We do not believe that disease is caused by one streptococcal superantigen, and it is quite possible that the 2 cases analyzed by Michie represent true expansion of Vβ2 T cells; however, it would be helpful to increase the number of cases analyzed and to determine the changes in the entire Vβ repertoire of these patients. It should be emphasized that the majority of studies that have investigated the in vivo effects of superantigens have documented the deletion of superantigen-specific Vβ bearing cells following a brief window of Vβ expansion [3–6]. In some cases, expansion was not detected and only Vβ depletion was seen [6]. I believe that this is related to the type of superantigen and the magnitude of the inflammatory cytokine response elicited in the host during the infection. Thus, it should not be surprising to observe Vβ depletion in STSS, which is consistent with the severe inflammatory response in these patients. However, as we indicated in [2], we cannot rule out that earlier expansion of these Vβ-bearing T cells may have taken place.

As to the method used to analyze the T cell receptor (TCR) repertoire, we agree that this method is quite difficult and if not done properly can generate misleading results. However, we have used this method for >5 years and optimized the conditions to the point that the technique is quite reliable and reproducible [7]. We [8, 9] and others [10] have compared the results with those obtained by flow cytometry and found very good correlation. Furthermore, because the number of anti-Vβ antibodies is limited, it is not possible to analyze the entire repertoire by flow cytometry and consequently it is not possible to determine changes in other Vβ elements. We agree with Michie that multiple methods should be used to analyze the TCR repertoire; however, because of the limited amount of blood that can be obtained from patients, this option is not always available. In addition, we fully agree that analysis of the repertoire in CD4 versus CD8 cells and in activated versus naive T cells is important, and we are developing methods to allow us to address this point in STSS patients.

Finally, although Michie reported the repertoire of only 2 STSS cases, his finding of a possible Vβ2 expansion and our finding of Vβ1, Vβ5.1, and Vβ12 depletion underscores the importance of analyzing the TCR repertoire of STSS patients in the context of the superantigen profile of their respective isolates. Such studies are currently underway in our laboratory and should help us better understand the mechanism of pathogenesis of severe streptococcal infections.

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Distinction between Parasitologic and Clinical Efficacy of Antimalarial Agents

To the Editor—Weiss et al. [1] compared the effect of different prophylactic regimens against *Plasmodium falciparum* malaria in 9- to 14-year-old children in Saradidi in western Kenya. They considered two outcome measures, the cumulative incidence of parasitemia and the cumulative incidence of "symptomatic falciparum malaria."

A diagnosis of symptomatic falciparum malaria in that study required the presence of malaria parasites in a thick smear and at least one of the following: history of fever or chills, axillary temperature >37.5°C, headache, musculoskeletal aches, or abdominal pains.

The authors acknowledge that ~20% of all blood smears were positive for *P. falciparum,* and hence that other illnesses would occur along with an incidentally positive smear leading to an overdiagnosis of malaria. In fact, it is well known that in children of the age range studied, clinical malaria attacks in areas of high endemicity are infrequent [2]. It is possible to estimate the proportion of clinical episodes attributable to malaria by considering the excess risk among the infected persons. When we did an analysis with data from areas of malaria endemicity comparable to Saradidi, we found that only a small proportion of the clinical episodes were attributable to malaria in children >10 years old [3, 4].

It seems likely that the vast majority of the clinical episodes reported by Weiss et al. are not in fact of malarial etiology. The authors are to be congratulated on their well-designed study and important contribution to the study of malarial prophylaxis in East Africa; however, it is doubtful that their analyses give a valid comparison of the clinical efficacy of the different regimens. It

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