Hyperactivity and the Feingold Diet

To the Editor.—Mattes and Gittelman, in the June 1981 issue, concluded that administration of food dyes contained in cookies had no effect on hyperactive children (ARCHIVES 1981;38:714-718). The authors reviewed the literature and made value judgments on previous reports as to their significance and shortcomings.

Thirteen children were included in this study. Seven (two of whom were dropped from the study) had continued hyperactivity on the Feingold diet. Three had behavioral disorders or irritability and had never been known to be hyperactive. Only three of the patients had a history of hyperactivity that was completely relieved by the Feingold diet at the time of evaluation.

The authors admitted to several defects in the study. Four patients were too young to take the distractibility test, equipment failure occurred in two, failure to eat the full number of cookies occurred in three, and no teacher rating was included in one who was not in school.

It is unfortunate that studies with such defects and unfounded conclusions obtain national attention. The *American Medical News* of July 10, 1981, mentioned this study. It appears that the authors have no awareness of food allergy (or food sensitivity). The concepts of masking, sensitivity reduction with time, withdrawal symptoms, addiction, and neutralization were ignored although it would seem these factors undoubtedly played a major role in this study. When one considers these phenomena, the "order effect" and "dosage effect" that apparently baffled the authors can then be explained.

The Feingold diet can be criticized, but this study in no way proves its point. Valid criticisms of the diet should relate to it being too broadly encompassing. The "one size fits all" concept of food sensitivity may be a popular notion, but it is not true. Chemically sensitive persons (as most hyperactive children probably are) react to different agents in different ways. Hence, even in this poorly designed study, it can be seen that only three of ten hyperactive children responded completely to the restrictions in the Feingold diet. Another criticism is related to the first, that in many chemically sensitive persons many restricted foods (especially those containing salicylates) can be eaten with impunity, especially if eaten on a rotary diversified schedule.

The knowledge of the effects of food additives is becoming increasingly widespread. Probably the most valuable service the Feingold Association provides is its commercial food research, allowing sensitive persons to avoid and to include products according to their nonlabeled additives.

The US government currently allows approximately 5,000 different food additives and this figure has been growing exponentially. In contrast, France allows only seven food additives, artificial colors have been banned in Italian drugs, and Sweden has banned the notorious tartrazine.

It has been estimated that hyperactivity occurs in one in 20 to as high as one in three American children. The incidence in France is one in 2,000. Can this be coincidental?

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In Reply.—Dr Traxel clearly belongs to the relatively small group of physicians who consider food allergy to be a major factor in the cause of psychopathologic disorders. Much has been published by this group.[1] However, the phenomena Dr Traxel refers to as if they were proved ("masking, sensitivity reduction ..., addiction, and neutralization," the value of a rotary diversified eating schedule) have not been demonstrated scientifically, and are not accepted as valid by most allergists. In addition, these phenomena, even if they exist, do not easily explain the order and dosage effects found in some studies, as these effects were not found in most studies (including ours) and so were most likely chance findings.

Most of Dr Traxel's criticisms are discussed in our article. In general, it seems that Dr Traxel has missed the main point of our study. Regardless of what we judged the children to be like while on the Feingold diet, all parents stated emphatically that diet violations brought clear and dramatic behavioral worsening. Though some of the children were hyperactive while on the diet, there was much room for making them worse, which we thought the experimental condition would do (given the Feingold hypothesis). There is no reason to think that *only* children who are asymptomatic while on the diet would show *any* effect from artificial colorings.

The cross-cultural issues Dr Traxel raises are interesting but his reported prevalence of hyperactivity in France is not documented. As far as we know, there is no basis in fact for the estimate he mentions. Even if it were veridical, which is not clear, it would not follow that diet was the factor responsible for the differences between the two countries; genetic factors, perinatal care, or standards of discipline and behavior at home and school might account for cross-cultural differences in prevalence, if these were indeed found to exist. Correlation does not mean causation, and only experimental investigations can provide meaningful information on issues of cause.

Our study did have defects, as all studies do, but in our view these were minor compared with its strengths, eg, the selection procedures designed to maximize the likelihood of identifying a dietary effect, the elimination of placebo responders, the high dosage, and the detailed evaluation by teachers, parents, psychiatrists, and psychological testing. Our conclusions, we think, are justified, ie, that artificial colorings do not appear to affect the behavior of children, even in those whose parents claim that they are clearly affected by these agents. We, of course, do not consider it onfortunate that our study received national attention when all too often unsubstantiated theories are widely publicized as verified, experimentally tested truths.

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