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Natural Hazards and Earth System Sciences
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Discussions

Interactive comment on "*Brief communication* "Earthquake–cloud coupling through the global atmospheric electric circuit"" *by* R. G. Harrison et al.

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Response to Anonymous Referee 1

We are grateful to the Referee for the thought given to our manuscript.

In addition to the current physical explanation in an ideal circumstance, I would like to suggest the authors to make experiments after considering more actual factors in specific earthquake events in the next study.

We would be happy to make such experiments, and now explicitly mention the need for experimental work in the conclusion of the paper.

Response to Anonymous Referee 2

We thank the Referee for the points made.

First of all I would like to mark that the "Charge exchange" contradistinction to the transport of ionization to the cloud level mentioned in the conclusion is a kind of slyness because there no publications proposing such ideas as ionization source transport to the level of cloud formation.

Our purpose in using the expression "charge exchange" was merely to highlight the vertical flow of electric current (i.e. a rate of exchange of charge) as an agent of communication between one part of the atmosphere and another. This also makes it clear that the ionisation source itself, gaseous, particulate or otherwise, is not transferred.

The basic numbers and ideas taken for calculation in the paper are very far from reality. For example, the radon rates before earthquakes usually reach values near 2000 Bqm^{-3}...so the ionization rate is much more strong than taken for calculation.

We agree that radon exhalations in such circumstances could well be much greater than the values assumed in our calculations, which was our reason for providing the Liperovsky et al (2005) reference. However we have based our estimates on the limited experimental data in which both atmospheric electrical changes and radon concentrations were measured simultaneously, and therefore we did not extrapolate beyond the range observed. If such a conservative approach is not adopted and a larger radon concentration is instead assumed, it will, if anything, greatly increase the conduction current density and the associated cloud effects anticipated.

The attributing of IIN to the Wilson supersaturated camera has nothing common with reality. It was shown that ions are very good centers of water vapor condensation in the normal atmospheric conditions [Svensmark et al, 2006]

To answer the referee's query, we should first explain that ions, particles and droplets are all distinct parts of the atmospheric aerosol size spectrum, spanning several orders of magnitude in size, from cluster-ions of a few nanometres to micron-sized droplets or larger (e.g. Carslaw et al, Cosmic rays, clouds and climate, Science 298, 1732-1736, 2002). In Svensmark et al (2006), cited by the referee, the ion-induced formation of particles occurred in a filtered but SO2 enriched atmosphere, under ultraviolet light, which was interpreted as due to formation of H2SO4 clusters. These clusters are many orders of magnitude smaller than the water droplets which form clouds, and the relevance of ion-induced nucleation in the atmosphere to the water vapour condensation forming cloud droplets has not yet been demonstrated.

Our point about the Wilson cloud chamber was made to emphasise that the cloud chamber requires vastly different levels of water vapour saturation from that naturally occurring in the atmosphere. This is because water droplet condensation on ions in the Wilson cloud chamber requires water vapour supersaturation of 400% or more, whereas actual supersaturation in the natural atmosphere is typically two orders of magnitude smaller, insufficient to achieve condensation on ions.

In [Sekimoto and Takayama, 2011] it is demonstrated that the formation of cluster ions takes place under any level of relative humidity. Just the IIN leads to formation of ion clusters of the aerosol size what was demonstrated in the paper of Pulinets and Ouzounov, 2011. And these ions are seeds for the earthquake clouds formation.

We agree that the hydration of small molecular clusters of ions is sensitive to humidity, indeed we have observed this in our own experiments (Harrison and Aplin, Water vapour changes and atmospheric cluster ions, Atmos Res 85, 199-208, 2007). However, a hydrated cluster ion (typically of nanometre or smaller size) is very much smaller than the minimum size of particle required for cloud droplet formation, which is typically a few hundred nanometers in diameter. The growth of ion clusters to these sizes in clean air requires many hours (e.g. Yu, F. and R. P. Turco, From molecular clusters to nanoparticles: The role of ambient ionization in tropospheric aerosol formation, J. Geophys. Res., 106, 4797-4814, 2001). Hence, even if this growth process can occur without interruption in the real atmosphere where conditions are much more variable,

the duration of the growth will allow the cluster ions concerned to be displaced by a considerable distance from their point of generation (about 100km for 10 hours, with a small surface wind speed of $3ms^{-1}$). A rapid but continuous process, such as the flow of electric current, is therefore required if surface information is to be imprinted in clouds directly above seismic activity.

The paper also does not take into account the bipolarity of the earthquake cloud effect which is formation of linear cloud structures, and also formation of linear translucidation in the cloudiness.

As we remarked in our second paragraph, many different cloud structures have been suggested as associated with seismic activity. It is not our purpose to comment on the validity of these claims, but we feel it is necessary again to reinforce our point that both the effects of orography, and detailed cloud climatologies in the region concerned must be well understood before anomalies can be appropriately statistically evaluated. Accordingly, we are confining ourselves to explaining what appears theoretically feasible in terms of a simple cloud structure– horizontal layer cloud - in order to provide a physical context in which these claims could be assessed.

And finally the oversimplified presentation of the Global electric circuit which does not take into account the conductivity anisotropy at the altitude higher than 60 km regardless this task was resolved as early as in 1998.

The global electric circuit is, in itself, a helpful conceptual framework (see, for example, K.L. Aplin, R.G. Harrison and M.J. Rycroft, Investigating Earth's atmospheric electricity: a role model for planetary studies, Space Science Reviews 137, 11-27, 2008) in which specific details can always be developed further. At the level presented here, the point being made is merely that such a system permits vertical current flow. Conductivity variations in the upper altitudes where the conductivity is already substantial will not affect this interpretation.

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