Letters to the Editor

A Small Outbreak of Spontaneous Abortion in Four Patients From Two Households

To the Editor:

Two sisters, Patient K (age 36) and Patient S (age 38) lived in the same household and shared all foods prepared in their home. Patient K was pregnant, and her sister, Patient S, became pregnant two months later. Patient K suffered a sudden, spontaneous abortion; two months later, Patient S also suffered a spontaneous abortion.

Two sisters-in-law, Patient L (age 33) and Patient Z (age 27), resided in the same household and shared all meals. About two months after Patient Z became pregnant, Patient L also became pregnant. Patient Z's pregnancy ended in spontaneous abortion, and two months later, Patient L also suffered a spontaneous abortion.

All four patients had positive indirect hemagglutination (IHA) tests for toxoplasmosis.

Over a five-year period, 139 pregnant women visiting the Mother and Child Care Center in Risafa, Iraq, tested IHA-positive for toxoplasmosis infection; however, only four women (two pairs of women, each pair sharing households) had spontaneous abortions. Women with serological evidence of toxoplasmosis infection prior to pregnancy do not infect their neonate; the fetus may be at risk only when primary infection is acquired during pregnancy.' Toxoplasmosis is transmitted to the fetus in utero during a pregnancy only as a result of parasitemia.[1,2]

The modes of transmission of toxoplasmosis in our culture, and especially in our four patients, have not been definitively established. Cooked meat cannot be considered a vehicle for infection in Iraq. However, direct contamination by flies[3] and cockroaches[4] serving as the mechanical carriers for the oecyst from cat feces is possible. There is a close association between the prevalence of toxoplasmosis in humans and the presence of toxoplasmosis in domestic cats.[5,6]

Toxoplasmosis infection is a preventable disease, and, even when primary infection occurs during pregnancy, early diagnosis and treatment can reduce the frequency and severity of disease in the neonate.[7]

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Parenteral Versus Oral Antimicrobials

To the Editor:

I read with great interest the recent report by Ehrenkranz et al.[1] However, I had several questions. The study was undertaken to promote a change from parenteral to oral antimicrobials with equivalent therapeutic actions. However, intervention also was regarded as successful if antimicrobial treatment was discontinued; this is not consistent with the study question. The rate of treatments that were discontinued rather than changed to an oral regimen should be reported. No sample size determinations were reported in this clinical trial. Because similar outcomes of mortality and secondary infections were observed, the power of the study should be stated to allow the reader to assess the probability of Type II error.[2]

Student's *t* tests and Fisher's exact tests were used to analyze the data. In Table 2, multiple com-

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parisons were performed, but the authors do not mention whether there was use of a method for adjustment for multiple comparisons.[3] Interestingly, days of corticosteroid use were reportedly longer in the compliant group compared with the noncompliant group. However, the different standard deviations suggest that the variances of the two groups may be significantly different. What method was used for adjustment of unequal variances, if it was necessary?

A higher proportion of patients in the noncompliant group had therapy for resistant pathogens, a marker for the severity of pneumonia. Therefore, one cannot conclude that patients in the noncompliant group were comparable with the compliant (or control) group. The small sample size precludes meaningful statistical analysis, revealing the power to detect a difference of approximately 33% in this particular example.[4] Furthermore, a new-site pulmonary infection was observed in patients of three compliant physicians compared with one in the control group; two deaths occurred in patients of the compliant group compared with none in the noncompliant group. This suggests that patients of noncompliant physicians may have a better outcome than patients of compliant physicians who discontinued parenteral therapy when contacted. Although there were no statistically significant differences observed in these patients, the study power was inadequate to evaluate these important outcomes.

Patients of noncompliant physicians had a longer length of hospital stay, a higher rate of phlebitis, and higher laboratory costs than the control group. Perhaps noncompliant physicians may differ inherently from control physicians, independent of the intervention. The results from the pilot study may have given further information that differentiated noncompliant physicians from others. The followup period of one month appears too brief to adequately detect late manifestations of infections at other sites, such as osteomyelitis and endocarditis.

In conclusion, the study addresses an important question and may be background for a larger study where the randomization process is not influenced by physician's compliance. Such a study has recently been published by Sanders et al.[5] The small sample size, the lack of control for potential confounders[6] in the analysis, and the duration of the follow-up limit the validity and the generalizability of the results.

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The author replies.

I thank Dr Widmer for his interest in our study. I fear, however, that he has misread the paper in a number of important aspects.

In answer to his questions, the study was undertaken solely to promote discontinuation of parenteral therapy (as indicated in the title). The stated endpoint defining physician compliance was cessation of parenteral antimicrobial treatment within 48 hours of unsolicited intervention, without regard to initiation of oral antimicrobials. All but one compliant physician, however, did discontinue parenteral treatment in favor of oral treatment.

Issues of sample size and study power were addressed in the discussion.

The multiple univariate comparisons set forth in Table 2 are for group comparisons of as many patient characteristics as can be identified. This was done to evaluate comparability of study populations and is essential to assess the outcome of the randomization process. A secondary question was whether pertinent differences existed between patients of compliant or noncompliant physicians. In this context, adjustments for multiple comparisons are meaningless; they would not increase the number of differences between various groups beyond the one already noted, nor would adjustment for unequal variances change the findings to make for longer treatment with corticosteroids in other patient sets.

I cannot fathom any biological relation between a physician's choice of oral or parenteral antimicrobial therapy for patient management during hospitalization, and posthospitalization occurence in the same patient of a new aspiration pneumonia or new lobar pneumonia at a new anatomic site, or death unrelated to the original pul-

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