been disorganised. When these

measures are appropriate they must be done early and quickly, just as laparotomy must be done in perforated gastric ulcer or ectopic gestation. Most disreputable statistics could be obtained from this latter brilliantly successful measure if it were left to the late stages, after unavailing medical treatment.

In the kind of cases indicated above, a conservatism which would insist on holding on till the intervention of nature, camouflaging toxæmia with narcosis pushed to the limit, disturbing the patient by troublesome gastric and colonic lavage, meanwhile permitting prolonged exposure of the mother to a virulent toxæmia, could not possibly be right. Such as did survive would be permanently damaged, and become victims of subsequent pathological pregnancies.

Regarding the use of narcotics, morphia or chloral cannot have any influence on the pathological effects of the toxins on the tissues of the mother, unless indirectly by putting the child out of action as a toxine-distributing agent; and possibly this may be the explanation of the sometimes favourable results of the enormous doses of those drugs which have been employed. The real intention of their use, however, has been to restrain convulsions and relieve discomforts. To this end they should be used only in sufficient amount to gain time for eliminative measures to become effective. Once this object has been attained, they should not be further pushed so as to blur the clinical picture. It is analagous to the use of morphia in acute abdominal disease. Once

the line of action has been determined on, its use is valuable to relieve the patient, and to facilitate treatment; but it must not be employed so as to mask symptoms which are the guide to the pathological condition.

V. COMMENTARY on the record of cases recently notified in New Zealand.

During the last two years there has obtained in New Zealand a system of voluntary notification of Eclampsia, which was arranged between the N. Z. Branch of the British Medical Association and the Health Department. By the kindness of Dr. H. Jellett, consulting obstetrician to the Department, I have been enabled to make an analysis of the notifications for the years 1927 - 1928, respectively 58 and 79 cases, together making a series of 137 cases. Although the information furnished by the notification forms does not give a detailed account of each case, the analysis provides some interesting matter; and at various points it confirms the contentions set out in this Thesis.

The record shows very clearly the value of ante-natal care, even when it is not successful in averting Eclampsia. Amongst the cases that had ante-natal care the mortality was 12.6%, while in those that had no previous attention it was 29.4%. Even in the cases that came on unexpectedly during supervision, there is a much better result than in those having no previous care. Probably many factors enter into this; but the most obvious and constant one is that eliminative measures were always promptly employ-

ed in the cases under supervision, and this doubtless avoided an accumulation of toxine to such an overwhelming extent as in the uncared-for cases.

The figures relating to the time of appearance of albuminuria are important. In 22 cases (16%) albumen was discovered only on the day of onset, although the urine had been previously and recently examined. In one case it was known that albumen appeared only three-quarters of an hour before the first fit. In 6 cases it appeared only after the convulsions; and in 7 instances there was no albumen at any time. Thus, in practically 25% of cases urinalysis failed to give the expected warning. This illustrates the fulminant and atypical character of a large proportion of cases, the need for close contact with patients during the later months and for taking other indications besides urinalysis into consideration. It also supports the infarction theory; for one looks in vain for any condition other than infarction, originating within the body, which is capable, under the conditions, of giving rise to such sudden manifestations. It appears that nearly all of those cases felt, and seemed, in perfect health up to the day of the occurrence.

Of 41 multiparae in the series, 5 (12.2%) had Eclampsia in previous pregnancies. This is noteworthy in relation to prognosis for future pregnancies for which the attendant is generally asked. If the incidence of recurrence be as much as 1 in 8, the common assurance that future pregnancies are safe is not justifiable.

No definite relationship of number of fits to mortality appeared in this series. Some patients died after one, two, or three fits; others recovered after one, two or three dozen. The former system of estimating the severity of the disease according to the number of convulsions appears to be unsound. It is not the number of fits, but the effect of the toxaemia on the general condition of the patient, which is the index of severity. A fit, presumably, is the result of irritative action of the toxins on the cerebral cortex, either directly, or indirectly through vascular changes. If this view is correct, it is understandable that intense toxination may rapidly, at any time, pass beyond the irritative stage to parietic action on the cerebrum, which will not be indicated by fits, but by symptoms indicating depressed cerebral function, such as amnesia, imperfect recognition of surroundings and of time, sensory deficiencies, and interference with cerebral inhibitions. It is difficult otherwise to understand the irregular distribution of deaths in relation to fits, although it is remembered that the brain is not the only vital organ subject to toxination.

The record demonstrates the impossibility of making any real comparison between the results of conservative and radical methods of treatment. The most that can be deduced is that in a given case, or group of cases, the mode of treatment was, or was not suitable. In New Zealand, in common with other British countries, in recent years there has been an increasing tendency towards conser-

vative measures; so that in the two years reported, in practically every case expectant and medical methods have been chosen to begin with, and radical treatment has only been resorted to when progress did not appear. Under such circumstances, when two methods have been tried, it would be manifestly inequitable in a fatal issue to put the death against the last-tried method alone: it should be debited to both methods, since both have failed. Further, there is no reliable indication that the cases in opposing groups are parallel - there is, in fact, strong indication that they are not parallel, and are therefore not open to comparison in their results.

For example, the group which in these statistics shows the best result is the "obstetrical assistance" group, in which low forceps were used to expedite delivery in otherwise expectantly treated cases. The mortality in these was 3.3%; whereas the "Radical" group, which includes Induction, Accouchement Forcé, and Caesarean Section, shows a mortality of 39.4%. Comparison here is impossible; for leaving out of court the difference in operative risk inherent in the two groups, the cases in which low forceps are used are of themselves proceeding towards a favourable termination; but those in the Radical group are not. Besides it is an easy matter to determine just when assistance should be given in low forceps cases; but it is exceedingly difficult to judge when, if at all, radical measures should be undertaken. This paper suggests means by which that judgment may be assisted.

One is not aware of the nature of the statistics on which the report of the Special Committee of 1922 was

based; but the knowledge gained from these New Zealand statistics raises a doubt in the mind as to whether the last word has really been said regarding the use of conservative and radical methods. This doubt is increased by the following features revealed in this analysis.

The maternal mortality in the 1927 cases was 20.7%, whereas in 1928 it fell to 13.9%. Coincidentally the number of cases treated by conservative methods only had risen from 68% in 1927 to 78% in 1928; and the natural assumption would be that the fall in mortality was due to the greater conservatism. Further enquiry, however, shows that the conservative mortality rose from 5% in 1927 to 11.3% in 1928, while the radical mortality fell from 53% in 1927 to 25% in 1928; so that the improvement in results in the latter year was really due not to increased conservatism, but to more restricted, and more successful radicalism.

It is, of course, beyond question that conservative methods are best for the majority of cases. The doubt is whether their merits are so exclusive of radical procedures as has been claimed of recent years. The indication is rather that certain radical methods have an honourable place, provided that the time of their application be suitably determined.

The mortality in the radical group in the analysis was very heavy. Induction had a death rate for mothers of 31.2%; Accouchement Forcé, 50%; and Caesarean Section, 45.4%. Although Induction during the attack has such a

serious mortality, figures relating to pre-Eclamptic cases indicate that it has an excellent scope in suitable circumstances, In a few cases where pre-Eclamptic symptoms were not yielding to treatment, Induction was carried out with 100% success. Accouchement Forcé was used in only six cases, and there is indication that death in two cases was due to it. Even with its great mortality, Caesarean Section should be considered a successful procedure in this series, for the cases did not appear amenable to any other treatment.

In present teaching as to the use of Caesarean Section reason and authority do not appear to coincide. Instead of considering why it is ever successful when other treatment fails, obstetricians in general insist on its being reserved as a last resort. It has been stated by an eminent authority, thus:--

"This operation is permissible as a last resource
 "when medical treatment in Eclampsia has failed. Occasion-
 "ally it may save a life. Death in Eclampsia, when it is
 "not due to suffocation by foreign particle in the lungs
 "or to cerebral lesion, is probably most usually the result
 "of hepatic degeneration. It is difficult to understand
 "how Caesarean Section can restore a badly degenerated
 "liver." 5

Thus it appears that Caesarean Section is authoritatively reserved for cases in which, by the showing of authority, it cannot be expected to do good. Here reason rebels. What explanation can there be of the occasional

recoveries, which are admitted, excepting either that Section does benefit the liver in some cases, (presumably those in which necrosis has not advanced too far before the "pathological chain" is broken), or that recovery would have taken place without operation? In the former alternative it follows that the same operation, in the same type of case, done earlier, would save more lives. In the latter, Caesarean Section is never justifiable.

Perhaps the most interesting and suggestive portion of this analysis is that showing the inter-relation of mortality of mothers and infants, to which I cannot discover attention previously being paid. For the purpose of demonstrating this relationship, in the case of twin births, when both infants were born alive, it is counted as one live birth; when both were stillborn, it is counted as one still-birth; and when one twin only was still-born, the mother is counted twice; so that in the series of 137 cases, the figures are:--

<u>Both lived.</u>	<u>M.lived: I.died.</u>	<u>I.lived:M.died.</u>	<u>Both died.</u>
85.	36.	11.	16.

It is in these figures that an antagonism of interests of mother and infant in Eclampsia is revealed which is not otherwise apparent. This is made clearer by the subsequent figures.

In the cases of the 23 mothers who died, the infant was born alive in 12, and still-born or unborn in 11 instances. That is, to 100% maternal mortality the infant mortality was 47.8%: and in 40 still-births the mother

lived in 32 and died in 8 cases. That is, to 100% infant mortality, the maternal mortality was 20%.

Further, in every instance, 8 in number, where the infant is known to have been dead prior to, or early in labour, the mother recovered.

A legitimate deduction from these facts is that Eclampsia may be classified into three groups according to the severity of its effects upon mother and infant, and that methods of treatment are allied to those groups. On the one hand, the disease is relatively mild, the toxæmia is controllable, and both mother and infant survive. In this group, comprising about 60% according to the series under review, expectant and medical measures are appropriate, with, occasionally, assistance by forcepation. On the other hand, the disease is so severe, the toxæmia so uncontrollable, that both mother and infant are destroyed. In this group, about 10% in this series, there will always be some inevitable mortality; but considerable improvement should be attainable by adaptation of the mode of treatment to the individual cases. Some in this group have probably been sacrificed by persistence, in the face of continued non-improvement, in conservative methods. Others certainly have been lost by ill-advised radical operations - Induction during the convulsive stage, and Accouchement Forcé, both of which should be abandoned.

Between those extreme groups is the third, numbering about 30%, in which the opposing interests of mother and infant become obvious. The toxæmia is controllable within safe limits only if the infant's agency in distributing toxine is terminated by its early death. It is a suggestive fact

that in the all-New-Zealand series I am at present reviewing, while the maternal mortality fell from 20.7% to 13.9%, for these years the infant mortality rose from 29.3% to 45.6%. If the infant dies in utero the mother's chances are enhanced. If it continues to live her interests are prejudiced; and the natural outcome depends chiefly on the promptitude of labour. It is in this group, mainly, that we must seek to improve results by more accurately judged use of the methods at our disposal. If the child continues to live, if the condition as indicated by blood pressure and the urine fails to improve, and if labour does not start and proceed quickly, persistence in conservatism will result in irreparable damage to the liver, and probable loss, or permanent damage at least, of the mother.

CONCLUSIONS, -

1. Eclamptic toxæmia and Eclampsia are due to autolytic poisons originating from placental infarcts.
(Young).
2. An important factor in the dissemination of the poison is the living child in utero, due to its absorbing autolytic products from the infarcts and re-liberating them into the maternal circulation.
3. Intrauterine death of the child generally checks the toxæmia by interrupting absorption from infarcts; while continued intrauterine life of the child perpetuates toxination of the mother.

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