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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Photochemical roles of rapid economic growth and potential abatement strategies on tropospheric ozone over South and East Asia in 2030" *by* S. Chatani et al.

Anonymous Referee #3

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Authors have modified the MS and they have added comparison with observations at some of the sites in South Asia. However, I feel that validation part of the model setup is still very weak, particularly, considering the future projection of ozone. Major flaw is simulation and comparison for a single year. Since ozone chemistry is highly complex and highly non-linear (with respect to growth in precursors), authors should make simulations for different years, say 1990 or 2000, and also compare with the available measurements. This will give confidence in using present setup for such studies. After comparing the observations for a set of two years (1990 and 2000 or 2000 and 2010), work on year 2030 would be more realistic. This argument would be

clearer, after my concerns mentioned below.

It should be noted that simulated surface ozone is higher by 15-20 ppbv for 3-4 sites in India. Therefore, model results are already biased. Co-incidentally, increase in ozone (during 2030) is also about 20 ppbv for India.

Similar to the above, simulated tropospheric column ozone is also higher (probably more than double) when compared with satellite data, mainly in winter and spring (Fig 8). This can be better seen in the percentage differences (not shown). Therefore, increase in ozone for year 2030 should to carefully seen and percentage difference (between observations and simulations) should be kept in mind.

Comparison of precursors like NO/NO2/NOx, CO, HC etc (from surface observations) are still missing.

More detail on PC0 and PC1 is also important for studying the impact on ozone. Like, how much decrease in NOx, VOC, etc is considered in these two scenarios. Then it would be better to compare ozone decrease in these two scenarios with increase in ozone from 2010 to 2030.

In case of BAU0, high ozone is seen along west coast of India and another hot spot around eastern India during winter. On the other hand, higher ozone are seen along south-east land mass of India and ozone levels are lesser along IGP region, one of the most polluted region. It is also interesting to see higher ozone along IGP region in Fig 8, but higher ozone along IGP is not very much clear in Fig 2.

Since surface ozone observations are available during different periods (from 1990s to present, though may not be continuous) at some of the sites in South and East Asia. Therefore, some discussion is also warranted on those observations and how do they compare with projected/demonstrated increase during 2030. I have also noted that there are several papers by this group on future projections. Since this MS is based on ozone, mere projected emissions scenarios and influence on ozone may not be

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sufficient.

I strongly feel that simulation should be made for two sets of year for showing model's capability in demonstration of future projections. This is important for ozone. Somehow, present MS shows significant enhancement in ozone over South Asia, I afraid that it may not be realistic. To have our confidence, it is essential to compare with past observations. I also feel that present MS lacks in-depth scientific discussion and I strongly feel that this MS is not suitable for ACP.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 9517, 2014.