Second 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 Izabela Pawlak, Anna Odzimek, Daniel Kepski and Jose Tacza

On the occasion of the Workshop on the Global Electrical Circuit in Warsaw, Poland earlier this month, I had good opportunity to meet with the coauthors of this manuscript to learn from them and to share my thoughts. I am attaching (after the main review) here a copy of the comments I had put together on first looking at the revision. That revision is much improved, reinforcing my earlier view that the paper is much improved and eventually deserves to be published. But for the moment, I have some very specific suggestions for a path to that publication.

Summary: Consider for publication after major revision

Suggestions for addressing remaining loose ends:

(1) Need for bipolar conductivity

A big step forward was the accessing of the Swider positive conductivity. But the very best quantity for treating the seasonal issue and the highly polluted Swider boundary layer in winter, one really wants the bipolar (total) conductivity, and based on my separate discussion with Marek Kubicki, these bipolar data are available in the Swider archive. The authors should get them and include them and come up with the best explanation for the seasonal variation of the electric field, and then make a firm judgment about local versus global influence/manifestation. In the present version, the conclusions are not firm.

(2) The expansion ratio threshold for the CN counter

Wilson (1897) showed (in work that eventually won him the Nobel Prize in physics) that when the expansion ratio of 1.25 was exceeded, a rainlike condensation on small ions appeared in his cloud chamber. This is essentially the threshold the authors have provided in supplying valuable new information about their CN counter. But they need to try to establish whether the counter was designed with the CTR Wilson finding in mind, so that they were just short of activating on small ions. I am not quite sure how that can be accomplished, but some effort should be made. At the very least, the Wilson (1897) work and threshold should be mentioned in the revised text.

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

Through the response to initial reviews, and through discussion in Warsaw, the authors have conveyed to me their general strategy in their selection of this CN threshold: an effort to achieve conditions sufficiently clean so as to achieve a globally representative measurement at Swider. (In their response to the reviews, they cited Landsberg (1938), Schonland (1953) and Mohnen and Hidy (2010), and now I have had a chance to consult the latter two references. Perhaps the best information in this context is contained in Table 2 of Mohnen and Hidy, and I encourage them to continue to focus on this information.

Having considered all of this information, my general reaction is that the value of 10,000 per cc is still quite large given the perceived objective of the authors. To be more specific, 10,000 per cc is large than the average values for 7 of 9 categories listed in Table 2. So the authors can say that they have selected a threshold value less than the means for "Town" and "City" in Table 2, but they need to say that this

general level is still “polluted” and that the finding that they are still unable to make globally representative measurements of the global electrical circuit from Swider is very believable. Remember the initial success of the Carnegie Institution ocean measurements of electric field (and CN!) in achieving global representativeness, but their values for CN were of the order of 100 per cc, and so two orders of magnitude less than the authors’ selected threshold. More discussion in the manuscript is needed about the full range of CN conditions one can have.

(4) Reference to Adlerman and Williams (1996)

The authors have been interested in checking on the role of aerosol variation on the seasonal variation of the global circuit. It would be valuable, after they checked carefully the seasonal variations of the Gerdien bipolar conductivity at Swider, to ascertain whether the inferences made by Adlerman and Williams (1996) are valid.

(5) Globally representative measurements at Swider

This issue came up at the recent Workshop on the Global Electrical Circuit and is deserving of some additional discussion in the present context. Two key points are worthy of discussion. First, Marek Kubicki informed me about days in which the Gerdien bipolar conductivity is essentially constant over 24 hours. This evidence for a fixed medium (yes, in polluted conditions and low values of conductivity) may provide an advantage to seeing global signals, and deserves additional attention. (The more common behavior at Swider is for highly diurnally variable conductivity.) Second, the boundary layer over land is often stabilized at nighttime by virtue of temperature inversions. This situation, well known to meteorologists but not exploited by atmospheric electricians in earlier work, might also provide a condition when convective transport of electric space charge is zero, and for which Ohm’s law is fulfilled. This makes it more likely that a measurement in this special time interval would be globally representative, because local perturbations are strongly suppressed.

I am now wandering beyond the scope of the manuscript, but the authors may benefit from giving these ideas further thought.

End review

Earle Williams

September 22, 2024

Comments communicated to the authors prior to the Workshop:

September 6, 2024

Below are my thoughts in blue text, in response to your responses on my review.

Brief responses to Response to Reviewer comments document

(Sorry I have that document only as PDF so will indicate here where I am inserting my remarks to respond. I will send return that document to you when I send these comments.)

Paragraph after “Summary: Consider for publication after…

Bravo for bringing the Swider conductivity to the table. This is a BIG improvement in the manuscript.

(1) Characterization of the medium with a CN counter

First paragraph of Reply:

I understand about interest in concentrating on the PG and aerosol, but if you want to address important scientific issues, then the emphasis on conductivity is also important. The interpretative attention on conductivity could be extended, in my opinion.

Second paragraph: This is a big improvement in the documentation of the instrumentation, though one key item about the expansion ratio is still needed. (Reviewer #2 also emphasized the need for greater elaboration on the instrumentation.)

With regard to "the adiabatic expansion ratio of 1: 1.25 could be achieved"

An accurate estimate here is quite critical, based on the earlier findings of C.T.R. Wilson with his cloud chamber. See in particular Wilson (1897), page 286, which states: "For adiabatic expansion to result in condensation in saturated air free of all foreign nuclei, we have seen that the final volume must exceed 1.252 times the initial volume." As shown later by Wilson, and in his Nobel Prize work, that amounted to a 4-fold supersaturation, which is just sufficient for nucleation on negative ions. So it is important to know for sure whether the expansion ratio in the reported work was consistently below this critical threshold established by Wilson. Maybe your instrument was designed that way, but this should be stated.

(2) The conductivity model used here

The discussion here is very much improved. Thanks very much for your efforts.

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

You aren't responding in any detail here to my comment, other than giving references I have not yet consulted. (The revised text has more info, to be sure.) My take-away here is that you are trying to find sufficiently clean air so that the measurements of PG at Swider can become globally representative. To justify such a procedure, you need to take care to compare this selected threshold with values one expects in places where globally representative measurements of PG can sometimes be achieved, most notably over clean oceans and at Vostok and Concordia stations in Antarctica. Recent analysis of many years of the Vostok PG data (paper being submitted for review now) do support a NH summer maximum of the GEC, contrary to the PG maximum in wintertime that you are documenting at Swider.

(4) The main troublement of the paper

After your Reply, first sentence:

Yes, but if the interest in the annual variation in the global electrical circuit is an important issue, why isn't this aspect given more attention in the revision? (It got more attention in the first version, but now you are (appropriately) bringing in the conductivity.) The polluted atmosphere as Swider is aliasing the GEC, and makes the PG measurements there unrepresentative of global variations.

"It did not, however, since the PG still has a maximum over winter months…"

OK, but you are not giving sufficient attention to the role of the polluted atmosphere at Swider in causing that maximum, in my opinion. (For me, Swider is the classic example of how wintertime pollution prevents access to globally representative measurements.)

"Perhaps we have not emphasized this conclusion of our paper enough."

I completely agree with this assessment, and think some changes are in order so that it is appropriately emphasized.

(5) The suggested resolution of the troublement

After Reply: The conducitivity measurements have been carried at Swider…"

Did Marek use previously only positive values, in the works that were discussed? (I will find out soon enough as I am meeting with Marek on Monday in Warsaw.) I had thought that Marek had access to bipolar conductivity from the Gerdien, an instrument I remember seeing at Swider on occasion of my last visit there in 2015.

"There is also a distinct effect of conductivity to lowering the affecting CN concentrations but there is no clear seasonal effect"

Isn't this tantamount to saying that even when you select data for which the CN are below this pre-selected threshold of 10,000 per cc, that you cannot undo the effect of the continental pollution at Swider in destroying a global signature? The attempt to find sufficiently clean air at Swider to enable global representativeness is not possible because those times are few and far between.

"We conclude the conditions of low condensation nuclei conditions does not change the character of the annual variation of the PG at Swider."

This finding needs to be better expressed in the revised manuscript, in my opinion, though I confess to not having read carefully the entire revision yet. You need to underline the lack of global representativeness in my opinion.

I am sending these comments with an aim to set up a discussion when I see you both next week in Warsaw, but only if you are interested, of course.

Regards
Earle