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Interactive comment on "ATTRICI 1.0 – counterfactual climate for impact attribution" *by* Matthias Mengel et al.

Anonymous Referee #2

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The authors present a new methodology to derive a counterfactual climate, relevant for impact studies. This is a very relevant and topical area to be looking at, and the PIK team are world leading in this area, so I was excited to see this paper from their team. However, I do think the paper is not exactly up to their usual standard, and in particular a thorough understanding of the attribution science is not clear in this paper.

My main concern is the framing and interpretation of the question being posed. I have to confess that it took me reading the entire paper to fully understand what they were doing, and how it is different to other methods. I think the authors need to work on posing the problem much earlier, and discussing how it fits in to the wider attribution methods. In IPCC WG1 attribution is often split into trend attribution, and event attribution – the authors mention this at one point, but do not really explain how their methodology fits into this concept. I was also very surprised not to see any of the available attribution impact work cited in this paper. I feel this is a very large omission from the paper, and the authors need a paragraph or two, maybe in their discussion section, introduction section, or both, that describes these papers, and explain how the author's current views and concepts fit into that. From the top of my head, these papers are all very relevant: Hydrology: Pall et al, 2011, Schaller et al, 2014; Health: Astrom et al, 2013, Mitchell et al, 2016; economy: Frame et al, 2020.

In a similar context, I also believe the authors need to highlight the weaknesses in their approach, as compared to other approaches. Two obvious ones are: 1) any attempt at making a counterfactual climate is difficult, and can be done multiple ways. Many other methods therefore provide an uncertainty in their modelled climate, but you do not. I understand why you don't, but the implications of this are important. 2) Many authors have argued that impacts are felt in the extremes of climate more so than elsewhere, that is why counterfactual attempts are often made with very large model simulation sizes. This is not so easy in your methodology, although I can see ways forward for it – this should be discussed.

The authors have worked in IPCC WG2 for a long time, and maybe a bit in WG1, but they need to be aware that their readers might be solely in one WG (or none at all), so the subject specific language needs to be very simple for a paper like this.

Other corrections

âĂć Title "counterfactual climate for impact attribution" – I see why you have this title, but it seems that your work would be very useful âĂć Line 9: "anthropogenic" is needed before climate change âĂć Line 9: "Other drivers change according to observations". Actually I think the other drivers should remain the same according to observations. âĂć Line 19-21: This sentence is very confusing without reading the paper, I suggest making it stand alone. âĂć Line 26: Citation needed. Haustien et al, 2017 is a good one, but there are others. âĂć Figure 1: In this figure you show "climate change" as the affected quantity – this should just be “climate”. Likewise for the driver in the second panel. I also do not agree with the caption that this is how the IPCC frame attribution. âĂć Lines 31-38: I like this description, and it is now clearer in my head what you are doing. If this section can be summed up on 1 line for the abstract, that would really help make things clear from the start. âĂć Line 63-66: This section is confusing me, in much the same way the end of the abstract did. Specifically, you say impact attribution does not need to address the causes of climate change. So, what is it addressing? You could state that explicitly here. I also think the attribution community would see this differently, and there is a danger that people will now be confused over what this term means. âĂć Data section: What is the spatial scale of the dynamically downscaled data? How much do we trust this data, especially in poorly observed parts of the world? What are the implications for these problems on the questions posed? âĂć Line 101: Lots of work has been done on pattern scaling recently, so I think a more modern approach should be cited here. E.g. Herger, 2015, although many others exist as well. âĂć Line 101-105: This paragraph makes it much clearer in my head what you are doing. I would use some of this text to explain this earlier, especially about the non-causal link with global temperature. âĂć Page 7: Why are the distribution names bolded? âĂć Line 144: I understand why you are including hurs, and commend it, but it is a bounded quantity (i.e. nominally constrained between 0-100), so would that cause problems when modelling with a Gaussian? âĂć Figures 4 and 5: These are very nice and informative figures. âĂć Lines 255-256: I agree it is rare, but there are still numerous studies that have done this (see major comment). âĂć Line 277-279: You should state clearly here why your data is useful in a complementary way to Gillett et al

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