eighth week. In addition, two cases in the quinine group and eleven in the controls started before this time; details of these labours are not included in the results. I am sure that the incidence of premature labour in the control group is abnormally high, because after the thirty-eighth week the time of onset of labour was approximately equal in both groups. Only one case—a control—went more than two weeks over the calculated date of labour. The other results are shown in the tables.

	Time	Quinine	Control
Stage 1:	Under 12 hours ...	33 (66 %)	20 (40 %)
	12 to 24 hours ...	12 (24 %)	21 (42 %)
	24 to 36 hours ...	3 (6 %)	6 (12 %)
	Over 36 hours ...	2 (4 %)	3 (6 %)
Stage 2:	Under 1 hour ...	34 (68 %)	20 (40 %)
	1 to 2 hours ...	12 (24 %)	21 (42 %)
	Over 2 hours ...	4 (8 %)	9 (18 %)
Stage 3:	Under 1/2 hour ...	45 (90 %)	47 (94 %)
	1/2 to 1 hour ...	5 (10 %)	3 (6 %)
	Over 1 hour ...	—	—

It will be seen that in 66 per cent. of the quinine cases the first stage of labour lasted under twelve hours, as compared with only 40 per cent. of the controls. In the longer first stages controls predominate. The second stage shows similar results—namely, 68 per cent. of quinine cases were under one hour in this stage, compared with 40 per cent. of controls, which again are in excess in the long second stages. In the third stage the results are very slightly in favour of the controls.

Haemorrhage.—There was no case of severe post-partum haemorrhage in either series, and no marked difference in the amount of blood lost. Pituitrin was necessary in four cases in the quinine series, and in six of the controls.

	Quinine	Control
Under 10 oz. ...	32 (64 %)	29 (58 %)
10 to 20 oz. ...	7 (14 %)	7 (14 %)
Over 20 oz. ...	6 (12 %)	6 (12 %)
Not noted ...	5 (10 %)	8 (16 %)
Pituitrin ...	4 (8 %)	6 (12 %)

Lacerations were almost equal in incidence.

	Quinine	Control
First degree ...	13 (26 %)	13 (26 %)
Second degree ...	4 (8 %)	6 (12 %)
Third degree ...	—	—

Puerperium.—In the quinine series four cases were morbid according to the B.M.A. standard. One of these was due to pyelitis. There were two morbid cases in the controls. None of these cases needed special treatment. **Stillbirths:** None in either series. **Forceps:** One in quinine series for foetal distress.

Conclusions

Although it is admittedly dangerous to draw conclusions from a small series of cases, certain salient facts do emerge from this investigation.

1. The tendency to onset of premature labour is, if anything, diminished by the ante-natal administration of quinine.
 2. The first and second stages of labour are accelerated without damage to mother or child. Inertia is not entirely abolished.
 3. The absence of cases of retained placenta or severe post-partum haemorrhage in both series makes it impossible to decide whether the incidence of these conditions is reduced or not. A much larger series of cases would be needed to prove this point. Bearing in mind the increased activity of the uterine muscle during the first and second stages of labour, one would expect a similar increase during the third stage.
 4. There is no evidence of any direct influence on involution and the puerperium. Here again a larger number of cases is required.
- The results suggest that the uterine action is strengthened by giving small doses of quinine during the

last month of pregnancy. If we accept this fact the method is surely to be recommended as a routine for use in normal, and especially in abnormal, cases, for there are few if any conditions in obstetrics where good uterine contractions are not a valuable factor. A method which helps to secure these, and which is simple and safe enough for use in domestic practice, appears to me to deserve more attention than it has had up to the present time.

In conclusion I wish to thank my colleagues on the staff of the Birmingham Maternity Hospital, who have kindly allowed me to use their cases in preparing this paper, and also the members of the resident medical staff for their help in collecting the figures.

REFERENCES

- ¹ Mitchell, D. A.: *British Medical Journal*, 1930, i, 144.
- ² Buddee, F. W.: *Ibid.*, 1934, i, 1159.

FURTHER EXPERIENCE OF THE USE OF QUININE IN NORMAL LABOUR

BY

D. A. MITCHELL, M.D.LOND., F.R.C.S.ED.
M.C.O.G.

AND

H. N. BRADBROOKE, M.A., D.M.Oxon.
M.R.C.P.LOND.

Over five years ago one of us (D. A. M.) published a paper¹ entitled "Use of Quinine in Normal Labour." The method used owed its origin to Dr. Philip Jones of Coleford, in Somerset, and consisted in the giving of a quinine salt in small doses (1½ to 2 grains thrice daily) for a period of two or three weeks prior to the expected date of labour. At the time this paper appeared Dr. Jones and the author (D. A. M.) had observed, and carefully followed up, something over 400 cases in general practice. The beauty of the method lay in its simplicity.

During the past five years it has been shown by letters to the medical journals, by discussions in medical societies, by the thesis of one of us (H. N. B.) accepted for the degree of D.M., and by personal correspondence, that the method has attracted wide attention both in this country and abroad. Some criticisms have been fairly, and some unfairly, made, and the time seems ripe to reply to our critics and to produce further support for the original claims.

Re-examination of Claims

We propose to discuss seriatim the four claims that were originally made for the method in the paper published in the *British Medical Journal* in 1930. We do this in the light of more recent experience, paying due regard to any criticisms that may have been put forward.

1. "The General Health of the Patients is Improved"

We hardly feel obliged to support this claim. The undeniable tonic action of quinine in small doses should alone be sufficient argument. There is, however, stronger evidence submitted in the correspondence of Green-Armytage,² Gair Johnston,³ and others in the *Journal*. Apart from this, we have noted since the earliest days of the investigation (some twelve years ago) that many patients liable to dyspepsia and heartburn in the later months of pregnancy are cured of these symptoms by the prescription of quinine. At first this was rather puzzling, as it was not realized how many of these patients were suffering from hypochlorhydria, and the tendency was to treat them—unsuccessfully—with bismuth and alkalis. If it should be argued that the improvement is due to the few minims of acid used to dissolve the quinine in the original prescription, we shall not feel inclined to dispute the point.

2. "Labour is Made Easier and Shorter"

Considerable evidence in support of this claim has been brought forward. Apart from the letters of Green-Armytage and Gair Johnston, previously mentioned, Gordon Luker⁴ considers pre-natal quinine, in combination with calcium, to be the most efficient means of preventing uterine inertia. After careful observation of fifty-three consecutive cases treated by one of us (H. N. B.) in the routine of general practice, the following conclusions were drawn:

"The first stage of labour (obliteration of the cervix and dilatation of the os uteri) takes place painlessly, and frequently without the knowledge of the patient, until it is nearly completed. The contractions of the uterus in the second stage are frequent and forceful, and the duration of the second stage is shortened. Taking as a criterion the period of incapacity due to pain, the duration of labour is conspicuously shortened. The average duration of severe pain in primiparae and multiparae was three hours and one and a half hours respectively."

It is quite obvious that this estimate of the duration of painful labour must depend upon several factors. In the first place some agreement must be reached upon the definition of "true pains" as opposed to "niggling pains." In the original paper on this method of pre-natal "quininization," "true pains" were defined, for the want of other definition, as those pains which made the patient, being up and about, stop and hold on to a chair, etc., until they had passed off. In this investigation we have both used this same standard of "true pains," but in addition one of us (H. N. B.) estimated the total duration of labour, and the following table shows the results at a glance:

General Average Figures

	Total Duration of Labour	Duration of Painful Labour
Primiparae (41)	12 hours	2½ hours
Multiparae (12)	8 "	1½ "

Effect of the Presentation

	Total Duration of Labour	Duration of Painful Labour
Vertex, occipito-anterior (39 cases)	10½ hours	2 hours
Vertex, occipito-posterior (10 cases)	16½ "	3½ "
Breech (4 cases)	5 "	5 "

The duration of painful labour is obviously affected by the decision of the individual practitioner on the advisability of affording relief to his patient by hypnotics, anaesthetics, and, in certain cases, by instrumental intervention. The duration of such assistance should, however, always be included in the period of painful labour, and has been so recorded in these figures.

The only serious criticism of this claim for shorter and easier labours in "quininized" patients came from Buddee,⁵ who used a modified method of pre-natal quininization at Queen Charlotte's Hospital upon 100 cases, with a similar number of controls. Apart from the fact that his modification of the method was, in our opinion, unsuitable and undesirable, he made no distinction between the total duration of labour and the duration of painful labour, and it is difficult to draw a just comparison between his figures and our own.

The claim for shorter and easier labours in cases subjected to pre-natal quininization has recently been substantiated by Ganner⁶ in a series of fifty hospital cases with controls. [See page 205.]

3. "Uterine Retraction is Uniformly Good"

It is difficult to provide positive and primary evidence of the truth of this important statement, but indirect evidence is abundant. Hewetson⁷ specifically noted this point in his practice in malarious districts over seven years ago. He also remarked on the apyrexial puerperium of these quininized cases, a point inevitably to be associated with good retraction and involution, or, in modern phraseology, the "blood-tight uterus."

In the above-mentioned series of fifty-three cases there was no rise of temperature above 99.4° F. recorded on the usual morning and evening charts, and we regard this as strong indirect evidence of good retraction. In a hospital attended by one of us (D. A. M.) it was noticed that clots were rarely seen in the forty-eight hours following labour in the quinine cases, whereas in other cases the occurrence was fairly common; surely this is evidence of firm and sustained retraction. Another pointer is the fact that in the considerable correspondence on the subject which we have examined only one case of post-partum haemorrhage is recorded, and this was associated with retained placenta in a patient who had taken the quinine for one week only (Rees⁸). Previous experience of the method, extending over five years, had left the author enthusiastic over the benefits.

4. "There is no Increased Tendency to: (a) Premature Labour"

No such tendency has been demonstrated by anyone who has used the method. Buddee,⁵ in his series at Queen Charlotte's, found a fractional increase of prematurity in the quinine cases compared with his normal controls. However, as previously mentioned, his modification of the original method might well have led to greater divergencies than this, and we do not regard it as a valid criticism.

(b) "Precipitate Labour"

It is curious that in the literature upon this subject we have found no complaints of precipitate labour after the quinine treatment. It probably occurs no more frequently than in patients who have had no quinine, although some of the labours observed by us have been remarkably quick. The explanation is twofold. In the first place there is a strong tendency to a painless, or relatively painless, first stage. When this is followed by a more than usually forceful second stage it is easy to see how apparently precipitate labour may result. This condition undoubtedly accounts very largely for the frequent "B.B.A." (born before arrival) cases noted in the early days of our investigations, but we have had no difficulty since we adopted the precaution of explaining thoroughly to the nurses how greatly the normal course of labour may be altered by the quinine treatment.

(c) "Retained Placenta"

Our own experience, and the comments of others, have shown only one serious instance of this complication. We have referred to it above under "uterine retraction." One of us (D. A. M.) has even had apparent success in treating with pre-natal quinine a few patients with histories of repeated retained placenta and post-partum haemorrhage.

(d) "Perineal Laceration"

The frequency of this is definitely not increased by the use of pre-natal quinine, and, as stated in the original paper, this is not surprising. Such accidents are more likely to occur when the patient has to use all her accessory muscles, aided possibly by fundal pressure

from the nurse or doctor, than when the uterus is fully equal to its task, and the attendant is free to exert restraining influence on the crowning head.

(e) "After-pains"

The frequency of these is not increased. In confirmation of this bald statement we can only say that the matter has never been mentioned (except by ourselves) in any correspondence on the subject.

Discussion

Such, then, is a short statement of more recent experiences in the use of quinine in normal labour, and we may well be asked to explain the limitations of the method. Claimed as a valuable aid to normal midwifery, the ante-natal use of quinine is also of undoubted value in certain conditions outside normality. This region, however, is vast and ill-explored, and it is wiser to admit at once that in the realm of the abnormal each case must be considered on its own merits. There are definite contra-indications to the use of quinine, such as the presence of albuminuria, flat pelvis, or, for that matter, any condition in which, for the purpose of moulding, slow labour is desirable.

When normal cases alone are under consideration it may justly be claimed that the method solves many problems of obstetric routine. It is rarely necessary to interfere or in any way to assist the patient during the first stage. The relative freedom from pain during this period is so frequent and so striking that the patient is often seen for the first time when the cervix is fully or nearly fully dilated. When the second stage is reached both humanitarian and obstetrical principles are well served by the use of short and simple hypnotics, analgesics, or anaesthetics, according to the taste and experience of the individual practitioner. There is seldom time or opportunity for the more complicated methods of "twilight sleep," or the more drastic routine of "tissue"—and dare we add "foetal"—poisoning.

In conclusion, we should like to emphasize a point which we have repeatedly had to explain in discussions. Our method of using quinine as an ante-natal prophylactic has little in common with the use of this drug in much larger doses for the deliberate induction of labour, and we strongly deprecate the natural tendency to strive for better results by giving larger doses. Such attempts, as we know from experience, never do any good, and can only bring the method into disrepute.

Nowadays the claim is made neither by pharmacologist nor clinician that quinine is a reliable, or even effective, *initiator* of uterine contractions. Rather should it be regarded as a true tonic—that is, an agent which increases the basic tone of the uterine muscle fibres, and thus reinforces contractions which are excited by other endogenous means. This tonic action is developed by a relatively small dose, and larger doses can even produce a state of inertia and increasing paralysis of the uterine muscle. For the purpose here discussed our combined experience shows that the dose of quinine should not exceed $1\frac{1}{2}$ to 2 grains given thrice daily for two to three weeks before labour is expected. Further simplification of the method, such as the use of a single daily dose of five grains, spells failure.

REFERENCES

- ¹ Mitchell, D. A.: *British Medical Journal*, 1930, i, 144.
- ² Green-Armytage, V. B.: *Ibid.*, 1933, ii, 397.
- ³ Johnston, D. J. Gair: *Ibid.*, 1933, ii, 265.
- ⁴ Luker, S. Gordon: *Ibid.*, 1933, ii, 58.
- ⁵ Buddee, F. W.: *Ibid.*, 1934, i, 1159.
- ⁶ Ganner, P.: Paper read before the Midland Obstetrical and Gynaecological Society, May 20th., 1935.
- ⁷ Hewetson, W. M.: *British Medical Journal*, 1928, i, 156.
- ⁸ Rees, W. A.: *Ibid.*, 1933, i, 587.

THE PHYSIOLOGY OF DEEP-SEA DIVING*

BY

WILLIAM A. R. THOMSON, M.D.

LATE SURGEON LIEUTENANT R.N.

The term "deep diving" defines any diving operation carried out at a depth exceeding 200 feet.

The scientific study of diving may be said to date from 1878, when Paul Bert¹ published *La Pression Barométrique*. The following thirty years witnessed the accumulation of many new facts concerning diving and caisson work, and in 1907 was published the "Report of a Committee appointed by the Lords Commissioners of the Admiralty to consider and report upon the conditions of Deep Water Diving."¹⁰ The recommendations of this committee, for which Professor J. S. Haldane was mainly responsible, rapidly gained universal adoption, and were instrumental in reducing the morbidity attached to such work to almost infinitesimal figures. Progress, however, was inevitable, and in 1930 the Admiralty appointed a further Deep-diving Committee—the members of which included Sir Leonard Hill—to investigate the possibilities of diving to greater depths than had hitherto been considered possible.¹¹

This subject can perhaps be best studied by a consideration of the three major difficulties to be overcome in deep diving. These consist of: (a) carbon dioxide poisoning, (b) oxygen poisoning, (c) caisson disease, or, as it may be more accurately termed, compressed air illness.

Carbon Dioxide Poisoning

Prior to the investigations of the 1907 Admiralty Committee one of the major difficulties encountered at depths exceeding 90 feet had been that the breathing of the diver became very laboured, and cases of loss of consciousness were not uncommon. This, the committee showed, was due to the pressure of carbon dioxide.

Normally, respiration is so regulated as to maintain a constant percentage of carbon dioxide in the alveolar air—that is, 5.6 per cent. At abnormal atmospheric pressures, however, it is the absolute pressure, not the percentage, of carbon dioxide in the alveolar air that remains constant—that is, a diver at 297 feet, or 10 atmospheres absolute pressure, will need ten times as much air as he would at the surface. At this depth a tension of 0.1 per cent. in the air breathed has the same effect as breathing 1 per cent. of carbon dioxide at atmospheric pressure. When the 1930 Admiralty Committee¹¹ began its deliberations on diving at depths exceeding 200 feet, it was faced by the problem of supplying sufficient air to the diver to prevent any accumulation of carbon dioxide, as a very large amount of air would be required to provide the necessary ventilation to an ordinary diving helmet when a diver is in deep water. The committee finally adopted a special deep-diving dress in which the diver carries a canister containing a carbon dioxide absorbent instead of the back weight—designed and produced by Messrs. Siebe Gorman and Co., of London. Air is supplied to the diver from a high-pressure air system through reducer valves which reduce the pressure from 2,500 to 180 lb. per square inch.

That this system is completely successful is shown by the figures for analysis of air in the diver's helmet at depths from 168 to 287 feet. The average figure for twenty-three dives was 0.13 per cent. of carbon dioxide. From the practical point of view it is equally successful, as by means of it very rapid descents are possible, one diver having descended to 290 feet in under two minutes without any inconvenience.

Oxygen Poisoning

The toxic effect of oxygen may be divided into two groups: (a) that resulting from short exposures to high pressures, and (b) that resulting from prolonged exposure to low pressures. The former takes the form of con-

* The substance of a lecture delivered to the Naval Medical Officers, Royal Naval Hospital, Haslar, December 6th, 1934.