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Interactive comment on "Effects of Wegener-Bergeron-Findeisen Process on Global Black Carbon Distribution" *by* Ling Qi et al.

Anonymous Referee #1

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In this work the authors seek to study the effect of the Wegener-Bergeron-Fidensen (WBF) process on the scavenging efficiency and global budget of black carbon (BC). The authors compile a large set of data and use a subset to generate a parameterization of the effect of temperature and ice mixing ratio on the scavenging fraction of BC. The parameterization is implemented within GEOS-Chem to study the global BC budget. Although the usage of English can be improved in some places, particularly in the introduction section, the work is readable and easy to understand. The compilation of data is also commendable and a strength of the paper. However the methods proposed do not really lead to the conclusions stated in regards to the WBF process. In fact, it is the effect of the temperature on black carbon scavenging, above all, what is being studied, not really WBF. As such the authors should modify the scope of the paper before it can be published in ACP.

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Discussion paper

General Comments:

My main criticism of this work is that that even though the authors seek to investigate the effect of the WBF process on the BC scavenging efficiency, no explicit parameterization of WBF (or even any other cloud microphysics) is implemented. In fact, the authors investigate the effect of using a temperature dependent scavenging parameterization of the scavenging efficiency (as the fraction of condensate being ice in the GEOS-5/MERRA is prescribed as a function of temperature, hence not independent), instead of the WBF process.

Data at a single site is used to develop the supposed WBF parameterization. However many factors may result in the observed temperature dependency, that are not directly related to WBF (fewer droplets at low temperature, weaker updrafts, weaker rates of conversion from hydrophobic to hydrophilic BC, decreasing BC concentrations, and so on). It is incorrect for the authors to assume that their parameterizations actually represent the WBF process, and in that sense the title ("effects of WBF. . .") and the scope of the paper is misleading. I'd recommend the authors should modify their paper to accurately represent the scope of their work and avoid statements where WBF is tacitly assumed as responsible for the observed effects. It is present form the paper is not suitable for publication in ACP.

In many places the authors describe their simulations as "improvements". However it is not clear what the model is being compared against. In fact, a control simulation, with the default configuration of GEOS-chem is missing.

Specific comments:

Page 1, Lines 10-30: The abstract does not mention anywhere that a parameterization is proposed.

Page 1, Lines 20-30. When using words like "discrepancy" and "increase" please indicate what the control simulation is. The way it is written is very confusing.

Page 1, Line 36. Aerocom is a project not a model. This sentence must be rewritten.

Page 2. Lines 14-30. It is not clear the relevance of this paragraph as WBF is by definition a process occurring in mixed-phase clouds.

Page 2, line 40. WBF is not the only factor that can explain such dependency.

Page 3, lines 3-5. How is this relevant?

Page 3, line 15. Increased with respect to what?

Page 3, Line 24. A very small fraction of black carbon (even hydrophobic) acts as IN and typically nothing detected at T above 255 K (see Murray et al. 2012).

Page 6, Line 5-10. This is a circular argument.

Page 7, line 18-20. How is this washout definition related to the one given before?

Page 7, Lines 30-32. This is an error. Even for hydrophobic BC, only one in a million is an efficient ice nuclei (See Murray et al. 2012).

Page 8, Eq. 5, 6. Nothing in these expressions indicate that the WBF is parameterized. In fact they are mostly a function of T.

Page 8, Lines 10-17. IMF is largely a function of T in GEOS-5, and the two expressions may not be independent.

Page 9, Line 14. If a reanalysis is being used (MERRA or MERRA2) it is incorrect to refer it as GEOs-5, which is the underlying model (see Rienecker et al. 2008).

Page 9, line 40. Only a very small fraction of BC particles are ice nuclei.

Page 10, line 22. The Figure barely shows any improvement.

Page 10, line 24. This is not shown.

Page 11, line 14. What is the control simulation for these experiments?

References:

Murray, B. J., O'sullivan, D., Atkinson, J. D., & Webb, M. E. (2012). Ice nucleation by particles immersed in supercooled cloud droplets. Chemical Society reviews, 41(19), 6519-6554.

Rienecker, Michele M., et al. "MERRA: NASA's modern-era retrospective analysis for research and applications." Journal of Climate 24.14 (2011): 3624-3648.

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