Response to Referee number 1

25th January 2021

The authors would like to thank Referee no. 1 very much for his/her expert, valuable and detailed comments to further improve and clarify the MS. We have considered all recommendations and made the appropriate alterations. Our specific responses are as follows, while the textual modifications amended can be traced in the marked-up version of the MS, which is available on the website.

Scientific comments:

One of my major concerns relates to the data from 2012 which were measured near city. Apparently, these are the only data from this region in this time range. However, when looking at the individual graphs (SI) one can see a clear difference between near city and city centre. The near city location exhibits lowest NO2 concentrations, lowest T and WS values and highest O3 which all influence NPF activity. Interestingly, the annual mean NPF occurrence frequency (Fig. 2) has the highest value near city (Y2012). What is the point of showing these data in Figs. 2 and 4? As far as I understand, they're not included in Fig. 3, and were not used anyhow according to the statement on line 722. Lines 694-697 even discuss the problem of varying types of oxidizing agents and concentrations with respect to NPF. I think more reasoning is needed why data have been merged here from two different locations. Probably, these data can be skipped without losing any information.

1. We have several reasons for including the near-city background site together with the city centre into the MS. The NPF phenomena at the two locations are connected to each other. It was described in lines 202–206 of the preprint that "the NPF events observed in the Budapest ordinarily happen above a larger territory in the Carpathian Basin (Németh and Salma, 2014) as a spatially coherent regional atmospheric phenomenon (Salma et al., 2016b)". (The conclusion does not mean that NPF events happen uniformly either in space or time within the whole basin since differences in atmospheric concentrations, meteorological data – some of them were correctly listed by the Referee – and amplifying or quenching effects over some sub-regional territories can cause systematic or accidental alterations. This explains why the annual f_{NPF} in the near-city background was larger than that in the centre.) The results and conclusions obtained for the background, hence, represent added values since the whole data set shows more detail picture on NPF process in the Budapest area. Furthermore, the differences and similarities between the two sites for f_{NPF} and environmental variables (Sects. 3.1, 3.2, 3.3 and 3.5) sensitively point to the importance of different urban environmental types in the process. We consider, for instance, the data point for the near-city background in Fig. 7 important even though, it was eventually not considered after a careful discussion in the line fitting. By skipping this data point and, more

importantly, the associated explanations, we would lose evidence for the role of different vegetation types on the start of the NPF occurrence peak in spring. At the same time, we strictly considered only the city centre data when average multi-year properties were dealt with. This was the case for Fig. 3 and in Sect. 3.4. The arguments for keeping the near-city background data in the MS were very briefly amended to Sect. 2.1 to enlighten their advantages.

Lines 505-507 discuss joint influence of variables on occurrence frequency by pair wise correlation. To me the question remains whether only correlation is important or also the absolute value of the variables.

2. We can agree with the Referee that the absolute values of the variables also play a role in the comparisons. The correlation analysis was mainly performed to gain first impression on pairwise relationships. Its results and the mean event-day-to-non-event-day ratios confirmed the need for a more comprehensive approach. The sentence was extended to express this and that the data sets are to be further evaluated in more detail as part of a dedicated multi-statistical analysis.

Lines 586/587: Who/what determines the limiting ratios (>1.1, <0.9, respectively) that favor or disfavor NPF? Are there references for these numbers or other scientific arguments?

3. The limiting rations serve only as indicative or guide values. The criteria were selected so that the daily mean values around the monthly mean ordinarily remain between them. This was based upon a simple exercise with the variabilities for non-event monthly time intervals. The procedure represents a pragmatic approach, though alternative limits could also be set. This was added to the text.

Table 3 (bottom of page 22): How was SD determined? Given the smooth increase of curves in Fig. S1, SD could probably be chosen narrower (< 10?). Connected to this: Why are error bars for SoS bigger than for start of NPF (seems to have much bigger scatter due to discrete daily appearance)? SoS may vary from year to year but can probably be narrowed down substantially for each individual year which would be much more representative for NPF activity/conditions in the corresponding year. Also related to this point is Fig. 7: the difference between the red and black lines is _20 days, which seems to be in perfect agreement with the difference of SoS and start of the NDVI increase in Figure S1. Wouldn’t the cutting point of linear fits through the pre-spring period and spring (increasing NDVI section) give a better estimate for start of vegetation activity?

4. The SDs of SoS and GuD were calculated for every pixel in a given land cover category, and those values were used to calculate the means and SDs. The processed remote sensing data are affected by noise due mainly to the atmospheric and illumination or observation conditions, and the land cover data set also have a relatively coarse (500 m × 500 m) spatial resolution. Moreover, the measured signal is a mixture of different vegetation types and species. Taking into

account pixels with high vegetation diversity and, therefore, with variable phenological properties, the derived metrics result in relatively large variability.

As far as the applied 20% cut-off method is concerned, it is commonly used to determine the start of the season (i.e. leaf unfolding) of a biome based on the NDVI data (e.g. Shen et al., 2015; Kern et al., 2016, 2020; Wang et al., 2018). The SoS marks the date when the annual NDVI curve of the selected pixel becomes larger than the $NDVI_{min}+0.2\times NDVI_{range}$, where the $NDVI_{range}$ is the difference between the yearly maximum and minimum NDVI during the first half of the year. A considerable advantage of this method is that it minimizes the potential effect of the noises in the data set (especially during the late winter/early spring) due to atmospheric effects or snow cover. In addition, both the SoS and the GuD vary during the years, and, hence, the time from the minimum NDVI until the SoS is also different in every year.

Both explanations above were included briefly into the text as extensions.

One remark about the conclusions: in the manuscript the influence of vegetation is solely discussed in relation to spring. The conclusion should also comment on the summer season where vegetation is fully developed and whether the winter minimum in vegetation activity explains the minimum in NPF frequency.

5. The relationships between vegetation and NPF event occurrence in spring (shown e.g. in Figs. 7 and S12) are regarded to be the main findings of the MS. The effects of vegetation on f_{NPF} was also investigated indirectly through T above vegetated territories in sultry summer intervals or in winter, but no obvious links could be proved in the present data sets. The issues raised by the Referee need further experimental and modelling studies. To her/his request, we briefly extended the conclusions with an additional sentence for completeness.

Technical, editorial comments:

Abstract, line 36: in the abstract I'd suggest to write out WS (wind speed)

6. The abbreviation WS was resolved in line 27.

Introduction, line 84: I'd use parenthesis: "(semi-)continuous"

7. Adopted.

Line 94: check language: “understanding OF? the role”

8. Adopted.

Chapter 2, line 118: move “(last five properties)” after the word “vegetation” in line 119.

9. Adopted.

Figure caption 1, line 189: write out “IGBP”.

10. The abbreviation IGBP was resolved.

Line 199: check language, word order

11. The word order was corrected.

Line 265: the wording “may be indirectly” does not sound convincing. Are there any stronger arguments for the use of the selected parameters?

12. The sentence was modified, and an extra citation was added.

Section 3, line 336: “overviewed”

13. The typo was fixed.

Line 342: “units”

14. Corrected.

Lines 351-357: Quite long sentence, hard to read

15. The sentence was divided into 2 parts, which were further clarified.

Line 428: “plots”

16. Corrected.

Line 503: “joint”

17. Corrected.

Line 535: “climate”

18. The typo was fixed.

Line 575: “necessarily”

19. Corrected.

Lines 580/581: I’d suggest to add the actual ratio here to clarify what is meant (as done on line 682 for some variables)

20. The sentence was extended to explain the meaning of the ratios better. We found the listing of all variables considered too extensive.

Lines 583/584: check language: . . .“ratios for modelled variables”. . .?

21. The sentence was modified.

Lines 594/595: Not sure I understand this sentence correctly. What’s the cause, what’s the effect?

22. A part of the sentence was deleted.

Line 602: the table indicates a value of 2.6 (not 1.64).

23. As a matter of fact, we dealt with the ratio of the monthly mean event-day-to-non-event-day ratio to its annual mean ratio (thus, ratio of ratios). For O_3, for instance, the mean winter ratio of 2.3 (calculated from December-February values of 2.6, 2.6 and 1.79, columns 3–5 of Table 2) was larger by a factor of 1.64 than its annual mean ratio of 1.42 (column 2). We split the sentence in 2 parts and clarified better its meaning.

Line 637: “later”

24. The typo was fixed.

Line 665: “confirms”

25. Corrected.

Table 3, bottom of page 22: “SD” appears twice. Maybe add superscripts SoS and GuD to avoid confusion.

26. The indices SoS and GuD were added in superscripts to SDs.

Line 718: regarding NPF event occurrence spring peak: add reference to text on page 5 where this is explained.

27. The requested reference was adopted.

Lines 720/721: Regarding statement "growth characteristics are different for various vegetation types as just concluded": I can't find discussion on this, please refer to position in text.

28. The discussion of this observation could be found in lines 705–710 of the preprint. This is located in a paragraph that is right before the paragraph containing the statement. Nevertheless, we specified the position of the discussion more precisely now.

Fig. 7: font type hard to read

29. The size of the labels and the resolution of the figure were increased.

Line 758: ". . .affect NPF. . ."

30. Corrected.

Chapter 4, line 763: ". . . with AN overall. . ."

31. Corrected.

Fig. S1, line 25: symbol assignment in figure caption (upper/lower triangle) is misleading, reformulate (e.g., triangle pointing upward/downward)

32. The specifications of the symbols were reformulated.

References:

Shen, M., Piao, S., Cong, N., Zhang, G., and Jassens, I. A.: Precipitation impacts on vegetation spring phenology on the Tibetan Plateau, Glob. Chang. Biol., 21, 3647–3656, doi:10.1111/gcb.12961, 2015.

Kern, A., Marjanović, H., and Barcza, Z.: Evaluation of the quality of NDVI3g dataset against Collection 6 MODIS NDVI in Central-Europe between 2000 and 2013, Remote Sens., 8, 955, doi:10.3390/rs8110955, 2016.

Wang, L., Tian, F., Wang, Y., Wu, Z., Schurgers, G., and Fensholt, R.: Acceleration of global vegetation greenup from combined effects of climate change and human land management, Glob. Chang. Biol., 24, 5484–5499, doi:10.1111/gcb.14369, 2018.

Kern, A., Marjanović, H., and Barcza, Z.: Spring vegetation green-up dynamics in Central Europe based on 20-year long MODIS NDVI data, Agric. For. Meteorol., 287, 107969, 10.1016/j.agrformet.2020.107969, 2020.

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