

## Response to Referee #1

The authors thank Referee #1 for his/her valuable comments to further improve and clarify the MS. We have considered all recommendations, and made the appropriate alterations. Our specific responses to the comments are as follows.

1. Two NSF factor have been used in the manuscript. The mixed usage of the two factors always confused me. I would like to suggest the authors to clearly state which factor they are referring to whenever possible, e.g., NSFs in Line 134-136. Also, is NSF<sub>nucl</sub> days larger than NSF<sub>all</sub> days? Looks to me that (N6-100/N100-1000)<sub>nul</sub> days is larger than (N6-100/N100-1000)<sub>all</sub> days by definition, isn't it? I would like to see more discussion on the relationship between the two factors.

We named the two versions of the NSF differently as NSF<sub>NUC</sub> for the concentration increment on a nucleation day, and NSF<sub>GEN</sub> for the concentration increase on a general day all over the MS to assist their differentiation. The interpretation of their meaning regarding the limiting values is valid for both of them. We also extended the text with several new aspects on their relationships. The changes are highlighted in red.

2. In table 2, wouldn't I get (N6-100/N100-1000)<sub>nul</sub> days from the numbers listed? For example, (N6-100/N100-1000)<sub>nul</sub> days can be obtained  $(1.72/1.03 = ((N6-100)Nucl/(N6-100)Non-nucl)/((N100-1000)Nucl/(N100-1000)Non-nucl) = ((N6-100)Nucl/(N100-1000)Nucl)/((N6-100)Non-nucl/(N100-1000)Non-nucl) = (N6-100/N100-1000)_{nul}$  days. However, my number is different from those listed in Table 3. What is the problem?

In data processing, concentration ratios were first derived from the individual data; later these ratios were averaged for nucleation and non-nucleation days for a specific time point considered, and finally the NSF values were calculated and averaged over the time interval selected. This does not necessarily and exactly results in the same final NSF value as that derived from the mean concentrations. The former data treatment is the correct method considering the dynamic character of atmospheric concentrations, uncertainties of the data, and the propagation of errors. This was actually utilised in the MS.

3. (Line 156-158), put "Data coverage for summer and autumn for the mean ratios" as a footnote of Table 2.

The requested footnote was added.

4. (Line 159-160) the lower background particle concentration on nucleation days in winter came from real measurements, right?

All the primary data presented were obtained experimentally, which was confirmed now in this section as well.

5. (Line 173) rephrase “its consideration in the averaging is justified”.

The sentence was modified to clarify its meaning.

6. Clearly mark the vertical coordinates (NSFnucl days or NSFall days) in Figures 1 & 4, and also include NSFnucl days or NSFall days in the body instead of the title of Tables 3 & 4.

The requested changes were fully adopted. See also the response to Comment 1.

7. In table 2, what caused the behavior of  $N_{6-100}$  on NPF days? Especially, the peak at night. Also, please expand to discuss why  $N_{6-100}$  is significantly larger between 6-9 am on non-nucleation days? Does this mean that nucleation was hindered by the high concentrations of preexisting particles?

We assume that the Referee meant Figure 2 instead of Table 2. The late evening/night peak was observed from the beginning of the measurements in Budapest (see e.g. Salma et al., 2010), and can likely be related to the combined effect of burning and heating activities at residences and homes, and of local meteorology. They are also influenced by the daily cycling of the boundary layer mixing height and mixing intensity. The exact interpretation of the evening peak, however, needs further dedicated investigations. As far as the  $N_{6-100}$  between 6 and 9 am is concerned, its higher level on non-nucleation days with respect to nucleation days is indeed related to higher pre-existing aerosol concentration level, and thus, larger condensation sink values, which hinder the chances for NPF. These explanations were now included into the text.

8. (Line 272) lower solar radiation in winter is understandable, but less biogenic pre-cursor gases are not justified. Is there any evidence that NPF in Budapest requires biogenic vapors?

There is indirect evidence that biogenic emissions contribute to the early stage of the growth process, and likely to the nucleation itself as well. Distribution of the monthly mean occurrence frequency, and relatively low (<20%) relative contribution of gas-phase  $H_2SO_4$  to the growth rate of particles can indicate the role of biogenic precursors. The sentence was extended to include this information, and two specific references were given now for further details.

9. (Line 277-279) rephrase “this observation raises the question ... for calculating the NSF”.

The sentence was modified.

10. Regarding the health effects (Line 385-387), I would like to suggest the authors to be more conservative. The relative short lifetime is one aspect, but the toxicity per particle is another aspect. The authors just can't evaluate the health effects of nanoparticles generated by NPF.

We can fully agree. The discussion on the health effects was modified to be more accurate, and the specific aspect raised by the Referee was included.

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