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REFERENCE


In Reply.—

Tasic and colleagues are correct in pointing out the potential hazards of zinc toxicity in treating infants with acrodermatitis due to low zinc in their mothers’ milk. Infants, especially premature infants, who have symptoms of acrodermatitis enteropathica while being breast-fed should be assumed to have normal capacity for uptake of zinc in the gut. If breast-feeding is continued during administration of zinc, the plasma zinc level can be expected to rise rapidly. This is due in part to the positive effect of human breast milk on zinc absorption as well as normal absorptive capacity.

Signs of zinc toxicity did not occur in either of our infants with acrodermatitis, although the serum level in infant 1 was 336 µg/100 ml (normal, 68 to 120 µg/100 ml) at the end of the first week of therapy with 75 mg of zinc sulfate per day (16.6 mg of Zn per kilogram of body weight). Infant 3 was given the same dose; however, the serum zinc level rose only to 164 µg/100 ml after three weeks of therapy.

Zinc toxicosis in humans is usually manifested by nausea, vomiting, fever, irritability, and drowsiness. The appearance of tremor and seizures in the patient described by Tasic et al occurred at a serum zinc level of only 34.4 µmoles/liter (225 µg/100 ml), a level at which one would not expect toxic signs. I would, therefore, question whether there might not have been other factors that precipitated seizures in their patient, such as prior neurologic damage, other nutrient deficiencies (eg, pyridoxine), infection, or acidosis. Perhaps the serum zinc level found by Tasic et al also did not fully reflect the magnitude of the “accidental zinc intoxication.”

Physicians are well-advised to use caution when treating zinc deficiency in infants and children until there is more knowledge of the side effects of acute and chronic therapy.

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1. American Academy of Pediatrics, policy statement based on
Eisenberg Defended

To the Editor.—

I have read Mittleman's letter1 commenting on the thoughtful article by Leon Eisenberg.2 I am surprised and dismayed that you were prepared to dignify this litany of invective by publishing it in your correspondence columns. Eisenberg's restrained reply was admirable—your decision to publish the letter was not.

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REFERENCES


Skull Roentgenograms; Soft Conclusions

To the Editor.—

We are sure that the title of the lead article of the February 1982 issue, "Mild Head Trauma in Children: When Is a Roentgenogram Necessary" (Pediatrics 69:139, 1982), caught the attention of many readers as it did ours. This article, according to the authors, examined "the incidence and clinical significance (italics ours) of skull fractures in an unselected population of children...". According to the abstract, the clinical criteria derived from the study are an attempt to "make efficient use of resources."

The question of the importance of a positive skull film after mild head trauma is raised several times by the authors. In their discussion, after having presented their data, they readily admit that "in general fractures are poor predictors of intracranial injury. Inevitably one is led to question the wisdom of trying to detect skull fractures in children after mild head trauma."

It was clearly not the intention of the authors to answer this question in their study. Yet if the authors themselves are unconvinced that detecting most skull fractures is important in clinical management several of their recommendations are of concern:

1. Why is a sensitivity of 100% chosen in setting criteria for skull films? Using a high sensitivity in a screening test implies that it is essential to detect true positives, and one accepts the inconvenience of identifying many false-positives in the process—a case which does not apply to skull films. It might have been more useful to set criteria to identify those fractures with implications for management—depressed and basilar skull fractures. The costs would be further decreased and the information gained would be more clinically useful.

2. In their proposed criteria for skull films, the authors mention several signs of possible basilar skull fracture. Yet, according to their own data, the skull film was a poor screen for this type of fracture; the diagnosis was made by complete tomography. Instead of listing clinical criteria for skull roentgenograms in order to achieve 100% sensitivity for all types of fractures, the authors could have emphasized the particular need for appropriate diagnostic methods to identify basilar skull fractures.

Perhaps if the article had been entitled "clinical correlates of positive skull films in mild head trauma," we would not have been disappointed in its concepts. Unfortunately, key words or phrases like "necessary," "efficient use of resources," and "clinical significance" imply far more clinically useful information than we were given.

In conclusion, we recognize that we cannot hold the authors responsible for a study that they did not do. We do feel that based on the data, more practical conclusions could have been derived.

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In Reply.—

We appreciate the thoughtful comments of Groner and Charney regarding the value of routine skull radiography in mild head trauma in children. They would be satisfied with less than perfect sensitivity in an algorithm designed to select children at high risk for fractures. If, as well may be the case, skull fractures are themselves poor predictors of clinical outcome, then one must raise an even more basic question—why try to detect skull fractures? Why use the presence of a fracture as a gold standard for performance? The answer may well lie in the nature of the physician's value: for the clinician (or perhaps for his malpractice carrier) there remains a marginal advantage to detecting such fractures.

The determination of the appropriate selection (or "cutoff") criterion for screening depends on five factors: (1) the prevalence of skull fractures in the population presenting for evaluation; (2) the sensitivity of the screening algorithm; (3) the specificity of the algorithm; (4) the benefit of detecting skull fractures; and (5) the cost or risk of radiography. Our study addressed only the first three factors. The last two factors are critical for making recommendations but depend on the value or utility system (ie, the patient's, the physician's, society's, etc) used to assess cost and benefit. Because we did not feel able to provide definitive answers to the latter two issues, we chose to couch our conclusions softly and conserva-
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The online version of this article, along with updated information and services, is located on the World Wide Web at:
/content/70/4/661.2