The TCCON and NDACC are two well-known international networks based on ground-based solar FTIR instruments. These two networks have observed globally over several years and their observations are extensively used in atmospheric physics and chemistry. The CH_4, HCl, HF, CO, N_2O are target species of both networks. A few studies have performed inter-comparison between the TCCON and the NDACC for certain gases, e.g., CO and CH_4. However, no inter-comparison between both datasets is available in literature for nitrous oxide (N_2O),which is the third most important anthropogenic greenhouse gas in the Earth's atmosphere after carbon dioxide (CO_2) and methane (CH_4). This study presents a global view of the XN_2O measurement differences between these two networks at seven sites (Ny-Ålesund, Sodankylä, Bremen, Izaña, Reunion Island, Wollongong and Lauder) covering a large latitudinal range from 45.0˚S to 78.9˚N. Furthermore, trends and seasonal cycles of XN_2O derived from the TCCON and NDACC measurements and the nearby surface flask sample measurements were compared with the results from the GEOS-Chem model a priori and a posteriori simulations. I would like to regard the novelty of this paper as moderate since previous studies have performed similar comparisons rather than N_2O but for CO and CH_4. However, this work can be a supplement of current understanding and should be in the literature. Generally, this paper is well written, fits well in the scope of AMT, and I recommend for publication with few corrections.

Specific comments.

1. In introduction part, the authors present many descriptions regarding why measuring N2O is important, how it can be measured by both the NDACC and TCCON networks, the usage of these measurements, and how they can be reproduced by CTM models. However, introduction for the key point of the paper, i.e., the scientific goal of the comparison is quite simple. More descriptions should be better, e.g., the authors can briefly introduce the previous comparisons between the TCCON and NDACC measurements for other gases, and what' the findings (optional request).

2. The reason why choose these 7 sites for comparisons because they covered a large latitudinal range from 45.0˚S to 78.9˚N. The global coverage is quite good. It is necessary to include this clarification. Besides, I think section 2 contain much

information and can be more structured, e.g., divide it into two subsections, with one for network description and the other one for theoretical analysis regarding what we can expect from the comparison, or why the difference exists.

3. What do you mean by “ only the TCCON measurements from the Bruker 125HR at Lauder are used”? You also used the NDACC measurements, right?

4. Not all acronyms in tables 3 and 4 are defined for the first time, e.g., MWs for the micowindows, TCCONap for TCCON a priori.

5. In table 3 and corresponding text, if the NDACC N_2O total column at Sodankyla is divided into 3 partial columns. The partial DOFS at each partial layer is less than unity, do you notice this?

6. Page7 line 4, there is no definition for VMR.

7. In figure 3, the differences between the TCCON and NDACC measurements at nyalesund and sodankyla are seasonal dependent. However, the differences at all other site are quite low and seasonal independent. The authors presented a detailed analysis for the seasonal dependent difference (sodankyla), but for the seasonal independent difference, the authors did not present any analysis. According to equation (4) and figure 2, the TCCON and NDACC avks are quite different. For the seasonal independent and the lower difference, the TCCONap could be more closer to the true state of the atmosphere, right?

8. In section 4, both nyalesund and sodankyla show seasonal dependent difference. Here the authors only select sodankyla for case study. The reason should be clarified.

9. Page 9 line 3 ,“…low XN2O values in the TCCON measurements in Figure 4 correspond to periods of high PV…”. As far as I judge from this figure, it is not always right. Please check if the plotting is correct.

10. Page 9 line 10, ACE-FTS is not the first time and should be defined in previous section.

11. Page 10 line 3, one “rapidly” should be removed.

12. Page 10 line 6. The smoothed ACE-FTS measurements are close to the NDACC retrieved N2O profiles for both inside and outside polar vortex cases, because the NDACC retrieval has a good sensitivity and the NDACC retrieval is able to capture the change in the stratosphere. However, the TCCON retrieval overestimates the deviation from the a priori in the stratosphere. Another reason is that you smoothed the ACE-FTS using the NDACC avk, but if you use the TCCON avk. The smoothed ACE-FTS measurements should also close to TCCON profile.

13. In figure 5, why you use the TCCON a priori profile rather than the scaled TCCON a priori profile (the retrieved) in comparison. In caption should state the error bars are included.

14. In table 6, I recommend to include the longitude and latitude information of the FTIR site.

15. The agreement in Figure 7 is improved, however, it still shows that the NDACC over/under estimated TCCON at low/high concentration.