Response to Letter to the Editor on “It’s Never Too Late: Neurological Outcome of Delayed Decompression in Tuberculosis of Spine”

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To the Editor,

At the outset, we would like to thank the authors of the letter for taking interest in our research. However, we would like to clarify and answer the points raised.

As mentioned in our article, the indications for surgery were taken as progressive neurological deterioration, failure to respond to chemotherapy, bowel/bladder involvement, spinal deformity, and instability.1 Kindly note that those patients who were improving on the antituberculosis therapy (ATT) were neither operated upon nor were they part of this study.

Authors in their letter have raised the concern about the timeframe for referring the surgical management as delayed. We would like to clarify that our patients in the study group were referred from peripheral centers and were already on medical management, that is, ATT. They underwent surgery for absence of neurological improvement or even progressive neurological deterioration in spite of medical management, which is not unusual in spinal tuberculosis especially with advent of drug resistance, poor compliance, and default by the patients on medical management especially in developing countries.2 Majority of patients in our study group were advised surgery at initial instance of failure of conservative treatment but they refused surgery because of various socioeconomic reasons and presented very late with worsened neurology after all other nonsurgical remedies failed to show any betterment. Hence, they were considered delayed, which is also evident in our study group by advanced stage of disease reflected clinicoradiologically by median time interval of 23.5 and 29.5 days, mean vertebral body involvement of 2.27 and 2.71, and mean maximal spinal cord compression of 0.37 and 0.357 in the satisfactory and nonsatisfactory improvement groups, respectively. Median time interval does not reflect the time interval of starting ATT and surgical decompression as mentioned clearly in our published paper. Hence, there is a misperception on the authors’ side in their letter that an adequate trial of ATT was not instituted.

Among the study population, some patients had satisfactory improvement (>10 LEMS [Lower Extremity Motor Score]) while others had less than satisfactory improvement. Thus, this study was a sincere attempt to understand the factors affecting neurological outcome in these 2 groups, including timing of decompression. Such a study of delayed surgery would not have otherwise been possible because of obvious ethical concerns.

Middle path regimen by Dr S. M. Tuli3 was advocated in the year 1975 when the spine surgery and instrumentation were a challenge in a resource-constrained country like India and major emphasis was laid on conservative treatment consisting of medical management, bed rest, and nutrition. Following Tuli’s philosophy, medical management is still the mainstay of management in our institutional protocol. However, there has been a significant shift in the management of tuberculosis of spine having neurological deficits. Recent advances starting from endoscopic decompression of the abscess4,5 to percutaneous instrumentation6 have been practiced at various centers with good functional outcome. The medical treatment combined with optimal surgical management helps not only in neurological recovery but also in faster rehabilitation in a controlled manner and avoids possibility of complications like surgical decompression as mentioned clearly in our published paper. Hence, there is a misperception on the authors’ side in their letter that an adequate trial of ATT was not instituted.

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late-onset paraplegia, known to occur with conservative management of advanced disease. The authors in the letter have described various classic stages of tuberculous paraplegia and have expressed reservation regarding LEMS score for its inability to identify or classify conditions like stage 1 where there is no motor weakness. We feel the authors of the letter have failed to take notice that patients in stage 1 were not part of study (outlined in indications for surgery) and LEMS was purely utilized for neurological evaluation in the setting of motor weakness. We would like to emphasize that functional outcome in tuberculous paraplegia is largely determined by final motor recovery achieved by patient at the end of treatment. In fact, LEMS score is objective quantification of individual muscle power in lower limb, which is expressed in terms of a numerical score, unlike the ASIA (American Spinal Injury Association) scale, and thus easing the application of statistical tools in evaluation of neurological outcome.

We are surprised by the observation made by authors in their letter stating mean LEMS score of 27.72 means stage 1 or 2 paralysis (stage 1 is no motor power loss). A score of 27.72 is significant motor involvement even leading to nonambulation and it assumes further significance in delayed presentation where neurology has worsened or conservative treatment in form of ATT has failed. LEMS is a well-validated and commonly used scoring system to evaluate outcomes of vertebral column resections.

We did not encounter isolated posterior involvement in our study group hence it was not commented upon. As there is limitation in sample size due to sheer nature of involving neglected cases, location of disease was not commented on separately as larger power study will be required. However, we agree with authors that location may have effect of neurological outcome and will be interesting to analyze in a larger power study.

The authors in their letter have mentioned about dry and wet forms of tuberculosis and their role in deciding prognosis. We agree with them that patients with liquified pus have better prognosis than thick inspissated pus. However, this distinction becomes difficult when patient is already taking ATT which itself causes reduction in liquified pus. This becomes quite subjective for assessment, and hence, this factor was not considered for the study. Our study has taken various radiological parameters like mean vertebral body loss and maximum spinal cord compression into consideration and same was subjected to statistical analysis. Hence, we do not agree with the statement that radiological parameters were not considered.

Authors in their letter have expressed reservations regarding need for anterior reconstruction by mesh cage or strut graft and have even termed it radical. Neurological decompression was the primary aim of surgery in our study. Pathological tissue like granulation tissue/abscess/sequestrum were debrided only if they were compressing on spinal cord. This is in contrast to radical debridement, where all diseased tissue irrespective of its contribution to neurological compression is removed and disease clearance is the aim of surgery. Transpedicular approach was used for decompression as mentioned in our published article in surgical technique section. However, we reconstructed defect created anteriorly after measured transpedicular decompression by using graft with or without cage. This approach does not leave any void created anteriorly after transpedicular decompression, and thus maintains the continuity of anterior column. We strongly believe that over reliance on ATT in presence of a large void anteriorly may have disastrous consequences like implant failure and difficult revisions. Hence, it appears the authors of the letter have misunderstood our approach of targeted decompression of neurological structures for radical debridement.

Once again, we would like to thank the authors of the letter for their keen interest and valuable suggestions for our research and hope our response satisfies their queries and benefits the well learned readers of this prestigious journal.

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