Comments to the Authors,

This manuscript conducted a case-control on the association between gouty arthritis and three candidate SNPs in a large Chinese population. This study provided new evidence that more SNPs would be taken as the new possible targets for diagnosis and treatment of gouty arthritis. The study was performed rigorously and the findings sound very interesting. In general, I'd recommend publication if the authors can address the following concerns

**Major Compulsory Revisions**

1, Because the demographic and clinical features between case and control were dramatically different. The chi-square and the allele frequency test between case and control would be biased without the adjustment of these significantly different features. Statistic model adjusted with these features should be applied.

2, In the Table 2, the detail sample counts for each category should be provided and how the P was inferred should be noted in the footnotes.

3, The result of the current study should be compared with other studies in Chinese and other populations, especially the OR and P-value as well as the heterogeneity between the different studies.

4, Although more and more significant SNPs were identified to be associated with gouty arthritis, it has been found the even with most significant SNPs in highest inherited disease, the prediction power was still very low (S Guo etc., Cancer Medicine 2014). How to interpret such problem and what’s the solution should be discussion in the section of discussion.

Minor Revisions

1, More detail information should be provided to “gout-free controls”. How to enroll them and the criterions?

2, Line 24 in Page 12, bonferronicorrection should be “Bonferroni correction”

3, Line 4 in page 15, “re6598541 and re1967017”, I think these should be typos. Please check the manuscript carefully again to avoid such problems.