Letters to the Editor

Correspondence pertaining to material published in the JOURNAL will be published, if found suitable, as space permits. Submit double-spaced copy clearly marked "For publication" and signed by all authors; references should conform to the JOURNAL format; maximum length; 500 words. Copyright assignment signed by all authors is required. The Editor reserves the right to edit such letters, which should be received within six weeks of publication of the JOURNAL article in question.

Neonatal Necrotizing Enterocolitis

Sir.—The article "Neonatal Necrotizing Enterocolitis in the Absence of Pneumatosis Intestinalis" by Kliegman and Fanaroff (JOURNAL 1982;136: 618-620) draws attention to the important observation that neonatal necrotizing enterocolitis (NEC) is not always associated with radiologic evidence of intramural intestinal gas (pneumatosis intestinalis). Nineteen of their 136 patients with confirmed NEC did not have pneumatosis intestinalis; five patients had radiologic evidence of ascites, a finding indicative of usually severe NEC in the proper clinical circumstances.[1] The authors do not state if the remainder of these patients had a normal radiologic appearance on abdominal films.

As Kliegman and Fanaroff point out, NEC without pneumatosis intestinalis (and without the other well-known signs of portal venous gas and pneumoperitoneum) has been reported before, including patients with histologic confirmation of NEC.[2,3] Such patients usually have what is known as a "disturbed bowel gas pattern" that is most often characterized by the presence of a number of parallel, elongated, dilated loops, often widely separated from each other.[1,2,4] This pattern replaces the usual appearance of many polygonal lucencies[5] seen in neonates with conditions other than NEC.

In their thoughtful "Comment" section, Kliegman and Fanaroff discuss the difficulty of establishing the diagnosis of NEC when there is no radiologic evidence of pneumatosis intestinalis and consider the potential value of other less specific diagnostic tests. A similar difficulty arises when minute bubbles are seen on abdominal films, but one is not certain that pneumatosis intestinalis, and not a mixture of gas and stool, is present. In such cases, the disturbed bowel gas pattern has been found to be very useful, although at times subtle, requiring some experience for recognition. It probably has less than perfect sensitivity and specificity, but very few diagnostic tests do.

I think the article by Kliegman and Fanaroff is important in emphasizing that pneumatosis intestinalis is not a sine qua non for the diagnosis of NEC. I believe, however, that most of the patients without pneumatosis intestinalis initially have disturbing radiologic findings, which should be looked for and which may be helpful in conjunction with the clinical appearance.

JOHN C. LEONIDAS, MD
Tufts-New England
Medical Center
Boston, MA 02111

1. Daneman A, Woodward S, deSilva M: The radiology of neonatal necrotizing enterocolitis: A review of 47 cases and the literature. *Pediatr Radiol* 1978;7:70-77.

2. Rabinowitz JG, Siegle RL: Changing clinical and roentgenographic patterns of necrotizing enterocolitis. *AJR* 1976;126:560-566.

3. Leonidas JC, Hall RT, Amoury RA: Critical evaluation of the roentgen signs of neonatal necrotizing enterocolitis. *Ann Radiol* 1976;19: 123-132.

4. Reid WD, Shannon MP: Necrotizing enterocolitis: A medical approach to treatment. *Can Med Assoc J* 1973;108:573-576.

5. Cipel L: *Radiology of the Acute Abdomen in the Newborn.* New York, Grune & Stratton Inc, 1978, pp 3-23.

Sexual Child Abuse

Sir.—We were delighted to see articles concerning the important issue of sexual child abuse in your February 1982 issue. Although both articles are generally consistent with current knowledge, we would like to present some concerns about the method or content in each.

The article "Sexual Abuse of Children: Sex-, Race-, and Age-Dependent Variations" by De Jong et al (JOURNAL 1982;136:129-134) emphasized several important points from earlier studies, including the fact that boys, as well as girls, may be sexually abused and that the symptoms of abuse may be nonspecific physical, psychosocial, or behavioral complaints. However, we find three problems with the study. First, the nature of the study population is unclear. The authors state that their study population consisted of 416 children who were examined for complaints of alleged sexual assault. They never state how many of these complaints were confirmed. Reporting bias is known to be an important problem in evaluating child abuse data.[1,2] Thus, although this type of protocol is not unusual, we suggest that this study and others with similar case selection procedures could be improved if cases are limited to those with confirmed or substantiated abuse. Second, several of the variables evaluated in the study were recorded in less than 70% of the cases. When the proportion of unknowns for any variable becomes too great, it is no longer possible to extrapolate to the whole population. Third, for virtually every variable, only percent distributions are given. The meaning of percent distributions depends on the characteristics of the community from which the cases were drawn. Thus, population-based, rate-specific data would be necessary to evaluate these results fully.

The article "Sexual Abuse of Children: Current Concepts" by Dr Jones (JOURNAL 1982;136:142-146) presents other difficulties. Dr Jones combines three separate studies concerning gonococcal disease in children and/or adolescents. These studies differ in a number of important respects, including the age group studied, the demographic setting, the time period of investigation, and the technique used to determine whether the patients were sexually abused. Although the combining of studies is a common technique for investigating problems for which limited data are available, doing this without noting potential biases is misleading. We are more concerned, however, with the author's statement, "The child may be given gonorrhea prophylaxis and diethylstilbestrol, or other medication . . . for prevention of pregnancy. . . ." The Centers for Disease Control are currently formulating recommendations for treatment of children suspected of being sexually abused. Until this process is completed, we suggest that recommenda-

tions for prophylaxis in adults be followed for sexually abused children. Gonorrhea prophylaxis should be given only if the abuser is known to have a gonococcal infection.[3] Follow-up care and counseling are essential for a sexually abused child, and the examining physician should treat the patient at follow-up if Gram's stain or culture results indicate infection. Finally, most sexual assaults of children do not involve intercourse. Thus, the need for pregnancy prophylaxis should be evaluated on an individual basis, considering the age of the child and the nature of the assault.

JANINE JASON, MD
Office of the Director
Center for Health Promotion and Education
DORINE KRAMER, MD
Center for Prevention Services
Centers for Disease Control
Atlanta, GA 30333

1. Garbarino J, Crouter A: A note on the problem of construct validity in assessing the usefulness of child maltreatment data. *Am J Public Health* 1978;68:598-600.
2. Light RJ: Abused and neglected children in America: A study of alternative policies. *Harvard Educ Rev* 1973;43:556-598.
3. *Gonorrhea: Centers for Disease Control–Recommended Treatment Schedules, 1979*, US Dept of Health and Human Services publication 97-796. Atlanta, Centers for Disease Control, 1979.

In Reply.—I appreciate the thoughtful comments of Drs Jason and Kramer. I will try to respond to their concerns.

The respondents are correct in that the three studies of sexual abuse in children with gonococcal disease are somewhat disparate. My purpose in combining the studies was simply to emphasize that the physician should strongly suspect sexual activity in a child who has gonorrhea.

The question of whether to provide gonorrhea prophylaxis after rape is more difficult. The reported incidence in the United States of gonorrhea in children who have been sexually abused varies from 4.7% to 7.4%.[1,2] The infrequent occurrence of gonorrhea in these children must be weighed against the potential psychologic disaster for the infected child and his or her parents. A satisfactory method of rapid identification of the presence of gonococci, currently under investigation, would alleviate this dilemma.

Finally, I agree completely with the statement that "the need for pregnancy prophylaxis should be evaluated on an individual basis, considering the age of the child and the nature of the assault." No case of childhood sexual abuse should ever be considered routine. The evaluation and treatment must be individualized.

JERRY G. JONES, MD
Department of Pediatrics
University of Arkansas College of Medicine
4301 W Markham St
Little Rock, AR 72201

1. Orr DP, Prietto SV: Emergency medical management of sexually abused children: The role of the pediatric resident. *Am J Dis Child* 1979;133:628-631.
2. Rimsza ME, Niggemann EH: Medical evaluation of sexually abused children: A review of 311 cases. *Pediatrics* 1982;69:8-14.

In Reply.—We wish to thank Drs Jason and Kramer for their letter and appreciate the opportunity to respond to their general comments, as well as their specific concerns, about our study.

Most childhood sexual abuse incidents are not reported,[1] and most studies of sexual abuse reflect the nature of the population served by individual sexual assault centers.[2] As a referral center, more than half of our patients were referred by the police, private physicians, local emergency rooms, social service agencies, or counseling services. We acknowledged the risks of reporting bias and comparison with dissimilar populations within the discussion of our findings.

The first specific concern is how many of the "alleged sexual assaults" were actually sexual assaults. Although it is often difficult to substantiate medically and legally prove complaints, we believe that the history given by the child and/or the physical findings indicated that more than 95% of the cases was inappropriate sexual stimulation of some form. The majority of the "probably unsubstantiated" cases were consentual acts between teenagers or the results of maternal projection of sexual concerns. The significant proportion of patients who are referred by outside agencies and a brief nursing triage help limit the number of unsubstantiated cases.

The second concern focuses on the effect that unknown or unrecorded individual case data can have on our conclusions. We recognized that when the proportion of unrecorded (unknown) information for any variable becomes too great, extrapolation may be inaccurate. Four variables evaluated in the study were recorded in less than 90% of the charts. These features deserved some mention; however, our analysis stressed those 15 variables that were recorded on 90% to 100% of the charts.

The third concern questions the use of percent distributions to express variables. Rate-specific data would enhance only certain data, while making the article more tedious. Percent distributions were used in the text to simplify the presentation of the findings.

We strongly agree with the recommendation to individualize the approach to gonorrhea and pregnancy prophylaxis. Our low frequency of positive gonorrhea cultures (3.1%) suggests a low frequency of gonorrhea infection among sexually abused children, which is consistent with other reports.[3] Routine gonorrhea prophylaxis would not seem to be indicated.

ALLAN R. DE JONG, MD
GARY A. EMMETT, MD
ARTURO R. HERVADA, MD
Department of Pediatrics
Thomas Jefferson University
Philadelphia, PA 19107

1. Kempe CH: Sexual abuse: Another hidden pediatric problem. *Pediatrics* 1978;62:382-389.
2. Greenberg NH: The epidemiology of childhood sexual abuse. *Pediatr Ann* 1979;8:289-299.
3. Tilelli JA, Turek D, Jaffe A: Sexual abuse of children: Clinical findings and implications for management. *N Engl J Med* 1980;302:319-323.

Poland's Syndrome and Leukemia

Sir.—Because the letter "No Link Between Poland Syndrome and Leukemia?" by Gilman and Miller in the February issue of the JOURNAL (1982;136:176) ended with the sentence, "The absence of comparable case reports from outside the United States is also a puzzle," we would like to mention that such a combination was observed in a 6-year-old girl in Turkey in 1977. The absence of a right pectoralis muscle and nipple with brachydactyly and a small left hand were the present abnormalities (Figure). Roentgenograms did not disclose any vertebral abnormalities or other skeletal deformities.

During a six-year period from October 1975 to October 1981, the conditions of 370 children with acute lymphocytic leukemia were diagnosed at Hacettepe Children's Hospital, Ankara, Turkey. The age range of the children was 2 months to 17 years. The previously mentioned 6-year-old girl