

## Celebrating Thirty Years of SPARC science - webinar series

13 June 13:00 UTC – online

### Questions that were not answered during the webinar

Veenus Venugopal: *Thank you, Professor. A basic question, if you could please answer. How long it will take the polar vortex changes to reflect in tropics through the Brewer-Dobson circulation? Like Sudden stratospheric warming?*

Ted: There are two aspects to this question. When the polar vortex changes, it does so for a reason, and that reason can also affect the tropics. This is the case with the onset of a SSW, which is driven by planetary-wave drag and this affects the tropics simultaneously as it is the balanced response to a torque. During the SSW recovery period, however, which can take a month or more, the disturbed polar vortex can affect the upward propagation of planetary waves, and this will affect the tropics. The time for that to happen is more like a week or more, reflecting the propagation time of planetary waves.

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Alexey Karpechko: *Ted, what kind of experiments would you like to see in CMIP7, if there will be CMIP7? Maybe model intervention experiments?*

Ted: Talk to Douglas Maraun, who is leading a discussion group around a possible JetMIP. However, to be useful a MIP needs a well-focused and well-posed question. And I would think that many of the mechanistic questions I have discussed are best addressed using single-model experiments.

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Georgios Fragkoulidis: *Very nice talk, thank you! I have a question on statistical significance. Given all the issues with hypothesis testing, does controlling for the false discovery rate provide any additional knowledge or rigor in a manuscript? It seems to me that it is ever more often being asked by reviewers, as a standard procedure.*

Ted: There is nothing wrong with addressing the question of whether a claimed effect might be due to chance, and indeed one must be able to address that question. I am simply arguing that we should do it with appropriate methods. The first thing is that there really does need to be a chance process behind it, so that the concept of sampling uncertainty is relevant. (That is not always the case with the significance tests we do, e.g. the difference between CMIP5 and CMIP6.) For purely data-driven approaches, there is a very high likelihood of false discovery and controlling for that is crucial in order to get anything sensible. But those are not the kinds of studies I was talking about. When there is a well-defined physical hypothesis being investigated, or some particular concern (e.g. in a study targeted at a particular risk), then prior probabilities, or consequences (what statisticians call 'utility'), enter into the calculation. That was the point of my quotation from Jeffreys in the context of the Kretschmer et al. (2020) study.

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Peter Hitchcock: *Forgive the second question here, but what role do you see, Ted, for using seasonal or subseasonal predictability as a means of exploring and understanding circulation changes?*

Ted: The simple answer is: enormous. I think that is the only way that we can develop confidence in our models for representing the causal linkages that lie behind circulation changes, and for estimating conditional probabilities for multiple causal factors (which can sometimes be quite nonlinear) given the small sample size provided by the observational record. It will not remove all the epistemic uncertainty around climate change, but it would strengthen the confidence we have in the storylines themselves (and their probabilistic consequences in terms of climate risk).

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Overland: *Was Bergen school more observation based and UK more fluid dynamics based?*

Ted: I'm not really the one to answer that question, and I'm not sure which UK school you are referring to.

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john scinocca: *Does the storylines/causal network approach suggest different or more useful types of experiments for climate models than the usual CMIP/IPCC cycle of experiments.*

Ted: The current approach for constructing storylines out of CMIP is based on regression across models, which is very crude. I think we would do better with single-model large ensembles from a smaller number of models, say 8-10. At least that way, we would have much better estimates of the forced response in each model, as well as the internal variability of each model. I think it is the very imprecise estimate of the forced responses in the models which is holding back progress on understanding the differences in the forced responses. Of course, such models wouldn't necessarily span the range of possible storylines, but that's not really necessary in a storyline approach. Perhaps for a particular purpose one might drive a model (e.g. through some kind of nudging) towards a particularly dangerous storyline for a high-end risk assessment, but I don't see CMIP being the place for that.

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Josie: *Thank you Professor! I was wondering is there any study focused on the causality analysis for the midlatitude jet streams latitudinal shift? As you mentioned before that analysis only depends on the dynamical paradigm. I am kind of interested in the casual link analysis for the midlatitdude weather and climate. Many thanks~*

Ted: Not as such, to my knowledge. However, a causality analysis can just be regression with a conscious attention to potentially confounding factors. You probably know that there was a claim of a causal mechanism by Kidston & Gerber (2010) which was subsequently debunked by Simpson & Polvani (2016). (Kidston & Gerber had not properly controlled for confounding factors.) See Breul et al. (2022, <https://wcd.copernicus.org/preprints/wcd-2021-78/>) for an update on this topic. But ideally, the causality should proceed from a thermodynamic starting point.