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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigating Diesel Engines as an Atmospheric Source of Isocyanic Acid in Urban Areas" *by* Shantanu H. Jathar et al.

Anonymous Referee #1

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General Comments:

Jathar et al present new measurements of isocyanic acid (HNCO) emissions from diesel engines to assess the role of selective catalytic reduction (SCR) systems in enhancement of HNCO emissions. The authors demonstrate, as one would expect, that NOx emissions are reduced when SCR systems are in place, but surprisingly SCR appears to have little impact on HNCO emissions. The authors take emission ratios (HNCO:CO) determined for diesel emissions, alongside emission ratios from other primary sources and estimates of secondary production to assess HNCO concentrations in a regional model. Both elements of this study of novel contributions to the literature. The paper is well written and should be published in ACP following the author's attention to the following comments:

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Discussion paper

Specific Comments:

1) There is a paper by Suarez-Bertoa and Astorga, Isocyanic acid and ammonia in vehicle emissions, in Transportation Research Part D: Transport and the Environment, which is not cited which has some discussion of HNCO emissions and SCR that are likely relevant to this discussion.

2) It would be helpful for the authors to provide more discussion on the choice and potential implications of using HNCO:CO vs EFHNCO. As I understand this is done because an emission factor for biomass burning is not trivial. However, I would like to know more about the implications of this decision for the gasoline and diesel emissions. If one were to implement an emissions factor based approach would you expect the same conclusions (e.g., that diesel emissions for HNOC are more important than gasoline emissions and that [HNCO] are routinely less than 100 ppt in urban areas?

3) The paper concludes that SCR does not enhance HNCO emission factors. However, I am struggling to see this so clearly in Figure 2. There is a tremendous amount of variability in each of the data sets. For example the blue dots (1500 rpm) span almost 2 orders of magnitude when [NH3] is zero? The reduction in NOx (Fig 2A) is very clear, but my interpretation of Fig. 2B would be that HNCO emissions when SCR is used do not change within an order of magnitude. Within the confidence limits of the data set, can it really be concluded that SCR does not impact HNCO emission factors? Perhaps I am missing something.

4) I was intrigued that the modeled dry deposition velocity of HNCO was taken to be equal to HNO3. I think it would be helpful to state what the corresponding HNCO lifetime is in the model wrt/deposition and how much this assumption impacts model [HNCO]. It is easy to imagine a factor of 2 if not much more uncertainty in this assumption. It would be helpful to the reader to know how important this term is.

5) I understand that benzene and HNCO should be strongly correlated near the source region, but these two molecules have very different atmospheric lifetimes. It would be

helpful for the authors to provide some comment on the limits of making such correlations for non-source regions.

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