Global distribution of sulfur and nitrogen deposition is of great concern for atmospheric chemistry studies. Affected by precursor emissions and meteorological condition, the deposition varied greatly in different regions over the world, resulting in various ecological impacts. It is thus important to improve the understanding of deposition level and its spatial distribution and at the global scale. This manuscript applied a Measurement-Model Fusion Approach, to improve the estimates of global sulfur and nitrogen deposition for 2010. It incorporated available data from site measurement and chemistry transport modeling, and adjusted the total amount of deposition and spatial distribution. In general the manuscript provided quite useful information on relevant topic, and was organized and written clearly. As a scientific paper instead of data report, however, I have some concerns that need to be further stressed or clarified. Those mainly include the motivation, scientific findings and result evaluation. Details follow.

- First of all, I feel the scientific motivation should be better stressed in the Introduction. What is the main purpose of the study? It should be clearly stated. Developing a new method for data fusion, or improving the estimation of global deposition (how to prove it then), or something else?
- Similarly, could the authors justify their main findings (e.g., the changed estimation of total deposition?) How to demonstrate the numbers were more reasonable compared to existing ones?
- Lines 97-100. I can understand that most of dry deposition were obtained based on this method. I am wondering, however, is it possible to collect some dry deposition data from direct observation instead of the inferential method. The latter actually bears some uncertainty from modeled dry deposition velocity.
- The procedure part. It is unclear to me whether the authors applied the same IDW method as before, or they made some improvement on the methodology? More importantly, I feel an evaluation on the datasets should be made before conducting the data fusion. For example, how were the observation data compared with simulation applied in this study? Moreover, if there was big difference between observation and simulation, is it still reasonable to apply the current data fusion method?
- Lines 202-203 (Figure 4). It is quite hard to read "higher observation in Asia are also
better reproduced with MMF”. Could some quantitative numbers be given?
- Similarly, lines 226-229. The analysis for the figures are quite descriptive and simple. Can you make more careful comparison and suggest the performance of the three modeling work compared to available measurements?
- Figure 6. Why compare wet NH4+ only? It is necessary to provide the comparison for all the species and to make a judgment on data fusion quality.
- Line 233-234. Does that mean TDep performed better than this work or the database was more reliable? Then what is the necessity of current work? Should think it over.

Minor issues:

The title could be quite confusing. “Budget ” might not be a proper word as current paper just focused on the deposition.

There is no need to repeat the reference when it is included in a sentence.

Figure 3: The x-axis and y-axis should be clearly labeled.

The language should be improved. Some clauses were not well organized.