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Gonadal function after testicular radiation for acute lymphoblastic leukaemia

Sir,

We were interested to read the recent paper by Leiper *et al* about the effect of direct testicular irradiation on Leydig cell function in boys treated for acute lymphoblastic leukaemia.[1] The questions they attempted to answer are important ones. We need to know if Leydig cell vulnerability to radiation damage is age related and if such damage is reversible with time.

In our own study, published early last year, the results showed that six of the seven boys irradiated during prepubertal life had an absent testosterone response to human chorionic gonadotrophin stimulation.[2] Two of the four boys irradiated during puberty had an appropriate basal testosterone concentration, but the testosterone response to human chorionic gonadotrophin stimulation was subnormal in three of the four. Thus we suspect that the Leydig cells of the pubertal testis may be less vulnerable to radiation damage than those of the prepubertal testis. We do not have sufficient data to indicate if age, as distinct from pubertal state, might influence the degree of testicular damage.

We found evidence that severe Leydig cell damage was present irrespective of whether the boys were studied within one year or between three and five years after irradiation, suggesting that recovery is unlikely. We note that in the seven boys studied sequentially by Leiper *et al*,[1] five showed no improvement in Leydig cell function, while two had subnormal peak testosterone concentrations of 4·6 and 6·8 nmol/l (1·33 and 1·96 ng/ml), respectively, after human chorionic gonadotrophin stimulation. The basal testosterone concentration may well have been adequate for pubertal development to occur in these boys, but unless there is a further improvement in Leydig cell function they will both probably require androgen replacement therapy once puberty is completed to allow normal libido and sexual performance and to avoid osteoporosis.

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Drs Leiper, Grant, and Chessells comment:

We welcome the comments of Shalet and Morris-Jones in response to our paper.[1] We agree that it is important to know if Leydig cell vulnerability to radiation is age related and if such damage is reversible with time.

In their paper Shalet *et al* documented severe Leydig cell damage after testicular irradiation. This was irrespective of whether subjects were studied within one year or between three and five years from the time of radiotherapy, suggesting that recovery of function is unlikely.[2] Our findings were similar to this in the majority of boys studied. In two of the seven cases studied sequentially using the human chorionic gonadotrophin test, however, at least partial recovery was clearly shown. The two subjects mentioned were studied two and three years after irradiation, when they were aged 10·9 years (Tanner Stage I) and 10·6 years (Tanner Stage II), respectively. The first boy had a basal testosterone concentration of 1·1 nmol/l (0·31 ng/ml) rising to 4·6 nmol/l after a three day human chorionic gonadotrophin test, while the second had a basal concentration of 0·8 nmol/l (0·23 ng/ml) rising to 6·8 nmol/l. The testosterone responses after human chorionic gonadotrophin stimulation in these two children only six months after irradiation were grossly inadequate.[3] Plasma testosterone rose from a basal concentration of 0·5 nmol/l (0·14 ng/ml) to a peak of 1·7 nmol/l (0·49 ng/ml) in the first boy and from 0·4 to 1·2 nmol/l (0·12 to 0·35 ng/ml) in the second. We acknowledge that there is a possibility that these children may need hormone supplementation in adult life for the reasons stated by Shalet and Morris-Jones in their letter, and vigilant follow up is mandatory.

With our present data on small numbers of subjects it is difficult to be sure that the degree of testicular damage sustained is age related rather than related to pubertal state at the time of radiotherapy. All our subjects were, however, prepubertal at the time of radiation. We are currently attempting to clarify this with further prospective studies.

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Prophylaxis of febrile convulsions: searching for the best

Sir,

As confirmed by Knudsen in *Archives*[1] and elsewhere,[2] the short term prophylaxis with diazepam seems sufficiently effective, feasible, and advantageous. His considerable effort[1] in codifying the risk of recurrence (age being the most predictive factor, on the basis of natural history, for the longer time span in which the central nervous system is evolving) must be considered as another step toward personalising the prophylaxis.

The temperature of 38·5°C, however, can be a near uncontrollable level of fever, adding an unnecessary extra

to the unrecognisable sudden onset fever (4% in his experience).[2] Hence the recurrence rate of Knudsen (mean 12%) is somewhat higher than possible and considerably higher than mine (4%).[3] I think that in everyday practice the signals for starting diazepam administration must be less dictated by a protocol and more personalised, so enabling the complete efficiency of this method. Those signals I have used since 1973 for oral prophylaxis (I reserve rectal administration for home treatment of overt seizures) are in my experience very useful. The symptoms can be as trivial as: sudden excessive thirst, listlessness, changes of mood, reduced scrotal turgidity, halitosis, yawning, feeling cold (without shivering), etc.[3] They are variably assembled in the single child, well known to his mother,[3] and are nothing more than a short preceding step in the way of discovering the stated fever.

Paediatricians must be grateful to Knudsen for showing once again that the distressing recurrences of simple febrile convulsions are preventable by intermittent treatment with diazepam and for re-stressing also the invaluable importance, to this purpose, of doctor-parents cooperation.[1-3]

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Dr Knudsen comments:

I am grateful to Dr Dianese for raising this important question. I agree that short term diazepam prophylaxis should be as personalised as possible, but it is equally important that the number of diazepam doses given to the child is kept as low as possible. In our clinic the parents are told to be aware of symptoms of illness in their children and take their temperature if necessary but not to let the thermometer rule the family. The parents are informed both verbally and in writing that recurrent febrile convulsions often take place very early in the feverish illness and that the first dose of diazepam is the most important one and should be given as early as possible, but only when the temperature is above 38·5°C. The method of prophylaxis proposed by Dr Dianese will—according to my data[1]—only result in a minor therapeutic gain (4%), as compared with the conventional one. After rectal administration in solution diazepam is absorbed very rapidly, and the anticonvulsant concentration is reached within 2–4 minutes. From a pharmacokinetic point of view oral administration may justify more early prophylaxis, because anticonvulsant plasma concentration of diazepam may not be reached so rapidly, especially when we are dealing with feverish children. To start prophylaxis on the vague, initial symptoms mentioned by Dr Dianese might result in overtreatment when administered by anxious mothers. I agree that a high degree of parental compliance is probably the most essential prerequisite for effective prophylaxis.

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Weight gain and height velocity during prolonged remission from acute lymphoblastic leukaemia

Sir,

We have read with great interest an article on weight gain and height velocity during prolonged first remission from acute lymphoblastic leukaemia.[1] The authors claim that none of the previous studies on growth after acute lymphoblastic leukaemia has reported data on weight gain. It might therefore be appropriate to point out that in our study of 27 children treated for acute lymphoblastic leukaemia[2] a similar phenomenon of excessive weight gain during the first year after stopping treatment was observed. This resulted in a higher than normal attained weight at four, five, and six years after the onset of disease; later on, however, weight became normal. Weight gain during the maintenance therapy observed by the Cardiff group[1] might be explained by the cumulative weight gain from recurrent steroid pulses. Our patients were treated without 'reinductions', and no such weight gain was noticed. The weight gain after stopping of treatment is more difficult to explain. In some of our patients it was so excessive that central nervous system leukaemia with hypothalamic obesity was suspected, but never proven.

In the analysed material[1] there was no difference in weight gain between those children who had received cranial irradiation and those who had not. It is therefore unlikely that a hypothalamic damage induced by radiation is responsible for the weight gain. We are, as the authors of the report, inclined to believe that psychological factors are likely to lead to weight gain. An additional factor may be the stopping of treatment with methotrexate, a drug that often causes malabsorption during the maintenance therapy.[3] When enteropathy is no longer present excessive consumption of high energy foods contributes to relatively greater weight gain.

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Drs Sainsbury, Newcombe, and Hughes comment:

We are grateful to Dr Moëll and his colleagues for drawing our attention to their paper on weight gain in 27 children treated for acute leukaemia.[2] The results from both studies show excessive weight gain after the stopping of treatment. We agree that stopping all cytotoxic treatment together with continued excessive food intake is the probable cause.