

Interactive comment on “On the meaning of independence in climate science” by J. Annan and J. Hargreaves

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Received and published: 26 September 2016

Thank you for the comments. Here we comment on some of the points raised.

The reviewer questions the structure of the paper. We did find this manuscript challenging to organise while writing. A major motivation of this work was to investigate the similarities and differences in how the concept of independence applies to the two situations considered here, making one paper seems a natural choice. However it might be a better read if rather than jumping backwards and forwards between the two cases as in the current manuscript, we first present a full analysis of model independence, then consider independence of constraints as a shorter second part, identifying key similarities and differences in the application of the theory. Therefore we propose to reorganise the paper on this basis, at which point it might be clearer whether a full split

C1

into two separate papers would be beneficial. Such a split could risk obscuring the link that exists between the two cases through the mathematical theory, and may also require a significant amount of repetition.

The reviewer notes that we do not make explicit recommendations regarding the treatment of dependent models. This was a deliberate decision. We don't think there is necessarily a single correct approach to take, although we do agree that this should be discussed or at least raised in the manuscript. One obvious choice would be to eliminate all dependency by simply removing dependent models from the ensemble prior to any analysis. This is at least in principle possible with the definition as presented, since independence is an absolute property rather than a matter of degree. It may also be possible to devise a down weighting according to the degree of independence. It seems likely that a low level of model dependence could reasonably be ignored without significantly affecting scientific conclusions. We certainly agree with the reviewer's point that the space of outputs is high dimensional and that dependence may be exhibited in some dimensions and not others. If the models which are (near-)replicated were themselves unremarkable and had no particular characteristics or a priori biases, then we would not expect eliminating a small number of dependent models to have any systematic effect on model projections or performance. On the other hand, model dependence would certainly be an important issue if a single model was to dominate an entire ensemble through say the inclusion of a huge initial condition ensemble. This is a situation that would probably be appropriately handled by scientists' intuition, but we think it is worthwhile to have a mathematical theory to underpin such treatment. We will discuss these issues in more detail in the paper.

The reviewer's comment in the middle of page C2 is hard to interpret. It seems to us that if we accept that "the ensemble has a common bias in its present day state" then this is indeed to conclude that it is not truth-centred.

The review only briefly discusses the section relating to constraints on climate sensitivity. An important point that we emphasise is that, as with the earlier case, the concept

C2

of independence is again a fundamentally subjective one. The purpose of the example is to illustrate that nevertheless, independence can be investigated within a specific modelling framework, and can then be considered as an objective and identifiable fact conditional on that framework.

Interactive comment on Earth Syst. Dynam. Discuss., doi:10.5194/esd-2016-34, 2016.