(according to the authors' assumptions) render the latter superfluous. Figures 1 and 2 [1] show that a CSF leukocyte count cutoff somewhere between 100 and 1000 provided a clean separation of nonbacterial meningitis from proven and presumed bacterial meningitis, as would be expected given the definitions used. Not addressed by the study was the more realistic clinical quandary: which postoperative neurosurgical patients who have a neutrophilic CSF pleocytosis but negative CSF cultures truly need antimicrobial therapy. It would be informative to know the outcomes for the patients from the present study who had "presumed bacterial meningitis" but did not receive antimicrobial therapy.

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Reply

Sir—We appreciate the comments of J. R. Johnson regarding our study [1], which demonstrated higher predictive values for CSF lactate (cutoff, 4 mmol/L) than CSF : blood glucose ratio (cutoff, 0.4 mmol/L) for the diagnosis of bacterial meningitis in neurosurgical patients. We agree that it would be clinically important to study the outcome of patients with "presumed bacterial meningitis" who did not receive antimicrobial therapy. However, at the time that we performed our study, all patients who qualified for our category "presumed bacterial meningitis" received antimicrobial therapy. We planned and performed our study because we had some doubt that leukocyte counts in the CSF are reliable as a single test for the diagnosis of bacterial meningitis in neurosurgical patients.

Neutrophilic CSF pleocytosis as a consequence of subarachnoid-space inflammation is found in various infectious and noninfectious forms of meningitis. The gold standard for the diagnosis of bacterial meningitis is the documentation of bacteria in CSF by use of gram stain or culture. Both CSF leukocyte count and documentation of microbial pathogens are inconclusive in patients who are receiving steroids or antimicrobial therapy. In spontaneously occurring meningitis, CSF lactate and CSF : blood glucose ratio have been found to discriminate bacterial from nonbacterial causes of meningitis [2–7]. Neurosurgery involving the posterior fossa can result in aseptic meningitis ("posterior fossa syndrome"). Signs of meningeal irritation appear rapidly, and although CSF analysis shows polymorphonuclear pleocytosis with elevated protein and low glucose mimicking bacterial meningitis, cultures remain negative. The obvious clinical dilemma is whether to treat these patients with antimicrobial therapy or withhold it.

Standard CSF studies (i.e., gram stain, leukocyte counts, or glucose and protein concentration) have proven unreliable for the diagnosis of bacterial meningitis after neurosurgery [8]. Therefore, in addition to standard CSF analysis, CSF : blood glucose ratio and CSF lactate levels are used in some centers to help differentiate postoperative bacterial meningitis from aseptic meningitis. In a retrospective study, we assessed which of the 2 ancillary tests, CSF lactate or CSF : blood glucose ratio, yielded better predictive values for the diagnosis of bacterial meningitis after neurosurgery. As correctly pointed out by J. R. Johnson, CSF leukocyte count accurately discriminated nonbacterial meningitis from presumed and proven bacterial meningitis, as shown in figures 1 and 2. However, this observation does not render CSF lactate or CSF : blood glucose ratio determination redundant, as the leukocyte count was used to categorize patients into the predefined groups. Our study showed that CSF lactate is superior to CSF : blood glucose ratio for the diagnosis of post-neurosurgical bacterial meningitis. This finding might prove helpful for designing a prospective study, as suggested by J. R. Johnson, that addresses the question: which patients need antimicrobial therapy?

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Impact of Infectious Diseases Specialists

SIR—The recent report of a study regarding the appropriateness of antimicrobial therapy for bacteremia [1] and the accompanying editorial [2] were disappointingly silent regarding the study's paradoxical and disturbing finding that although infectious diseases specialists' involvement was associated with administration of more appropriate therapy, and appropriate therapy was associated with improved survival, involvement of infectious diseases specialists was not significantly associated with improved survival. The authors' speculations as to the reasons for this lack of association would be welcome. Until improved clinical outcomes from infectious diseases specialists' involvement can be demonstrated, skeptics will remain unconvinced that such specialists should routinely participate in the management of treatment for patients with bacteremia (or other infectious disease syndromes), regardless of improvements in abstract process outcomes such as "appropriateness of therapy."

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Reply

SIR—We read with great interest the comments of J. R. Johnson, but we do not share his skepticism about the evaluation of the benefit of the advice of infectious diseases specialists in the management of bacteremic patients. Indeed, several factors may explain why the involvement of an infectious disease specialist is not significantly associated with improved survival among bacteremic patients, despite an increase in the appropriateness of therapy. First, we think, as do others, that physicians solicit the assistance of infectious disease specialists more often in complicated cases, for which the prognosis is probably worse [1]. In our series, 55% of patients about whom an infectious disease specialist was consulted had already been treated by antibiotics for a previous infection, compared with only 38% of other patients ($P < .01$).

Second, the relative risk of death related to infection is reduced by ~30% for patients who are initially treated by infectious disease specialists (group 1), compared with the risk for patients who are initially treated by other physicians (group 2). This improvement could probably have achieved a significant threshold if more patients had been included, or if the treatment of patients initially treated by other physicians with a poor rate of appropriateness (group 2) had not been altered in subsequent days by infectious disease specialists, which resulted in a higher rate of appropriateness.

Indeed, of the 428 patients (groups 1 and 2), only 58 (14%) were not followed by an infectious disease specialist between the time of blood culture and the day on which the results of susceptibility testing of the germ were available. This shift of patients from group 2 to group 1 has 2 consequences. First, the real impact of the infectious disease specialist on outcome can be analyzed only for the empirical period, before the shift. Second, from gram staining results, it can be seen that the influence of the infectious disease specialist from day to day resulted in more and more patient-treatments with greater appropriateness than for other physicians at each step. As a consequence, when the laboratory results are available, the resulting appropriateness of the treatment for nearly all the patients is influenced by the advice of infectious disease specialists. This also explains the ~100% appropriateness of treatment when susceptibility data became available in this series, in contrast with the findings in other series cited in the discussion of our article.

Moreover, the role of the infectious disease specialist in the correct use of antimicrobial drugs is certainly of importance, for both epidemiological and economic reasons. Although it was not the primary end point of our study, we observed a significant reduction in the use of broad-spectrum drugs by infectious disease specialists compared with that by other physicians.

For all these reasons, we think that our article, such as another recently published in this journal [2], will help convince our colleagues that patient-specific management advice from infectious disease specialists will improve both the quality of medical care and the outcome for patients.