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Interactive comment on "Limitations of Emergent Constraints on Multi-Model Projections: Case Study of Constraining Vegetation Productivity With Observed Greening Sensitivity" *by* Alexander J. Winkler et al.

Anonymous Referee #3

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Please accept my apologies for this very slow response to reviewing: "Limitations of Emergent Constraints on Multi-Model Projections: Case Study of Constraining Vegetation Productivity With Observed Greening Sensitivity"

Emergent Constraints (ECs) have become a very popular mechanism to collapse inter-GCM differences, and in order to make more refined future projections. It is therefore highly relevant to verify how robust the methodology is, and/or find counter-examples which illustrate potential issues with the technique.

This is a slightly superficial review, but what I would encourage the authors to do is to focus more tightly on the issue of potential problems with ECs – maybe at the expense of some of the other text describing so fully the particulars of vegetation greening.

While there are some concerns surrounding the EC approach, some of the criticisms levelled by the authors are only valid if the approach is applied carelessly. So I am not convinced these are limitations, and instead, a better title might be "Careful Application needed by ECs. . ...".

The Abstract raises two concerns.

(*) "The method critically depends on first an accurate estimation of the predictor from observations and models". This is true, but this is not particular to ECs any more than it is for any other environmental science modelling exercise. It is always essential to ensure that measurements align tightly with models to – for instance – allow model calibration. For example, the need for "like-for-like" comparison is routinely addressed when utilising Earth Observing data to constrain terrestrial ecosystem models.

(*) "Second, depends on a robust relationship between inter-model variations in the predictor-predictand space". This is really what lies at the heart of emergent constraints, which by definition is the search for emerging regressions across "X" and "Y"-axis space. However, if no relationship is present, then clearly the method would not be used. An interesting question to ask, however, is if intuitively a relationship is expected, but is not seen inter-GCM, then what does this imply?

The Conclusions are much more nicely set out, and I think clearer to understand. However, to just run through the points raised:

(*) The paragraph starting "The importance of how the observational predictor. . ..." again raises the need for all EC modellers to ensure a direct 1-1 mapping between modelled "X"-axis quantity and measurements. The next paragraph correctly identifies the importance of accurate spatial aggregation, when the GCMs themselves are predicting bulk quantities, defined as only valid over large regions (e.g. mean “greening” levels”).

(*) “A large source of uncertainty is associated with temporal variability”. The EC method does account for uncertainty in the measured “X”-quantity, which is why the standard diagrams place bounds on that – in addition to uncertainty associated with the model-based regression. If only one measurement is available, based on averaging over multiple years, then standard statistical techniques can be used to build error bounds. These can include, for instance, sampling only subsets of the years. Methods like this can also be applied where there is a mismatch in window length, to ensure larger uncertainty bounds where the measured quantity is over a short period.

(*) The conclusion hints at the issue of the importance of both identical “X”-axis temporal length (both model and measurement), and additionally the need for identical time-periods. In its most extreme for instance, it would not be appropriate to take 30-year segments of GCM period 1850-1889, comparing to 1990-2019 measurements. This is because an EC can change in time. Such variation is sometimes used to question ECs, but as long as the “X” model and “X” data are for the same period, then the method remains valid.

(*) Indeed here, the authors acknowledge dGPP v dLAI_max relationships do change for increasing CO2 levels. These changes are not a failure of the EC method, simply that (i) timescales need to line up correctly for present day (data versus models), and (ii) users need to be aware of what CO2 level is being considered for the “Y”-axis.

(*) I think the authors might miss a trick here, and especially for vegetation analysis. Where the EC approach is at risk of failure is if all GCMs currently miss an important process, and that will only become critical into the future. One prominent example is where, until recently at least, very few GCMs describe possible future down-regulation of fertilisation through geochemical cycles such as that of Nitrogen.

I certainly do not want this review to appear defensive of ECs, and this is indeed a very

interesting and thought-provoking manuscript. There are definitely things that require investigation associated with the technique. It is just that most of the points raised do not invalidate the EC approach – the examples are much more a case of “please use ECs carefully”?

Sorry, this is a short review, but if another version if generated then I would be happy to see the paper again.

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