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Comment

## ***Interactive comment on “Projections of air pollutant emissions and its impacts on regional air quality in China in 2020” by J. Xing et al.***

### **Anonymous Referee #1**

Received and published: 9 November 2010

The manuscript written by Xing et al. forecasts several emission scenarios of air pollutants in 2020 in China on basis of comprehensive analysis of energy consumption, control techniques of air pollutants and policies. This work is important and very helpful to understand the situation of air quality in the near future in China. The authors further analyze the impact of different emission scenarios on concentrations of various air pollutants using a CMAQ air quality model. The part is difficult to follow. A major revision is needed for publishing in ACP. This reviewer likes to review the revised version. My critiques are mainly related to the modeling result as listed below:

1) Page 26906, line 22, Are the calculated concentrations of SO<sub>2</sub> and NO<sub>2</sub> at ground level or the average concentrations of SO<sub>2</sub> and NO<sub>2</sub> in the boundary level? The same comment is applicable for other pollutants. 2) Page 26906, lines 25 to Page 26907,

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line 1, this reviewer totally lost on the discussion. This reviewer believes that primary emission of NO<sub>2</sub> should account for a minor fraction of NO<sub>x</sub> emission. NO<sub>2</sub> should be dominantly from the conversion of NO to NO<sub>2</sub>. The analysis of the ratio of emission changes of NO<sub>2</sub> to NO<sub>2</sub> concentration responses does not make sense to this reviewer. 3) Page 26907, lines 1-3, this reviewer does not understand what the authors were discussing. 4) Page 26907, lines 7-10, the authors should summarize the general trend rather than giving an example. The PRD, ECH, YRD and NCP are nuisance going back and forth in the manuscript to remember what these terms mean. It is unnecessary to save letters in writing. 5) Page 26907, lines 15-17, “Although in January, the increase of NO<sub>x</sub> emission in REF[0] will reduce the ozone concentrations by –4% in NCP, –7% in YRD, –1% in PRD, and –1% in ECH.” Why? If it is due to the titration reaction, why not use concentration of (NO<sub>2</sub>+O<sub>3</sub>)? 6) Page 26907, lines 20-22, “These results suggest that the effects of different ozone chemistry regimes in different seasons should be considered during policy-making for NO<sub>x</sub> control.” What does it mean? 7) Page 26908, lines 1-3, an increase or a decrease relative to what? 8) Page 26908, lines 8-9, “PM<sub>2.5</sub> concentration is more sensitive to primary PM emissions in January due to lower atmospheric oxidation activities” The meaning of this sentence is not clear to this reviewer. 9) Page 26908, lines 11-12, “decreases in SO<sub>2</sub> emissions in PC[2] reduce the PM<sub>2.5</sub> concentrations by 5% in NCP, 1% in YRD, and 3% in ECH.” The statement is problematic since an increase of SO<sub>2</sub> emissions in PC[2] in the Pear River Delta shows in Table 6. 10) Page 26908, lines 17-19 “NO<sub>x</sub> controls are more effective in April and July in NCP/YRD with an emission to concentration scale of 6–12, while are less effective in PRD with scale >20 due to NH<sub>3</sub>-poor condition.” This reviewer cannot follow the logic of the statement. The sentence should be clarified. 11) Page 26908, line 27 “SO<sub>2</sub> is the dominant sulfate species in PM<sub>2.5</sub>.” The sentence is problematic. 12) Page 26909, lines 4-7 “The growth of NO<sub>x</sub> emissions has positive impacts on the sulfate reduction because of the ozone chemistry, especially in January, April and October when VOC-limited regimes are dominating. Extra NO<sub>x</sub> emission will react with OH to obstruct its reaction with SO<sub>2</sub> to generate sulfate; the reduction ratio of sulfate

is 6%.” This reviewer lost here and this reviewer believes NO<sub>x</sub> emissions should, in general, increase OH in the regional scale, although it may be not the case in urban center. 13) The section of “Total sulfur deposition and nitrogen deposition” is difficult to be understood because the two terms are not defined in this manuscript.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 26891, 2010.

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