

Interactive comment on “Agnotology: learning from mistakes” by R. E. Benestad et al.

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Scafetta accuses us for making a number of “unjustified accusations” toward the authors of the papers we have selected in our study. This is a misrepresentation of our work – in fact, we provide a discussion why we think the papers draw misguided conclusions based on replication. We do provide the source code of our work (replicationDemos).

Scafetta is the author of several of these papers and he is a contributing author to the NIPCC report discussed. We are not surprised that he dislikes our paper. His ‘short comment’ is quite extensive, with many arguments that are off-topic. I will address the most important here.

We will argue that we do not simply “highlight secondary apparent discrepancies of the critiqued theories in reconstructing the data with an “absolute” precision claiming

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that such minor discrepancies invalidate the proposed theories” - instead, we look at aspects on which the conclusions hinge.

Scafetta associates his work and situation with that of Galileo to Einstein, and argues that our paper “does not serve a scientific purpose, but a political one”. I think that the association is misplaced and the accusation that our paper has a political purpose is unwarranted.

Scafetta makes reference to a recent paper of his in Pattern Recognition in Physics also available on arXiv), which we will include in the revised version of our paper – this paper clearly demonstrates how he (deliberately?) has unacceptably misrepresented the work Benestad and Schmidt (2009; BS09, available from <http://pubs.giss.nasa.gov/abs/be02100q.html>).

In the abstract of BS09, it is stated that “We demonstrate that naive application of linear analytical methods such as regression gives nonrobust results”. The paper iterates this point further “The regression analysis ... should in this context be regarded as a naive approach that is prone to yielding biased results, and we caution against using such techniques without a critical interpretation”, and “Here we use the regression to demonstrate how spurious results may arise from colinearity and “noise” by examining the variability in the coefficients”.

Scafetta (2013a,b) turned this around and accused the paper for inappropriate use of this method: “An improper application of the multilinear regression method is found in Benestad and Schmidt (2009), indicated herein as BS09” and “The first way BS09 multi-linear regression fails is mathematical. The predictors of a multilinear regression model must be sufficiently linearly independent, i.e. it should not be possible to express any predictor as a linear combination of the others”. Furthermore, Scafetta incorrectly gave the impression that a regression with 10 covariates was used for the comparison and the conclusion of a 7% solar contribution.

It’s easy to check this – the evidence is black-and-white – written in the cited papers.

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I find it difficult to engage in a serious discussion with Scafetta, based on this starting point. It is important to discuss the actual message, not some misconstrued notion. Furthermore, a great deal of Scafetta's comment is off-topic and not directly relevant to our paper. We are not really discussing "IPCC models" (the IPCC has no models, but he means the climate model simulations presented in the IPCC reports), but rather the method used to analyse these model results presented in one of his papers. Nor are we discussing the climate sensitivity in our paper.

Scafetta has his own idea about how science is progressing and what is 'proper' procedure concerning criticising past work. We argue in our paper that it is also necessary to look at a number of papers as a group too. Furthermore, this discussion paper allows the authors of the original papers to comment on our paper, just as what Scafetta has done. Hence, his major point number 1 does not apply. I also disagree about our paper being poorly written – this is Scafetta's own subjective view.

I disagree with Scafetta's point #2 about our paper containing "numerous misconceptions and/or falsehoods in addition to philosophical, mathematical and physical errors that cannot be fixed without making their paper completely useless". For the reference to McKittrick, please see my response to his comment. I see that Scafetta has a strong interest in our paper being rejected because it exposes some fundamental weaknesses in his own work and papers on which the NIPCC is based.

Our work is not as Scafetta presents it: 'Simply arguing that there might be some error here and there is not a "demonstration" that the error truly exists'. In fact, we have done his analysis and showed why he has reached incorrect conclusions. We are not looking at "straw men", using "red herring" tactics, or presenting "personal doubts".

In his defence, Scafetta picks our differences between 60 or ~66-year oscillation (or ~20-year period), which relates to the question whether the earth's climate is subject to the influence from planets in the solar system. The point made in our paper is that there are some broad spectral peaks not even centered on the frequencies cited in

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Scafetta's work, and it is naïve to attribute these to planetary forcing the way he does.

We will more carefully describe the curves in Fig. 2 in our paper in the revised version of the paper. Scafetta judges that his fit is superior based solely from a visual inspection, but a proper evaluation needs to take into account the number of fitted parameters. His fit involves more fitted parameters (e.g. a quadric trend model), which will result in a closer description. That should not be confused with a better model – over-fit is indeed discussed in our paper.

The ENSO-analogy demonstrates that the method used by Scafetta in general is not appropriate for studying cycles. However, it is true that ENSO is not equivalent with the global mean temperature, nor are the paloclimatic indexes of the Pacific Oscillation, Atlantic Oscillation, or Indian summer monsoon (which have different phase relationships). The question is not whether there are natural variations present with timescales in the range 50-80 years, but the methodology that Scafetta used to represent the global mean temperature. Furthermore, it's particularly the association to an astronomical origin which we criticize, in addition to the curve-fitting. We also show that the search for astronomical signal in the climate models is logically flawed, for reasons explained. Hence, Scafetta's attempt to demonstrate that the climate models fail to reproduce the ~60-year variations is misplaced and based on an invalid approach.

Scafetta never explains properly the physics of the astronomical influences, other than some vague resonance to gravitational forcing. We do indeed discuss how this physical explanation fails in our paper.

Part of the Scafetta's comment is off-topic, which we can leave for later. However, we have never stated that 'science on climate change is already perfectly understood and "settled"' - I think this is a misconstrued idea of Scafetta's. Our paper is concerned with falsification of some controversial articles, for which we have provided a source code and data, in addition to an explanation. With our analysis and Scafetta's comment, I hope we can make some progress. However, it would be helpful if Scafetta could

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disclose his own methods, so that others could examine his analysis in detail [1].

It is interesting to note Scafetta's view "given the fact that the climate is a dynamical system, not just a stochastic system". On this account, he deviates fundamentally with ours and e.g. those who think that long-term persistence may provide an explanation for the recent trends. Furthermore, he dismisses the effect from non-linear chaos, and the rich complexity of the earth system. The world is not as simple as that.

In analysis, one should not pick one sample (here one model: GISS ModelE) and from it deduce that the whole sample (here ensemble of climate models - CMIP3) is wrong. This is a logical flaw in Scafetta's argument. Furthermore, Scafetta refers to a "free-phase" climate model – which is a very strange way to put it. We argue that one cannot expect to see a signal from the planets in the climate models with the same phase as in the real world, because we know the models do not include this type of forcing. Furthermore, we think that the slow fluctuations arise from non-linear chaos, and the phase essentially is unpredictable. There is no reason to believe that the models should replicate the phase of these oscillations. What we show is that they more or less reproduce their magnitude.

Scafetta does not think our calculations were clear, but their recipe is provided in the R-packages 'replicationDemos'. His remark "a reader is left to just "trust" their words that they have done this and that" is completely off the mark. Furthermore, our equation is so simple (it's in the R-code) that we thought it would not be needed. The results from Scafetta were digitally copied from a PDF-version of his paper.

Again, Scafetta's remark 'model reconstructs the temperature patterns "well" because the phases do not matter!' suggests that he does not understand the situation. This is exactly right because we know the model should not reproduce the phase of the internal variations in the non-linear and chaotic dynamical system, and because we know that any astronomical forcing is absent. His comment that "This is pure nonsense" reveals a fundamental lack of understanding of the climate models, the climate

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system, and the underlying physics.

The testing of the climate models have been done elsewhere, and is really off-topic here.

Scafetta needs to explain why Pearson's chi-squared test is not relevant for his paper – I think the global mean temperature can be approximated as being Gaussian. We didn't mention the "synchronization" issue because we didn't believe it and because we thought that point was fairly minor to the other issues.

We have different views on trend representation. I think Scafetta's definition is non-persuasive and ad hoc.

Scafetta refers to blogs when referring to a previous paper by – ironically he accuses our paper for having a blog-like character. Again, the paper he cites in Pattern Recognition in Physics (why that journal?) that provides an unacceptable misrepresentation of the regression analysis in Benestad and Schmidt (2009). This can easily be checked by reading the two papers (both open access). In any case, the R-code for replicating the work is available in 'replicationDemos', and we would urge Scafetta to divulge his own code [1].

The editorial resignation is a matter of fact, proving there to be controversies. We do not say it's a "science demonstration". This is a "straw man" argument.

We have noted that Humlum et al. (2011a) do not make a claim about the giant planets anywhere in their paper – see our response to their comment. We still believe that Humlum et al (2011) performed a curve-fit that is not suitable for attributing causes.

Also, see our response to the comments posted by Loehle and to Solheim et al.

Scafetta's concern about editorial review do fit in our discussion about agnotology and peer reviewed papers. We too argue that the reviews sometimes are too weak, letting through papers such as those from Scafetta. The gravest example is probably Scfetta (2013).

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References:

Benestad, R. E. and Schmidt, G. A., 2009. Solar trends and global warming, *J. Geophys. Res.* 114, D14101, 2009. Open access from <http://pubs.giss.nasa.gov/abs/be02100q.html>

Humlum, O., Solheim, J.-E., Stordahl, K., 2011. Identifying natural contributions to late Holocene climate change. *Glob. Planet. Change* 79, 145–156.

Scafetta N., 2013b. Discussion on common errors in analyzing sea level accelerations, solar trends and global warming. *Pattern Recognition in Physics*, 1, 37–57. DOI: 10.5194/prp-1-37- 2013.

Scafetta, Nicola. “Discussion on Common Errors in Analyzing Sea Level Accelerations, Solar Trends and Global Warming.” arXiv:1305.2812 (May 13, 2013a). doi:10.5194/prp-1-37-2013.

[1] Sceptical Climate Researcher Won t Divulge Key Program - Environment - 18 December 2009 - New Scientist. Accessed July 5, 2013. <http://www.newscientist.com/article/dn18307-sceptical-climate-researcher-wont-divulge-key-program.html#.UdaMk86UAqU>.

Interactive comment on *Earth Syst. Dynam. Discuss.*, 4, 451, 2013.

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