Third Review of "Diurnal, seasonal and annual variations of fair weather atmospheric potential gradient, and effects of reduced number concentration of condensation nuclei on PG and air conductivity from long term atmospheric electricity measurements at Swider, Poland " by I. Pawlak, A. Odzimek, Daniel Kepski, and José Tacza

Moving this paper forward to publication has proven to be challenging. I have exchanged with the authors (Pawlak and Odzimek) and shared my reviews with them (and visited with them in Poland at a Workshop on the Global Circuit and at the recent AGU meeting) to assist with a difficult topic. The encouragement to interact more closely with Marek Kubicki has been useful in getting the involvement with the conductivity variable (PC), but has also led us into this "dust" topic (see below) which becomes a new complication for Swider, though one that could also be addressed with CN analysis, since dust particles are also CN. Further exchange with Marek is needed here.

The author's observation location at Swider is a polluted continental site but you have tools to investigate that. The authors have long had an idealistic goal of getting globally representative measurements of the GEC by compensation in making observations in conditions of reduced CN (<10,000 per cc), and so conditions much closer to clean maritime ones. Such conditions are unfortunately infrequent. From the time of my first review, my recommendation has been to shift attention from CN to Gerdien conductivity, as the latter is a quantity more closely connected with GEC behavior and the values remain valid even in highly polluted conditions. Here I will summarize again the difficulties with bringing in the CN in this study, and which have still not been overcome.

(1) The authors are not yet addressing the important "dust" issue with the CN measurements. Yes the appeal to the earlier Kubicki et al. (ICAE, 2007) work raises dust, so to speak. Otherwise, bringing in Kubicki et al more strongly is helpful here, because that study makes use of the all-important Gerdien conductivity data (which are rarely available in atmospheric electrical observations), and conductivity is much more important for the global circuit interest than the CN observations. The authors are now working with that archived quantity. This is valuable even if it is only unipolar conductivity data. Marek Kubicki has additional info on the dust (D) quantity that should be followed up.

(2) The use of reduced-CN data, intended to enable study of the PG observations in cleaner conditions, is still not reaching appropriately clean conditions. 10,000 per cc is not clean. The authors recognize this difficulty in multiple places (one example is lines 61-65).

(3) The authors lack a reliable means to estimate the electrical conductivity with the measured CN observations. Please correct me if my claim is incorrect. This problem stands in the way of drawing firm conclusions in this study. The absence of a simple relationship between conductivity (well measured with the Gerdien tube) and CN (measured with the CN counter) is clear from Figure 13 in the revised manuscript. The connection between conductivity and CN needs to be more quantitative than what is expressed in lines 481-482. The authors quote changes in PG (of order tens of %) as CN values are decreased from 10,000 per cc, but they do not use their conductivity model to predict what these changes should be. Even rough agreement could be used to declare partial success with the conductivity model.

Another key interest in this work is the seasonal variation in the DC global electrical circuit. I mentioned in an earlier review that this variation was not well-established, largely because of contamination from local effects. Since that time, recent work by Russian scientists (Slyunyaev et al. 2024 in JGR) has

demonstrated a northern hemisphere summer maximum by making use of Vostok, Antarctica measurements of potential gradient which are not contaminated by aerosol/CN and or dust, and so the results are convincing. I have been a reviewer of this work. These findings also raise the bar in verifying the seasonal variation of the GEC at Swider, in polluted conditions.

Summary: The authors should be encouraged to produce a revised manuscript that gives greater attention to the conductivity observations than the CN observations, and which sheds further light on the physical role of the "dust" at Swider. See further details below.

Additional comments on the revised manuscript appear below.

Lines 13-15 This is the dust issue and raises a key question that is left unanswered by the revised manuscript, even when CN is returned to as a topic of key interest. Dust particles should also serve as CN, so why does the CN counter not see the large seasonal variation evidenced in the work of Kubicki et al. (2007)?

Line 37 This question on the seasonal variation of the GEC has now been investigated in considerable detail by the Russians and papers in JGR should be appearing soon. The NH summer maximum in the DC GEC is supported by Vostok measurements of PG, running for many years.

Line 45 The statement about the air conductivity is unclear.

Line 50 It is challenging to find "low levels of nuclei number" at Swider as we have discussed. This situation thwarts the authors' main interest in finding conditions needed for a look at global representativeness. The improvement here is that the authors are now facing up to what conditions are needed.

Lines 64-65 I agree, and this thwarts the main goal of the study.

Line 101 You should say that the air conductivity is dominated by small ions, but all ions contribute. The air conductivity should also be influenced by the presence of dust, which is also aersosol. This aspect should be investigated further for Swider.

Section 2.3 Important new documentation of CN measuring equipment has now been added, including maximum supersaturation attained.

Line 142 Normally one is using "foul" for bad weather conditions.

Line 166 10,000 per cc is still a polluted condition.

Lines 171-172 Why is summer more polluted than winter? What is the seasonal variation of the dust?

Appeal to Kubicki et al. (2007) is needed here.

Line 482 The authors do not answer this question about whether PG data could ever be used to infer the annual variation of the GEC. The likely reason is that one never has a sufficiently clean condition to have globally representative results.

Line 485 Other aerosol types: the authors should strive to address the nature of "dust" in the earlier study by Kubicki et al. (2007), who first addressed the seasonal variations in Gerdien conductivity. Why

isn't this "dust" measured with CN counters? Kubicki shows an annual variation of dust substantially larger than CN at Swider. Why? Further interpretation is needed here.

My best recommendation, and in keeping with my initial review: Make use of the high-quality Gerdien tube data the authors have now demonstrated access to and go beyond what Kubicki et al (2007) achieved with the seasonal variations. The authors have made progress with organizing the Swider Gerdien data but they need to improve on the interpretation of the seasonal behavior. You will have better temporal resolution than you had with the CN data and you will be investigating a quantity (conductivity) more closely connected with the DC GEC than the CN observations. This effort may also expose more information about the dust component of aerosol (emphasized by Marek Kubicki in 2007) and its quantitative impact on the conductivity. How was that dust quantity measured? It is not explained in the abstract. One wants to understand why the seasonal change in conductivity is much larger than the variation of CN.

Adlerman and Williams (1996) is discussed in the Introduction, but after looking at the seasonal variations in conductivity and PG, the authors do not return to the seasonal aerosol variation is a plausible explanation for the seasonal variation in PG. And since the authors now have seasonal variation in both PG and PC, why don't they have a look at the air-earth current to see if this is compatible with a NH summer maximum in storm source currents?

If the authors wish to emphasize the CN observations in this study, they need to tie them in more closely with conductivity than is achieved at present.

End review

Earle Williams

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