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Earth System Dynamics
Discussions

Interactive comment on "Comment on "Scaling regimes and linear/nonlinear responses of last millennium climate to volcanic and solar forcing" by S. Lovejoy and C. Varotsos" *by* K. Rypdal and M. Rypdal

Anonymous Referee #2

Received and published: 22 May 2016

The Lovejoy & Varotsos (L&V) paper that this is a comment on has some issues that this comment on it identifies; I thus think it is essential that this comment be published, although it does need some minor revisions.

1. I started with re-reading L&V, and some brief comments are in order. First note that, in my reading of L&V, there are many places where statements are made that are flatly not true or highly misleading, as well as the presentation being unnecessarily confusing and imprecise. With a particular beef about their failure to carefully distinguish between (i) whether the underlying system equations are nonlinear, which

no-one would disagree with, and (ii) whether the response to small-amplitude external forcing can be sufficiently well approximated by the linear response for many purposes. No-one would ever claim that linearity is "valid", but simply that it is a sufficiently good approximation for some purposes. (And as an aside, regarding the observation on page 143 that a linear scaling system corresponds to a filter that is a power law... since we know that the filter is not a power law, then for this sentence to be true the scaling assumption must not be valid, invalidating the analysis. And I also object to sloppy usage of the word "feedback" which they tend to incorrectly invoke as an explanation for nonlinearity.) Personally, I would never have recommended acceptance of L&V manuscript without major modifications.

2. In addition to the observations about the L&V presentation being unnecessarily confusing and that the conclusions there are not actually supported by the analysis, I do think it worth pointing out somewhere that the ZC model is wholly inappropriate to the addressing the question in the first place. This 1986 model was designed solely to capture ENSO dynamics, and 30 years of subsequent research has made it clear that the parameter values assumed in the 1980's were not correct. The model is not stable, and a self-sustained ENSO arises as a result of chaotic dynamics (see papers by Tziperman in 1990's), rather than being the result of a stable heavily damped oscillator driven by climate variability as most researchers now believe ENSO dynamics result from in the real climate system. The characteristics of variability in the ZC model are therefore not relevant to reality (nor the behavior of GCMs) as it wasn't designed to capture them in the first place – I would not have been surprised if the ZC model is nonlinear in its response to forcing, but I don't actually care one way or the other. It is a complex but toy model, not a GCM intended to capture the dynamics of the actual climate. (Also, as a result, variability in ZC may have nothing to do with variability in GISS. I could generate a long control run for you if you really wanted to assess unforced variability.)

3. Some of the criticisms of L&V seem too directed at the individuals rather than de-

scribing the paper itself (e.g. “L&V are blind to this fact”). I understand your frustration with their paper, but criticize the paper, not the authors.

4. Citations on line 28-29, could add MacMynowski et al (2011), where the transfer function to solar forcing was explicitly computed in a GCM, Myrrhvold & Caldeira (2014), where they fit the response of CMIP5 models to either semi-infinite diffusion (long-memory power law) or multi-exponential, or Held et al (2010) with a two characteristic response time in GFDL models.

5. Could also be a bit clearer in places about “linearity”. We all know that the response isn’t exactly linear, the question is whether it is sufficiently linear for our purposes. E.g., line 35, these don’t demonstrate that CMIP5 is linear, simply that it is approximately linear for the amplitude of forcings considered, and for estimating the forced response (which is a different question from asking whether the forcing alters the statistics of the variability, as the authors note above eq. 1). Again, line 149 ought to have a qualifier like “substantively influence” or something like that; of course it will to some level that we are hoping we can ignore for most purposes. It is clear from the authors’ response to the online discussion that they understand what they mean, some of the clarity in their responses may be useful in the paper.

6. Top of page 3, point (i), need to say “the distribution of the internal variability” or something like that (the actual realization will of course change.)

7. Page 4, 5, some additional discussion is missing; in order to separate out the variability from the forced response, you either need to make some additional assumptions (as you do) that the high-frequency content is only variability and not forced (i.e., that the transfer function from input to output is small at high frequencies, which is reasonable), or actually estimate the input-output response, which would be difficult due to limited data. The linear response to solar forcing could be frequency dependent, rather than the functional form in equation (8). In principle, one could subdivide the time series and estimate (nonparametric) transfer function fit to the response (e.g. MacMynowski

& Tziperman, GRL, 2010), or do a least squares fit of the time series to a more complicated (but a priori) linear model with more than the two parameters you have (even AR(1) I would believe more than what you use in (8).) And use that to estimate the residual variability. This is highly unlikely to alter any conclusions, so I don’t object to what you did, other than to more explicitly mention in your discussion that the separation into forced response and variability requires an assumption on the nature of the linear input-output relationship. (And note that you already described some of the literature on that relationship only a page earlier!)

8. Figure 1d and 2, is it easy to put confidence bounds on these? (Without the plot becoming too excessively messy.) (I know you said you didn’t need to bother, though if it were trivial to do it might add weight.)

9. Sentence on page 6, line 174, needs to be fixed. . . I would generally assume “the statistics are so poor” to mean that the differences are NOT statistically significant.

10. Line 201 might be a good point to point out that the ZC model was never intended to get the statistics of variability correct, and so there is no basis for assuming anything about the magnitude of it relative to GISS. (If I can get it to work, still, I could go generate some ZC runs for you. . . I do have a 1000-year output file still that is probably from the right parameter values, though not absolutely certain; I’ll try posting that as a supplement).

11. Line 224, not clear that it contradicts L&V insofar as one could get the wrong variability at low frequencies for different reasons – nonetheless it clearly contradicts everything we know about the climate response to dynamic forcing, you already have lots of citations earlier on this point (and the MacMynowski 2011 GRL paper quite explicitly computes the transfer function for one GCM and shows that it is not a power law). So I think you can be pretty strong here in pointing out that this assumption in L&V is simply wrong. (I find it astonishing that L&V would make such an obvious error, yet it appears from my reading of the manuscript that they do.) Point III is also strong.

I don't feel qualified to argue regarding point #2

12. Typo line 264 (of vs on) âĂČ I will also make some brief comments on the back-and-forth "discussion" between R&R and L&V, typed chronologically as I skim:

13. (SC1,AC1). L&V criticize only the second of the 3 issues that R&R raise regarding L&V's analysis; they appear here to ignore the other two. They then go on to claim R&R make an error when, if I read correctly, R&R simply have reverted to the usual structure function definition rather than the abnormal one L&V use. I don't see anything in here that would suggest that changes to the manuscript are needed, though some of the authors' comments in response may be helpful in clarifying the manuscript. (I think the phrase, "The memory in the response smears out the volcanic spikes. This is a linear effect." might be useful to include somewhere, along with some of the opening comments.)

14. (SC2). a. If I didn't know that it was the same people, I would have thought from the initial sentence here that its authors' hadn't even read L&V let alone R&R, insofar as the original paper reads almost entirely as an argument that approximating the system response as linear is not a good approximation, while the first sentence here would have one believe that the original paper was simply pointing out the (obvious) idea that it isn't precisely linear. R&R simply observe that one can't statistically rule out the response being linear (i.e., can't distinguish it from the response of a linear system), which seems to me like a pretty good argument that linearity is a sufficiently good approximation, they never claim that the response actually is linear. b. The statement that L&V's first response indicated assumptions I-III as being irrelevant is utterly false, the first response only stated that assumption II was irrelevant and ignored the fact that there were two others, which are both relevant. Further, #I was clearly stated as an assumption (albeit one not actually satisfied by the climate system as I noted in point 1 above) in the original L&V paper. Most of this note seems to not actually respond directly to the comment under consideration, and certainly doesn't seem to refute any results in that comment. c. Minor comment #1 seems to utterly misconstrue the intent

of R&R's comment. It would seem that some clarifications in the comment might be in order in case some other readers are also unable to detect the distinction between "the response is linear" and "the response is not statistically different from that of a linear system and therefore the linear approximation is sufficient for most purposes". I thought the wording in R&R could have been improved in places, but nonetheless thought this point was blindingly obvious.

15. (SC3) a. Minor criticism, but the presence of temperature-albedo feedbacks does not in and of itself suggest that the response to forcing will be strongly nonlinear. b. And again, this comment fails to distinguish between the concept of the response being nonlinear, which is trivially obvious and no-one disagrees with, and the response being significantly different from linear, i.e., whether or not using a linear approximation is sufficient. c. I agree that the NorESM analysis does not directly argue whether L&V's original analysis was correct or not.

16. SC3, SC4: a. I'm not going to dig in myself as to why the analysis in R&R regarding ZC noise levels appears to be inconsistent with the SC3 computation of noise levels, either one of them is wrong, or there are different definitions going on here. (I see R&R subsequently responded to this.) b. I will comment on the statement: "We take their AC3 response (R+R, 2016c), - the analysis of a completely different set of model runs (NorESM) – to be an indirect admission that our original paper was correct." While I am not formally reviewing the discussions, I find this passive-aggressive behavior to be childish and inappropriate.

17. SC5 – this adds nothing other than reinforces my conclusion that it's authors did not actually read or understand R&R.

Bottom line: Having read most of the discussion, I find (a) almost all of it is utterly irrelevant to the R&R comment under consideration (as both parties agree), and more importantly (b) none of it appears to refute the conclusion in R&R that a properly constructed test without arbitrary and erroneous assumptions does cannot distinguish the response from linearity. I therefore see nothing in the posted discussion that lead me to worry that I missed something in my reading of the R&R response.

Please also note the supplement to this comment:
http://www.earth-syst-dynam-discuss.net/esd-2016-10/esd-2016-10-RC2-supplement.zip

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