

***Interactive comment on “Can we trust simple marine DMS parameterisations within complex climate models?” by P. R. Halloran et al.***

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**Response to Referee 2: S. Elliott**

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We are very grateful for Scott Elliott's positive and emotive review, and are encouraged that he finds himself in 'complete agreement' with our major conclusions. We make all of the specific changes mentioned in the review. To make the reader aware of the issues carefully highlighted by the referee, without imposing on the work that the referee's group appear to be undertaking at present, we point the reader to Scott Elliott's review encouraging them to read the enclosed discussion.

**Referee point 1**

I find that I am in complete agreement with their major conclusions. In fact our group is now working with a combination of high profile correlations (e.g. Vallina and Simo, 2007) and process models of varying complexity. But given an opportunity here in the present review, I would like to propose ahead of time that the arguments be extended in several ways.

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## Response 1

We found the referee's discussion surrounding the issues associated with process based models most interesting and provocative. We feel that this discussion is beyond the immediate scope of this paper, but will be of considerable interest and importance to many of the readers. To both acknowledge the considerable knowledge and thought that the referee has put into this discussion, and to maintain the focus of our study on empirical DMS parameterisations, we take advantage of the open review format and direct the reader to the reviewers thoughts by adding a reference to the review in page 1307, line 8 of the original manuscript (see text A below).

**Text A:** '...Vogt et al. (2010) and Elliott (2009). It should however be noted that many of the broad issues discussed in this paper may also apply to process based DMS models, and indeed to the simulation of variables other than DMS (see Elliott (2010)).'

## Referee point 2

There are a few minor editing problems I can point up for the Halloran et al paper. The most important is consistency of the referencing style. The authors have tended to cite the same work in several ways. The original Kettle climatology offers the best example. It occurs as Kettle et al. (1999) in some cases -which is correct- or as Kettle 2000 in others -which is not.

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## Response 2

All incidences of the Kettle citation have been standardised to either; Kettle et al. (1999) when we refer to the DMS database as it existed in 1999, or Kettle et al. (2000) where we refer to the updated climatology (which is the climatology used throughout this study). The abbreviation 'Kettle climatology' is introduced at the start of the 'Predictive Capacity in the Present Ocean' section, whereafter the abbreviated form is used to improve readability. We find occurrences where the abbreviation for Simo and Dachs (2002) (abbreviated to Simo (2002) - specified in page 1299 line 2 of the original manuscript) and Anderson et al. (2001) has not been adhered to, and modify these. We intentionally revert to the unabbreviated form of all references for the conclusion section to provide clarity for any reader focusing primarily on this section.

## Referee point 3

Halloran et al. emphasise the ability of the simple equations to outperform the Kettle interpolations-extrapolations in a synthetic experiment involving new data. My take is that this is understandable, not critical and probably fortuitous to a large degree. Kettle tuned carefully to the older values, which now appear to be incorrect.

## Response 3

We agree with the referee's point, and certainly did not intend to in any way criticise the Kettle climatology, however as we say in page 1301, line 9 of the original manuscript 'This approach [i.e. testing the climatology alongside the parameterisations] allows us to subjectively assess whether running a present-day coupled ocean-atmosphere climate simulation with an interactive DMS scheme can capture present day DMS fluxes

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equally well, or better than, a model where DMS fluxes are calculated from the standard climatology' - we believe that this is an important aspect of the paper. Acknowledging the point that the outperforming of the climatology by the parameterisations is likely to be to some degree fortuitous we amend the line in the abstract (page 1296, line 11) '...find them to perform marginally better than the standard DMS climatology at predicting observations from an independent global dataset' to '...find them to perform comparably with the standard DMS climatology when predicting observations from an independent global dataset', and the line in the conclusion (page 1311, line 4 of the original manuscript) saying '...can predict present day surface ocean [DMS] with a level of skill better than and similar to that of the Kettle et al. (2000) climatology respectively.' to read '...can predict present day surface ocean [DMS] with a level of skill comparable to that of the Kettle et al. (2000) climatology.'

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