when public interest in design as such has been awakened, it is regrettable that insufficient has been done to make generally available a well-designed model of this fundamental aid to the cripple.

London, W.1. J. T. BACH.

EARLY-MORNING CRAMP

SIR,—Some years ago there was a correspondence in your columns on the cause of cramp.

I remember that at that time I had a hunch that the cause was central and not peripheral—nervous and not vascular. The intervening years tend to confirm that hunch and suggest that for the relief of early-morning cramp one might turn to Sherrington's reciprocal innervation of antagonistic muscles. Contract the antagonists, and the cramp should disappear. As an example: cramp in the quadriceps extensor should be alleviated by voluntarily flexing the knee-joint. It is so in me.

Will other sufferers from cramp put this idea to the test? It's difficult to think out the antagonists in the agony of cramp in a muscle, but, in a simple case like the above, quite possible.

Donhead St. Mary, near Shaftesbury. V. H. MOTTRAM.

EFFECT OF ACETYLSALICYLIC ACID ON FŒTAL MICE AND RATS

SIR,—In their comment on our letter [1] Miss Earley and Mr. Hayden [2] state that it was impossible to deduce from our data exactly how much acetylsalicylic acid our animals received.

In fact, we stated that the mice and rats were given 1·2 g. and 0·6 g., respectively, of acetylsalicylic acid per kg. body-weight. The average body-weights of the mice and rats were not stated. They were 25 g. and 185 g. respectively. The diet of the mice and the rats was identical; it contained 1% acetylsalicylic acid. The mice ate, on average, 3 g. food daily, and the rats, on average, 12 g. food daily.

Miss Earley and Mr. Hayden suggest that uncoupling of oxidative phosphorylation in the mother animal might be one cause of the lethal effect of acetylsalicylic acid on fœtal rats. Naturally, this possibility has been seriously considered in the Amsterdam laboratory, but there is experimental evidence against it. In experiments carried out several years ago by one of us (H. J. K. O.) 55 pregnant rats were given dinitrophenol, the typical uncoupling agent (35 intraperitoneally, 10 by stomach-tube, and 10 in food) for 1 to 18 consecutive days covering the 3rd to the last day of the pregnancy; 54 litters with 581 living young were born with a normal average birth-weight of about 5 g. While the litters were kept under control they showed normal growth and development.

In these experiments the mother animals received the maximum tolerated dose of dinitrophenol. With doses of about 30 mg. dinitrophenol per kg. body-weight given daily by intraperitoneal injection to adult rats on 4 consecutive days, only a few deaths occurred. With a single dose of 50 mg. per kg. body-weight no animal survived. When dinitrophenol is given by stomach-tube, the toxic and lethal doses are in the same range as with intraperitoneal injection. When the dinitrophenol is mixed with the food an amount of 0·2% is tolerated. These findings agree with the data tabulated by Farris and Griffith.[3]

We found anophthalmia and microphthalmia in 14 young from 8 litters out of a total of 389 young from 37 litters after administering dinitrophenol by intraperitoneal injection during pregnancy. Since in none of the 20 litters with 192 young born from mothers which received dinitrophenol in food or by stomach-tube were eye defects seen, they should probably be attributed to the technique of the intraperitoneal procedure. Dokter [4] has reported that various mechanical stimuli applied to the amnion sac can cause malformed fœtuses, without any toxic agent being administered. Moreover, in 64 stock litters with 638 young born in the same period only 1 anophthalmic animal was found. We concluded from these experiments that intraperitoneal injection is an unsuitable method of administering drugs to pregnant animals for a study of teratogenic effects. Nevertheless these preliminary experiments show clearly that uncoupling of oxidative phosphorylation does not cause fœtal death in rats.*

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1. Klein Obbink, H. J., Dalderup, L. M. *Lancet*, 1964, i, 565.
2. Earley, P. A., Hayden, J. *ibid.* p. 763.
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GLUCOSE-6-PHOSPHATE-DEHYDROGENASE-DEFICIENT ERYTHROCYTES

SIR,—Dr. Beutler and Miss Baluda (Jan. 25) have erred in their evaluation of the method which we used in calculating our results with respect to the disappearance of diisopropyl-fluorophosphate^{32}-labelled red cells during drug-induced hæmolysis in heterozygotes for glucose-6-phosphate-dehydrogenase deficiency.[1]

Their statement that " The calculations made failed to take into account the fact that only half of the enzyme-deficient cells are destroyed by drug administration " is erroneous, since the initial destruction of only half the enzyme-deficient cells was, in fact, one of the important cornerstones for the calculations. After initial destruction of half the erythrocytes in enzyme-deficient individuals, if drug administration is continued as it was in our studies, the remaining enzyme-deficient cells also undergo premature destruction, but at a slower rate. The red-cell survival in the two heterozygotes reported did not fit the hypothesis of a mixture of normal and enzyme-deficient cells. The suggested explanation offered by Beutler and Baluda for these findings—namely, that acute hæmolysis of enzyme-deficient cells was affecting the survival of normal cells—is improbable in view of the fact that the cell-survival measurements of interest were made after the completion of the initial, major, hæmolytic episode.

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WECHSLER TEST

SIR,—With the necessity to demonstrate subnormality of intelligence to satisfy the basic requirement of the definitions of subnormality and severe subnormality in Section 4 of the Mental Health Act, 1959, an accurate intelligence quotient assumes more importance than under the Mental Deficiency Acts. The intelligence test used should given an I.Q. which is valid for the individual tested. I am concerned that psychologists in the Prison Service apparently use the Wechsler adult intelligence scale (W.A.I.S.) to test individuals in whom subnormality of intelligence is suspected.

Fisher [2] has cast considerable doubt on the validity of this scale in this group of individuals, finding that the W.A.I.S. I.Q.s averaged 15 points higher than the Stanford Binet (S.B.) I.Q.s for subjects aged 18–54 and 23 points higher than the S.B. I.Q.s for subjects aged 55–73.

My own experience has been similar to Fisher's, and I always regard the W.A.I.S. I.Q. with considerable suspicion in a subject who appears clinically to be mentally subnormal.

I recently saw such a man on remand in prison who had been classified as of dull normal intelligence on the basis of a W.A.I.S. I.Q. of 86, but who was subsequently found to have an I.Q. of

* Tables with detailed data are obtainable on request.

1. Brewer, G. J., Tarlov, A. R., Powell, R. D. *J. clin. Invest.* 1962, **41**, 1348.
2. Fisher, G. M. *J. cons. Psychol.* 1961, **25**, 192; *ibid.* 1962, **26**, 391; *J. ment. Defic. Res.* 1962, **6**, 41.