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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of HONO on global atmospheric chemistry calculated with an empirical parameterization in the EMAC model" *by* Y. F. Elshorbany et al.

Anonymous Referee #1

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This paper presents a simple parameterization for improving the simulation of HONO in chemical transport models. I found the analysis of field observations to construct the parameterization interesting and thorough. The observations are mainly urban. Extension to the global atmosphere is highly questionable. In fact, the global model analysis presented here focuses on polluted environments, which seems most appropriate. I recommend publication after consideration of the comments below.

1. Title is misleading in that the paper does not focus on 'global atmospheric chemistry'. It focuses on polluted environments, in the context of a global model, and with just a few global surface maps. Examination of the impact on global atmospheric chemistry would

require discussion of the implications for the global budgets of tropospheric ozone and OH, also perhaps with zonal mean effects. I don't really encourage the authors to do that because their parameterization is only constrained for polluted environments. But then they should change the title. 2. The introduction needs some review of the HONO atmospheric measurements – can they be trusted? Different measurement methods are used when constructing the parameterization. Have they been intercompared? 3. I found the introduction to be very thorough but also too long, rambling and uncritical – I recommend tightening and shortening it. 4. I'm surprised that the parameterization recommended by the authors for HONO production doesn't depend on aerosol surface area, considering that the missing process for HONO formation is thought to be heterogeneous. Is it assumed that the dependence on aerosol is implicitly contained in the dependence on NOx? That might apply in urban areas but doesn't seem appropriate globally. 5. Page 12,904: I'm surprised by the authors' statement that HOx cycling is ineffective for NOx below 1 ppb. I know it to be effective down to about 0.1 ppb NOx. 6. Figure 13: such high values of ozone over the eastern US (160 ppb!) don't make sense, and a 40 ppb enhancement of ozone due to HONO chemistry seems highly suspicious. 7. Page 12,905, bottom: the good agreement with local models is not surprising since the comparison presented here is for the polluted environments from which the parameterization was derived. 8. Page 12,906, line 14: not clear what is meant by 'buffering effect'? 9. Page 12,907, line 4-5, also in text: the dependence on wind speed is not clear.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 12885, 2012.