Tests for growth hormone secretion

SIR,—Professor Brook and Dr Hindmarsh claim that children growing at a less than 3rd centile velocity carry a 97% chance of showing an abnormality on investigation.

1 In a leading article in the British Medical Journal in 1986 they wrote: 'If a third centile velocity is chosen for immediate action the chances of investigating a normally growing child are only 3%.

2 The latter statement is capable of two interpretations. It could mean that 3% of normal children would be investigated, which is true. It could also be taken to mean that 3% of investigated children would be normal, which is not true, unless by chance. Only this second interpretation, however, fits with the assertion that 97% of investigated children would be abnormal.

The percentage of children below the third centile who are abnormal is:

Number of abnormal children in the group (<3rd centile) × 100
Total number of abnormal + normal children in the group

Growth chart centiles concern only normal children. They cannot tell us about the number, or even the existence, of abnormal children and therefore cannot provide the figures necessary for the calculation.

The chances of finding an abnormality on investigation depend on the sensitivity of the methods of investigation, and on what proportion of those perceived as having the problem have it and what proportion represent simply the extremes of biologival variability. Brook and Hindmarsh give no indication that they have taken these factors into account in arriving at their figure of 97%.

D P ADDY
Dudley Road Hospital, Dudley Road, Birmingham B18 7QH


Professor Brook and Dr Hindmarsh comment

We thank Dr Addy for his interest and we accept his point.

In practical terms it makes not the slightest difference because the total number of abnormal children will so greatly outweigh the number of normal children that the fraction he gives will approach unity or 100%, which is the point we were trying to make.

Gut blood flow velocities in the newborn: effects of patent ductus arteriosus and parenteral indomethacin

SIR,—The paper by Coombs et al claims to show indomethacin has direct effects on the splanchnic circulation that are independent of its desired effect, that is, closure of the duct. Indomethacin is such a widely used and important drug that such a claim must be carefully evaluated.

The observed effects of drug administration were a decrease in systolic gut flow velocity with a change to antegrade diastolic flow by one hour. In the group given a rapid bolus the systolic velocity fell further and more rapidly; it is claimed that this is due to splanchnic vasocstriction. The evidence provided does not support this. The conclusions are pre-supposed based on the assumption that left ventricular stroke volume does not change after indomethacin administration. In fact several papers have shown that left ventricular stroke volume falls dramatically, with a fall in systolic aortic velocity, if the duct closes. An abnormally raised cardiac output returns to normal. The decrease in systolic mesenteric flow velocities observed in this study could therefore merely reflect aortic flow changes secondary to rapid ductal constriction.

The observed velocity changes could have occurred in the descending aorta, the renal arteries, and even the femoral arteries. In fact in any systemic artery. Indomethacin normally closes the duct; therefore, to study its generalised effects, the haemodynamic consequences of ductal constriction must be taken into considera-tion. The observation that the fall in gut flow velocities decreases with second and third doses could be explained by the duct becoming progressively smaller and therefore the haemodynamic consequences of ductal constriction becoming less marked.

The suggestion that rapid administration of indomethacin causes an undesirable fall in splanchnic circulations is probably a misinterpretation of the observations presented in this paper. We note that none of the 19 infants given indomethacin suffered serious side effects and that, in every case, retrograde or absent diastolic flow changed to normal antegrade flow.

JONATHAN R SKINNER
ISAYAN MATTHES
EMUNOD N HEY
Department of Child Health, Princess Mary Maternity Hospital, Great North Road, Newcastle upon Tyne NE7 7DN


Hyperinsulinaemia hypoglycaemia in small for dates babies

SIR,—The paper by Collins et al on hypo-glycaemia in the small for dates infant raises two important issues about the pathogenesis and hence management of such infants. The authors suggested that the low glucose concentra-tion in the subgroup without overt evidence of hyperinsulinism might be explained by a transient deficiency in glucagon secretion. This implies that they believe that the remaining infants had normal plasma glucagon concentra-tions. However it is not equally likely that a proportion of their ‘hyperinsulinaemic’ group were also relatively deficient in this vital glucagonemic hormone? Thus could not the persistense of the fetal insulin effect be due to a relative deficiency of normal postnatal glucagon release? In the absence of glucagon, the key enzymes of the glucagonemic pathway (for example, phospholipase pyruvate carboxy-kinase) would be inoperative and it has recently been shown that glucose-6-phos-phatase also requires glucagon for catalytic activity.

The second and related issue is that given the wealth of theoretical evidence of the poten-tial importance of this hormone, why is the glycemic response so poor after an intramus-cular injection? In pilot studies before the original paper on transient glucagon defici-ency, we observed that infants who failed to respond to intramuscular glucagon neverthe-less showed a brisk, sustained rise in peripher-al plasma glucose concentration after an intravenous glucagon bolus (20 pmol/kilo). The most likely explanation is that the sustained peak plasma concentrations of this hormone after intravenous injection might be sufficient to raise the concentration in the portal vein above the local insulin concentrations and thereby reverse the direction of net hepatic glucose flux. Thus the early use of intravenous glauc-a in all hypoglycaemic infants is likely to shorten their dependence on intravenous glucose supplementation and in addition prevent rebound hypoglycaemia when oral feeding is introduced.

ANIL MEHTA
Department of Child Health, Ninewells Hospital and Medical School, Dundee DD1 9SY


Dr S Collins, Leonard, Teale, and Marks comments

We thank Dr Mehta for his letter. In the original protocol it was planned to measure plasma glucagon concentrations in all the babies, but there was insufficient blood in all cases. We have a few samples from 20 babies. In two patients who were hyperinsulinaemic (cases 6 and 9) glucagon was not detectable in plasma at the time of hypoglycaemia (limit of detection 25 pmol/l). Some of these results are consistent with the hypothesis that the hyperinsu-linaemic babies were relatively glucagon deficient as well.

Development of intestinal motility

SIR,—In Dr Bisset’s article on development of intestinal motility he notes that little is known of the development of colonic motility in humans and that most infants pass their first...