degree of comparability which is felt acceptable should be specified. If the answer is 'yes' then it would be reasonable to use the simpler Robinson method instead of the more complicated (but better validated) Dubowitz one. Correlation does not answer this question. We expect two measures of the same thing to be correlated; what we are really interested in is the difference between the gestational ages calculated by the two methods. The simplest approach is to calculate the mean and SD of the differences between the two ages. A paired *t* test can be used to test for any systematic difference between the two measures. A significant result looks likely in this case as most of the points in their figure are below the line of identity. From the SD a 95% confidence interval can be calculated. Alternatively, a regression of the Dubowitz age on the Robinson age could be carried out. Each age by the Dubowitz method could then be compared with its estimate, using the regression equation and the observed and estimated ages compared in the same way as before.

The authors have not established that the Robinson method is a good approximation to the Dubowitz system, and their assertions about the accuracy of the method are unsupported.

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Drs Serfontein and Jaroszewicz comment:

We thank Mr Altman for his interest in our paper, although the points he has raised, some of which are valid, are really minor ones and in no way affect the essence of the study.

Regarding the design of the investigation, at the start of the study a trial assessment was made by both of us in which each used the same method of assessment of gestational age on the same infant and no significant differences were disclosed. For the actual study it was felt that the fact assessments were made by two different observers (one doing only the Robinson technique and the other only the Dubowitz method) would result in greater objectivity. Independence of observation was ensured by the results of one examiner not being made known to the other.

With the term 'reasonably sure' it is meant that these were Cape Coloured mothers who could state definite dates of their last normal menstrual periods, had no bleeding within one month of those dates, and had regular menstrual cycles of 28 ± 2 days, the last period being normal in amount and duration. The first day of the last menstrual period (LMP) was used as the reference day and the expected date of delivery was calculated as 280 days (40 weeks) from that day.

Regarding the statistical model and analysis of our study we reiterate that the Robinson method for estimation of gestational age is only pertinent between 29 and 37 weeks' gestation and it is between these limits that we assessed its value against the Dubowitz method. For this reason a paired *t* test comparing the two methods is undesirable. Should one want to do so it would have to be

by means of a 4-fold contingency table with age groupings according to both methods. In this way the same restrictions are imposed upon the Dubowitz method as upon the Robinson method. There is no significant difference between the two methods when compared in this manner.

With respect to the correlation, we should like to point out that a correlation of 0.85 significant at $P < 0.01$ suggests that there is one in 100 chance that this correlation is due to chance factors, i.e. reasons other than that warranted by actual strength of data. What this correlation means is that 67% of a rating on one scale is predicted by the ratings on the other scale, which is very high agreement considering other variables that might also influence the ratio. From any statistical view point this magnitude of correlation, which is also not due to chance, is a high one.

We agree that a spuriously high positive relationship may exist because each infant was subjected to both methods of examination. However, we feel the magnitude of the correlation is strong enough to counter this point. Considering that both the Dubowitz and Robinson assessments are subject to error, a correlation of 0.85 is in fact a very good one. Good (positive) correlation means that low values obtained with the Robinson method would also be low with the Dubowitz method and vice versa. Further response to this part of Mr Altman's criticism is hampered by the usage of terms such as 'in this context' which we, for our own part, find vague.

Our statement that 'the 95% confidence interval for a single estimation of gestational age is $\pm$ one week' is incorrect. This was due to a misinterpretation on our part of the statistician's results. We should, however, like to point out that the standard error of estimate for the Robinson method when measured against the gestational age as determined by LMP is 1.37 weeks, whereas that for the Dubowitz method is 1.94 weeks.

For these reasons and given the magnitude of correlation with only a small chance factor associated with it, it would seem that the Robinson method is a highly suitable alternative to the Dubowitz method for gestational age between 29 and 37 weeks.

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Oral rehydration in infantile diarrhoea

Sir,

Chatterjee *et al.* (*Archives*, 1978, 53, 284) prefer a solution containing 50 mmol/l sodium for the oral treatment of children with diarrhoea, instead of the 90 mmol/l recommended by the World Health Organisation (1976). In their study of 39 children they found the two solutions were equally effective for rehydration, but that the solution with higher sodium concentration resulted in somewhat more periorbital oedema and hypernatraemia. This difference was not, however, statistically significant

and the problems were not shown to have clinical importance. The investigators rehydrated the children using an intragastric drip.

We successfully treated a large number of children from 2 months of age upwards with the solution recommended by the WHO. We normally give the solution as a drink, when the children's thirst controls the amount they take in, and we have found no problems with salt overload. Only in the relatively few children given the solution by intragastric drip for specific reasons (most commonly stomatitis, prostration, or excessive vomiting), have we sometimes encountered periorbital oedema among the children a few months old, but this did not appear to influence recovery.

We agree with the generally accepted practice that the oral solution should normally be given as a drink. This is not only pleasanter for the child, but it is the only method suitable for mass use in developing countries, where shortages of trained staff and equipment will generally be much greater than in the conditions of the study in the Calcutta Medical College Hospitals. This method also allows for the close collaboration of the mother in her child's treatment with its potential for health education; and evidently it may be safer.

The common problem in poor conditions with inexperienced mothers and lack of staff is not overhydration but that the child will be given insufficient fluid. In these conditions the solution of higher sodium content is more practical since the volumes needed are less. Additional free water is neither necessary nor recommended. In Mozambique the national policy (Melamed and Segall, 1978) is to use the solution recommended by the WHO.

References

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Dr Chatterjee and co-workers comment:

We thank Dr Vinhas *et al.* for some very pertinent comments on our paper on oral rehydration in infantile diarrhoea. They treated a group of children with diarrhoea with the oral solution recommended by the WHO and did not have any significant problems of salt overload. Furthermore, they administered the fluid as a drink, except in a few cases where a nasogastric tube was used. We preferred the use of a nasogastric tube for the following reasons: (1) we studied only those children with moderate and severe dehydration; they were very sick and were not well enough to drink, and rapid rehydration was mandatory; (2) as this was a controlled clinical trial, use of a nasogastric tube was preferred to reduce the variability in fluid administration, and (3) aspiration of

stomach before administration of fluid, particularly in infants who received loads of antidiarrhoeal medicines before admission, reduces the chance of vomiting. Vomiting is also less likely if the child sleeps while receiving the fluid through a nasogastric tube and is not disturbed to take the drink. We agree that for mass use, if rehydration can be started early, fluid given as a drink is the method of choice and the use of a nasogastric tube should be limited to severely dehydrated infants in cases where quick rehydration is necessary.

The question of salt overload needs a detailed analysis. Although Dr Vinhas *et al.* did not mention the severity of the dehydration, we assume that most of the children had milder dehydration as they were well enough to drink. Even so, while attempting adequate hydration in a few of the severe cases through a nasogastric tube with the solution recommended by the WHO, periorbital oedema occurred in some of them. According to Dr Vinhas, in children receiving the oral fluids by mouth, amelioration of thirst limits the intake. In our experience, however, this is not always so and on many occasions the children continued to drink excessive amounts and developed periorbital oedema. It is pertinent to mention that the estimated episodes of diarrhoea in Asia, Africa, and Latin America in 1975 is 348.2 millions in children aged under 2 years (Rohde and Northrup, 1976). Therefore, if hypernatraemia occurs even in one in a thousand, it will lead to a problem of salt overload in an enormous number of children. We strongly feel that a recommended oral fluid for mass use in the field where supervision is inadequate, should have a higher margin of safety and that the low sodium solution being equally effective should serve this purpose.

Reference

- Rohde, J. E., and Northrup, R. S. (1976). In *Acute Diarrhoea in Childhood*, Ciba Foundation Symposium 42 (New Series). Edited by K. Elliott and J. Knight. Elsevier: Amsterdam.

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Prophylactic diazepam in febrile convulsions

Sir,

We read with interest the paper by Knudsen and Vestermark (*Archives*, 1978, 53, 660) shedding some new light on an old controversy about preventing febrile convulsions (FC). The authors stated that: (1) continuous treatment with phenobarbitone had no advantage over intermittent diazepam; (2) the administration of diazepam was not optimal, so that 80% of children received it too late; (3) diazepam is safe, quickly absorbed, free from undesirable side effects, and, unlike phenobarbitone, is subject to little parental resistance; (4) diazepam may be an alternative prophylaxis of FC, but further controlled investigations are needed.

We performed such an investigation in 1973-75 and checked the results in 1977 (follow-up 18-52 months).