



## Answer to the Letter to the Editor of T. Imamura concerning “The incidence of myocardial infarction after lumbar spine surgery” by Harwin B, Formanek B, Spoonamore M, Robertson D, Buser Z, Wang JC (Eur Spine J. 2019; doi:10.1007/s00586-019-06072-4)

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To the Editor,

We appreciate the enthusiasm and interest of reviewer in our paper, and the points brought up by the reviewer are important questions for future studies.

We agree that the paper addresses perioperative risk factors but does not take confounders into consideration. Our initial goal was to employ propensity score matching to account for various confounding factors. However, given that 664 patients in our study were identified with myocardial infarction and were further stratified based on the approach and timing, the propensity matching was not possible. We believe that information of the surgical approach and timing of myocardial infarction after lumbar surgery is novel and important information. We agree that the possibility for confounding cannot be ignored, and thus, we hope that this study will serve as a foundation for further future studies which will test scoring systems along with treatment options.

Looking at the effect of treatment on postoperative outcomes was also something that we considered. However, we found that further stratification of the data would cause difficulties regarding the power of our numbers. Furthermore, it would be challenging to categorically organize the array of different treatments in a systematic way. For meaningful conclusions to be drawn regarding the effects of intervention on perioperative myocardial infarctions, a randomized control trial would be necessary. We hope that results from our study will be used as a starting point to address certain risk factors in future studies. We sincerely thank the reviewer for putting forth these important comments and appreciate the consideration of the editors.

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