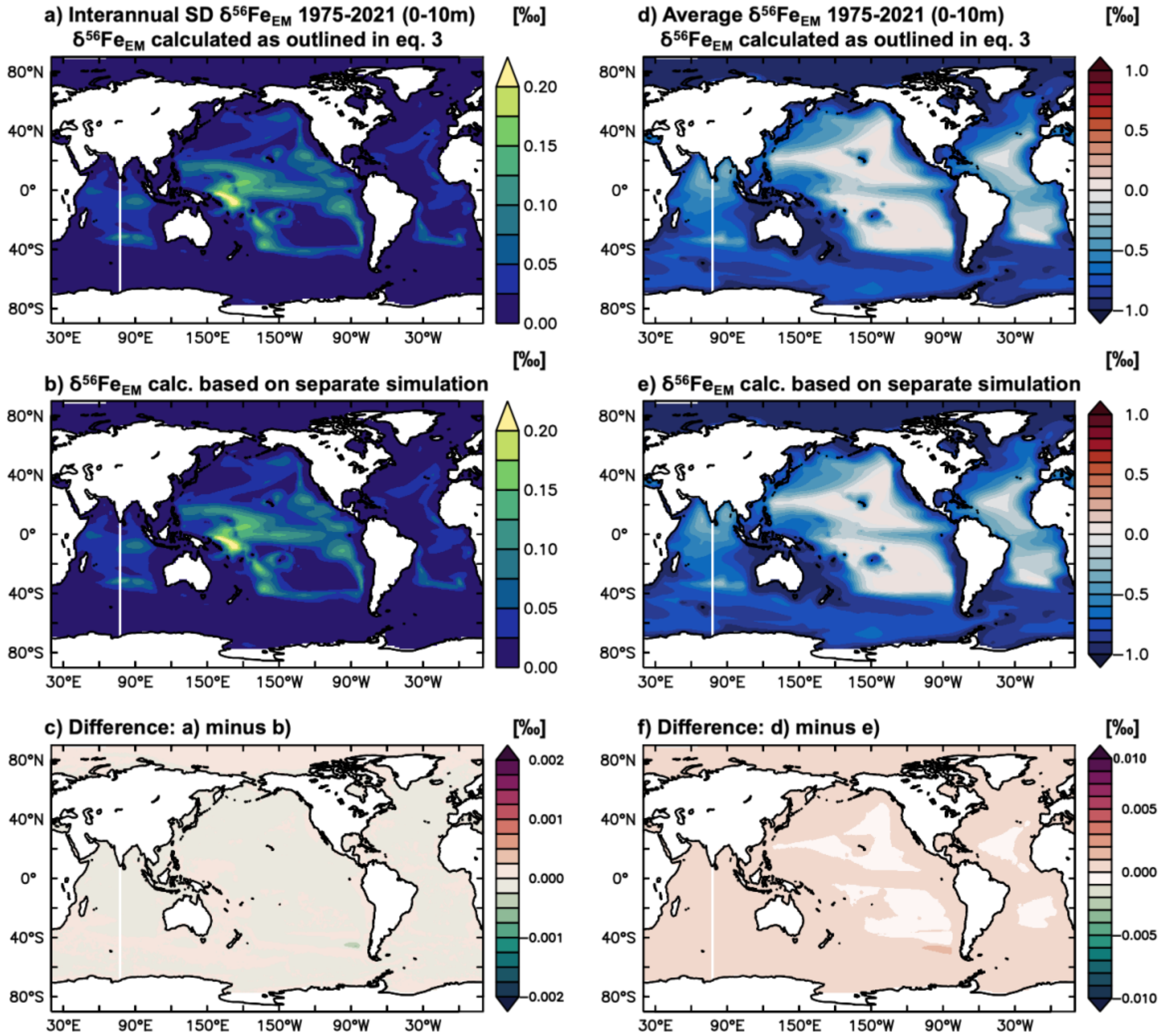
The authors applied a global ocean model describing the Fe and 56Fe cycling to study the impact of climate variability on surface distribution of iron concentration and its isotopic signals. The model considers different isotopic compositions of sources and fractionation by biological uptake and organic complexation of iron. Their previous publication (König et al. 2021) presented the modelled distribution of delta56Fe and a thorough comparison with observations. In this study this model was driven by different climate forcing data and a series of sensitivity experiments were conducted to quantify the contribution of single factors to the inter annual variability of delta56Fe. Strong responses of delta56Fe to climate change were found in the model. I find the idea to study climate variability with Fe isotope fingerprints highly interesting and the article was well-written and easy to follow. However, I have some concerns about the analysis of model results and kindly ask the authors to give explanations for the following points:

1. Line 120-123: The effect of two single components, fractionation by biological uptake and organic complexation, on delta56Fe is estimated from the difference between an experiment with all components switched on and another one with only one component switched off (Eq. (1) and (2)). But the effect of the third component, isotopic compositions of endmembers, is estimated in a different way (Eq. (3)) which assumes that the three components act independently on delta56Fe in a linear relationship which is not true. An experiment with all endmembers set to 0 ‰ is to my opinion necessary to disentangle the effect of all single components, as the authors mentioned themselves as well (L. 125-126). If this experiment was already done I would like to see if the result is identical to the estimation presented now in the manuscript and why.

We did not originally perform additional experiments with endmembers set to 0‰, since the way our model is set up, the impacts of uptake/complexation fractionation and endmember effects add up linearly. To confirm this, we have now run such a simulation (for the hindcast set-up) and compared the corresponding $\delta^{56}Fe_{EM}$ (calculated similarly as in eq. 1,2) to the "residual" $\delta^{56}Fe_{EM}$, as described in eq. 3. This comparison shows that, beyond rounding errors, the calculated $\delta^{56}Fe_{EM}$ and SD $\delta^{56}Fe_{EM}$ are the same (within 0.002‰ and 0.0005‰, respectively) - confirming the linearity and our original approach.



We agree that the linearity of the endmember and fractionation effects is not obvious, and will add this figure to the supplement and reference the figure in the main text (line 122 of the original manuscript):

“Thanks to the additive nature of fractionation and endmember effects in our model, which we confirmed for the hindcast experiments (Figure SX), the endmember effect $\delta^{56}Fe_{EM}$ could be calculated by subtracting the two fractionation effects (Eq. 3) from $\delta^{56}Fe_{diss}$.”

2. Line 135-138: If I understand it correctly, the authors calculated SD of each distribution of delta56Fe resulted from Eq. (1) to Eq.(3) and then the fraction of each single SD in the sum of them. SD can demonstrate the variability around the mean state but tells nothing about the mean state itself. Responses of the three single components to the interannual climate variability can be reflected in SD but also in the mean state of delta56Fe. So I don’t quite understand why just SD of different runs are used to examine the contribution of single components.

We agree with the reviewer that climate variability causes substantial changes in the mean state of $\delta^{56}Fe_{diss}$ and its components, especially over the longer time scales of the climate change simulations. However, since the standard deviation was calculated over the entire time period (i.e., not relative to a running interannual mean), changes in the mean state are accounted for, and are, indeed, responsible for the majority of "variability" in the climate change simulations (e.g., compare Fig. 2a vs. Fig. 6 in the submitted paper). We do agree that the contribution of mean state changes to temporal variability over the 21st century should also be emphasised, and will include this in Section 3.1.2 (l. 169):

"Whereas over the shorter period of the hindcast experiments (1975-2021), elevated $\delta^{56}Fe_{diss}$ SD is mainly due to temporal variability around a mean $\delta^{56}Fe_{diss}$ value, for the climate change experiments, elevated $\delta^{56}Fe_{diss}$ SD is also related to a change in the mean $\delta^{56}Fe_{diss}$ over the next century (Fig. 6a)."

Furthermore, the sum of three SDs is not the same as SD delta56Fediss of the experiment with all components switched on, due to the non-linear relationship between the single components and different signs of the effects. The authors only discussed about the latter in the manuscript. I have no doubt that the results of the three experiments are interesting and can help us to understand how the marine Fe isotope cycle responses to climate variability. Different SDs of the three distributions indicate that each component is differently sensitive to climate variability. However, the interpretation of the relative importance in percentage needs a justification.

The earlier response hopefully persuades the reviewer that the system is linear, and we discussed how overlaps and variability on different frequencies contributed to the results in the original manuscript. The percentage plot is aimed to illustrate what effect can be considered to be dominant in different ocean areas.

At this stage I would like to encourage the authors to revise the analysis and interpretation of the model results. After that, I would be happy to provide more detailed comments.

We have responded to all the comments provided by the reviewer (as well as those proposed by reviewer 1) and hope that this has provided the necessary reassurance to the reviewer.