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Reply on RC2

Attila Buzás et al.

Author comment on "Revisiting the long-term decreasing trend of atmospheric electric potential gradient measured at Nagycenk, Hungary, Central Europe" by Attila Buzás et al., Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2021-7-AC2, 2021

Authors' response to the 2nd review

First of all, we would like to thank the Referees for their thorough and constructive comments and the positive reception of our manuscript. Their precious work contributes to the refinement of the manuscript and hopefully, ultimately to the publication of the revised paper.

Major point 1:

Review:

"*Conductivity is a key parameter in the physics of the Global Atmospheric Electric Circuit (GEC) which affects, dfor example, the atmospheric electric field magnitude. In the present study, the conductivity issue is not treated adequately; therefore, I ask the authors to be more specific in dealing with this parameter in their paper.*

In line 101 it is stated that "all objects in the model were initially treated as perfect conductors", which is certainly a gross simplification. First, what does it mean here "perfect conductor" when electrically the wood is a "perfect insulator"? They need to explain and discuss this idealized assumption and if should be applied here. Apparently, it is presumed that the conductivity of the objects (trees and building) is considered equal to that of the conducting ground. This is not well explained. Even the ground conductor concept needs to be discussed and clarified, since its conductivity varies and it may depend on season or is subject to a trend with years.

In validating their model, the authors have been obliged to deal with their assumption on conductivity, because the model led to systematic differences between the predicted PG values and the measurements (Figure 4a). Thus, in line 200 is stated that: "the model with perfectly conducting objects overestimates the shielding effect of the trees". To account for this discrepancy, the authors adjusted the objects' dielectric constants to values that fit better their data. However, they do not explain how the dielectric constant is used here to account for the effect of conductivity. Also, they need to explain why they adopt dielectric constants for the objects which are much larger than published values. For example, the authors use wood dielectric constant values of 120 to 130 which differ

considerably from those of 25 to 85 reported in relevant publications. This is an issue that needs to be considered. To state it in other words: can the dielectric constant of the objects be used as a "free parameter" to fit the data in order to explain the observations? Are the large dielectric constants used in the present simulations realistic?

*I recommend that the conductivity issue is first discussed in the "Model setup" section and explained as how it relates to the dielectric constant in the model. Then the initial calculations should be carried out by using published dielectric constant values, instead of considering the objects to be "perfect conductors". Once this is done, new model calculations can be done by applying larger effective dielectric constants, which, however, need to be justified as being physically realistic. All this requires a major revision of the paper.***"**

Authors' response:

The major problem with the paper according to the second review were the inappropriate handling of the conductivity parameter during the setup and validation of the model. Indeed, we agree with the referee on that treating trees as "perfect conductors" is an inappropriate way and we would like to thank them for drawing our attention to this problematic point. Therefore, we accept the proposed method and will firstly use dielecteric constant values from relevant publications, do the model calculations with them and omit the "perfect conductor" part. Furthermore, more attention will be payed for the conductivity parameter and any discrepancies will be discussed more thoroughly in the revised paper.

However, the model results still imply that trees at NCK have greater (120-130) dielectric constant values than those reported in relevant publications (25-85). Please note that the dielectric constant of living trees is highly dependent on the actual conditions of the trees, especially on their moisture content. As we do not have data about the wetness of the trees we are bound to fit the dielectric constant to the measured shielding profiles. Another reason for this that we can not model the exact, rather complex geometry of overlying branches and the foliage in our 2D model. This introduces an uncertainty in the dielectric constant values so we use an effective dielectric constant which is a bit higher than reported values. Again, this difference originates from the unkown moisture content and the complex geometry which can not be fully incorporated in the model. Realistic dielectric constants of the trees could be derived with this method if the wetness of the trees would be known and the 3D geometry of the tree and the foliage would have been modeled in more detail.

Major point 2:

Review:

"*Finally, I wonder why the authors do not consider possible seasonal changes in the objects' conductivity when discussing the seasonal (winter, summer, and spring) variations. Especially since ground moisture and various degrees of wood wetness, which vary with season, are expected to affect the conductivities, and therefore the model predictions.*"

Authors' response:

The principal aim of this study is to correct for the shielding effect in the long-term annual averages of PG at NCK. The wetness of the trees and ground moisture should indeed have an annual variation but we do not see such an effect in the shielding profiles measured in the winter and summer unambiguously (Fig. 4a-b in the manuscript). Furthermore, the seasonal variation in the PG is present in the uncorrected data as well so it is not

introduced by the correction. This phenomenon is worth investigating, however it lies outside of the scope of this paper.

Minor point 1:

Review:

"*Text in Page 6 and Figure 2: It is not clear why the mobile sensor fair-weather PG measurements range from 0.0 to 0.7 kV/m and those of the stationary sensor range between 0.0 to 0.275 kV/m.*"

Authors' response:

It is an unexpected behaviour indeed as the two instruments are of the same type (Boltek EFM100 field mill). The two field mills have different sensitivites. Moreover, during the field measurements the orientation of the head of the field mills differed from each other. In case of the stationary field mill, the head was oriented downwards whereas in case of the mobile one, the head was pointed upwards. The different orientation distorts the ambient atmospheric electric field in case of the two instruments differently. The field mill with the upward orientation (the mobile instrument) measures higher PG values as equipotential lines are somewhat denser at the top of the mounting pole. On the other hand, the downard-faced stationary field mill measures smaller PG values as its mounting pole shields the ambient electric field. Please note, however, that we are not interested in the absolute PG values in this study rather in the relative PG, the ratio of the PG measured by the two instruments after cross-calibrating the two field mills. We will describe this part more carefully in the revised paper.

Minor point 2:

Review:

"*Is it justified to have 3 and 4 significant figure accuracy for the quantities shown in the various tables? How can you have such accuracy when you deal with measuring a quantity that is highly variable?*"

Authors' response:

Thank you very much for your pertinent remark. This level of accuracy is indeed too much for so highly variable parameters. As this problem was noted by the first Reviewer as well, we addressed it in our response to the first review. We will handle the accuracy problem more carefully and pay more attention to the error propagation and uncertainties throughout the revised paper. For a more detailed answer please see our response to the first review.

Minor point 3:

Review:

"*In page 13 the authors rely on Figures 5 and 6 to conclude that the time series of the mean annual PG values at NSK are similar with those at Swider, Poland. However, I note that: (a) there is no Swider data plotted in Figure 5 (!), and (b) the upper panel in Figure 6 shows large differences in magnitude and variability between the electric field annual means at NSK and Swider. How can the authors claim that the two time series are well correlating? From what I see, there seem to be a problem with the fair weather field measurements done at Swider; this is also recognized by the authors but not explained (see lines 281 – 285). The Swider mean fair weather E fields are on the average too large,*

exceeding in most cases 200 V/m. I suggest the NSK-Swider comparisons to be omitted. "

Authors' response:

Figure 5 do not contain the Swider PG data, it is used to demonstrate the similarities between the NCK and Swider data which cannot be seen on Figure 6 because of the different value ranges these two datasets bear. To demonstrate the strong correlation (Pearson's correlation coefficient is 0.8) between the long-term PG data at NCK and Swider we plotted the annual corrected PG means at NCK on Fig. 5/a and the annual PG means at Swider are presented on Fig. 6 upper panel. These two figures are to used for the comparison. All three long-term trends that were found in the corrected PG time series at NCK appear in the Swider data as well. The Swider data is only shown in Fig. 6 because this figure was adopted from another publication. We put the corrected NCK annual PG time series on Fig. 6 upper panel to demonstrate the difference in magnitude (but not in the variation) between the two datasets. PG values at Swider indeed exceed 200 V/m in most cases, however it does not mean necessarily that there is a problem with PG measurements at Swider. Please note that the Swider Geophysical Observatory is located ca. 15 km away from Warsaw, the capital of Poland and is surrounded by settlements. The anthropogenic pollution is likely to be higher at Swider than at NCK as NCK is located in a Natural Park and only a smaller city is located near it. Higher pollution decreases the air conductivity thus resulting in higher PG values. Air conductivity at Swider after the perturbed period of atmospheric nuclear weapon tests is around $3\text{-}4*10^{-15}$ S/m whereas the average fair weather air conductivity is greater by one order of magnitude (around $1.3*10^{-14}$ S/m) according to *Rycroft et al., 2000*. Please note that PG measured at different sites can have highly different magnitude. For instance, in a paper where 17 PG stations were compared, the non-disturbed PG median of all the investigated data ranged from 21 V/m to 404 V/m (Nicoll et al., 2019). The high variability of PG at different sites, alongside with the different sensitivity of instruments at NCK and Swider, are likely to be the reason behind the different absolute PG values at the two sites. Therefore, we do not agree on omitting the comparison with the Swider PG from the manuscript.

Minor point 4:

Review:

"*Finally, omit "Central Europe" from the title, Hungary is enough."*

Authors' response:

We would like to emphasize the region (Central Europe) in the title as PG data from Swider, Poland (the same region, Central Europe) were used to support that the corrected long term PG time series is correct. One of the conclusions of the submitted study is, that long-term fair weather PG time series measured at NCK are representative at least in a regional scale. That means PG recorded at NCK can mirror variation in the atmospheric electric field at least on a regional scale which do not mean necessarily that the absolute magnitude of the PG at different sites in the region are the same as the PG is highly dependent on local factors. Therefore, we suggest to retain "Central Europe" in the title.

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