Review of " Analysis of diurnal, seasonal and annual variations of fair weather atmospheric potential gradient at reduced number concentration of condensation nuclei from long-term measurements at Swider, Poland" by I. Pawlak, A. Odzimek, D. Kepski and J. Tacza

The main goal of this study, the assessment of the seasonal variation of the global electrical circuit from a polluted continental location, is very worthwhile and strongly appreciated, and from that standpoint every effort should be made to get the work published. The true seasonal variation of the DC global circuit is still not firmly established, even in measurements of the ionospheric potential (with contradictory seasonal variations from Muhleisen/Fishcer and from Markson). My main concern, already communicated to the second author, is the limited approach taken here: the measurement of condensation nuclei to characterize the atmospheric medium, rather than the measurement of air conductivity with a pair of Gerdien tubes. In the following, suggestions are made for improving the present approach, but in the end we suggest a conductivity approach that rests on observations readily available to the authors at the same Swider location (by Marek Kubicki).

Summary: Consider for publication after major revisions (and possible inclusion of conductivity measurements)

Substantive issues:

(1) Characterization of the medium with a CN counter

For reasons of time, the authors have been reluctant to get involved with the Swider conductivity measurements, and instead have chosen to rely on a CN counter. (The distinction between large and small ions of atmospheric electricity is not mentioned.) If the large ion population is reliably measured with the CN counter, then the air conductivity can be inferred (though this is not the best approach to obtaining air conductivity, as it is an indirect one). Unfortunately, the documentation on what is being measured with the Scholz counter is thin, even to the point of not disclosing what supersaturation value is achieved. It would also be valuable to know the instrument response to clean oceanic air but that is of course not easily obtained. In any case, a big improvement in the characterization of the Scholz counter is essential here.

(2) The conductivity model used here

Section 6 describes a conductivity model, but without sufficient details to thoroughly check its viability and origin. Equation (1) represents this model, but this is not an equation found in Tinsley and Zhou (2006). It may be an equation taken from Israel's text, but that is not identified. I for one do not recognize equation (1) from available references, though the inverse relationship between conductivity and N is reasonable. In addition, all parameters used here should be properly quantified and justified. One piece of evidence that this conductivity model is not working properly (even if equation (1) is taken at face value) is a simple check on Ohm's Law and air-earth current. One need only check equation (2) numerically (though it should be born in mind that the GEC air-earth current may vary annually). For winter, a value σ = 2.28 x 10^-15 and E = 370 V/m, J = 0.84 pA, too small by at least a factor of two. For summer, σ = 1.76 x 10^-15 and E = 370 V/m, J = 0.65 pA, and so too small by a factor greater than three. The evidence here is that the conductivity model is giving too small a conductivity, and that inference is backed up by the large values of N coming out of the CN counter (with values per cc larger than ones typically reported in the literature, even for cities, see Chalmers (1967). The authors should make these points and these calculations. They also need to take a careful look at their conductivity model.

(3) The arbitrary CN threshold of 10,000 per cc

This threshold in CN is mentioned repeatedly (lines 8, 45, 99, 103, 111, in captions for Figures 7 and 8, lines 177, 341 and 347), but it is not made clear why this value was selected. No reference is given for justification. The suggestion is that the authors are seeking a characterization of the medium in cleaner conditions, but again the best parameter for that purpose is the Gerdien-measured conductivity, since this quantity is dominated by small ions with mobilities orders of magnitude larger than those for large ions/CN). In the end, the selection of this threshold is not resolving the main troublement at present (lines 13-14 of the Abstract and lines 360-362 of the Conclusions).

(4) The main troublement of the paper

The authors' main troublement, linked directly to the important interest in the annual variation in the source of the global electrical circuit, is that the seasonal variation in N (and conductivity inferred from the model that is N-dependent) is quite small in comparison with the potential gradient, leaving the impression that NH winter is still dominating the DC global circuit, as Lord Kelvin had inferred (probably incorrectly) more than a century ago. Based on the weight of the evidence now available, something is wrong with using the CN measurements to infer the true air conductivity.

(5) The suggested resolution of the troublement

The good news here is that long-term Gerdien tube measurements of the air conductivity are also available at Swider and have been presented in earlier unrefereed work by Kubicki et al. (2007), in a work that is cited in the present manuscript (page 2) but not elaborated on in the present context. The air conductivity is always dominated by the small ions (whose mobilities are orders of magnitude larger because of their small size, but these are not the ions measured with typical CN counters). Figure 1b of Kubicki et al. shows that the wintertime conductivity of air is reduced by a factor of two in comparison with summer, and is inverse with the measured seasonal change in potential gradient, making the seasonal variation in air-earth current much less than either conductivity or PG. The most important point here is that the seasonal change in air conductivity is MUCH LARGER than the variation of CN, the main source of the authors' troublement.

One last aspect of Kubicki et al. (2016) that also has relevance is Figure 1a, showing that the seasonal variation of dust exceeds a factor of 5, and so shows the largest seasonal variation of all. It is conceivable that the dust (however it is measured, and this is not explained in this brief abstract) is dominating the removal of small ions over the seasonal cycle. One must also be aware however (from Figure 6 of the reviewed work) that the true seasonal variation of CN is not captured by the 3-hour per day sampling of CN, a serious shortcoming.

Summary:

When air conductivity measurements at Swider are considered, and the authors can surely do that in a revised submission, the main troublement of the manuscript is removed. (This finding would not conflict with the Kubicki et al findings because the seasonal issue was not focused on in that work.) The revised findings would then support the general conclusions of Adlerman and Williams (1996) that the wintertime maximum in potential gradient is caused by the enhanced pollution expected in wintertime at extratropical locations such as Poland and the UK of Lord Kelvin.

End review

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