

## ***Interactive comment on “Exploration of the atmospheric chemistry of nitrous acid in a coastal city of southeastern China: Results from measurements across four seasons” by Baoye Hu et al.***

### **Anonymous Referee #1**

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The manuscript “Exploration of the atmospheric chemistry of nitrous acid in a coastal city of southeastern China: Results from measurements across four seasons” by Baoye Hu et al. reports year-long observations of HONO together with gaseous, particulate, and meteorological parameters which are relevant for investigating HONO sources. The manuscript adds valuable informations on HONO concentration level and its temporal variation under costal condition. Discussions on the HONO sources and on the HONO impacts on OH radical production is however inline with the current understanding. I would recommend the publication if my following comments are well addressed.

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### **General comments**

There are plenty of published papers describing HONO measurements, most of which are also using the similar methodology to investigate HONO sources and draw similar conclusion that daytime HONO is mainly originated from photolysis of nitrates. In order to make the manuscript more valuable to the community, I would suggest the authors put more efforts on summarizing the findings on HONO production in different environments (e.g., inland or costal, downtown or suburban or rural, seasons, RH levels,  $\text{NO}_3^-$  levels, etc.) and compare those with this work. I think the comprehensive data set shown in the manuscript would well support the comparison. The current comparisons listed in Table 1 and Table 3 are too general and not quite informative compared to those already shown in many other publications.

### **Specific comments**

**Line 93–96, Page 4:** The author should make a clear description that the time needed for the sampling period and the later IC analysis on the MARGA system. When synchronize high time resolution data (i.e., HONO,  $\text{NO}_x$ , J values) to MARGA data, it should be done exactly for the MARGA sampling period. This should also be clearly described.

**Line 110, Page 4:** I would recommend the Section 3.1 focus on reporting the measurement results, discussions on HONO sources and OH production by HONO photolysis can be moved to the following specific sections.

**Line 111–115, Page 4:** As mentioned in the General Comments, I suggest to make the comparison in terms of environmental conditions.

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**Line 128–131, Page 5:** How about the sea salt concentration observed in this study? The argument is based on the assumption that HONO is mainly formed by photolysis of sea salt, which could not be confirmed at this moment.

**Line 134–135, Page 5:** It would be helpful to confirm this by using the measured J, NO<sub>x</sub>, RH, etc.

**Line 162–163, Page 6:** I could not follow the argument that higher HONO/NO<sub>x</sub> ratio indicate unknown daytime HONO sources. The authors should first describe what is “unknown” and if the observation could not be explained by the well accepted theory.

**Line 184, Page 6:** How is the duration of air masses been determined?

**Line 242, Page 8–Line 243 Page 9:** Seeing from the summer plot in Fig. 4, most blue points lie in values below 0.03 when RH is above 90%. It looks to me that the trend of orange line is biased by only few data points which have high HONO to NO<sub>2</sub> ratio.

**Line 312, Page 11:** Should be “Eq. (7)” instead of “Eq. (6)”.

**Section 3.5, Page 13–14:** First of all, there are various assumptions on HONO production pathways been made in the previous sections. I would be better to provide a full picture on how large of the each contribution to the HONO formation. Secondly, under the title “Parameterization”, the reader could not even find a formula used for predicting HONO production or concentration. Moreover, why Eq. (10) is suitable in other place than in this work? Would the parameterization described in this work more

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reasonable and can be better used in the future?

**Section 3.6, Page 14–15:** I think the authors should make a clear statement that they are evaluation the primary production of OH radical. As shown in many publications investigating HO<sub>x</sub> budgets, the production of OH during daytime is mainly by HO<sub>2</sub>/RO<sub>2</sub> + NO reaction.

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