despite "multiple standard courses of antibiotics." A more appropriate question is whether respondents would retreat a patient who failed the 10- to 28-day course of antibiotics endorsed by IDSA, as previous studies have shown.[14,15] Question-framing bias is known to influence responses negatively and to preclude the drawing of valid conclusions.[2,3] In summary, the extensive bias in this survey renders the results uninterpretable and invalid.

R.S. serves, without compensation, on the medical advisory panel for QMedRx, Inc. L.J. declares no conflicts of interest.

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Reply

To the Editor:

We thank Stricker and Johnson for their careful reading of our study. They state that "Lyme-literate" physicians—that is, those physicians who diagnose and treat patients with chronic Lyme disease—would be reluctant to respond to our survey for fear of identification and persecution by the Connecticut Medical Licensing Board. We clearly stated in the introduction of our survey that responders and nonresponders could not be specifically identified, and that their survey answers would be anonymous. The study had the approval of the University of Connecticut Health Center's Institutional Review Board. Also, there was no second mailing, because responders and nonresponders could not be identified. It is reasonable to believe that any Lyme-literate physicians who received this survey would have been happy to voice their points of view and be counted.

When Stricker and Johnson state that Lyme-literate physicians fear prosecution by the Connecticut Medical Licensing Board, they are referring to a case of a Connecticut physician who diagnosed two siblings from Nevada (a non–Lyme-endemic state) with chronic Lyme disease over the telephone. Without seeing or examining these children, this physician prescribed antibiotics and made them home-bound because they were too ill to attend school. It was the children's father (who did not have custody of the children) who reported this unorthodox medical practice of diagnosing and treating patients with chronic Lyme disease over the phone to the Connecticut Medical Licensing Board. In Connecticut, Lyme-literate physicians are not being bullied, persecuted, or prosecuted by non–Lyme-literate physicians. The risk of interacting with disgruntled patients or families exists for Lyme-literate and non–Lyme-literate physicians alike.

We totally disagree that patients with more complicated Lyme disease are cared for by Lyme-literate physicians. Stricker and Johnson's references 8 and 9 do not support this statement and can be classified as testimonials. Patients with complicated Lyme disease (eg, those with lymphocytic meningitis or third-degree heart block) frequently require initial hospitalization. These patients are often seen in the hospital by a non–Lyme-literate physician like myself, and after hospitalization are followed in the outpatient setting by the same non–Lyme-literate physician. In Connecticut, Lyme-literate physicians are mostly outpatient-based. They diagnose patients with chronic Lyme disease in the outpatient setting and may order peripherally inserted central catheter lines to administer intravenous antibiotics in the outpatient setting. It is not unusual for patients with chronic Lyme disease to be hospitalized with complications (not of the presumed chronic Lyme disease) from the prolonged antibiotic therapy or related to a peripherally inserted central catheter line, such as neutropenia, pseudocholelithiasis, cholecystitis, *Clostridium difficile* colitis, thrombophlebitis, and catheter-related bloodstream infections.[1-5] If Stricker and Johnson are referring to patients with complicated Lyme disease as those who have complications of prolonged oral and intravenous therapy, then we concede that Lyme-literate physicians are more experienced in this area.

Stricker and Johnson state that Lyme-literate physicians are specialists and that they would not have been included in our survey. But there is no specialty board of Lyme literacy, and thus most Lyme-literate physicians have evolved from the ranks of family physicians, internists, and pediatricians. Stricker is an internist and hematologist, but he would have been eligible for our survey as an internist.

Stricker and Johnson state that one outlier provider who diagnosed and treated patients with chronic Lyme disease was eliminated, and that this elimination changed our results. Outliers were defined as physicians who exceeded 3 standard deviations from the mean with respect to the number of patients with Lyme disease or chronic Lyme disease that they treated. There was one outlier among the physicians who diagnosed and treated chronic Lyme disease, there were two outliers among the physicians who were undecided with respect to the existence of chronic Lyme disease, and there were two outliers among the physicians who did not believe that chronic Lyme disease exists. All data were presented with and without outliers. The outliers did not affect our conclusions.

Lyme-literate physicians argue that a large percentage of primary care physicians in the field commonly diagnose and treat patients with chronic Lyme disease.[6] Our study's aim was to survey primary care physicians. We found that 2.1% of responding primary care physicians (outliers included) diagnose and treat patients with chronic Lyme disease, and that none (outlier included) prescribe intravenous or oral antibiotic courses for longer than 1 year. We believe our study is accurate and reproducible. We have read the many "letters to the editor" written by Stricker, Johnson, and colleagues, and challenge them to do the research to confirm their many testimonials.

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Genetics of the coenzyme Q10 pathway and rosuvastatin-induced muscle effects

To the Editor:

We have read with great interest the report by Avis et al evaluating the impact of rosuvastatin treatment on coenzyme Q10 (CoQ10) status in children with heterozygous familial hypercholesterolemia (FH).[1] Although relatively uncommon, statin-induced muscular side effects are the most common cause of withdrawal, which is sometimes not clinically justified in terms of ratio between protective and side effects in patients at high cardiovascular risk.[2] Furthermore, inappropriate discontinuation of statins could lead to increased vascular events.[2]

In our opinion, the demonstration of CoQ10 reduction without affecting mitochondrial adenosine triphosphate (ATP) synthesis in children with FH is an interesting finding, in the presented model of tolerant subjects, only in the sense that this mechanism is not sufficient to induce muscular intolerance. However, such an evidence reinforces previous contradictory data that CoQ10 supplementation did not improve muscle tolerability to statin treatment.[3,4]

Nevertheless, the CoQ10 pathway has been reported to be involved in muscular side effects induced by statins,[5] but specific pharmacogenetic studies confirmed a putative genetic influence in this setting with specific differences among statins and suggested a prominent role for SCLO1B1 mutations.[6-8] Recently we have extended such observations, demonstrating a rosuvastatin-induced muscle intolerance in association with a specific polymorphism within the COQ2 gene (rs4693075) and relative CoQ10 metabolism.[9]

In our opinion, the lack of data from intolerant subjects in terms of CoQ10 levels and ATP synthesis present a limitation to the results of Avis et al. A genetic study should be performed to assess the response to each statin in terms of CoQ10 involvement in putative muscle intolerance in the setting of children with FH. Further studies are necessary on the basis of the present body of evidence, as suggested by Avis et al in their conclusions.

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