

preceded by disease of sufficient severity to have required treatment, the risk of stillbirth is 20%–27%, and these (20% of all cases) provide food for thought. It is especially in these that a further method of assessment, whether by antibody titrations, tests on liquor amnii, or other method, is needed.

The time at which stillbirth occurs must also be taken into account, and this varies, the chance of the pregnancy reaching 35 weeks satisfactorily being only 30% following multiple stillbirths but 70% for "first affected" stillbirth. The nearer a pregnancy approaches term satisfactorily the less the risk of stillbirth, so that the later induction is delayed the greater its apparent benefit unless early stillbirths are taken into account. Induction at 37 to 38 weeks can possibly prevent 40% of all stillbirths, but less than 30% of those with a bad obstetrical history.

We still require an accurate method of forecasting stillbirth in haemolytic disease, and, having forecast it, a better method than premature induction to forestall it.—We are, etc.,

W. WALKER.  
S. MURRAY.  
J. RUSSELL.

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#### REFERENCES

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- 2 Davies, B. S., Gerrard, J., and Waterhouse, J. A. H., *Arch. Dis. Childh.*, 1953, 28, 466.
- 3 Allen, F. H., jun., Diamond, L. K., and Richardson-Jones, A., *New Engl. J. Med.*, 1954, 251, 453.
- 4 Walker, W., Murray, S., and Russell, J. K., *Lancet*, 1957, 1, 348.

#### Deformities of the Chest

SIR,—Your annotation on deformities of the chest (*Journal*, March 16, p. 637) comments on the little work which has been done in Britain on this subject. After referring to the suggestion of H. A. Brodtkin<sup>1, 2</sup> that funnel chest and pigeon breast might be due to congenital weakness of the anterior segment of the diaphragm the annotation states: "His arguments, though interesting, are mainly theoretical and proof should be forthcoming before they are accepted. It should be possible to learn something from post-mortem studies. . . ."

In his Hunterian lecture at the Royal College of Surgeons of England, May 17, 1956, Mr. E. F. Chin discussed the surgery of funnel chest and congenital sternal prominence.<sup>3</sup> I carried out a histological study of muscle biopsies from the anterior portion of the diaphragm taken by Mr. Chin at operation and compared the findings with biopsies from normal controls in the same age group. The histological picture has done a great deal to confirm Brodtkin's work. In the cases of funnel chest the anterior portion of the diaphragm proved to be grossly deficient in muscle, and the most common change was a marked increase in fibrous tissue. Some cases showed complete replacement of the muscle by fibro-fatty connective tissue. These findings are illustrated in Mr. Chin's paper, and, in our opinion, they fully explain the clinical findings in these cases. I would submit therefore that the congenital defect in the development of the diaphragm suggested by Brodtkin has now been fully proved.—I am, etc.,

Southampton.

R. A. GOODBODY.

#### REFERENCES

- 1 Brodtkin, H. A., *Pediatrics*, 1949, 3, 286.
- 2 ———, *Dis. Chest*, 1951, 19, 288.
- 3 Chin, E. F., *Brit. J. Surg.*, 1957, 44, 360.

#### Chemotherapy in Primary Tuberculosis

SIR,—I was interested to read the comments of Dr. F. J. Bentley (*Journal*, March 9, p. 584) on my recent article on this subject (*Journal*, February 9, p. 324). I am in entire agreement that a major planned trial is called for. As I stated, "The results obtained in the present relatively small series of cases . . . cannot take the place of an adequately controlled large-scale trial."

In recent years treatment of primary tuberculosis in the Indian and Eskimo child has been carried out on a large scale in this country, and it is the general opinion among workers in the field that the clinical course now very closely

resembles that in the white child. In view of this, despite the smallness of the group, and with the reservations mentioned, I feel that it is justifiable to conclude that there is no *obvious* increase in the natural frequency of segmental lesions as a result of antimicrobial treatment. As Dr. Bentley has reported a 10% incidence of haematogenous complications in children under 2 years of age suffering from a primary infection,<sup>1</sup> it would appear to me unwise to discourage the use of isoniazid in any of this group in view of its excellent reputation in the control of haematogenous spreads unless the results of a controlled trial should show a very real increase in segmental lesions among cases who receive chemotherapy.—I am, etc.,

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Ontario, Canada.

J. B. RYDER.

#### REFERENCE

- 1 Bentley, F. J., Grzybowski, S., and Benjamin, S., *Tuberculosis in Childhood and Adolescence*, 1954. London.

#### Neodymium 3-Sulpho-isonicotinate and Blood Coagulation

SIR,—Professor R. B. Hunter and Dr. W. Walker have reported (*Journal*, January 26, p. 227) that they have found haemoglobinaemia in all of four patients who received daily injections of neodymium 3-sulpho-isonicotinate 5 mg./kg. for more than five days. It is not stated, however (as I have heard by personal communication from Dr. Walker), that the haemoglobinaemia was demonstrated only by spectroscopic methods. It may be remembered that the principal bands for neodymium (594–562, 534–498 m $\mu$ ) are the same as those of oxyhaemoglobin (578 and 542 m $\mu$ ). It is therefore not possible to show by simple spectroscopy that neodymium will produce haemoglobinaemia. There is also no evidence that it was possible to convert the pigment into reduced haemoglobin, methaemoglobin, and haematin with the corresponding changes in the spectrum.

German authors have seen haemoglobinaemia or haemat-uria in only some of many thousands of patients, which is similar to the observations following the application of heparin or dicoumaroles, where it is thought that these effects are associated with a greatly prolonged clotting-time rather than being caused by the anticoagulant itself. The proof of a drug-induced haemoglobinaemia is also difficult, because destruction of erythrocytes at venepuncture or on sharp centrifuging of the blood can result in haemolysis.

I believe that it is not justified to draw conclusions from such obviously inadequate experiments and results as regards the clinical application of neodymium sulpho-isonicotinate, a remedy in clinical use for seven years.—I am, etc.,

Hamburg.

E. VINCKE.

#### Natural Childbirth

SIR,—I have no desire to enter into any controversy regarding the merits and demerits of training programmes for natural childbirth. In my letter (*Journal*, December 29, 1956, p. 1545), I merely wished to add support to Dr. Constance Beynon's<sup>1</sup> contention that it is now time to take stock of the situation regarding the value of these training programmes. I would, however, like to correct certain misunderstandings created in the mind of Dr. J. G. Scott (*Journal*, February 9, p. 343) by my letter.

First, I have a very great respect for Dr. Read's work and consider that he has done us obstetricians a very great service in helping to wean us away from "unnatural childbirth." With most new ideas, however, the pendulum has always a tendency to swing too far due to excess of enthusiasm, and it is necessary to check it before any actual damage is done either to patients or to the nation's purse. Secondly, in more than twenty years of obstetric practice, I have yet to come across an "unsophisticated girl" who has had any education in preparation for labour during pregnancy by a midwife, whether "native" or foreign, excepting those girls who have already been attending antenatal clinics. These girls receive a certain amount of simple but very valuable instruction by the health visitors during