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Comment on angeo-2021-7

Anonymous Referee #2

Referee 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-RC2, 2021

Referee report on the paper: "Revisiting the long-term decreasing trend of atmospheric electric potential gradient measured at Nagysenk, Hungary, Central Europe", by Attila Buzas et al., submitted for publication in Ann. Geophys.

The authors use an electrostatic potential model to estimate and correct biases in atmospheric electric potential gradient (PG) measured at Nagysenk (NSK), Hungary, which are presumed to be caused by electrostatic shielding effects of the growing trees surrounding the station. The PG measurements at Nagysenk are made for long time (> 60 years) and can be used to study long-term trends of atmospheric electricity parameters; therefore, they need to be unbiased of unwanted influences. In dealing with this problem, the authors aim also in resolving a dispute that goes on for some time, regarding the reason behind the long-term decline that has been detected in the NSK fair weather electric field.

The present study does contribute towards resolving existing issues and understanding the behavior of the long-term PG time series measured at NSK. This is a good work that is well-written and presented. I am willing to recommend its publication after the authors consider in a revision the following issues.

Major Point.

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.

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.

Some additional issues that need to be considered in a revision are:

1) 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.

2) 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?

3) 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.

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