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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of nitrous acid chemistry on air quality modeling results over the Pearl River Delta region" *by* R. Zhang et al.

Anonymous Referee #3

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The manuscript by Zhang et al describes a 3D chemistry and transport modeling study of the Pearl River Delta region with a focus on testing new nitrous acid formation mechanisms and their impact on HONO and ozone chemistry. The study is based on the CMAQ model, adapted and tested for the Pearl River Delta region. The authors carefully review the various proposed HONO formation mechanisms and discuss how these mechanisms are parameterized in the model. The main conclusion from the model study is that implementing additional HONO sources improve the comparison with direct measurements of HONO, in particular during the day. The authors conclude that heterogeneous chemistry is responsible for 60% of HONO, followed by surface photolysis, and direct emissions. Inclusion of HONO chemistry lead to an increase of maximum O_3 of $\sim$8% and an increase of PM2.5 of $\sim$12% compared to the base case. The authors also show that inclusion of more accurate HONO chemistry impacts the

design of ozone control strategies.

In general, this is a very interesting and well thought through manuscript. The authors spent considerable effort in considering all currently proposed HONO sources and including them in the model. The results confirm some earlier, much simpler, studies that predict that inclusion of HONO chemistry will have a 5-10% effect on ozone. The discussion of the impact on ozone control strategies has implications that go beyond pure science.

I recommend this manuscript for publication in ACP after some revisions and copy-editing. Below are some specific comments.

Page 15083, line 7: Please clarify what you mean with "O_3-to-NO production rate".

Page 15086, lines 10 – 25: Treating aerosol surfaces and the ground by the same physical mechanism seems inaccurate, because of transport limitations towards the ground surface that do not exist in the same way for the aerosol. Please explain why you can treat ground surface and aerosol heterogeneous chemistry by the same parameterization. Also, how do you reconcile treating heterogeneous conversion on the ground surface and the use of deposition velocities for surface loss separately?

Page 15088, equation 5: Define z_s in the text.

Page 15089, line 17: Please define "index of agreement".

Section 3: The most serious omission in the manuscript is the missing comparison of the observed diurnal variation of HONO at Xinken and Guangzhou with the results from the various model runs. This must be added to convince the reader that

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HONO is better described in the expanded model. Figure 4 is not sufficient for this purpose.

Figure 6: It is not completely clear what is shown in Figure 6. Is this the 24h average in the lowest grid cell of the model? In this case the plot is skewed towards the high nocturnal HONO cases. Figure 6 also does not seem to show any information that is not also shown in Figure 7.

Section 3.4, Figure 6 & 7: The results in Figures 6 and 7 are surprising in that the heterogeneous conversion plays such a major role. Current literature suggests a much higher relative contribution of the photolytic source. The authors should comment on why they think that the heterogeneous pathway is so important in their case.

Figure 8: Please show separate plots for daytime and nighttime, following the arguments outlined above for Figure 6.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 15075, 2011.