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Comment on wcd-2022-32

Anonymous Referee #3

Referee comment on "Reconciling conflicting evidence for the cause of the observed early 21st century Eurasian cooling" by Stephen Outten et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-32-RC3, 2022

This paper reviews the evidence for the causes of the 1998-2012 wintertime cooling over Eurasia. This has been a controversial topic with some studies arguing that the cooling trend was caused by sea ice loss, while others have argued that the trend was caused by internal variability and that sea ice loss has played no role. The authors review the literature, find common ground, examine where disagreements still exist, and provide some guidance for ways forward. They argue that both views can coexist and that whether the cooling trend was caused by sea ice loss or internal variability shouldn't be a 'yes-or-no' question.

Overall, I thought this was a very nice and balanced review of the literature. Although there have been quite a few reviews on the broader topic of Arctic-midlatitude links, the specific topic of Eurasian cooling is certainly worthy of its own review. I think this will be an important paper that will help to motivate future research. I do however, think there are a number of issues that need to be addressed/clarified before publication.

The authors argue that a better question to ask is "whether the cooling trend was more likely given the observed sea ice loss". Does this mean (1) more likely compared to some period prior to the large observed sea ice loss (or preindustrial period), or does it mean (2) more likely compared to a hypothetical world with global warming like we have observed, but with no sea ice loss. These are two different question with potentially two different answers. It is entirely plausible that the answer to question (1) is no, but the answer to (2) is yes.

These questions are interchanged throughout the paper and in the wider literature. Most modelling studies that are discussed (including the *a* and *b* categories outlined in section 3) are attempting to answer question (2), or whether sea ice loss by itself makes these trends more likely (which is equivalent assuming the responses can be added linearly). However these studies are often misinterpreted as addressing question (1). I would argue that question (1) is more practically relevant.

It is important to note that the reason these questions are different is not just that thermodynamic warming from CO2 will make the cooling trend less likely. It is also because of dynamical effects. The same models that show a high pressure response over the Urals in response to sea ice loss (sometimes with weak Eurasian cooling) also show a low pressure response (with strong Eurasian warming) in response to CO2 warming without sea ice loss (e.g. Hay et al. 2022). These dynamical effects tend to oppose easch other resulting in weak circulation responses overall in historical simulations and future projections. Thus, for the models where the answer to question (2) is yes, the answer to (1) is still likely no. Some clarification and discussion of these issues are needed.

The authors argue that whether the cooling trend was caused by sea ice loss or internal variability shouldn't be a 'yes-or-no' causal relationship. However, most modelling studies don't treat it as a 'yes-or-no' question. Most modelling studies just happen to find that the forced response to sea ice loss shows no or very little to cooling, but that does not mean that they did not consider that it could be a combination of both. A new perspective that authors point out is that it is possible that sea ice loss increases the magnitude of internal variability, such that the cooling trends could more likely even without impacting the forced response. This idea seems to form the bases of one of the main messages of the paper, that both the 'ice-driven' and 'internally-driven' viewpoints can coexist. This is an interesting idea that is plausible, but it is basically a hypothesis with little evidence to support it at this point.

The authors do show that because of the higher standard deviation of Eurasian temperatures, the trend was made more likely during 1998-2012. However, the authors cannot attribute these changes to sea ice loss without additional evidence. Much like with seasonal mean temperatures, the standard deviations will vary from decade to decade just due to internal variability, so attributing these changes needs much more careful analysis. There is even a period in the 1960's and 1970's that had similar or even higher standard deviations relative to the recent period (Fig S4b), when sea ice was (presumably) much higher than today.

The authors should either provide additional evidence to support this idea, or make it clear that this is just hypothesis that could be considered. To explore this further, the authors could do some very simple analysis using the data that they already use. For example, do the standard deviations of Eurasian temperatures (or the probability of a large cooling trend) increase along with sea ice loss in the CESM-LE?

Although overall, I thought the interpretation of the literature was well done, there was one issue I thought was misrepresented. Throughout the text, it is portrayed that most modelling experiments find some weak Eurasian cooling in response to sea ice loss. I don't think this accurately represents the published literature. The studies that are cited to support this all use single models and/or small ensembles. Surveying the literature of these single model studies that focus on the cooling is inadequate because it will be susceptible to selection biases. If groups run the modelling experiments and find cooling, they will focus on it, but if none is found the study will not focus on this (if it gets published at all). This could be made worse if small ensembles are used, such that the responses seen may not even be real.

To minimize these issues, large, multimodel ensembles should be used to more accurately reflect the state of the modelling evidence. These should be given much more weight than the single model studies, even if they don’t specifically focus on the Eurasian temperature response. So taking a look at large multimodel ensemble studies:

Ogawa et al. 2018 : No cooling from an average of 6 models (Fig 1c) and 5 out of 6 models show no cooling (Fig 2c).

Blackport et al. 2021: No cooling from an average of 4 models (Fig 12d) and all 4 models show no cooling (Fig 13 b)

Liang et al. 2021: Average of 9 models shows no cooling over central Eurasia (Fig 7a).

Smith et al. 2022: Average of 16 models shows no cooling over central Eurasia (Fig 1b)

Hay et al. 2022: Average of 5 coupled models show no cooling over central Eurasia (Fig 6a).

Note that these last three do show some weak cooling only over East Asia, but this is not where the cooling trends are in observations. Also since these three studies do not show individual models results, we do not know how many show cooling, but if it is not seen in the average, it is unlikely that many do (but some probably do).

My interpretation of this is that most models show no cooling, but some do show weak cooling. This is just my interpretation of the literature, and the authors may disagree (maybe I am missing other large multi model studies that do show more evidence of cooling?). If so, this should be backed by evidence that does not only rely on a couple single model studies.

More specific comments:

L9: “with a small contribution from sea ice” Isn’t the whole point that we do not know this? It could no contribution (as indicated by most models), or it could be a small contribution (as indicated by some models), or it could be a much larger contribution (if models substantially underestimate the response).

L15-16: This seems a bit misleading. My reading of the paper is that internal variability had a substantial contribution to the 1998-2012 cooling trend and will continue to play a large in future trends. On the other hand sea ice loss may have altered the likelihood of the trend and may contribute to the likelihood in the future. Treating these two factors on equal footing in the abstract seems a bit misleading.

L122-125: By ‘disappear’, do you mean that they actually disappear or do they only become non-statistically significant?

L247: Related to my comments above, Honda et al. 2009 and Liu 2012 are bad examples to use here. Liu et al. 2012 used only 20 years from a single model for both their high and low sea ice runs, which is too few realizations to be meaningful. Honda et al. 2009 ran their experiments for 50 years from a single model, but “selected” the 28 years where the signal was the largest and note that the responses were much weaker if all 50 years were used.

L248-249: “but also an inconsistency…” What is this based on? From what I have seen all sufficiently large ensembles show a consistent warming signal over recent decades (e.g. Blackport et al 2021, McCusker et al 2016, in addition to some the studies cited later in the paragraph).

L508-513: Is the trend more likely because of the larger standard deviation or is the larger standard deviation because of the trend? The trend itself will cause a larger standard deviation.

Additional references:

Hay, S., and Coauthors, 2022: Separating the Influences of Low-Latitude Warming and Sea Ice Loss on Northern Hemisphere Climate Change. *Journal of Climate*, **35**, 2327–2349, https://doi.org/10.1175/JCLI-D-21-0180.1.

Liang, Y.-C., and Coauthors, 2021: Impacts of Arctic Sea Ice on Cold Season Atmospheric Variability and Trends Estimated from Observations and a Multimodel Large Ensemble. *Journal of Climate*, **34**, 8419–8443, https://doi.org/10.1175/JCLI-D-20-0578.1.

Smith, D. M., and Coauthors, 2022: Robust but weak winter atmospheric circulation response to future Arctic sea ice loss. *Nat Commun*, **13**, 727, https://doi.org/10.1038/s41467-022-28283-y.