Responses to Reviewers

Manuscript: “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, D. Kepski, and J.Tacza

We would like to thank all Reviewers of this manuscript for very careful reading and valuable comments.

Response to the comments made by Earle Williams, the Reviewer #1, in the third review:

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.

Response:

We sincerely thank the reviewer for the detailed comment and the long-standing engagement with our work. We would like to clarify a key point that may have led to a misunderstanding. Our intention in this study was not to obtain globally representative measurements of the Global Electric Circuit (GEC). Rather, our specific objective was to investigate the role of condensation nuclei (CN) in modulating potential gradient (PG) variations at a polluted continental site like Świder.

By examining PG under varying CN concentrations, including instances when CN values were relatively low as possible at the site (generally <10,000 cm^{-3}). We aimed to understand the extent to which local aerosol loading influences PG at this location. This was not motivated by wanting to generalize our results to globally representative GEC conditions, but instead to characterize the local impact of aerosols on PG behaviour, and check in what way this may alter the annual variation.

(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.

Response:

Indeed, the dust data for the Świder observatory were collected during at least some of the periods analyzed in our work. However, the old data are not digitized, and the more recent data are the result of cooperation with the State Sanitary Inspectorate, are not publicly available and their inclusion in the work would require a significant expansion of work and staff, if possible at all. Introducing such significant changes and analyzing another large data set at this stage of the publication process is impossible. Additionally, most of the information on dust until 2010 is available only on a daily basis - filters were changed once a day, obtaining the weight of dust deposited on the filter. The result of these observations would be difficult to compare with the measurements of condensation nuclei performed 3 times a day and presented in our work, especially knowing that relationship between condensation nuclei number and particulate mass is complex (Leng et al. 2014).

Leng, C., et al. "Variations of cloud condensation nuclei (CCN) and aerosol activity during fog–haze episode: a case study from Shanghai." Atmospheric Chemistry and Physics 14.22 (2014): 12499-12512. https://acp.copernicus.org/articles/14/12499/2014/acp-14-12499-2014.pdf

At present we cite some old results published by Haberko (1961) which provides some justification for the study.

(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).

Response:

We appreciate the reviewer's observation regarding the limitations of using CN < 10,000 cm^{-3} as a threshold for cleaner conditions. We fully acknowledge that this level does not correspond to truly clean air in a global or maritime context. However, this threshold was selected pragmatically, based on the data availability at the Świder station. Actual range of CN considered is between 5000-8000 cm^{-3}, which still is polluted compared to Arctic or Antarctic.

As noted in lines 255-259, reducing the dataset further by applying a stricter CN threshold would result in a significant number of data gaps. This would compromise the statistical robustness and temporal continuity necessary for meaningful analysis of PG variations. Therefore, the chosen threshold represents a balance between aiming for relatively lower aerosol conditions and maintaining sufficient data coverage for the study's objectives.

(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.

Response:

We think are simplified model was developing in the right direction, and we hope to make progress on it.

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.

Response:

We thank the reviewer for this valuable comment and for highlighting the recent findings by Slyunyaev et al. (2024), which indeed provide an important contribution to our understanding of the seasonal variation in the global electric circuit (GEC) under roughly clean conditions.

However, we would like to clarify that the main objective of our study was not to establish or verify the 'real' seasonal variation of the DC GEC itself. Rather, our aim was to investigate how seasonal changes in aerosol loading - quantified via CN concentrations - affect the seasonal behavior of the PG at a polluted continental site such as Świder. In this context, we explored whether reducing the CN concentration (e.g., to values <10,000 cm^{-3}) could help reveal clearer patterns in PG variability that may be obscured by local pollution effects.

We agree that measurements at cleaner sites, such as Vostok, are better suited for identifying unambiguous seasonal signals in the global circuit. Nevertheless, we believe that studies like ours offer complementary insights by quantifying the extent to which local aerosol variability modulates PG measurements, which is essential for interpreting long-term records from continental sites.

Moreover, even in Antarctic there may be some seasonal change in the concentration of aerosol which may have an effect on the annual PG variation. We hope this was taken into account.

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.*

Response:

We thank the Reviewer for the suggestion. After more digitization efforts we have considered the conductivity and effects of dust, and what is even more clear from the preliminary analysis, the situation is complicated, and there must be other processes like the convection affecting PG at a site like this. Without more comprehensive study and measurements, it could be very difficult to infer any GEC variation from the PG variation at such a site.

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)?

Response:

Theoretically, in the case of atmospheric electricity, the most important charge carriers are single molecules and this is why we focused on condensation nuclei measurements. It is known that seasonal fluctuations in the number of aerosol particles are not large in Central Europe (Asmi et al. 2011), but the mass of particulate matter is much higher in winter, which results in frequent occurrence of smog (Czernecki et al. 2017). The high number of aerosol in summer (which balances the CN amount from pollution in winter) can be explained by secondary aerosol formation with the participation of volatile organic compounds and high insolation events (Dall'Osto et al., 2018). Obviously, it is true that "dust" also serve as CN. It may be very interesting to study which particle sizes and origin influence the most electric field strength, but unfortunately we are not able to perform such an analysis for the study period. We conclude that there may be a dust fraction invisible to our CN counters.

Asmi, Ari, et al. "Number size distributions and seasonality of submicron particles in Europe 2008–2009." Atmospheric Chemistry and Physics 11.11 (2011): 5505-5538. https://acp.copernicus.org/articles/11/5505/2011/acp-11-5505-2011.pdf

Czernecki, Bartosz, et al. "Influence of the atmospheric conditions on PM 10 concentrations in Poznań, Poland." Journal of Atmospheric Chemistry 74 (2017): 115-139. https://link.springer.com/content/pdf/10.1007/s10874-016-9345-5.pdf

Dall'Osto, Manuel, et al. "Novel insights on new particle formation derived from a pan-european observing system." Scientific reports 8.1 (2018): 1482. https://pmc.ncbi.nlm.nih.gov/articles/PMC5784154/pdf/41598_2017_Article_17343.pdf

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.

Response:

Thank you for pointing us to the latest research results, although relating the results from such a different and clean environment is difficult to relate directly to the results obtained here.

Line 45 The statement about the air conductivity is unclear.

Response:

We clarified the sentence. Now it states: "while the air conductivity variability is higher in the summer" (line 47).

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.

Response:

We accept this remark and are aware that it is a certain limitation as to the usefulness of the Świder data for determining global changes in the electric field. We hope that now, in the revised version of the manuscript, this shortcoming has been clearly commented on.

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

Response:

Unfortunately we are limited by the site characteristics. Nevertheless, we believe that this data matters and the conclusions from this work can be valuable to the community.

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.

Response:

We admit that the emphasis of small ions here is not entirely correct. We have changed 'small ions' to 'ion mobility' in the text. We are aware of the predominance of small ions and that all ions affect conductivity, but we decided not to expand on this topic here.

In 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.

Response:

Thank you for pointing this out. We replaced "bad" by "foul".

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

Response:

We are awared that 10000 is still not clean air conditions, but we decided for such threshold to separate days with exceptionally high particle counts from our analysis and see if this improves the relationship with the electric field.

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

Response:

We believe that we explained it before. Probably this is the effect of secondary aerosol formation on sunny summer days. We think it is better to talk about higher concentration of aerosol particles than

pollution, because in winter the air is certainly more polluted. In summer, the greater part of the aerosol will be of natural origin, which we should not talk about as pollution.

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

Response:

Although we are aware that in Kubicki et al. 2007 was mentioned secondary aerosol formation that may be referred also here, we decided not to cite it here as we just describe our results here. Reference to this effect and work of Kubicki was done in new line 455.

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.

Response:

We want to emphasize again that this was not the main intention of the work. We do not agree with the second sentence. With almost 50 years of data, it is possible to find days with nearly perfect air quality to analyze global electric current, although it is true that Swider data as a whole for sure are not representative for annual potential gradient variability. Here we present seasonal variations between PG and CN trying to focus on relatively clean conditions.

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.

Response:

Dust is also measured by CN counters. The average CN number is higher in winter than in summer at the Swider observatory, but the difference is not that large in number, but great in aerosol mass (which we know from data presented in new Figure 15). It seems that different aerosol types influence electric field in different ways. This may suggest that bigger (and heavier) particles present in winter have larger effect on electric field properties than we previously thought and we should study this effect more thoroughly in the future. There is also a possibility that we should consider not solely on CN as decisive factor, but take into account also e.g. air humidity.

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.

Response:

We added new two Figures to analyse the effect of particulate matter (dust) on conductivity records and new text in section 7 to describe this new data. We hope that this will complement the previous results and provide more complete look at the Świder observatory data.

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?

Response:

In the revised version of the manuscript we paid more attention to the air conductivity data and their relationship with the variability in aerosol concentration and mass. We do not wish to extend this work further but we hope that the general relationship between aerosol conditions, air conductivity and potential gradient measurements is now more comprehensively explained in case of Świder, and the associated air pollution problem in this area. Analysis of conduction current density would extend this work enormously. In the revision we indicate that such an opportunity exists but it is also not free from problems.

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

Response:

New figures and text was added to address this issue. In the two latest revisions the CN influence is analysed both in relation to the potential gradient and the air conductivity.

Thank you for all your valuable comments on the text. We believe that the corrections presented here thanks to your suggestions have made this work more valuable.