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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Global patterns of lightning properties derived by OTD and LIS" *by* S. Beirle et al.

Anonymous Referee #2

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This is a well organized and splendidly written manuscript. It present results of long-term statistics of lightning events detected by the OTD and LIS sensors on board NASA satellites. The authors provide detailed analysis of lightning parameters derived from the optical data, and attempt to distinguish between the characteristics of lightning in different geographical areas as a function of season, time of day. This analysis is of importance to lightning climatology per-se and to the derived LtNOx produced by lightning, which constitutes a significant input to the chemistry included in global climate models.

There are some issues that deserve the authors' attention, and a minor revision is needed. The manuscript can be accepted to NHESS provided adequate responses are given to the points below.

1. No reference is made to the special properties that lightning in coastal regions have, and the analysis is divided strictly according to land/ocean areas. Many research papers have been published which account for the special properties of lighting flashes occurring within several $\sim$10s of km from the land-sea border. These flashes exhibit properties which differ from marine and oceanic flashes, as well as from those that occur inland. For example: (Seity et al., JGR, 2001; Petrova et al., Atmos Res. 2014;). The 1°x1° grid resolution is probably too coarse to achieve this distinction and so important details differentiating between lightning characteristics are lost. 2. What is urgently missed from the analysis presented in section 3 is a depiction of lightning activity as a function of time, namely an hourly diurnal cycle. While the authors use the day/night division in tables 2 and 3, the type of hourly lightning number (perhaps shown by grouping flashes in different continents or latitude strips) is of unique importance to the study of the Global Electrical Circuit as exemplified by the Carnegie Curve (see for example reviews by Rycroft et al., SSR, 2008; JASTP, 2012). The authors should add this extra analysis to this section, and compare it with the daily curves of the ionospheric potential gradient (Harrison, 2013). 3. The authors correctly point out that the IC/CG ratio is latitude dependent and that one cannot use a single "US type" value (if such a value even exists). The topic should be expanded in view of earlier work, such as Price and Rind (GRL, 1993), Soriano et al. (JGR, 2007) and Mackerras et al. (JGR, 2006). It is always better to relate these differences to thermodynamics and cloud physics considerations, as exemplified by Mushtak et al. (Latitudinal variation of cloud-base height and lightning parameters in the tropics; Atmos. Res., 222-230, 2005) than to just leave them as a statement. 4. The discussion of the origin of the land-ocean differences in lightning properties seems incomplete without focusing on the properties of thunderclouds over the different surfaces. See for example the paper by Williams and Stanfill (The physical origin of the land-ocean contrast in lightning activity; C. R. Physique 3, 1277-292, 2002). The authors need to enlarge this part of the manuscript. 5. The discussion of the possibility to compare flash multiplicity derived from NLDN and other like systems and the LIS/OTD statistics is interesting

and offers intriguing possibilities. I expect the authors to review the topic more thoroughly and to suggest a practical way of doing this. The multiplicity of strokes is a common and well-used parameter in lightning climatology, with special importance to the detection efficiency of lightning detection system. It is latitude/season dependent and has been thoroughly studied by many groups (see Pinto et al., 1999; Schulz and Diendorfer, 2006). It would be interesting to compare this space-based flash duration vs. multiplicity values to the video-based studies conducted by Fleenor et al. (AR, 2009), Saba et al. (JGR, 2010) and Ballarotti et al. (JGR, 2012). 6. The issue of positive ground flashes is not well covered and is discussed only in passing, and in my mind is the weakest point in the paper. The reference to the classic Kitagawa and Michimoto (1994) paper misses many new findings concerning winter thunderstorms and I urge the authors to update their section 4.6 to reflect this (the Rakov and Uman 2003 book is a good place to start). Also, it should be noted that the high percentage of +CG does not occur only in winter thunderstorms but also in MCSs at different stages of evolution (Stolzenburg et al. MWR, 1994) and as shown by Lyons et al. (Science, 1998) may reflect ingestion of smoke aerosol particles into the clouds that alters their charge structure and increase the fraction of +CGs.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/2/C496/2014/nhessd-2-C496-2014-supplement.pdf

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