The Journal of Arthroplasty 32 (2017) 2033–2036

Contents lists available at ScienceDirect

The Journal of Arthroplasty

journal homepage: www.arthroplastyjournal.org

Letters to the Editor

Letter to the Editor on 'The Prevalence of Positive Findings on Metal Artifact Reduction Sequence Magnetic Resonance Imaging in Metal-on-Metal Total Hip Arthroplasty': Part 1

To the Editor:

We read with great interest the article by Lindgren et al [1]. The authors use ordinal logistic regression (OLR) to correlate predictor variables against primary outcome variables. OLR is used to report proportional odds ratios which authors do not report. The interpretation of the proportional odds ratios relies on the proportional odds (PO) assumption, which is the most important underlying assumption when performing OLR. The PO assumption relies on the equal relationships between each pair of outcome groups. This means that predictor coefficients that describe the relationship between absent pseudotumor (PT) and type I/II/III PT are the same as those that describe the relationship between absent PT/type I and type II/III.

As is the case with assumptions of proportional hazards with Cox regression analysis, the assumption of PO should be investigated in some way. The authors do not take into account the PO, or at least fail to report it. Authors state that predictor variables were assessed while controlling for age and sex. With fixed-sized metal-on-metal bearings, as in the present study, the effect has been reported or there has been a strong trend toward it [2–4]. It is very likely that the effect of gender is not constant across different outcome variable partitions (PT absent vs type I/II/III, absent/type I vs type II/III, and so forth) resulting in the violation of the PO assumption. Hence, gender should be included as a predictor variable on its own, or alternatively, the partially proportional model should be reported.

Another worrisome issue in the present study is the implementation of multivariate linear regression to assess the association between the serum metal ion levels and the predictor variables. As numerous studies have clearly shown and which is also readily seen in Figures 3 and 4, the distribution of untransformed metal ion levels is highly skewed and clearly not normally distributed. The very same issue is seen with Harris Hip Scores. The normality of the error terms is an underlying assumption when performing linear regression analysis. Owing to the nature of the investigated variables, it is inevitable that regression diagnostics will be performed, that is, to investigate the distribution of error terms and residuals to see whether it is appropriate to use linear regression. The authors even report the line of fitted values in the scatter plots (Figs. 3 and 4), but the aforementioned diagnostics are not performed or at least not shown. Moreover, as can be clearly seen in Figures 3 and 4, there are few extreme outliers, and, due to the estimation method in the linear regression, the results are most likely rendered severely toward significant β coefficients by these outliers.

Finally, we fully agree with the authors and their conclusions. It should be noted, however, that in addition to the aforementioned important issues with statistical methods, the authors' conclusions are based on a patient cohort with loss to follow-up of 87% (379 of 434 hips). The risk of selection bias is, therefore, enormous.

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http://dx.doi.org/10.1016/j.arth.2017.02.050

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Letter to the Editor on "The Prevalence of Positive Findings on Metal Artifact Reduction Sequence Magnetic Resonance Imaging in Metal-on-Metal Total Hip Arthroplasty": Part 2

To the Editor:

We read with great interest the article by Lindgren et al [1]. Unfortunately, we have concerns regarding the statistical methods used in their study.

DOI of original article: http://dx.doi.org/10.1016/j.arth.2016.01.011.

One or more of the authors of this paper have disclosed potential or pertinent conflicts of interest, which may include receipt of payment, either direct or indirect, institutional support, or association with an entity in the biomedical field which may be perceived to have potential conflict of interest with this work. For full disclosure statements refer to http://dx.doi.org/10.1016/j.arth.2017.02.050.

DOI of original article: http://dx.doi.org/10.1016/j.arth.2016.01.011.

One or more of the authors of this paper have disclosed potential or pertinent conflicts of interest, which may include receipt of payment, either direct or indirect, institutional support, or association with an entity in the biomedical field which may be perceived to have potential conflict of interest with this work. For full disclosure statements refer to http://dx.doi.org/10.1016/j.arth.2017.02.051.