tives. Finally, given that substantial variations subsist at the level of individual clinicians,[3] focusing on performance at this tier rather than at the hospital level may be better suited to foster improved outcomes.[4]

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No potential conflict of interest relevant to this letter was reported.

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THE AUTHORS REPLY: Bray expresses concern that changes in coding for pneumonia in hospital records may be the reason for our findings. There was no incentive for providers participating in the program to increase their coding of admissions for pneumonia, which increased by similar percentages in the two areas over the course of the study: 28.9% in the northwest region and 29.7% in the rest of England. Changes of this nature in the volume of patients could influence the mortality and may not be accounted for by the case-mix adjustment, so the difference-in-differences design that we used was necessary. In addition (as described in the Supplementary Appendix, available with the full text of our article at NEJM.org), we estimated our models again with the volume of patients admitted to each hospital in each quarter as a potential influence on the risk-adjusted mortality. Patient volume was not an important determinant of the mortality among patients with pneumonia, and the estimated results of the program were unaffected.

Maruthappu et al. emphasize that the reductions in mortality that we observed were larger than would be expected from the improved performance on the process quality measures. We emphasized in our article that providers adopted a wide range of quality-improvement initiatives in response to the program and that the reductions in mortality could not be attributed to the improvements in the measures alone. Moreover, we agree that our findings reflect the NHS context; this point was reinforced in the accompanying editorial by Epstein.[1]

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Index Case for the Fungal Meningitis Outbreak, United States

TO THE EDITOR: In their report on fungal meningitis, Pettit et al. (Nov. 29 issue)[1] describe an "immunocompetent man" who received the "latest in a series" of epidural injections of methylprednisolone. The description of the patient as immunocompetent is not completely accurate. The systemic effect of localized glucocorticoids (e.g., epidural, intrathecal, intraarticular, and inhaled) is underappreciated.[2] Cushingoid changes,[3] reduced bone-mineral density, and elevated blood glucose levels[4] have been reported after the administration of epidural glucocorticoids. Methylprednisolone plasma levels peak 3 to 6 hours after intrathecal administration in dogs and persist for up to 2 weeks.[5]

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No potential conflict of interest relevant to this letter was reported.

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TO THE EDITOR: The summary of the index case of fungal meningitis, the subsequent "Preliminary Report"[1] on the outbreak, and the feature "Updates for Clinicians" on NEJM.org do not address the question of whether these injections were appropriate for low back pain. In fact, epidural injections appear to be only moderately effective for short-term (not long-term) symptom relief in patients with radiculopathy caused by disk herniation,[2] and ineffective for other forms of low back pain.[2,3] However, the use of epidural injections for low back pain is common and rising in the United States.[4] Physicians, patients, payers, and regulators should question the role that the overuse of injections for low back pain has played in this outbreak.[4]

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THE AUTHORS REPLY: We agree with Lauter and Sims that the potential immune modulating effects of epidural glucocorticoid injections are not well described. Data from reports on hematopoietic stem-cell transplantation have shown that the prolonged use (>2 to 3 weeks) of high-dose (>1 mg per kilogram per day) systemic glucocorticoids is a risk factor for invasive aspergillosis.[1] However, epidural glucocorticoid use has not previously been reported as a risk factor for invasive fungal infections.

The effectiveness of and appropriate indications for epidural glucocorticoid injections are controversial. An additional review[2] reported conflicting results compared with those referenced by Gérvas and Kovacs. This may be due, in part, to heterogeneity of studies with regard to many variables including the causes of back pain, diagnostic criteria for ascertainment of cause, duration of symptoms, target tissue for the intervention, anatomic approach to intervention, pharmacologic agent choice and dosage, cointerventions, comparator treatments, and outcome measures. In addition, the methodologic quality of studies included as well as the methods of the review itself may affect conclusions. Additional data regarding the efficacy of epidural glucocorticoid injections from large, well-designed, placebo-controlled randomized trials are needed, given the frequent and rising practice of this procedure in the United States.

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Since publication of their article, the authors report no further potential conflict of interest.

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