This paper presents a useful contribution to the attribution of the 2023 high temperature anomaly, discussing the role of shipping emission sulphur reductions.

The paper is novel, topical and makes interesting findings on the short-term climate response to policy implementation.

I have questions over the quantification of the response as it seems very much on the high-side and this is not discussed. Also, for the paper to be of wider use its findings need to be contextualised better. The results also need to be better presented as it was hard for me to work out what they did or what the results meant, so the average reader might struggle to make sense of the results.

Nevertheless, I strongly support publication of this work after changes to better contextualise and explain their results.

Major points

1. Lines 32-41. From their figure 1 as radiative forcing of order 0.2 Wm-2 is stated, supported by three references. None of the references refer to the 2020 IMO regulation. Hodnebrog et al. does not even mention shipping emissions I believe. So the authors need to explain where their 0.2Wm-2 estimate come from. Other published work, not referenced, estimate forcing values of 0.2 Wm-2 over oceans – which is around 0.12 Wm-2 globally (e.g. Yuan et al., 2024; Forster et al., 2024). The authors also, and most importantly, diagnose their 0.2Wm-2 forcing estimate from their figure 1a's absorbed solar radiation. However, eyeballing the figure it looks to be at least 0.4 Wm-2. I think these forcing values need to be properly quantified. As if their forcing is closer to 0.4Wm-2, this would go a long way in explaining their high surface temperature response. Ideally, they should run an ensemble with prescribed SST and sea-ice to make a proper estimate of effective radiative forcing.

2. Fig 2 is quantifies the temperature response to shipping and is crucial but I found Section 2.2 did not do a good job describing this figure or its quantitative results and it took me as a reader a long time to work out what the figure meant. Panels 2 a) and b) don't seem to be referred to. The green lines seem the important ones to compare to the orange to estimate the role of shipping but these are not referred to or discussed. The figure is clearer than the text in my view. I think the section should be rewritten to walk the reader through the figure and quantitatively estimate the temperature change from shipping emission changes.

3. The paper would generally benefit a discussion about the high forcing estimate its uncertainty and how this may flow through into their high temperature response. This temperature response should also be put in the context of the other causes of high 2023 temperatures, such as the El Nino. The El Nino discussion as written, makes it look as if El Nino is not important – it may not be

important for modulating the shipping response, but otherwise it was. Also Forster et al. 2024, suggest that the aerosol radiative forcing, actually became more negative overall due to Canadian wildfires. In their global estimates, this more than compensated for shipping emission changes. Forster et al. may not be right, but this provides important context. This should be mentioned here.

4. The methods could be clearer – were the emission changes applied globally or over the ocean. I think comparing a 10 member ensemble to a 100-member control warrants explicit discussion as well. I notice that the green lines are more variable in Figures 1,2 and 3. This obviously affects the overall results and the uncertainty of the attribution, but how?

Other comments

5. Line 87. Planned non-CO2 policies do affect carbon budgets to a certain extent as non-CO2 warming scenarios are factored into carbon budget estimates. I think your statement needs more explanation.
6. Line 95, I think it's not only policy we need to worry about but possible human and natural emission changes in general?