

Answer to the reviewers

To Reviewer 1

Thank you for your comments.

The statistical analyses should be verified again by a professional biostatistician. To me, it is very hard to understand the data. Thus, the Results and Discussion part should be clearer and concise (it is very hard for the reader to follow the text in this part of the manuscript). Maybe the use of logistic linear regression and expression in odds ratio, 95% confidence interval and p value is a better option.

We can understand the reviewer's opinions, but we have no statistical defects and we cannot change them.

Professor Atsushi Oshio, co-author of our article, is a professional biostatistician and an expert in quality of life investigations, rather than a Medical Doctor. He was responsible only for statistical analysis of the collected data, and was not involved in the design or assessment of the PGSAS study. We asked Professor Oshio to review your comment once again. Subsequently, we decided that there were no issues in the statistical section that need correction. There are two reasons. First, the results should be interpreted using the effect sizes of the beta and R^2 values because even clinically insignificant differences can easily reach statistical significance and be misjudged in such large-scale samples. Second, the two explanatory variables in this study, age and postoperative period, are continuous variables, and binarizing these explanatory variables in multivariate analysis is an arbitrary and risky statistical interpretation. Please refer to the other PGSAS studies; the statistics were performed in the same manner and all papers were accepted without any problems.

We also asked Professor Yasuo Iida, a biostatistician who was not involved in this PGSAS study, for his input and he agreed with our view.

Why did the authors choose as background factor only postoperative period, age, sex, surgical approach (laparoscopic or open), and the status of the celiac branch of the vagal nerve? Did you consider exploring some other factors such as co-morbidities, performance status?

The opinion seems reasonable. However, as described in the article, this is an additional analysis using the dataset of the PGSAS study. These were the five background factors collected in the PGSAS. There was no data about co-morbidities in the dataset, and additional assessment of co-morbidities in 2400 patients is physically impossible. The performance status is also important; but there was also no data on performance status in the dataset. All of the patients in this study were

relapse-free outpatients, and almost all were PS0.

Please use the term gender instead of sex.

This paper is a scientific paper that examines post-gastrectomy conditions. The use of 'sex' in this study has no social implications, as only biological attributes have implications. Sex, not gender, is used in accordance with common conventions for medical writing.

Please eliminate redundant text. For example, the statement "Of the 2368 patients evaluated, 909 underwent DGBI, 475 underwent DG, 393 underwent TGRY, 193 underwent PG, 313 underwent PPG, and 85 underwent LR" is already given in Table 1. Figure 1 is rather a Table. Please consider to change from Figure to Table.

According to the suggestions, the relevant text has been deleted. Figure 1 has been changed to Table 1.

The manuscript needs should be polished for English fluency.

Although the first author is not a native speaker, this paper has been reviewed by a native speaker several times. Please see the attached certificate.

To reviewer 2

Thank you very much for your helpful comments.

1. Title should be shortened.

The title has been changed.

2.All figures should have figure legends.

The information in the figure has been presented in table form in the revised manuscript, negating the need for a figure legend.

3.Some grammatic mistakes should be corrected by native speaker.

This paper has undergone several checks by a native speaker.

To reviewer 3

Thank you very much for your helpful comments.