We would like to thank the topic editor for his/her comments. We hope the following responses and the revised contents in the manuscript sufficiently addressed the comments.

1. All three referees commented on the models producing features (especially a "hotspot in the Southern Ocean west of South America" – referee 1) where there are no fCO2 data. You really should respond to these comments in the final text – the comments will be public and readers will be able to judge your responses. I think you can look at the independent (input) variables: do these extrapolated fCO2 features correspond to features in one or more input variables? Especially important for confidence in the models: is the set of input variable values closely approximated somewhere else so that the model features where there is not fCO2 data are in fact constrained by fitted fCO2 data?

   Response: We added a paragraph in section 7 to discussed the hotspot issue. The hotspots in the Southern Ocean west of South America can also be seen in the modeled CO_2 of Takahashi et al. (2014). For comparing with their results, we changed the color scheme of figure 3 to the same color scheme. At this stage, it is not realistic to precisely answer the question "is the set of input variable values closely approximated somewhere else…", because this requires removing input variables one-by-one and retraining all models multiple times. Tracking "somewhere else" is not possible for FNN. It is possible for SVM and SOM, but requires revising their source code. In section 5, we added a paragraph to plain that the SOM and SVM do not have over extrapolation risk. Since the hotspots appeared in all models, the risk of accepting them would not be high.

2. Page 2 equation (1). Referee 3 commented on your use of a single value of trend for all locations. You have partly answered this at the end of section 5 but you should relate the statements there to the referee's question. "normalisation" does not make a clear link.

   Response: We added "The trend in Eq.(2) cannot be modelled directly by the models. One approach to deal with the problem is to normalize the measurements to a reference year using a global rate and only model the nonlinear component." in section 4 to make "normalisation" more clear.

3. Page 2 equation (2) and dSST. I suspect that dSST is not described properly in the text. "difference between the monthly and annual means of SST" is not continuous, it means 12 discrete values, same values on January 1 and January 31 and different values on January 31 and February 1. But your response says you are avoiding this problem.

   Response: You are right. The dSST is not continuous in time. While month changes mechanically, dSST reflect better the effect of season on seawater property. For example, the season does

affect the equator as much as the Northern and Southern oceans. We revised the last sentence in section 2 to address this.

4. Section 4. Perhaps there should he sub-headings to make clearer that page 3 lines 20-25 are about SVM, page 4 lines 1-15 are about FNN and page 4 lines 16-25 are about SOM.

Response: We added sub-headings as suggested.

5. Appendix A3. I think the kernel function definition (A15) should come directly after A(9) where φ is introduced.

Response: We rearranged the equation as suggested.

6. Equations (A12) and (A14). Somehow "c" seems to have been replaced by "α" but the relation between them is not stated.

Response: We are very grateful that this comment makes us to re-examine equations in the appendix. Derivations for the equations are very long and complicated in textbooks. In trying to use a minimal number of equations to deliver the concepts of the models, we made mistakes, especially for SVM. Now we added the missing links between "c" and "α". We avoided using the dot product operator for vector all together. It can be a trap for mistake.