**LETTERS TO THE EDITOR**

**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 a problem. If a problem is real it and its proportion represent simply the extremes of biologic variability. Brook and Hindmarsh give no indication that they have taken these factors into account at arriving at their figure of 97%.

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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 patentenral 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.\(^3\) 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 vasoconstriction. The evidence provided does not support this. The conclusions are pre- sumably 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.\(^4\) 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 secondarily 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 circulation 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 ante- grade flow.

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**Hyperinsulinaemic 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.\(^1\) The authors suggested that the low glucose concentra- tion in the subgroup without overt evidence of hyperinsulinaemia might be explained by a transient deficiency in glucagon secretion.\(^7\) 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 persistence of the fetal insulin effect be due to the relatively long fall of normal postnatal glucagon release? In the absence of glucagon, the key enzymes of the glucagonemic pathway (for example, phosphoenyl pyruvate carboxykinase) would be inoperative and it has recently been shown that glucose-6-phos- phatase also requires glucagon for catalytic activity.\(^8\)

The second and related issue is that given the weight of theoretical evidence of the poten- tial importance of this hormone, why is the glycaemic response so poor after an intramus- cular injection? In pilot studies before the original paper on transient glucagon defici- ency,\(^1\) we observed that infants who failed to respond to intramuscular glucagon neverthe- less showed a brisk, sustained rise in peri- pheral plasma glucose concentration after an intravenous infusion. 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 gluca- gon in all hyperglycaemic infants is likely to shorten their dependence on intravenous glucose supplementation and in addition prevent rebound hypoglycaemia when oral feeding is introduced.

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Dr S Collins, Leonard, Teale, and Marks comment

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. In a few samples left over to do this, 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). These results are consistent with the hypothesis that the hyperinsuline- miaemic 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...