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Reply to 'Comment on *The physical origin of the land–ocean contrast in lightning activity*'

Earle Williams *, Sharon Stanfill
Parsons Laboratory, Massachusetts Institute of Technology, Cambridge, MA 02139, USA

Pontikis et al. [3] (hereafter PHM) have recently raised questions about the validity of analyses in Williams and Stanfill [5] (hereafter WS) that support the thermal hypothesis over the aerosol hypothesis as the primary explanation for the land–ocean contrast in lightning activity. A recent literature exchange on the same general issues has recently appeared [2,6], and an extension of the island analysis in WS [5] has also been completed [9]. We welcome this opportunity to address in print those issues discussed with the Guadeloupe group at a Workshop on the Physics of Lightning there in May 2004.

In raising their questions, PHM [3] address three arguments from WS [5]. Here we respond to them in the same order.

(*i*) PHM [3] assert that island maximum elevation is an important additional parameter in the analysis of island lightning, that features of the distribution of island areas are creating an artificial transition in the ATD (Annual Thunder Day) number-vs-island area plot, and that the ATD parameter has questionable value as an electrification index.

We agree that elevation may be important for this problem, one reason these numbers were included in WS [5]. The difficulty here is that terrestrial islands with ATD lightning documentation are insufficiently numerous to allow control studies with island elevation alone. Island area and island elevation are clearly positively correlated, as PHM [3] show. For self-similar conical islands with fixed aspect ratio, the island maximum height will scale as the square root of island area. In practice, the power law scaling is weaker than $\frac{1}{2}$ (not shown), but still clearly evident. The main point is that if ATD or lightning activity is positively correlated with island area, then these parameters will also correlate positively with island elevation, as PHM [3] demonstrate.

These authors then go on to argue that the "absence of islands higher than 1000 m with areas lower than 600 km^2 [...] is, most probably, at the origin of [...]" the transitional scale found in WS [5]. If the island area–elevation relationship follows a power law, one does not expect to find a surplus of island elevations greater than 1000 m for areas smaller than 600 km^2. In a later study, Williams et al. [9], using different sets of islands and different measures of electrification, as suggested by PHM [3], found the same general transitional area.

Evidently, PHM [3] are of the view that the role of island elevation is the forced lifting of air "by large and mesoscale perturbations". In such a case, we would expect lightning activity to be induced in both daytime and nighttime, since these large-scale wave phenomena are not diurnally driven. In contradiction to this expectation for the island of Guadeloupe, all lightning flashes documented over this island by the Lightning Imaging Sensor in space have occurred in the daytime hours [9], when heating of the island surface is expected. This would seem to affirm the importance of "local thermal convective developments", as hypothesized in WS [5].

* Corresponding author.
E-mail address: earlew@ll.mit.edu (E. Williams).

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PHM [3] question the use of ATD as a measure of island thunderstorm activity. When the WS [5] study was undertaken, ATD observations [10] were the only data available to characterize electrical activity over different islands in any uniform way. WS [5] used these data at face value. Undoubtedly, different procedures are followed for thunder day reporting on different islands, and this may afford one explanation for the apparent discrepancy between Sumatra and Guadeloupe noted by PHM [3]. Alternatively, this discrepancy may arise because Guadeloupe and Sumatra are widely separated islands and are situated in different synoptic regimes, both meteorologically and oceanographically. In the more recent study of island lightning [9], islands were grouped geographically as a control on this possible source of variance, and we still found transitional island areas similar to that in WS [5].

(*ii*) PHM [3] disavow the twofold interpretation of the global distribution of 'warm' clouds documented in WS [5]. PHM agree with the aerosol role, but they disagree with the role for updraft, as voiced earlier by Jorgenson and Lemone [1]. PHM [3] essentially deny the presence of radar 'first echoes' in the 'warm' portions of continental convection [7]. This claim is not supported by either simulations [4] or personal communication with Rosenfeld et al. (2003), or by observations [5,7]. With regard to the simulations, we welcome quantitative comparisons on the effects of aerosols and cloud base updraft speed on model simulations by the Guadeloupe group and Hebrew University.

(*iii*) Regarding the postulated CCN concentration threshold for continental-style convection, we reject the idea of a fixed cutoff, independent of the cloud updraft velocity, based on other modeling results [4] and personal communication with Rosenfeld et al. (2003). The authors' conclusion that the "observed contrast is more consistent with the aerosol hypothesis than with the thermal hypothesis" is simply not justified without a consideration of the thermodynamics differences in instability and cloud base height between the "green ocean regime" and the "dry-to-wet" transition [8]. We agree with the washout of aerosols in any regime with abundant rainfall [8], and the consequent attenuation in the importance of CCN on either the regime-integrated rainfall or electrification.

In their concluding remarks, PHM [3] urge greater care in selecting indices for cloud electrification. Williams et al. [9] have made progress here more recently by using lightning flash rate and flash rate density from the Lightning Imaging Sensor over islands. The general findings for transitional island areas are essentially the same. Additional evidence is presented there in favor of the thermal hypothesis. PHM [3] also recommend the use of GCMs to compare calculated and observed distributions of global lightning. This approach is questionable at present because the crucial aerosol and ice microphysics are inadequately implemented in the large-scale models, including those at the mesoscale. Better evidence on the quantitative comparison of the thermal and aerosol hypotheses will come from the kind of convective scale observations and modeling with which we have all already been engaged.

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