EGUsphere, referee comment RC2
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Comment on egusphere-2022-105

Anonymous Referee #2

Referee comment on "Causal deep learning models for studying the Earth system" by Tobias Tesch et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-105-RC2, 2022

The work seems to bring causality research in AI (more specifically Pearl, 2009) into hydrologic analysis. While I am no expert on causality analysis, it occurs to me there is some novelty in the authors' valiant effort in venturing into this realm alone and presenting a stab for hydrology, but there are also some concerns regarding clearly defining the real merit of the method. If the authors call for more research in this direction, the limitations and potential should be carefully discussed. The grand goal of the paper was to "learn causality", but the reality is that this is still very difficult from purely data-driven basis. I personally appreciate such explorations and think this concept is new to hydrology. I think the paper can be considered for publication after some substantial revisions. However, the authors will have to carefully qualify the applicability and limitations of the technique.

The most important issue --- as far as I can read, the key appears to be defining a sufficient set, which requires lots of subjective decisions and prior assumptions. The authors included previous-day precipitation and previous-day soil moisture because they think these variables will influence today's soil moisture. Also included are precipitation, daily temperature, humidity, wind. By the time you are done providing the sufficient set, you already need to inject lots of knowledge. We might wonder why we still need to run this causality test in the first place. I do see the point -- some of the decisions can be based on prior knowledge while the main causality gradient of interest (is soil moisture leading to more rainfall) may be unclear from our prior knowledge. This raises two issues: (i) there is only a niche of questions where this approach is meaningful: where we know enough to identify a causal graph and a sufficient set, but do not know the answer to the main question. This niche does exist; (ii) it will be much harder to apply where the causality or even the important factors are unknown, so the sales language of "learning a causality link" does not fit reality and should be carefully qualified.

As an initial demonstration the study also lacked a control experiment. In other words, if you replace today's soil moisture with a potential highly-correlated confounder, will the analysis show it is non-causal? This has not been demonstrated.

The writing of the article is also problematic:

(i) there should be a simple logical explanations for Theorem 1. I mean, the mathematical form can be accurate but does not help many people to understand the logic. You should translate this into simple, ordinary language. I don't believe the underlying logic is that remote.(ii) the Methods and Results are intermingled in an unhelpful way. Try to have more clear sections with dedicated functions.(iii) By the time I reach section 4 I am totally tired and cannot understand the rather complicated logic. Can you make this simpler?

Many unclear places:
(iv) How does the UNet represent the causal links in Figure 2? To my understanding all the inputs were treated in the same way(i) define "blocking a path"(ii) line 204 "further input variables" like what?(iiii) Page 6 needs lot of plain-language explanations.(iv) don't understanding "By including antecedent precipitation as input variable, or, in other words, conditioning on antecedent precipitation, we canexclude this correlation from our analysis."