

Interactive comment on “Measurements of electric charge separated during the formation of rime by the accretion of supercooled droplets” by R. A. Lighezzolo et al.

Anonymous Referee #3

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1. Page 2: “The authors concluded that the magnitude of the charges on the fragments was too small to be able to account for the observed electrification rates in thunderstorms”. This appears again on page 8: “Although they concluded that this mechanism is unsuitable to explain the observed electrification rates in thunderstorms. . .”. I also thought this, but looking back on the paper, it neither states that it is of little significance, nor is it actually a small charge. In fact the estimate is two orders of magnitude greater than the charge transferred during collisional charging during those particular experiments. My suggestion is to remove these two statements from this paper.

2. One hypothesis that should be mentioned for further investigation by the scien-

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tific community relates to the consequential implication of graupel charging positively (and contributing to the lower positive charge region) from the emission of negatively charged secondary ice crystals. In a situation where the storm has an inverted polarity, with midlevel positive charge sandwiched between upper and lower negative charges, the amount by which the secondary ice production helped enhance the lower positive charge region, should be the same amount by which it reduces the lower negative charge region given that the proposed charging mechanism of the secondary ice particles presented here is seemingly of one polarity only (i.e. doesn't produce positively charged splinters). Is there any evidence that the lower negative charge region is less substantial than the lower positive charge region? If not, this is something future observational studies may be able to look for. If current evidence doesn't support this, and the lower negative charge centre is comparable in magnitude, this then implies that this splintering charging mechanism may not be having as significant an effect as suspected. This is worth discussing, at least hypothetically, in the paper.

3. In the event that the magnitude of charge separation via this mechanism is enough to contribute significantly to the formation of the lower positive charge region (and negate the lower negative charge region), in the grand scheme of thunderstorm electrification, it is not a big player given its limited scope of explanation of all thunderstorm charging observations. In other words, like the inductive theory, if it has a non-zero effect, the effect can be seen to be limited and supplementary to a greater charging influence (the Relative Growth Rate theory). I always feel it's important to point this out in humility, as it helps to minimise confusion for readers of the literature in this subject area.

4. There is a reference to one of your previous studies on page 7 on the first paragraph starting "there are few previous studies". While I appreciate why you put it in there from an advertisement point of view, I do feel that it is out of place and unnecessary. It adds nothing to the points being made about H-M splintering and its presence is only very weakly justified. However, what really concerns me about it being present is that the interpreted observation being referenced is contentious and not supported yet by

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any other researcher. If highlighted in this way in the new paper, it suggests greater importance than is scientifically justified. This is an issue because it leads readers in this field astray and could cause all kinds of confusion and difficulties in the future for us. My suggestion is to earnestly consider removing the reference, or raising my points in the paper to ensure the reader is aware, in the interest of scientific clarity and honesty.

5. Please could you consider avoiding the use of the term “non-inductive mechanism”? In the penultimate paragraph in section 5, I would suggest rewording the sentence as: “In addition, charges larger than around 5 pC found on the precipitation particles could also be acquired by collisions with other ice particles (ice crystals or other graupel pellets) via field-independent collisional charging described by the Relative Growth Rate theory (. . . same references. . .).” This is for a very important reason: avoiding the wrongful labelling of the Relative Growth Rate theory and the knock-on consequences. The explanation below is not directed at the authors of this paper explicitly, but at the community in general.

There is a meme (a transmitted idea) in the literature by many, many authors in this subject area (mostly not by those explicitly working in cloud charging I hasten to add, but by readers of it) that promotes the use of the term “the non inductive charging mechanism” or equivalent phrasing. This is such an awful term to use and it is wrong for multiple reasons that go far beyond “just a name”.

Firstly, it is not reasonable to describe something positive in terms of what it is not: the ‘NON-inductive’ mechanism. You wouldn’t describe a male as a “non-female”, a dog as a “non-cat”, or a particular star as a “non-Sun”. Secondly, it implies there is a single specific mechanism with the official name “the non-inductive mechanism”- there isn’t. There exist ‘field-dependent’ mechanisms and ‘field-INdependent’ mechanisms, and therein lies an important point: these are categories of charging mechanisms and there is more than one such mechanism in each category. So, back to the first point: the term “non-inductive” should never be used in favour of “field-independent”-which at

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least does positively describe something in terms of what it actually is. Relating to the second point: given that there are many such field-independent mechanisms, the one almost always being referred to when the term “non-inductive mechanism” is used is the one which is favoured now and for which there is substantial supporting evidence, and that specific mechanism has a specific name: the Relative Growth Rate theory. To fail to refer to it by its official name, and instead use a grammatically inept label to a non-theory, is somewhat insulting toward all the research that has gone into supporting it, and is akin to describing the theory of General Relativity as “the non-electromagnetic theory”-it tells you nothing and demonstrates vast ignorance of what’s being discussed. This is one factor helping to measurably foster a lack of understanding in the general thunderstorm community of progress in the cloud charging sub-community and of the Relative Growth Rate theory itself.

In summary, please consider using “field-independent mechanisms” when referring to the mechanism class, and “the Relative Growth Rate theory” when referring to the specific mechanism.

I also want to point out that I don’t support the idea that the “non-inductive theory/charging/mechanism” term needs to appear once in a paper so that readers will understand what is being referred to from historical uses of the term. This merely propagates the term like a virus in the literature and doesn’t correct it. If it is to appear at all, it really should be attached to an explanation flagging it as a poor term. Instead, simply consider using the correct terminology as in the example given at the start.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 23349, 2009.

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