

Interactive comment on “A mechanism for biologically-induced iodine emissions from sea-ice” by A. Saiz-Lopez and C. S. Boxe

Anonymous Referee #1

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This article reports a very interesting model to explain the high levels of iodine oxide (IO) from biogenic I₂ and HOI emissions. While the model appears compelling and attractive, it should be highlighted that there is an urgent need to study the biological (algal?) sources of these iodine emissions. Biogenic iodine pumps such as kelp (*Laminaria* sp.) are well-studied in temperate latitudes, however it is unclear to what extent the physiological knowledge of iodine metabolism in such seaweeds (and their emission rates of inorganic iodine and iodocarbons) can be transferred to polar, high-latitude environments where fundamentally different environmental conditions exist. Sea ice is a peculiar, unique algal habitat, and there are good reasons to assume that the halogen metabolism of its algal communities may exhibit several unique features as well. At the current stage, we know virtually nothing as to whether sea ice algae actually me-

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tabilize iodine, whether they accumulate it, what the physiological function of iodine would be, and in what form they would contribute to gaseous iodine emissions into the atmosphere. I would therefore caution against using the knowledge from ecosystems and model organisms in temperate climates to draw far-reaching conclusions about biogenic iodine fluxes in the Antarctic.

Also, I should highlight that the biological sections of this paper include a couple of inaccuracies: While the accumulation factor of 30,000 applies to iodine accumulation by the temperate kelp *Laminaria digitata*, it is far from certain whether this can be transferred to any Antarctic algae or whether *Laminaria* might not be a fairly unique case. Contrary to what this paper states, it is not really known either whether this also applies to microalgae / phytoplankton. Also, there seems to be some confusion about the localization of haloperoxidases and iodide oxidation (cf. the recent paper by Verhaeghe EF et al., 2008: *J. Biol. Inorg. Chem.* 13(2), 257-269).

To be very clear, I would consider the model presented here a very attractive hypothesis rather than a sound explanation of an atmospheric process in the Antarctic environment, which needs to be substantiated both by field / biological oceanographic studies as well as laboratory experiments with live / cultured representative model organisms from the Antarctic coastal and sea ice habitats. This should have been stated more clearly, but it does not reduce the value of this work.

In other words, this constitutes a very valuable piece of work that should lead to some interesting follow-up studies.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 8, 2953, 2008.

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