

to denote merely the thick lower border of the upper active segment.

Rupture of the uterus always occurs or commences in the lower segment where it is abnormally thinned out, generally in unduly protracted labour.

Even under such unfavourable conditions, that is, when the lower uterine segment is considerably thinned, so long as the presentation is the ordinary vertex or occiput little danger need be anticipated during version, and this is so because of the shape of the body to be turned. In this condition the child, lying in normal attitude and position, vertex presenting (Diagram I will help to explain this), the back of the child and its occiput form almost a segment of a circle. Consequently, when we draw down a leg and the child revolves on its transverse axis, no resistance is offered by the prominent ring of Bandl to the upward movement of the head. In other presentations, however, the conditions are totally different; the outline of the dorsal surface of the child is no longer represented by the segment of a circle, and this fact constitutes a possible element of difficulty and danger; this is easily recognised by reference to Diagram II, which represents a face presentation, the head lying in the lower segment

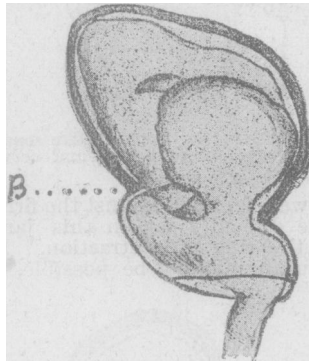


Fig. 3.—Arm presentation.

of the uterus and cervix; the back and head no longer form one continuous curve, but instead we find two curves, one formed by the back and one by the head of the child, with a markedly acute angle between them corresponding to the neck. The longitudinal axis of the body and the axis of the head meet at an angle. What happens now on drawing down a leg? Instead of the one continuous curve gliding over the projecting Bandl's ring, this projecting ring catches in the angle of the neck, the head becomes hitched and turning difficult or impossible. If traction be persisted in there will be danger that the lower segment will be stretched until its thin wall gives, and rupture takes place. The same state of affairs may occur in a shoulder presentation, when the lower segment is thinned and Bandl's ring well marked. Diagram III represents this condition—the condition found in my case just narrated.

In this case the difficulty was not due simply to a firm contraction of the uterine body over that portion of the child contained within it; this condition I have met with on several occasions. When the whole uterus is in a state of tonic retraction there is great difficulty in introducing the hand and reaching the legs. In my first mentioned case no such obstacle was met with; the hand passed readily into the uterus when once the Bandl's ring was passed; above this there was room for easy manipulation.

I would point out that the cause of difficulty and danger to which I have here called attention is one of malpresentation of the child coupled with a pronounced differentiation of Bandl's ring; a condition not to be confused with a contraction, or as it ought to be called a rigidity, of the cervix, whether at the external opening or at the internal os. The treatment I found successful under such circumstances is first to deeply anaesthetise (chloroform) the patient, then to disengage the part hitched under the ring of contraction by drawing it downwards and towards the axis of the canal, and finally to push the child *en bloc* backwards into the cavity of the uterus; version is then easily performed.

CHLOROFORM OR ETHER?

By SURGEON-MAJOR EDWARD LAWRIE, M.B. EDIN.,
President of the Hyderabad Chloroform Commission.

THE BRITISH MEDICAL JOURNAL has performed a signal service to the Hyderabad Commission in placing before the profession Dr. Julliard's views on chloroform and ether. The leading article in the JOURNAL of April 25th, 1891, is incomplete, however, and ought to have included the London statistics so opportunely brought forward by Mr. Roger Williams in the *Lancet* of February 8th, 1890.

According to Dr. Julliard's statistics, deaths from chloroform amount to 1 in 3,258, and from ether to 1 in 14,987 administrations. According to Mr. Roger Williams, the statistics of the London hospitals show that deaths from chloroform amount to 1 in 1,236, and from ether to 1 in 2,754 administrations. On the other hand, the statistics of chloroform administered on Syme's principles form an unbroken record of inhalations from 1848 to 1891 without a death. The BRITISH MEDICAL JOURNAL regards Dr. Julliard's figures as "a most valuable statistical summary," but this summary would obviously be much more useful if it were accompanied by a description of the method of administration pursued in all the cases from which it is compiled. There are two distinct methods of chloroform administration in vogue. In one the pulse, as well as the respiration, is taken as a guide; in the other the pulse is never under any circumstances taken as a guide; and it is manifestly unreasonable to compare the risks of ether and chloroform without stating with regard to chloroform which of these methods is employed. The importance of this point lies in the fact that there is not one case of death from chloroform recorded, in which it is proved that the pulse was not taken as a guide; whereas, in Syme's practice and in my own, where the pulse has never been taken as a guide, no death from chloroform has ever occurred. It should be stated that in Syme's practice, as in my own, the anaesthetic was always administered by students and not by specialists. *If the pulse is affected under chloroform, it indicates chloroform poisoning either direct or through abnormal respiration. All the chloroformist has to produce is harmless anaesthesia, with regular breathing, and without poisoning, and of this the pulse can never be any test whatever; it is, therefore, positively dangerous and useless to take it as a guide.* The following table places the available figures in a most striking light:

Mortality Statistics of Chloroform and Ether.

Anaesthetic Employed.	Source of Statistics.	Period.	Number of Deaths to Administration.
Chloroform	Julliard	Not stated	1 to 3,258
Ether ...	Julliard	"	" 14,987
Chloroform	St. Bartholomew's Hospital (Roger Williams)	10 years, 1878 to 1887	" 1,236
Ether ...	" " "	"	" 2,754
Chloroform	Syme and Lawrie ...	43 years	No death.

If statistics are of any value, this table ought to carry conviction with it, because it shows clearly that chloroform administered on Syme's principles is even less dangerous than ether administered in accordance with the most approved methods. But the Hyderabad Commission has no desire to institute further comparisons between them. All we say is, let anybody use ether who chooses, but if chloroform is to be employed, let it be given in the right way. Surgery cannot yet do without chloroform, and the only way to give it with invariable safety is to be guided, as Syme was, not by the circulation, but entirely by the respiration. What Dr. Julliard says about ether I can say, *mutatis mutandis*, about chloroform. During fourteen out of the seventeen months that have elapsed since the Hyderabad Commission demonstrated that the key to the safe administration of chloroform consists in regular breathing, I have given chloroform several

¹ For three months I was absent on leave.

times daily. Not only have I not had any deaths, but I have met with no accident of any kind. I have not once had to do artificial respiration or to pull forward the tongue. Neither have I had to interrupt an operation in order to ward off any accident due to chloroformisation. There is no element whatever either of luck or of chance about these results. Any surgeon can administer chloroform without risk who will take the trouble to assure himself that the patient's breathing is normal and regular throughout the administration, and to stop the inhalation in good time, that is, directly full anaesthesia is produced. Statistics such as those of Dr. Julliard and Mr. Roger Williams, which are intended to show the danger of chloroform, are, as my table proves, susceptible of a very different interpretation. If they help to prove anything, it is that no anaesthetic is absolutely safe except chloroform administered on Syme's principles, and the more proof we have of this kind the better.

NOTE ON MALTA FEVER.

BY SURGEON DAVID BRUCE, M.S.,
Netley.

THE BRITISH MEDICAL JOURNAL for May 16th, 1891, contains a paper by Staff-Surgeon Charles C. Godding entitled "Malta (Remittent) Fever," in which the conclusions arrived at are so opposed to those held by myself that I feel compelled to offer a few remarks in the way of criticism. In extenuation of my thus coming forward as a critic, I may state that I have had ample opportunity of studying this most interesting and important fever, as I was stationed for five years in the Military Hospital, Valletta, during which time more than 400 cases came under my notice. I am, therefore, interested in a very high degree in all that pertains to this fever, and am most desirous of procuring a proper recognition of what I consider to be its true nature at the hands of English medical men generally, and especially those of the army and navy. Although there are extremely few of the deductions given by Staff-Surgeon Godding as the result of his experience with which I perfectly agree, I shall limit myself to a consideration of his classification of the disease, as upon this the important questions regarding treatment, and the propriety of early removal from the infective area, evidently depend.

The disease, in his opinion, is "a paroxysmal fever with daily remissions." Turning up the word "paroxysmal" in Quain's *Dictionary of Medicine*, I find it is "used to indicate the periodic attacks or fits which characterise certain diseases, such as ague, gout, and asthma;" but there are no periodic attacks or fits in Malta fever, which, on the contrary, is one of the most monotonous of diseases, and therefore I take it that the word is not used here in the sense quoted. The word is also used commonly as synonymous with malarious, and this is, I presume, the meaning Staff-Surgeon Godding gives it; hence the definition may be read "a malarious fever with daily remissions." Now this is the point I wish to combat. In my opinion Malta fever is not a malarious fever; there is no tiniest thread of malaria running through it; it is a species of fever perfectly distinct from enteric or malarious fever, having its own definite parasitic cause, and is as worthy of specific recognition as diphtheria or tuberculosis. This is my position, assumed and held mainly by a belief in the following line of argument.

It is slowly creeping into the minds of English medical men—slowly chiefly on account of the impassable difficulties placed by legislation in the way of original research in England—that infectious diseases are caused by specific parasites. It is slowly but surely becoming established that enteric fever is not enteric fever without the presence of Eberth's bacillus, and that Asiatic cholera is not Asiatic cholera without Koch's cholera vibrio. In the same way malarious fever, in whatever part of the world it occurs, is coming to be associated with an ameba-like parasite found in the blood of those suffering from the disease. The demonstration of this parasite, supplemented clinically by the very specific action of quinine, constitutes the most rational and surest method of diagnosis. Now, if an ameba-like parasite could be demonstrated in the blood of those suffering from Malta fever, and if quinine could be shown to have its specific effect on the course of the dis-

ease, then I would be the first to admit that Malta fever is malarious. In this case the term remittent fever, in the officially recognised nomenclature of diseases would exactly describe the disease, and could be used instead of the very misleading term simple continued fever, under which heading it is at present principally returned. But in Malta fever no one has been able to find any such parasite in the blood, and it has been proved over and over again, and partly admitted by Staff-Surgeon Godding himself, to be absolutely and totally refractory to quinine. On the other hand, to prove the specificity of this disease, I undertake to demonstrate in every fatal case of true Malta fever a definite micro-organism which has not the slightest resemblance to that found in malaria. This parasite I have shown to be capable of giving the fever to certain animals, with all the symptoms met with in man. It can be cultivated with the greatest ease from the spleen and other organs of fatal cases of Malta fever, and has never been found in the organs of cases of malarious fever or any other disease. From these, and many other, considerations, I hold that Malta fever is not a malarious or a paroxysmal fever, and that Staff-Surgeon Godding's definition of the disease is utterly misleading and not altogether harmless, leading, as it is sure to do, to the indiscriminate use of large doses of quinine in the treatment of the fever.

MEMORANDA:

MEDICAL, SURGICAL, OBSTETRICAL, THERAPEUTICAL, PATHOLOGICAL, Etc.

HYPERPYREXIA IN INFLUENZA.

CASE I.—On May 26th I saw a man suffering from influenza. The temperature was 103.8° F. On May 28th the temperature was 99°. On May 29th it rose to 101°, and on the following morning it was 104.8°. Antifebrin gr.x were given, but in about an hour the temperature had reached 108.4°. He was then unconscious and dying. He was lifted into a slipper bath partly filled with lukewarm water; cold water was then added until quite cold. The temperature went rapidly down until it became subnormal. During the twenty-five minutes he was in the bath he gradually returned to consciousness. He was lifted out, clothes stripped off him and well rubbed down, put into blankets, and placed back in bed. He went on very well for three or four hours, taking plenty of nourishment. The temperature then rose again to 106.4°, when he was again placed in the bath; he was sensible and was able to take brandy during the bath, which lasted twenty minutes, when the temperature had fallen to 98°. He went on favourably until about 8 p.m., when the temperature rose again to 106.2°, and still continuing to rise; the bath was again suggested, but was not used. He soon became unconscious, and died about an hour after.

CASE II.—On May 27th a young married woman, aged 21, was confined. On May 29th she was seized with influenza. The temperature was 103.6°. She went on pretty well until May 30th, when the temperature ceased to fall, and soon began to rise, reaching 104.8° F. Antipyrin, antifebrin, and quinine reduced this very little, the tendency being upwards. About 2 a.m. on May 31st the temperature rose quickly to 108°. She then became unconscious, and was evidently dying. She was given a bath of lukewarm water, and cold water was poured over her chest until the water became quite cold in the bath. She was kept in the bath until she came to and began to speak (probably 25 minutes) the temperature had fallen to 97.4. She was then lifted out and rubbed down, and put to bed. During the early morning she became maniacal, but suddenly became sensible again. She continued well until about 2.30 p.m. on May 31st, when the temperature again went up to 106.7°. The cold bath was again resorted to; this time she was able to ask for the bath, and told us to "be sharp" with it. My assistant again put her in, and this time kept her in fifty minutes; the temperature had then reached 97.4°. She conversed freely during the bath, and took support and stimulants. She was again taken out, rubbed down, and put to bed. The temperature one hour and a half after was normal, but about three hours after it again began to rise. Anti-