Comment on " 'Agnotology: learning from mistakes' by R. E. Benestad, H. O. Hygen, R. van Dorland, J. Cook, and D. Nuccitelli" published in

http://www.earth-syst-dynam-discuss.net/4/451/2013/esdd-4-451-2013.html

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Abstract

This comment contains a response to Benestad et al. (2013) where the authors critique a set of papers that they dislike and make a number of unjustified accusations toward their authors. I am going to demonstrate that, despite their "good intentions", Benestad et al. (2013) is filled with misconceptions and/or falsehoods, and with severe mathematical and physical errors. In brief, this paper is a very superficial and unprofessional internet-blog style study. The arguments advanced by the authors simply originate from poor reading and understanding of the critiqued works and general lack of mathematical, statistical and physical knowledge. They even promote their critiques by claiming that the IPCC climate models are NOT supposed to reconstruct the correct phases of the temperature patterns! Simultaneously, the authors do not use their same logic to critique the alternative theories that they favor and advocate such as the IPCC AGW. In fact, one important aspect of the scientific method is comparing alternative theories to determine which one better agrees with the data. Benestad et al. (2003) carefully avoided this direct comparison. Often, the authors simply highlight secondary apparent discrepancies of the critiqued theories in reconstructing the data with an "absolute" precision claiming that such minor discrepancies invalidate the proposed theories. However, the authors do not provide any alternative theory capable to better interpret the major patterns of the same data. Moreover, despite their "good intentions", Benestad et al. (2013) do not really demonstrate anything because often they do not even make explicit the functions they use or their data analysis results or figures. They simply provide some R-routines that in their opinion the reader is supposed to run by himself to find out what happens. Therefore, a real direct comparison with the critiqued works cannot even be made in most cases. In history of science the same flawed superficial logic has been often used by those who have opposed the emerging physical theories proposed since the 16th centuries from Galileo to Einstein and beyond. This work does not serve a scientific purpose, but a political one.

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1. Introduction

Benestad et al. (2013) critique some papers including a few authored by me (e.g. Scafetta, 2010, 2012a; Loehle and Scafetta, 2011; and other studies) that claim that the anthropogenic global warming theory as advocated by the IPCC (which says that humans contributed about 100% of the warming observed since 1900, as also explained below) is somehow “erroneous”.

However, data analysis and an increasing number of papers are establishing that the climate system is very likely characterized by large natural oscillations. Moreover, contrary to Benestad et al. (2013) claims, the criticized papers used not only global surface temperature records since 1850, where these oscillations (e.g., the quasi 60-year oscillation) are quite evident, but also much longer climatic records covering millennia and centuries.

Simultaneously, data analysis establishes that the IPCC models do not reproduce these oscillations also during the period 1850-2013 that these models are supposed to reconstruct accurately. For example, the temperature standstill after 2000 is missed by the models, but also the strong warming between 1850 and 1880 and between 1910 and 1940 and the cooling from 1880 to 1910 and 1940 to 1970 remain unexplained by the models. These models are, therefore, very likely flawed in the sense that they are missing something (e.g. natural oscillation mechanisms) while overstating something else (e.g. the effects of anthropogenic emission climate forcing).

Once these oscillations are taken into account, Scafetta demonstrated that about 50% of the observed 20th century warming can be interpreted as due to the detected natural variability, and his proposed model also well forecasts the standstill after 2000. This result implies that the correct climate sensitivity to CO_2 doubling is about half of the average 3 ^oC value currently implicit in the IPCC models. The real climate sensitivity at the observed scales should therefore be about 1.3-1.4 ^oC and likely between 0.9 ^oC and 2 ^oC. Similar results are being more recently confirmed by researchers simulating the 60-year temperature oscillation with the quasi-60 year Atlantic Multidecadal Oscillation such as Tung and Zhou (2013) on PNAS that Benestad et al. (2013) do not cite nor critique with the same zeal. As a consequence of the lower climate sensitivity to GHG emission, Scafetta concluded that the same 21st century IPCC emission scenarios would produce far less alarming 21st century warming projections than what currently proposed by the IPCC and its advocates such as Benestad et colleagues: see also Scafetta (2010, 2012a, 2013c).

I waited that two anonymous referees submitted their review hoping that they could emphasize better the evident flaws in Benestad et al. (2013). Because, unfortunately, this did not happen, I decided to write a response. I will also address some of the anonymous referee’s arguments. In the following I refer to Benestad et al. (2013) paper as BHDCN2013.

In brief, I do not believe that BHDCN2013 can be accepted for publication for two major reasons:

1) BHDCN2013 naively critique a large number of papers authored by numerous people published in the peer reviewed scientific literature without adding anything to science. If BHDCN2013 believe that their arguments are scientifically correct they should submit proper critical scientific comments to the original journals and let the criticized authors to write proper responses so that the readers may properly evaluate the arguments by considering both sides. Sadly, both anonymous referees failed to properly emphasize this elementary point, despite the fact that BHDCN2013 is so poorly written and argued that both referees disliked the paper for other reasons and also suggested its rejection in the present form. This

point is, however, partially mitigated by the fact that Earth System Dynamics allows open comments.

2) BHDCN2013 contains numerous misconceptions and/or falsehoods in addition to philosophical, mathematical and physical errors that cannot be fixed without making their paper completely useless. In the following, I am going to discuss some (not all) of BHDCN2013 errors. The extent and severity of these errors was not caught by the anonymous referees, who also appear not having properly read the critiqued works: Referee #1 explicitly says that he has not verified BHDCN2013 calculations and Referee #2 handwaves. The referees' behavior is quite bizarre, indeed, and only demonstrates some of the serious limits of the anonymous peer review process. Essentially, checking the facts is hard and time consuming, and requires scientific skills as well as academic integrity. It is just much easier to just follow the "politically correct" theory of the time, in this case the IPCC AGW. As a consequence, people that advocate the "politically correct" theory can easily get very shallow, inadequate and insufficiently critical "positive" reviews due simply to the confirmation biases of the referees and perhaps to their own personal opportunism and interest, while a minority opinion is easily unfairly mistreated for the same reasons.

Moreover, in his comment McKitrick has revealed that this same paper was already rejected by another journal and that BHDCN2013 have not truly addressed the issues raised by those referees that yield the rejection of their paper. And in its reply Benestad revealed that the paper was apparently submitted and rejected not once but twice!

Therefore, I do not see how the editor of the Earth System Dynamics can accept BHDCN2013 by ignoring: (1) the overall negative review of his two anonymous reviewers, although these reviewers were quite cynical toward the criticized authors; (2) the negative reviews this paper received elsewhere; (3) the rebuttals of the accused authors without also publishing (free of charge) the responses from the criticized authors such as the present one.

2. My general "agnotological" philosophical impression

The general impression that I had is that Benestad, Hygen, van Dorland, Cook, and Nuccitelli have not understood at all the criticized works and engaged in "straw-men" and "red-herring" tactics to mislead the scientific community and society about ongoing frontier research that they personally dislike up to the point that they try to defame the critiqued authors with a number of undemonstrated "accusations".

Most of their critique refers to the scientific problem of "replication", which is evidently an important part of science. However, the correct way of proceeding in an objective scientific critique is first to accurately replicate the result and acknowledging the logic of the critiqued studies within their own "full" hypothesis, which demonstrates that the critics well understood the critiqued study, and then demonstrate whether factual errors are present. Simply arguing that there *might* be some error here and there is not a "demonstration" that the error truly exists. However, often Benestad et al. construct "straw-men" arguments based on partial and misleading presentations of the supporting arguments used in the critiqued works. They also do not report equations, figures and tables with their data analysis results that can be point-by-point contrasted versus those reported in the criticized works. For some curious reason, the authors think that such tedious work, which is necessary for providing scientific demonstrations and to make explicit the facts to a reader, should not be their primary responsibility but it should be left to the readers themselves by using some R-codes that Benestad et al. provided!

Moreover, the demonstrated "errors" must be objective and so serious as to invalidate the analysis and the scientific conclusion of the critiqued studies. It is evident that minor irrelevant discrepancies in data analysis as well as inappropriate application of the proposed methodologies outside the physical constraints of the original analysis, do not invalidate the interpretative scientific logic proposed in the critiqued studies. Small discrepancies are typical when slightly different data or slightly different analysis methodologies are used to reproduce some result.

On the contrary, BHDCN2013 focus their critique emphasizing secondary irrelevant data analysis details or systematically misapplying the adopted mathematical methodologies outside the physical time-scale of validity proposed by the authors. They do this often using red-herring tactics, while neglecting the major scientific message contained in the works they critique (e.g. the existence of large natural climatic oscillations at multiple scales from the 60-year cycle to the millennial one not captured by the IPCC models) that, evidently, they could not properly disprove.

BHDCN2013 ended up expressing more a litany of "personal opinions" and "personal doubts" (which are not even supported by convincing numbers and figures) misleadingly presenting them as "incontrovertible facts" on a number of issues than presenting accurate and convincing scientific demonstrations disproving the results or the theories proposed in the critiqued works.

BHDCN2013 also carefully avoided using their same critical logic to scrutinize the works that support their own advocated AGW theories that, as demonstrated in the peer reviewed literature BHDCN2013 criticize, contain far more serious shortcomings such as the macroscopic failure of the IPCC general circulation models in properly reproducing the observational temperature data at multiple scales such as the temperature standstill after 2000.

BHDCN2013's attempt to dismiss scientific works with just a "philosophical" approach instead of using very accurate, explicit, detailed and extended physical and mathematical calculations and graphs is naïve, at least. They should have scientifically disproved the papers being critiqued before attempting to write a "philosophical" treatise. On the contrary, BHDCN2013 mislead a reader by giving an impression that the critiqued papers have been already so robustly rebutted in the literature that they can now propose a philosophical "agnotological" summary and interpretation of the case.

Let us now analyze BHDCN2013 "science" and its claims mostly referring to my own works.

3. A "quasi 60-year period" or a "65.75-year period"?

One of the issues discussed in my papers criticized by BHDCN2013 is that the global temperature records since 1850 present a "quasi 60-year" oscillations with local maxima around 1880, 1940 and 2000. At page 467 BHDCN2013 criticize such a claim by simply stating that according to them the period would be about 65.75 years instead of quasi-60 years! Below I discuss BHDCN2013 mathematical error first and their physical/scientific error later.

3.1 The mathematical error.

BHDCN2013's claim is mathematical erroneous simply because they forget to estimate the statistical error associated to their 65.75 year estimate, and taking it into account. In Scafetta (2010, Table 2) it is written that the statistical analysis gives an average period of 62 ± 5 year. It is evident that BHDCN2013's "sharp" 65.75-year oscillation falls within the statistical error bar of Scafetta's "quasi-60 year" oscillation. So, there is no contradiction in Scafetta's calculations versus their "sharp" and errorless 65.75-year result.

In the same way, BHDCN2013 claim that Scafetta quasi-20 year oscillation is "wrong" because their calculation would provide a better fit of 21.5 year. Again, BHDCN2013's claim is mathematical erroneous simply because they forget to estimate the statistical error associated to their 21.5 year estimate. In Scafetta (2010, Table 2) it is written that the statistical analysis gives an average period of 21 ± 1.4 year. It is evident that Scafetta's "quasi-20 year" oscillation and BHDCN2013's "sharp" 21.5-year oscillation fall within the statistical error bars of Scafetta's measurements. Therefore, BHDCN2013 did not demonstrate any mathematical error in Scafetta's calculations.

On the contrary, it appears that, despite their boasting "robust" and "errorless" analyses, BHDCN2013 do not know how to properly interpret the results of their own statistical analysis. It appears that they do not know how to calculate the error bars in a regression analysis or at least give an estimate of it. Indeed, BHDCN2013 do not disprove anything but, on the contrary, they end up confirming Scafetta's analysis!

Indeed, that BHDCN2013 calculations may be statistically not optimized (despite their claim that they are using the "best" harmonics) is evident in their figure 2 reproduced below with the original caption.

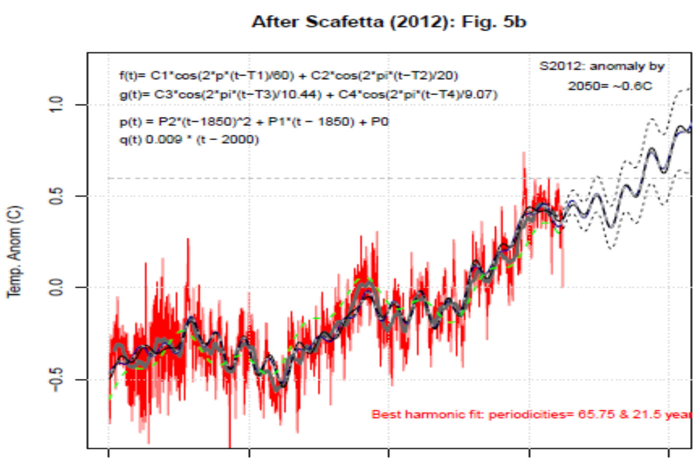


Fig. 2. A reproduction of Fig. 5 in Scafetta, 2012a (also available on-line from http://arxiv.org/pdf/1201.1301v1.pdf). The reproduction was done calling the function "Scafetta2012()" in replicationDemos. The grey horizontal dashed line marks the level where Scafetta's curve intersects year 2050. An interative search for periodicities in the vicinity of those suggested by Scafetta, gave a best fit to the pair of harmonics if they were 21.5 and 65.75 yr respectively.

It is quite unclear to me what BHDCN2013 want to demonstrate with this figure, and a reader should be very careful because this figure shows something "good" and something "bad": the "good" part is my model; the "bad" part is what BHDCN2013 added. Indeed, contrary to their "accusations" that they do not reproduce my figure, it appears to me that they well reproduce my figure 5b (Scafetta, 2012a): a reader should note that my figure is made of the **red**, the **blue**, the **black** and the grey lines and the error bars curves after 2010 and everything looks "good". The only difference with my figure is that they added their thin **green-model** dash line whose meaning is not explained. It appears to me that such green-model may be the one obtained with the so-called "best harmonics" reported in "red" at the bottom-right of the figure.

It is evident also at naked eye that their additional **green-model** curve (which is almost invisible in the figure) performs far worst than my **black** and **blue** curves in reconstructing the patterns of the temperature record (red curve). So, BHDCN2013's own added model (green-curve) seems for sure not optimized as claimed, while my proposed model reconstructs the temperature quite well. It is, therefore, hard to interpret the above figure as a "demonstration" that my result is "wrong" and theirs "right". They get my result which well reconstructs the temperature patterns, but apparently when they use their "best model" they get a poor result!

3.2 The scientific errors.

The temperature is characterized by a quasi 60-year cycle is clearly visible for example in Figure 1 of Scafetta (2010) reproduced below, and this oscillation is not captured by the climate model simulation. The figure reports the GISS model E ensemble simulations and its divergence from the data pattern before 1970 is quite evident to the naked eye. Note the failure of the model to get the cooling in 1910, the maximum in the 1940s and many other patterns, which BHDCN2013 do not discuss nor explain.

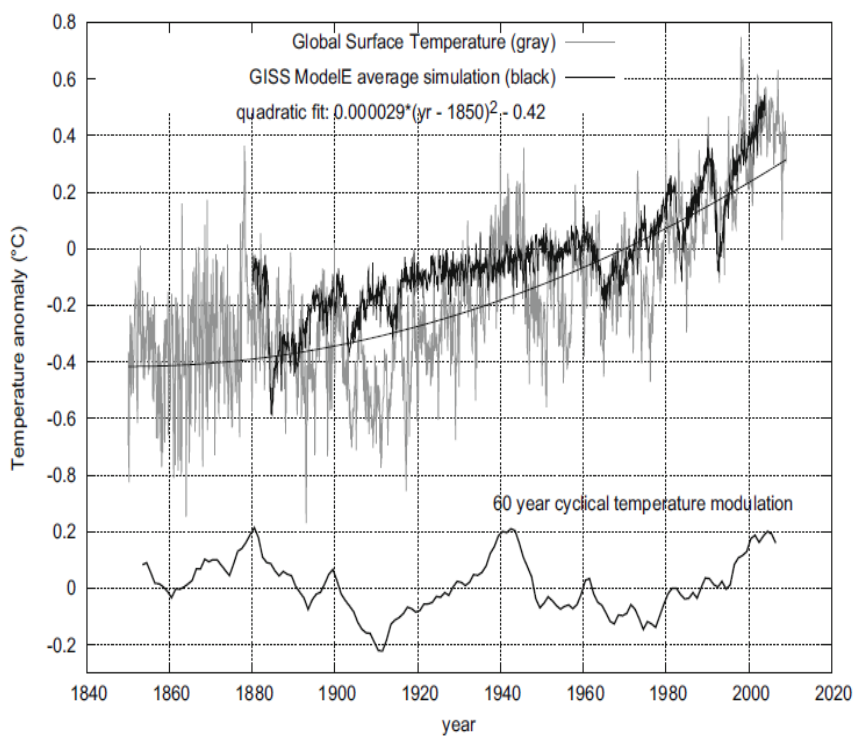


Fig. 1. Top: global surface temperature anomaly (gray) (Brohan et al., 2006) against the GISS ModelE average simulation (black) (Hansen et al., 2007). The figure also shows the quadratic upward trend of the temperature. Bottom: an eight year moving average smooth of the temperature detrended of its upward quadratic trend. This smooth reveals a quasi-60 year modulation.

BHDCN2013 mislead the reader by arguing that only two 60-year oscillations exist since 1950 and that this fact "demonstrates" Scafetta's argument "wrong". They even plot in their Figure 4 a “demonstration” using the ENSO signal from 1980 to 1990 (!), without realizing that such an argument is nothing but a red-herring fallacy because what the ENSO does from 1980 to 1990 is an irrelevant topic presented in order to divert the attention of a reader from the original issue referring to the quasi 60-year oscillation.

However, BHDCN2013’s red-herring argument plays an important and strategic purpose. It is used to mislead the reader about the fact that Scafetta’s works demonstrated and argued that this quasi 60-year oscillating modulation cannot be interpreted just as a simple stochastic pattern observed from 1850 to 2010 that coincidently resembles a 60-year oscillation between 1850 to 2010, but is one of the physical characteristic oscillations of the climate system because it has been found in numerous paleoclimatic records for centuries and millennia, together with other oscillations, as acknowledged by numerous authors properly referenced in my papers.

BHDCN2013 misleading attempt is particularly serious because my papers not only reference numerous works, but also make explicit several figures showing centuries of data manifesting this quasi 60-year oscillation. For example, this is one of the numerous figures that can be found in Scafetta (2012c) among the other papers, which was also partially reproduced as figure 4 in Loehle and Scafetta (2011):

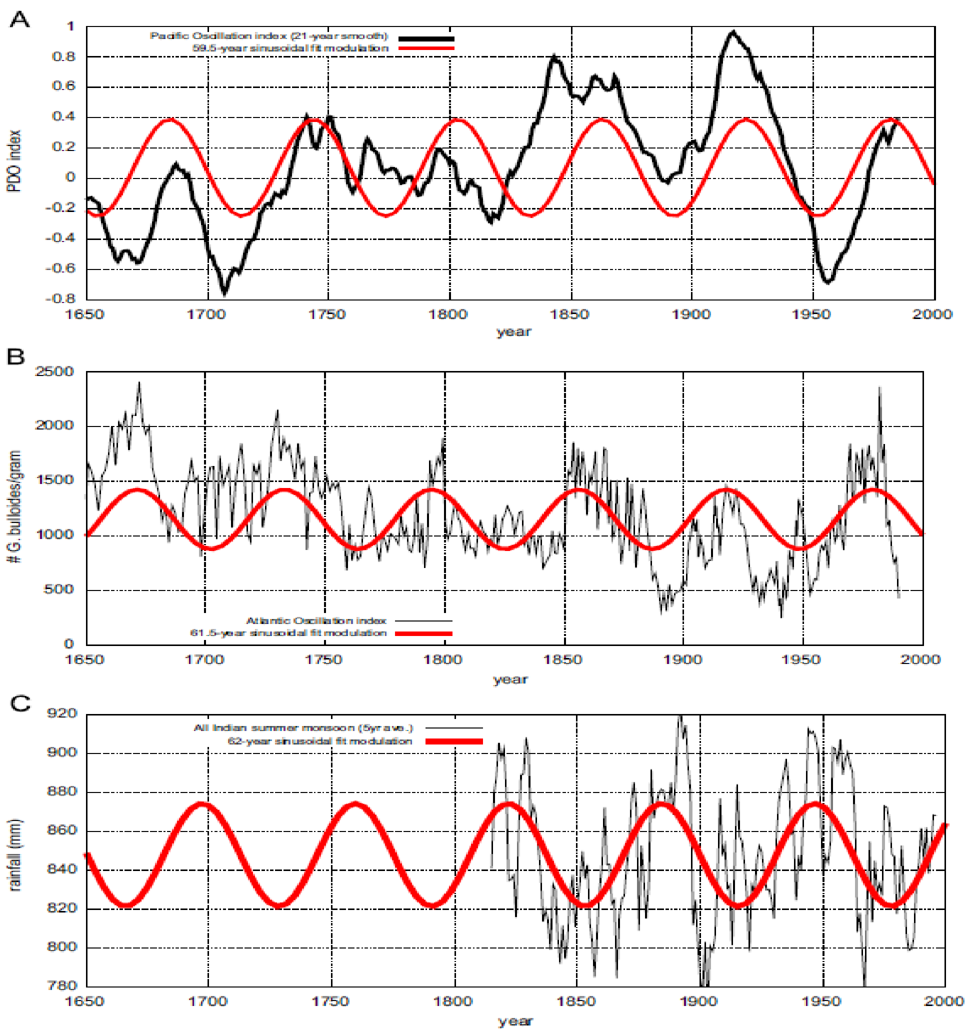


Fig. 3. (A) Twenty-year moving average of the tree-ring chronologies from Pinus Flexilis in California and Albertain: this record is used as a proxy for reconstructing the Pacific Decadal Oscillation (MacDonald and Case, 2005). (B) Record of G. Bulloides abundance variations (1-mm intervals) from 1650 to 1990 AD (black line) (Black et al., 1999); this is a proxy for the Atlantic variability since 1650. (C) Five-year running average of the Indian summer monsoon rainfall from 1813 to 1998 (Agnihotri and Dutta, 2003). All three records show clear 60-year cyclical modulations that are (positively or negatively) well correlated to the 60-year cycles of the global surface temperature and the aurora records. The records are best fit with sinusoidal functions that give a statistical error about the 60-year period of ± 4 years.

In fact, this 60-year oscillation has been found in numerous records such as in the multisecular reconstructions of the AMO, PDO, NAO, ice core, sea level records, monsoon rainfall, fish catches, etc. Just for example, Klyashtorin et al (2009) analyzed numerous multisecular records and found an average predominant frequency peak at 59.2 year, as shown in the table below:

Table I. Predominant periods of climatic fluctuations within the range of 20–100 years according to all data available.

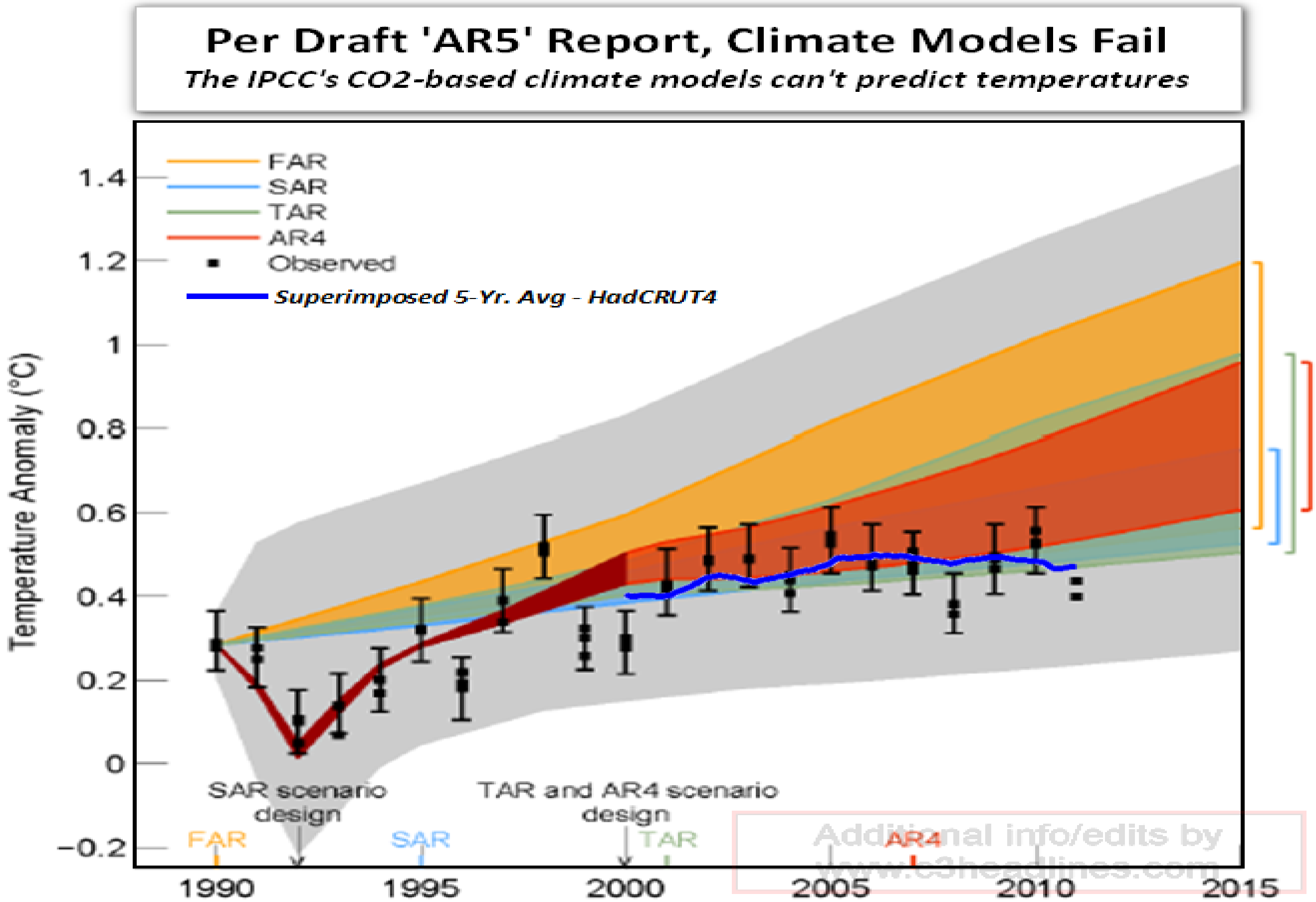
Time series	Series length, years	Predominant peak, years	Secondary maxima, years
Ice core samples	1420 (552–1973)	54	32
Arctic pine tree	1480 (500–1980)	60	32
California bristlecone pine tree	1500 (479–1979)	76	32
California bristlecone pine tree	8000 (–6000–1979)	55.4	20-35
Sardine (sediment core samples)	1730 (270–1970)	57 and 76	56, 33
Anchovy (sediment core samples)	1730 (270–1970)	57	72, 99
Global dT	140 (1861–2001)	55	18.0

A recent summary of this literature is also contained in Scafetta et al. (2013). where it is shown that this oscillation is present since 1350 A.D. al least in ice core temperature based records, and in Scafetta (2013a, 2013b, 2013c).

This quasi 60-year oscillation clearly appears to be one of the major natural harmonics of the climate system and not just a red-noise fluctuation that since 1850 resembles a 60-year oscillation. Scafetta (2010, 2012a, 2012b, 2012c; Loehle and Scafetta, 2011; Mazzarella and Scafetta, 2012; Scafetta and Willson, 2013; Scafetta et al. 2013; Scafetta 2013a, 2013b) showed that this quasi 60-year oscillation is very well correlated to solar/astronomical oscillations. Also independent studies such as Ogurtsov et al. (2002) found this 60 year oscillation using millennial long solar proxy records. Thus, it was hypothesized that these natural oscillations could have an astronomical origin, although the exact mechanisms are not fully understood yet. More recent works (e.g. Scafetta and Willson, 2013) found other important multidecadal oscillations at 40-45 years and at 80-90 years, which need to be summed to the secular and millennial ones.

Scafetta (2012a) also demonstrated that the general circulation models used by the IPCC macroscopically fail to reproduce this quasi-60 year oscillation both in their individual runs and in their ensemble means and in their power spectrum. This implies that these models macroscopically fail to include important physical mechanisms capable of reproducing such oscillations. In Figure 1 above, the GISS ModelE average simulation is directly compared against the global surface temperature data and the failure of the model in reproducing correctly the data patterns before 1970 is macroscopic.

Also the failure of the models in reconstructing the post 2000 temperature record is becoming more and more manifest as evident in the IPCC figure below



BHDCN2013 completely miss the above important scientific result and claimed that Scafetta's result is "wrong" simply because Scafetta estimated a "quasi-60 year" oscillation (with a statistical measure of 62 ± 5 year) while BHDCN2013 get a trial "errorless" period of 65.75 years! They never mention the rich literature confirming this oscillation for centuries and millennia (together with other oscillations) misleading a reader to believe that Scafetta claimed the existence of a 60-year climatic cycle on the base only of the global surface temperature record available since 1850, when this oscillation is macroscopically evident. In any case, BHDCN2013 did not disprove that the data since 1850 can be interpreted with these cycles. Thus, BHDCN2013 argument is scientifically baseless.

Moreover, BHDCN2013 misinterpret Scafetta's works by claiming that I am simply doing some curve fitting. The important point of my works is that the detected climatic harmonics are approximately coherent to measurable astronomical/solar harmonics, which are the ones that are used to reconstruct the temperature patterns. Scafetta's method is somehow equivalent to the harmonic constituent astronomical models used to efficiently predict ocean tides. This simple concept is however systematically misinterpreted by BHDCN2013.

Thus, in their curve fitting exercise BHDCN2013 miss completely the physics of my papers that they criticize. For example, BHDCN2013 fail to realize that the 20 and 60-year oscillations I use do not derive from mere cure-fitting but from astronomical considerations that form the physical hypothesis used in the paper. BHDCN2013 never understand this point.

In addition, the figure below reproduces Figure 10 in Scafetta (2012c).

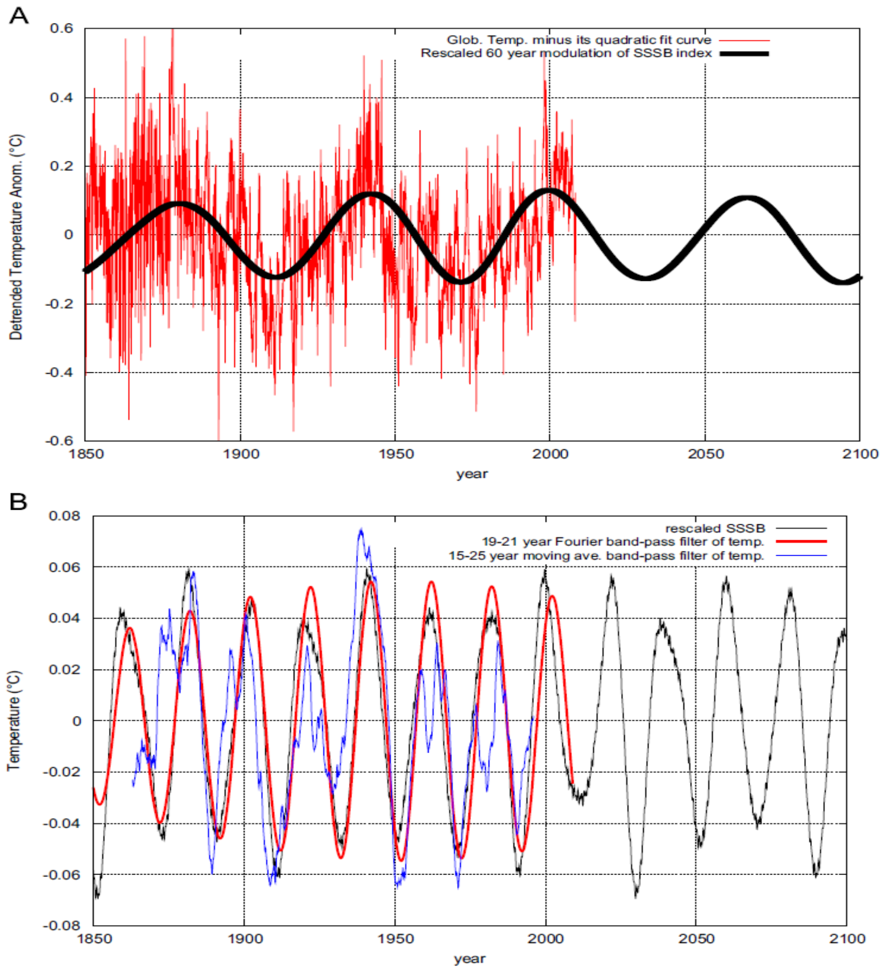


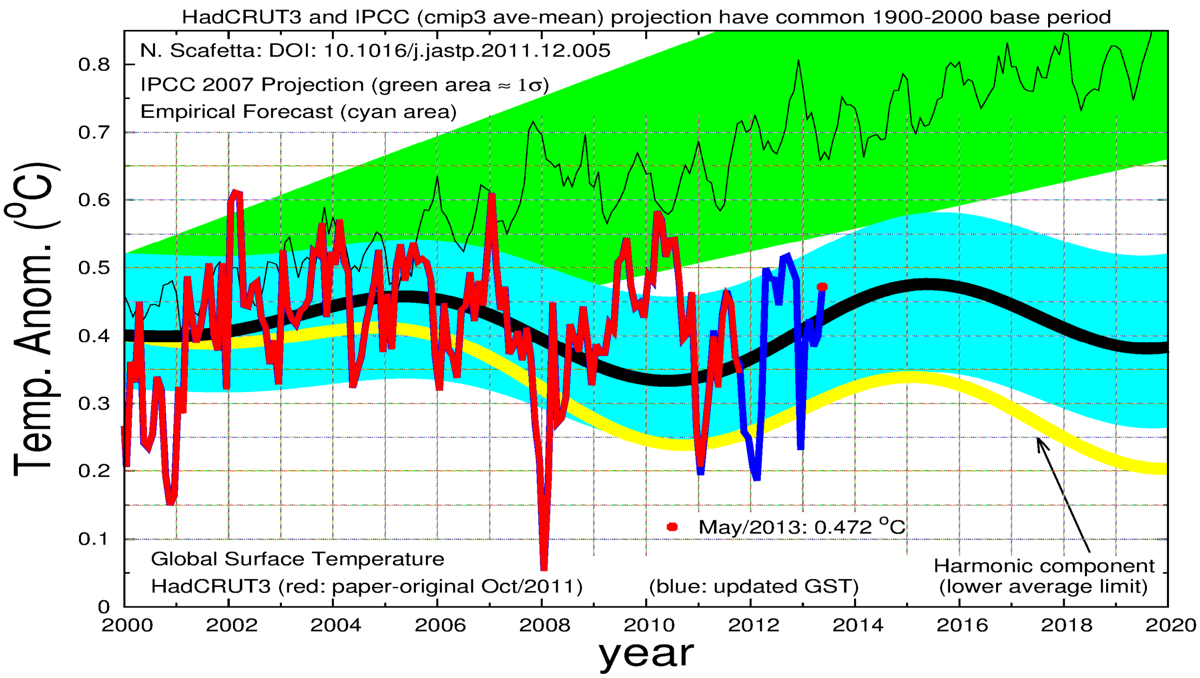
Fig. 10. (A) Rescaled 60-year modulation of the solar speed relative to the solar system barycenter (SSSB) (black) (see Fig. 6B) against the global surface temperature record detrended of its quadratic fit. (B) Rescaled modulation of the solar speed relative to the solar system barycenter (black) (see Fig. 6B) against two alternative pass-band filtered records of the temperature around its two decadal oscillations. The figures clearly indicate a strong coherence between the astronomical oscillations and the oscillations observed in the climate system.

In the above figure, the red and blue curves represent filtered temperature anomaly, while the black curves are not curve fitting but are oscillations deduced directly from astronomical considerations. The synchronicity of the oscillations would be evident to any unbiased reader.

Thus, the physical message of Scafetta's result is that the true physical origin of these quasi 20- and 60-year climatic oscillations should be found in missing astronomical forcings of the climate system. This simple logic is however misinterpreted by BHDCN2013 with hand-waving and inaccurate arguments.

Finally, it is evident that simply arguing that Scafetta's works are "erroneous" because the exact physical mechanisms responsible of these cycles are still unknown, is a logical and scientific fallacy. In fact, for millennia people have concluded that the ocean tides were linked to the lunar phases and caused by the moon without knowing the physical mechanisms. Also today the ocean tides are predicted with harmonic models based on astronomical oscillations, as Scafetta's models, because the physics is not known with sufficient accuracy to predict them using general circulation models.

In general, all geophysics is based mostly on observational theories (such as Scafetta's one) while analytical models such as the general circulation models adopted by the IPCC have failed to properly reconstruct and predict the temperature patterns with comparable accuracy (Scafetta 2012a). On this very point Scafetta's model has been demonstrated to greatly outperform the IPCC model as demonstrated in Scafetta's works (2010, 2012a). This is also evident in the figure below that reproduces and updates Figure 5b in Scafetta (2012a).



In the above figure the cyan area represents Scafetta forecast model since 2000, the green area represents the IPCC projections based on their adopted general circulation models and the red curve is the global surface temperature updated in blue since 2011. There is no need to emphasize that Scafetta's astronomical model performs much better than the IPCC models. Moreover, Figure 5b in Scafetta (2012a) shows two harmonic models that essentially coincide. One of the two curves is calibrated during the period 1850-1950, which indicates that Scafetta's model would have been able to predict the steady temperature observed since 2000 well 60-years ago, in 1950!

Essentially, BHDCN2013 arguments are mostly based on their erroneous and false opinion that the science on climate change is already perfectly understood and "settled" and that they have already won the debate. Yet, they claim that the large patterns observed in the climate system that the models fail to reproduce are simply due to "unpredictable noise", instead of accepting the existence of missing physical mechanisms. So, they engage in hand-waving arguments to dismiss papers investigating the missing physical issues. Yet, they fail to indicate a single climate model that performs better than the model proposed in the criticized works in reconstructing the climate patterns at multiple scales.

In fact, the IPCC general circulation models are demonstrated in Scafetta (2010, 2012a) to perform far poorer than the harmonic constituent astronomical model. But this comparison is missed in BHDCN2013. In addition, in Scafetta (2012b) a theoretical solar model is also developed whose oscillations well correlate with the temperature oscillations such as the quasi-60 year cycle, which is particularly evident since 1850.

The fact that the theoretical harmonics may differ slightly from what could be deduced from statistical analysis of the data is here irrelevant. In fact, it is well known that complex oscillating systems use the theoretical frequencies as limit cycles around which the physical realization fluctuates chaotically.

Thus, it is not surprising that small divergences between the theoretical frequencies and data analysis might emerge in particular when limited and noisy time series are analyzed. On the contrary, BHDCN2013 needs to determine the physical origin of their claimed departed frequencies and demonstrate that the result is at clear odds with the proposed theory, which is something that they do not do.

BHDCN2013 simply interpret the climate system as stochastic “noise”, but this is hardly a plausible alternative physical explanation given the fact that the climate is a dynamical system, not just a stochastic system. In fact, it is common among researchers to describe a complex signal that they do not understand as “noise” but this only highlights the researcher’s ignorance about a specific phenomenon. On the contrary, Scafetta (2010) demonstrated that the observed patterns are not noise because they are simultaneously present in alternative global surface temperature records such as in the North and in the South hemisphere, land and ocean.

4. The physical errors of Benestad’s “free-phase” climate model

About some other surprising physical claims, at page 462 BHDCN2013 continue questioning Scafetta (2012a) methodology to test whether and how the IPCC model simulations (all of them) accurately reconstruct the temperature patterns. My method used cycles with phases and amplitudes found for the temperature observations and used these as a yard stick for testing the ability of the GCM to reconstruct the observed patterns. BHDCN2013 claimed that my approach is “wrong” because according to them the phases had to be left “unconstrained” in the analysis. They write: “*A more appropriate null hypothesis would be that the amplitudes seen for the 20 and 60 yr variations would be due to noise. Hence, it is important to allow the phase to be unconstrained in the analysis, as we have done (Fig. 3).”*

The irony about BHDCN2013 argument is that Figure 9 in Scafetta (2010) compares the spectrum produced by the temperature, by the astronomical harmonics and by the GISS ModelE. The power spectrum does not contain information about the phases. I demonstrated that the astronomical harmonics are far more coherent to the temperature power spectrum than is the GISS ModelE. So, also BHDCN2013 “free-phase” climate model would perform far worse than the astronomical harmonic constituent model, and this is already demonstrated in my papers. However, in their “agnotological” argument BHDCN2013 neglected to mention this finding, which also demonstrates poor reading of my papers and continuous straw-man tactics that they employ to mislead the readers. Scafetta (2013c) further demonstrates that all IPCC general circulation models (the CMIP5 models that are to be used in the IPCC 2013) do not reproduce the spectrum of the temperature records which already rebut BHDCN2013.

In any case, more specifically BHDCN2013 claimed in their Figure 3 that they repeated the calculation in some different way than those in Scafetta (2012a) by treating the phase of the oscillations as “free” parameters (I will come back later on this point), and of course they got some different values.

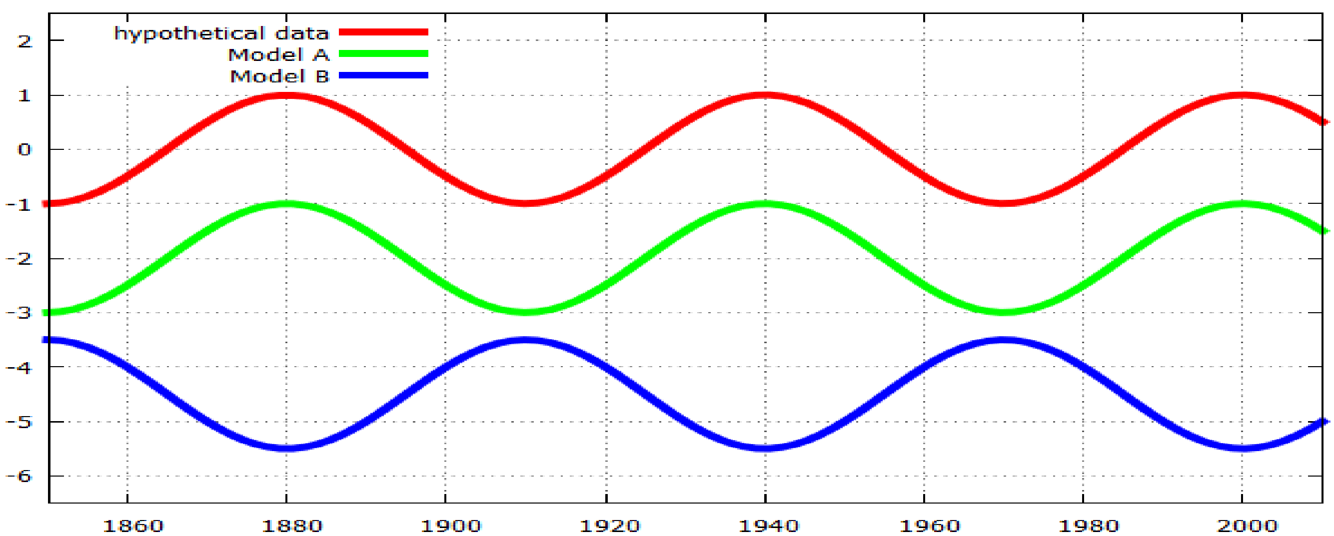
First, it is not really clear how they made the calculations nor which results they really got. On the contrary, in Scafetta 2012a and in the supporting file of that paper, very detailed equations and calculations for each computer model and for each computer model simulation were explicitly detailed. Also figures for each computer simulations were plotted so that a reader could easily note the failure of the models to reproduce the data at naked eyes. Thus, BHDCN2013 claims cannot be truly verified, and a reader is left to just “trust” their words that they have done this and that. In any case, also in the BHDCN2013 calculations (yellow and red estimates) the models perform quite poorly requiring the 90% of the confidence interval (almost 2 sigma) to get the 20 and 60-year oscillations, but failing the 1-sigma (~66%) confidence, which is what really matters in statistics.

BHDCN2013’s calculations are very sloppy indeed, considering that they want to demonstrate that the calculations made in another papers (e.g. Scafetta 2012a) are wrong. Scafetta’s paper contains about 30 tables full of data results plus about 40 figures, and BHDCN2013 could not even write down the equations that they are using for their calculations and could not show a single figure!

But now let us discuss the physics of their proposed “unconstrained” or “free” phase model, which in their opinion is the “right” way to deal with the IPCC climate models.

If I understand well BHDCN2013 and their “free-phase” climate model, if the global surface temperature presents local “maxima” around 1880, 1940 and 2000 (the 60-year cycle) while a climate model produces local “minima” around the same periods, e.g. minima in 1880, 1940 and 2000, then the model reconstructs the temperature patterns “well” because the phases do not matter!

This is pure non-sense. A climate model is supposed to reconstruct the temperature patterns in the correct timing as they occur in reality, not at random dates as BHDCN2013 think. Let us explain this simple concept with an ideal example that used the figure below:



The figure above demonstrates how unphysical BHDCN2013 “free-phase” climate model is. The figure shows a hypothetical physical time series in red and two models A (green) and B (blue) supposed to reconstruct the data. According to BHDCN2013 the phase should not matter and must be kept “unconstrained” when testing whether the model reproduce the data. Therefore, according to BHDCN2013, Model A (which accurately reconstructs the data phase such as the model proposed in my papers) and Model B (which is completely out of phase with the data like the GCM of the IPCC which peak usually in 1960 while the temperature peak in the 1940s) equally “well” reconstruct the red physical record! I fear that such a curious reasoning is more “unique” than rare in science!

By claiming that the phases of the harmonics must be free parameters and do not matter to test the ability of a model to reconstruct a physical record, BHDCN2013 are implicitly acknowledging that the IPCC climate models do not reconstruct nor they are supposed to reconstruct the climate system patterns as they are in reality but they are supposed to only produce random noise. In BHDCN2013’s understanding of climate science, it is the reader that needs to manually and arbitrarily shift the phases of the computer climate model simulation patterns despite their physical meaning to find a better match with the data patterns!

For example, it would not matter that the models do not reproduce the standstill temperature from 2000 to 2013, as observed. What according BHDCN2013 would matter to confirm the “accuracy” of the models is to check whether the models produce a temperature standstill during some unspecified period and for any reason. For example if a model may reproduce a standstill from 2050 to 2060, another model produces a standstill from 2040 to 2050, etc, then BHDCN2013 would say that these models are “correct” despite the fact that they do not actually reproduce the standstill from 2000 to 2015.

This way of reasoning is “non-sense” in science. In fact, according to the scientific method, if the climate models are supposed to do what BHDCN2013 think they should do, the climate models are essentially useless, they cannot be really tested (in fact any model-data discrepancy would never matter) and, consequently, these models would not belong to the realm of science. The truth is that BHDCN2013 cannot point to any model nor any model single simulation that agrees with the data better that Scafetta’s model and, to cover their embarrassment, BHDCN2013 invented the scientifically novel and unsatisfactory concept of a “free-phase” climate model philosophy.

The physical flaw of BHDCN2013 argument is self-evident. If a model produces a specific geometrical pattern at the wrong timing, the evidence is that the model is not modeling the real physical system, but something else, and that the modeled patterns may be due to a physical cause different from those causing the real patterns. For example, while the temperature pattern shows an oscillation peaking around 1880, 1940 and 2000 which may be due to an unknown mechanism causing this oscillation (e.g. Solar activity, see Loehle C. and N. Scafetta, 2011), the IPCC models usually peak just after 1960, which is a pattern that was due to the GHG warming suddenly interrupted by large volcano eruptions in 1961-1963. It is evident that it is not possible to shift the 1960-1963 volcano eruptions in the 1940s to reproduce a better matching of the IPCc climate model outputs with the temperature cooling from 1940 to 1970.

However, the major interpretative error made in BHDCN2013 is in not understanding that in my papers I am proposing an alternative climate model, one that claims that the climate patterns are synchronized to a set of astronomical oscillations, more or less as it happens for the tidal system. Therefore, the model proposed in my papers does not need to manually “randomize” the phases as BHDCN2013 must do with their “free-phase” climate model to attempt a better agreement with the data. As my papers demonstrate, the interpretative power of my model is far superior to the IPCC models that on the contrary would require a manual “randomization” of their phases.

In general, it is the comparison aspect between the astronomical model and the IPCC models addressed in my papers that BHDCN2013 completely miss. The numerous math and physical errors made in BHDCN2013 simply demonstrate that they have not properly studied Scafetta’s works and that they have a curious understanding of physics and math.

5. Miscellaneous math and physical errors

There are numerous and severe math and physics errors in BHDCN2013 here and there. Let us analyze three cases.

5.1 Excuse me, how is χ^2 defined?

At page 461 they state: *“Other mistakes in the (Scafetta) paper included a misapplication of the chi-squared test used to assess the global climate models (GCMs) against the observations, where Scafetta used the squared error-estimates in the denominator; conventional chi-squared tests do not square the denominator, see e.g. Wilks (1995) and Press et al. (1989).”*

This is severely incorrect. In equation 11 in Scafetta (2012a) the comparison is correctly made between the square of the deviation of the model from the temperature and the estimate of the measured variance (= square of the standard deviation) of the model itself. So, I need a square both at the numerator and at the denominator. BHDCN2013 apparently do not know that the denominator must contain the square of the standard deviation also because the ratio must be dimensionless.

In general, if I have a theoretical function f(x) and a set of data (x_i,y_i) where y_i has standard error σ_i , the chi squared function is defined as

$$\chi^2(x_i) = \left(\frac{y_i - f(x_i)}{\sigma_i} \right)^2 = \frac{(y_i - f(x_i))^2}{\sigma_i^{\,2}}$$

that is equivalent to what I have used in my equation 11, with the square of the standard deviation of the error at the denominator.

On the contrary BHDCN2013 misunderstood it for the Pearson's chi-squared test defined sometime as

$$\chi^2 = \sum_{i=1}^{n} \frac{(O_i - E_i)^2}{E_i}$$

where in the denominator the theoretical value E_i (without square) is used simply because such a test is valid ONLY when the observational value O_i is Gaussian distributed around the theoretical expected value E_i with a standard deviation given by the root of E_i, which is an assumption that sometimes is made in statistics in specific cases. Thus, in the above equation, the denominator value E_i represents the "square" of the theoretical standard deviation. In the case discussed in my paper, the "square" of the standard deviation is not given by the absolute value of the model prediction itself but by its own measured variance, as correctly used in my equation.

It appears to me that BHDCN2013 need to take some class in statistics! In any case, in my case I need to use the equation as I used it because its meaning is to provide an estimate of the statistical deviation of the model from the data.

5.2 What is the difference between "resonance" and "synchronization"?

Several times BHDCN2013 claim that some of the comments reported in my papers are wrong. For example, at page 460 they say: *Scafetta (2012a) can be reviewed in terms of the physics and the statistical analysis. The paper failed to acknowledge that resonance is an inherent property of a system, and will pick up any forcing with matching frequency.* And they go on claiming that *"Noisy forcings embed a range of frequencies, as well as transient functions, and can therefore feed a resonance";* "*if such a resonance implies positive feedbacks, these should also be present in a situation of GHG forcings." "L&S2011 assumed similar resonance as Scafetta (2012a), with the same weaknesses."* And in Table 1 they even provide a resonance simulation.

The above is a typical straw-man argument that uses a false premise on which BHDCN2013 develop a criticism. In fact, in his own papers Scafetta talks mostly of "synchronization" of coupled oscillators (see Scafetta, 2010, appendix), and when he refers to "resonance" he refers to that specific resonance that emerges from such synchronization. BHDCN2013 miss completely the issue and go on developing their own misinterpretation of the facts to mislead a reader.

Essentially, because in Scafetta's theory the astronomical forcings are essentially oscillators and because the climate system is regulated by an internal circulation dynamics, what likely happens is a "synchronization" of the internal climatic circulation to the external harmonic forcings. This is exactly what happens for the tidal system that is self-synchronized to the gravitational harmonics. Scafetta (2010) explain extensively this concept in his appendix. Yet, BHDCN2013 never get the issue up to the point that they never use the word "synchronization" even once!

With their straw-man argument BHDCN2013 mislead the reader letting him to think that Scafetta does not understand sufficient physics. On the contrary, the truth appears to be that BHDCN2013 do not understand the phenomenon of "synchronization" which differs from a mere "resonance" of a "non-malleable body" whose internal frequencies are "fixed" (which is what they are thinking of). In fact, in cases of "synchronization" the system is sufficiently malleable to adapt its own internal dynamics to the harmonics of the forcings and resonate with them. Again, this is what happens for the ocean tidal system and may happen for the climate system as well.

It is true that a system can self-synchronize to specific internal resonances activated even by noise. However, the problem with such interpretation referring to the climate is that Scafetta showed that the climate oscillations appear synchronized to astronomical oscillations at multiple scales from the decadal to the millennial one. Thus, BHDCN2013 want the reader to believe that the fact that the climate system presents oscillations synchronized to multiple astronomical oscillations is just a "coincidence". Even if so, BHDCN2013 do not explain the physical origin of the presumed coincidence and therefore they do not disprove the theory proposed by Scafetta but simply express their "disbelief" in it. They simply conceal their true intention with a straw-man argument that misrepresents Scafetta's proposal by arguing from a "resonance" point of view while Scafetta argues from a "synchronization" point of view. But, just expressing a "disbelief" is not a "scientific demonstration" that Scafetta's proposed theory is necessarily erroneous.

5.3 How are the temperature trends defined by Scafetta?

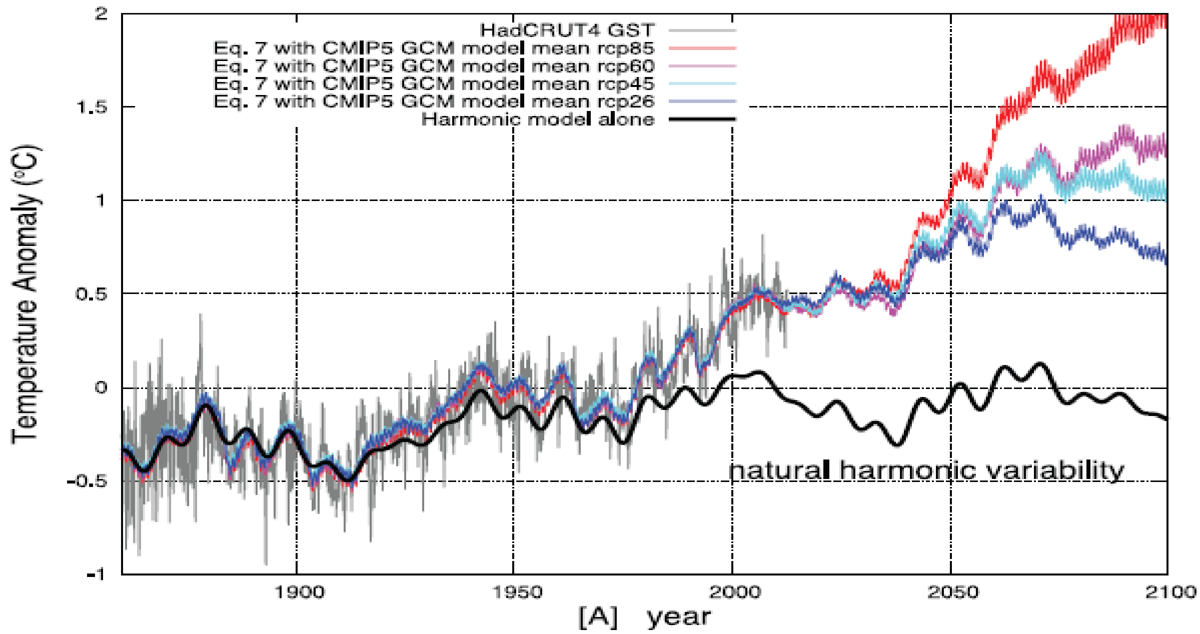
Another curious BHDCN2013's comment is made in page 461 when BHDCN2013 state: *"Another weakness in the analysis presented in Scafetta (2012a) is the handling of trends, as a quadratic trend that conveniently fitted the data was used for the period 1850–2000, and then a linear fit with a warming rate of 0.009 Cyr^{-1} was used after 2000. Hence, the method used by Scafetta implicitly assumed that the rate of warming was abruptly reduced in year 2000 for the future. It also implied that the future warming rate was smaller than the range reported in Solomon et al. (2007), and much of the recent warming was mis-attributed to natural variations*"

This comment is a typical example of poor reading and understanding of my paper (2012a). As clearly stated in my paper I used a linear trend from 2000 to 2050 (Eq. 9 in Scafetta 2012a) because the IPCC projections can be roughly approximated from 2000 to 2050 (up about 2100) by linear trends with some errors bars (which are ignored in BHDCN2013) as demonstrated at page 47 of the supplement file.

As also clearly explained in the paper the estimate of this trend is reduced by about 0.4 relative to the *0.023 Cyr^{-1}* central estimates of the IPCC models because the missed 60 and 20 year oscillations would imply that the anthropogenic component is overestimated by about the same factor. This is an important point of my hypothesis that BHDCN2013 misinterpreted completely.

Essentially BHDCN2013 did not realize that the quadratic fit from 1850 to 2000 simply captures, at a second order approximation, the secular trending of the warming from 1850 to 2000. This 1850-2000 warming trending is due not to anthropogenic forcing alone, but to whatever is causing it. On the contrary, the linear component after 2000 is supposed to capture and simulate only the anthropogenic contribution as deduced by the models but with a reduced climate sensitivity, as estimated by taking into account the existence of the oscillations such as the 20 and 60-year oscillation that would be responsible of about 60% of the warming observed from 1970 to 2000. So, the two components cannot be directly compared as BHDCN2013 misinterpreted.

The model proposed in Scafetta (2012a) using parabolic and linear curves to simulate the secular trending from 1850 to 2000 and the anthropogenic warming projections after 2000 is not part of the harmonic astronomical model itself but simple first order geometrical approximations of these trends that at the time could not be more accurately reproduced because the millennial and secular astronomical oscillations were not yet identified with sufficient accuracy. The problem of identifying these additional long astronomical oscillations was addressed in Scafetta (2012c) and a new harmonic model that does not use any more parabolic and linear curves is proposed in Scafetta (2013c). This updated model is presented in Scafetta (2013c). This represents a progression of the theory.



There is no need to comment about the excellent agreement between the data and my model. Note that before 1970 and 2000 the model secular trend is produced by a full hindcast because the secular trend is calibrated during the 1970-2000 period alone.

6. Physical flaws and math errors in Benestad and Schmidt (2009).

BHDCN2013 write:
page 456: *"Often the methods can be tested (Pebesma et5 al., 2012), and some of these claims have already been revealed as flawed analysis (Benestad and Schmidt, 2009)."*

page 472 *"Benestad and Schmidt (2009) demonstrated that the strategies employed in Scafetta and West (2005, 2006a, b, 2007) were unsuitable for analysing solar-terrestrial relationships"*

The above claims are quire curious given the fact that immediately after the publication of Benestad and Schmidt (2009) I published a partial rebuttal where I demonstrated some of the major mathematical errors made by Benestad and Schmidt in applying the Maximum Overlap Discrete Wavelet Analysis. This strong rebuttal was published on July/22/2009 at Dr Pielke Sr. Blog at

Nicola Scafetta Comments on "Solar Trends And Global Warming" by Benestad and Schmidt

http://pielkeclimatesci.wordpress.com/2009/08/03/nicola-scafetta-comments-on-solar-trends-and-global-warming-by-benestad-and-schmidt/

The same article was then published and commented in several other blogs, e.g. at WUWT

Scafetta: Benestad and Schmidt's calculations are "robustly" flawed.

http://wattsupwiththat.com/2009/08/04/scafetta-benestad-and-schmidt%E2%80%99s-calculations-are-%E2%80%9Crobustly%E2%80%9D-flawed/

http://rankexploits.com/musings/2009/arent-end-points-pesky-sciaffetta-responds-to-bs-paper/

where a few hundred people could verify that indeed Benestad and Schmidt (2009) contains the math errors that I pointed out. Benestad too knowns well about my rebuttal, but in BHDCN2013 they completely ignored to mention it, which is quite curious indeed.

More recently, I have published a formal paper in the peer review literature where I demonstrate in detail some of the math and physical errors made in Benestad and Schmidt (2009). This is

Scafetta N., 2013a. 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.

where I discuss both the collinearity errors made in the regression algorithm adopted by Benestad and Schmidt (2009) (they used ten collinear constructors in their regression model) and the wavelet filtering errors using erroneous padding (they used the periodic instead of the reflection one) and the erroneous sampling of the data they used.

Therefore, it is not clear to me how a study such as Benestad and Schmidt (2009), which contains seriously flawed mathematics, can be used to demonstrate anything about the works of other people.

On the contrary, Benestad may need to consider withdrawing his 2009 JGR paper with Schmidt, given the fact that his paper has been demonstrated to be seriously mathematically flawed since 2009.

In addition, even ignoring the major mathematical errors, the argument advanced by Benestad and Schmidt (2009) is nevertheless flawed. For example, they questioned my 11-year solar cycle signature evaluation on the temperature (that gives a max-to-min amplitude of about 0.1 ^{o}C, which is also confirmed by numerous other studies and by the IPCC 2007 AR4 too, as highlighted in my papers, e.g. Scafetta, 2007, 2009) by simply applying my same wavelet decomposition analysis proposed in earlier works not to the real temperature record, as I did, but to the GISS Model E simulations! As also better explained in Scafetta (2013a), Benestad and Schmidt (2009) did not realize that a mathematical filtering approach used to separate a signal from the noise would not work well when applied to the GISS ModelE simulations because these simulations do not reproduce the temperature patterns but only produce a very large stochastic noise with an upward trend against a very small 11-year solar signature. This is at least 3 times smaller than what has been measured by numerous people, as shown in my papers (e.g., Scafetta, 2009; Scafetta 2010; Scafetta 2012a; Scafetta 2013a). Thus, in the case of the GISS Model E simulations the signal-to-noise ratio is too small to properly extract the 11-year solar signature using a filtering methodology as the one used by me in my earlier 2005-2006 works that Benestad and Schmidt (2009) criticized.

In general, the result of the analysis of a given physical sequence (e.g., global temperature records) cannot be rebutted by simply applying the same methodology to some unrelated sequence (e.g., computer simulations that do not reconstruct properly the data) because the data analysis methodologies are chosen also by taking into account the signal-to-noise ratio and other things that are characteristic of the analyzed signal.

7. The logical fallacy that "editorial resignation" = "scientific demonstration"

BHDCN2013 also contains numerous curious arguments such as when at page 455 they write:

"It is well-known that there have been some glitches in the peer review: a paper by Soon and Baliunas (2003) caused the resignation of several editors from the journal Climate Research (Kinne, 2003), and Wagner (2011) resigned from the editorship of Remote Sensing over the publication of Spencer and Braswell (2010). These papers have not been retracted, however, correction or errata are expected to be published when severe flaws are discovered to avoid that others unfamiliar with the papers later on base their work on incorrect information."

BHDCN2013's argument is clearly logically flawed. In science only a definitive mathematical/physical demonstration can determine whether a published scientific claim is erroneous. It is evident that the mere resignation of a number of editors who simply disagreed with the results published in a paper does not demonstrate by itself that the incriminated papers (in some case published with the previous approval of the same resigned editor as in the case of Spencer and Braswell) are necessarily fundamentally erroneous: note that small imprecisions may always exist.

In any case, contrary to what BHDCN2013 lets a reader to believe, cases such as Soon and Baliunas (2003) are very complex, as documented for example here:

http://en.wikipedia.org/wiki/Soon_and_Baliunas_controversy
http://newzealandclimatechange.wordpress.com/2011/11/27/climategate-2-and-corruption-of-peer-review/

where even some external pressure on the editors, who may have felt intimidated, may be suspected. For example, the Wikipedia article says that "Jones replied Mann that "I think the sceptics will use this paper to their own ends and it will set paleo back a number of years if it goes unchallenged. I will be emailing the journal to tell them I'm having nothing more to do with it until they rid themselves of this troublesome editor", referring to de Freitas." And "By May the journal's editors Hans von Storch and Clare Goodess were receiving numerous complaints and critiques of the paper from other scientists, to such an extent that they raised the issues with de Freitas and the journal's publisher Otto Kinne. In reply, de Freitas said they were "a mix of a witch-hunt and the Spanish Inquisition".

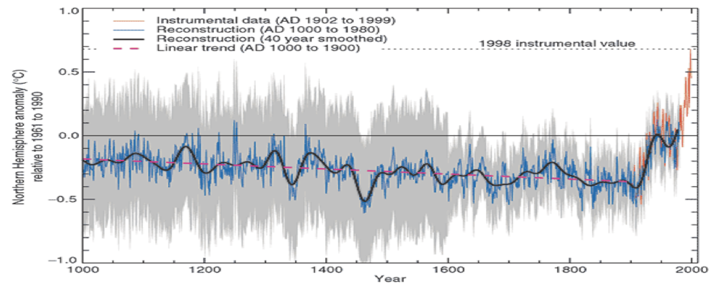
Note that the accusations against de Freitas (the editor handling Soon and Baliunas (2003)) were unjustified, as demonstrated by Otto Kinne (the director of the journal) here

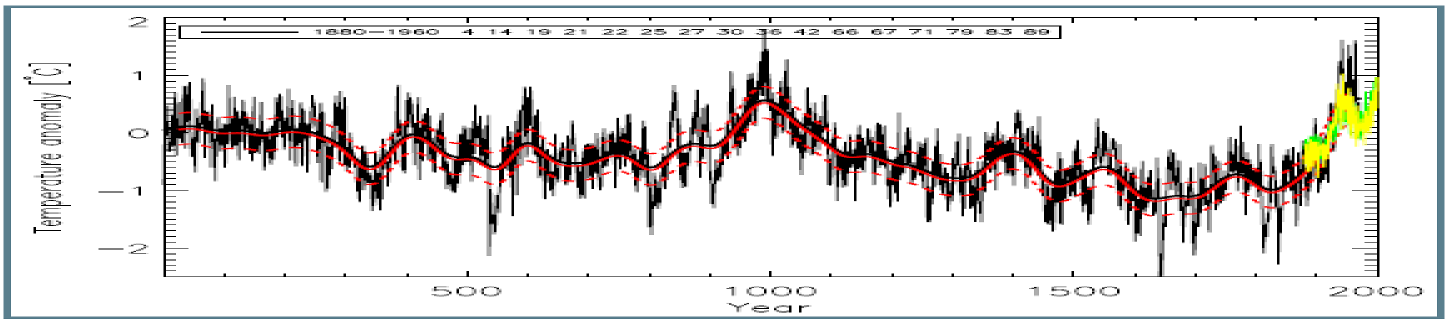
http://wattsupwiththat.com/2011/11/28/a-response-from-chris-de-freitas/

Although herein it is not possible to demonstrate the real cause of the resignation of a number of editors (if they did it in good faith or under some kind of pressure or something else), it remains a logical fact that such editorial resignations, by themselves, do not demonstrate the incriminated papers to be scientifically erroneous and misleading. Therefore, BHDCN2013's argument is logically flawed, and reveals prejudices from the authors.

I do not want to discuss the scientific issues of each case in detail, but I simply observe that Soon and Baliunas (2003) and later the far more detailed Soon et al. (2003) questioned the Hockey Stick temperature reconstruction by Mann et al. (1999) observing that a large number of paleoclimatic climatic sequences suggested that the Medieval Warm Period was more significant than what was suggested in the original Mann's Hockey Stick temperature reconstruction used in the IPCC 2001.

Indeed, more recent publications are apparently confirming to some extent the claim of a widespread Medieval Warm Period. For example, the recent extra-tropical Northern Hemisphere temperature reconstruction by Christiansen and Ljungqvist (2012). Below I show a comparison between the record by Mann (top) and that by Christiansen and Ljungqvist (bottom):





The difference between the two records about the significance of the medieval warm period is remarkable, and Christiansen and Ljungqvist record may partially support the claims of Soon and Baliunas (2003) and Soon et al. (2003) of a widespread medieval warm period.

Curiously, on climate audit McIntyre reports (http://climateaudit.org/2012/07/01/lonnie-and-ellen-serial-non-archivers/) that Tom Wigley apparently acknowledged that Soon and Baliunas might have had a point that 20th century precipitation was not unusual (a theme revisited in AR5 Zero and First Draft). Writing to Mann and others (2003-06-06 682.) Wigley wrote:

Mike,
Well put! By chance SB03 may have got some of these precip things right, but we don't want to give them any way to claim credit.

In any case, reconstructing past climate is a very difficult task also because only proxy models are used to reconstruct past temperatures. I do not see problems if different researchers can have different opinions on complex topics and propose different hypothesis. So, the resignation of those editors as well as Wigley's claim that people such as Soon and Baliunas should not be given credit even if found correct, sounds strange and academically improper to me.

After all, Soon and Baliunas were strongly accused to confuse temperature and precipitation proxies because according to Mann "it is fundamentally unsound to infer past temperature changes directly from records of drought or precipitation" (read the reference here:
http://stephenschneider.stanford.edu/Publications/PDF_Papers/MannSenateQuestions.pdf.).

However, claims as Mann's absolute certainty that a given record can be a temperature proxy or a precipitation proxy but not both in some degree are tenuous given the fact that these sequences are, after all, used as "proxies". In fact, "proxy" models are not rigorous and experimentally tested "physical" models. This is why they are called "proxy" after all.

For example, the same record (the Monsoon SW Asia) was used as a temperature proxy in Moberg et al (2005) and as a precipitation proxy in Treydte et al (2006) (read the comments here: http://climateaudit.org/2006/04/27/treydte-moberg-soon-and-baliunas/). But neither Moberg et al (2005) nor Treydte et al (2006) were accused, as it happened in the case of Soon and Baliunas, nor editors resigned anywhere. Also the AR5 Zero Order Draft in language reminiscent of Soon and Baliunas, stated that "multiple studies suggest that current drought and flood regimes are not unusual within the context of last 1000 years" (see the comments here: http://climateaudit.org/2012/08/01/hide-the-megadroughts/)

Thus, BHDCN2013's language and accusations claiming papers wrong just because some editors resigned over some publications are scientifically unjustified and improper: BHDCN2013's argument appears to be a political ploy, not a scientific "demonstration".

The future will tell whether papers such as Soon and Baliunas (2003) were "absolutely" wrong. History is filled of cases were specific theories and hypotheses were first strongly opposed and, in some case, the scientists proposing them were ridiculed and even persecuted and arrested, and later it was found that the theories were essentially correct.

After all, as explained above, since 2009 I have demonstrated that Benestad and Schmidt (2009) present some serious math errors, but no editor at JGR resigned for that yet.

8. The logical fallacy of Cook's "97% consensus" argument.

At page 454 BHDCN2013 write a section advocating the classical "consensus" argument as a "demonstration" that the criticized works had to be considered suspicious: *"Cook et al. (2013) reviewed nearly 12000 climate abstracts and received 1200 self-ratings from the authors of climate science publications. Using both methodologies, they found a 97% consensus in the peer-reviewed climate science literature that humans are causing global warming."*

More seriously, BHDCN2013 argue their case by inferring that the incriminated papers are essentially "guilty" to create misinformation in the public. In fact, a mismatch apparently exists between Cook's 97% "expert" consensus finding and the public perception where they report that half of the population apparently does not believe in the IPCC AGW.

BHDCN2013 fail to realize the irony of what they say because if just a few papers from a few critics were able to convince well 50% of the population then these few papers and these few critics had to be very convincing indeed!

In any case, events such as the Climategate emails and the more important fact that the IPCC models have predicted an average warming of about 2 ^{o}C/century since 2000 while no warming has been observed are facts, and they might also have contributed to generate in the general population some doubts in the IPCC AGW interpretation. Yet, BHDCN2013 ignore to discuss these cases and their effect in the general public.

The "consensus" argument is a logical fallacy known as *Argumentum ad populum*, that is an appeal to widespread belief, bandwagon argument, appeal to the majority, appeal to the people where a proposition is claimed to be true or good solely because many people believe it to be so. Yet, Galileo Galilei is quoted to say that *"In questions of science, the authority of a thousand is not worth the humble reasoning of a single individual*."

However, BHDCN2013 argument is fallacious also for another important objective reason.

A careful reading of the Cook et al. (2013) paper demonstrates that the claimed 97% consensus refers to the claim that humans are contributing more than 50% of the global warming since the mid-20th century. Indeed, a careful reading of the database of Cook et al. (2013) even includes in the 97% consensus one of my papers. In fact, my papers argue that about 40% to 70% of the observed warming might have been induced by natural factors (solar effects, oscillations etc.). Therefore, also some of my papers can be interpreted as crossing the 50% borderline criterion adopted by Cook et al. (2013).

The problem with Cook et al. (2013) "97% consensus" argument is that it is severely misleading.

The Anthropocentric Global Warming (AGW) theory as advocated by the IPCC since 2001 (which is the theory that an increasing percentage of the population does not believe correct any more) states that the net anthropocentric forcings have contributed about 100% of the total warming since 1900 and even more than 100% (since suppressed by aerosols) of the warming since mid-20th century. However, Cook et al. use the claim that the AGW is quantified by the IPCC advocates as 50+% of the total observed warming. This is very misleading.

The correct interpretation is clearly evident in figures 9.5a and 9.5b of the IPCC report http://www.ipcc.ch/publications_and_data/ar4/wg1/en/figure-9-5.html which for the benefit of the reader I report below:

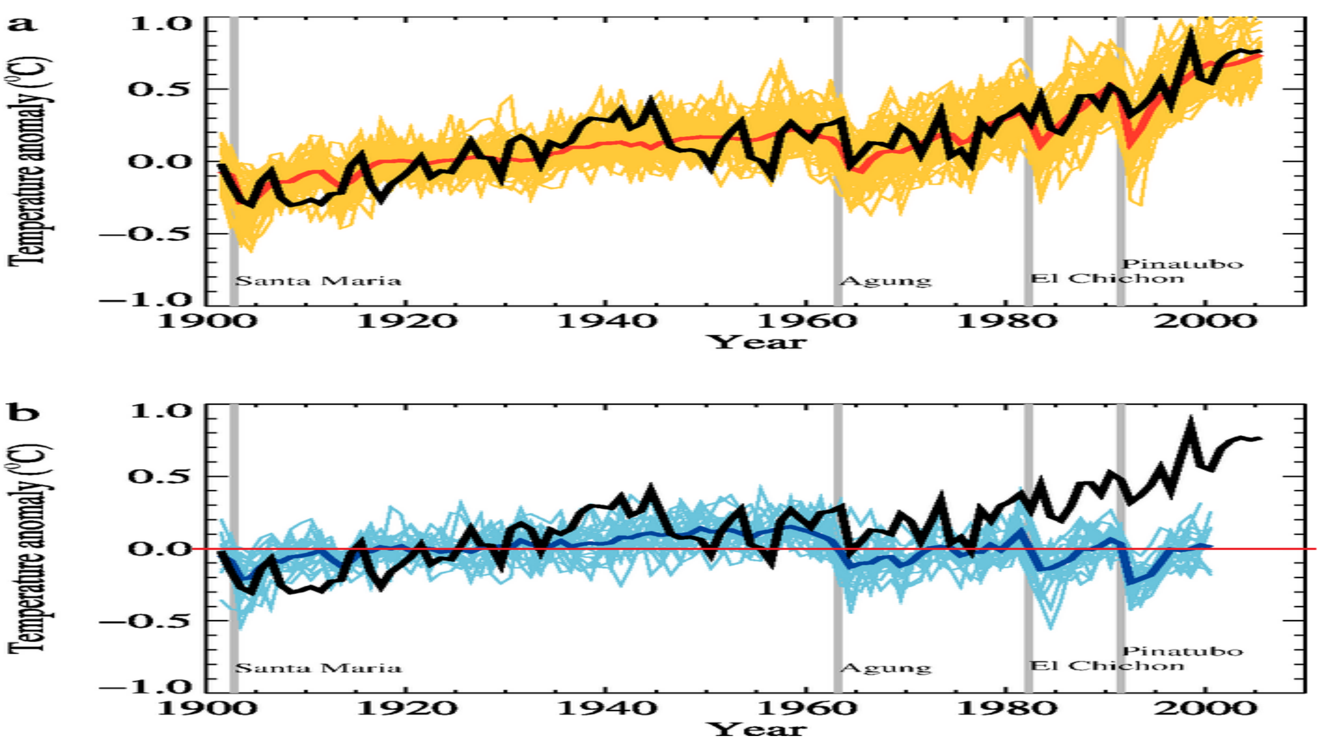


Figure 9.5 shows a comparison of general circulation model simulations made [A] with all used radiative forcings (human + natural) and [B] with natural forcings (solar radiative forcings + volcano) alone.

As it is evident from the figure, when the GCMs are forced only with the supposed natural forcings alone (see blue curves in Figure 9.5b) the IPCC model result is that natural forcings alone have not contributed at all to the total warming observed from 1900 to 2005. The same forcings would have caused even a slight cooling from 1950 to 2003.

On the contrary, as the IPCC Figure 9.5a shows, only the addition of the claimed anthropogenic forcings could have let the GCMs to reconstruct the observed 0.8 K warming from 1900 to 2005. Thus, the figure clearly indicates that according to the IPCC climate models practically 100% of the warming since mid-20th century was caused by anthropogenic forcing alone.

In fact, the IPCC 2007 AR4 synthesis report section 2.4 explicitly states: "*The observed widespread warming of the atmosphere and ocean, together with ice mass loss, support the conclusion that it is extremely unlikely that global climate change of the past 50 years can be explained without external forcing and very likely that it is not due to known natural causes alone. During this period, the sum of solar and volcanic forcings would likely have produced cooling, not warming.*"http://www.ipcc.ch/publications_and_data/ar4/syr/en/mains2-4.html

It is the claim that anthropogenic forcing has contributed about 100% of the post 1950 warming that is questioned in my works and by other IPCC critics because the IPCC models fail to reconstruct detectable oscillations such as the 60-year AMO oscillation from 1850 to 2000 and failed to properly reconstruct the standstill temperature since 2000, which suggests an effect of the cooling phase of the 60-year cycle (Scafetta, 2012a), which should also have greatly contributed to the warming from 1970 to 2000. And, apparently, half of the population does not believe in the IPCC claim too.

Cook et al. (2013), however, used the misleading and meaningless borderline of 50+% that could practically include almost all published papers that address the issue of climate attributions including those of notorious "skeptics" (Idso, Soon, Morner, Shaviv, Carlin, Scafetta) as demonstrated in this web-site:

http://www.populartechnology.net/2013/05/97-study-falsely-classifies-scientists.html

Indeed, even if some of these papers fall within the 50+% criterion they do severely contradict the AGW theory as proposed by the IPCC, as in figure 9.5, that advocates the 100% AGW claim.

Thus, Cook's 97% figure is not surprising at all once the methodology employed to obtain it is well understood. Cook included in their 97% figure every paper claiming that the anthropogenic contribution to the recent warming has been 50% or larger. This is, however, not what the IPCC has indicated in its climate model simulations such as in their figures 9.5a and 9.5b shown above.

Cook's 97% interpretation given in BHDCN2013 is, therefore, highly misleading because Cook et al. adopted the "broad consensus" definition of human have caused "some" warming (50+%) while BHDCN2013 interpreted it as meaning the "specific" AGW definition of the IPCC, which refers to a quasi 100% anthropogenic contribution claim to the global warming.

BHDCN2013 have "misunderstood" Cook et al (2013) even by having Cook as one coauthor, which, evidently, questions the logical consistency of Cook et al. (2013) as well!

9. Additional math and physical flaws in some of the 17 "agnotological" cases

BHDCN2013 discuss 17 "agnotological" cases. Some of the claims were rebutted above. Some refer to papers different than mine and were refuted in the comments by McKitrick and Solheim et al. So I do not repeat. I simply highlight some details.

Case 1: ignoring data which do not agree with the conclusions (!)

BHDCN2013 start criticizing Humlum et al. (2011a) claiming that they suggested the giant planets in the solar system play a role in climate change on Earth. Yet, Humlum et al. (2011a) do not make such a claim anywhere in their paper. Only this fact suggests that none of the five authors of BHDCN2013 have carefully read Humlum et al. (2011a), which alone questions the credibility of their entire paper. Humlum et al. have simply expressed in later publication a positive interest in some of my papers proposing such a theory.

Even interpreting the above as a lapse, the major BHDCN2013 argument is mathematically flawed. In their figure 1 they extended the 3-frequency model proposed by Humlum et al. (2011a) to roughly reconstruct the last 4000 years of the GISP2 record to the entire Holocene, that is back by 6000 years, as show below (note that the blue lines are added by me):

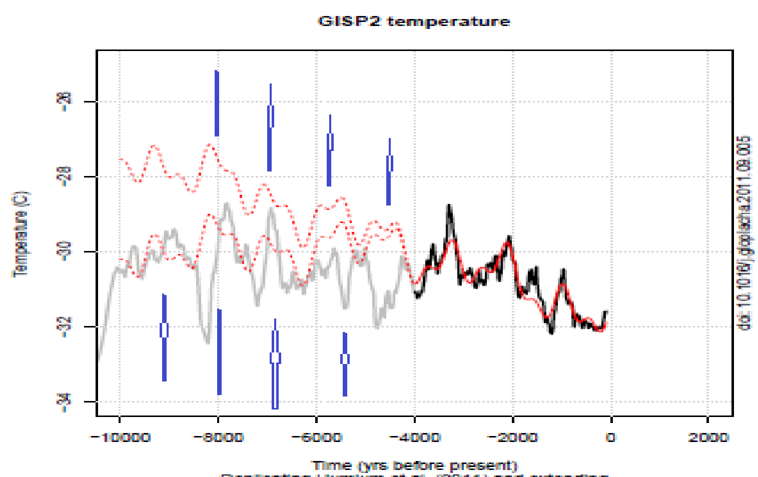


Fig. 1. A replication of Humlum et al. (2011a)'s model for the GISP2-record (solid red) and extensions back to the end of the last glacial period (red dashed). The two red dashed lines represent two attempts to extend the curve fit, one keeping the trend over the calibration interval and one setting the trend to zero. The black curve shows the part of the data showed in Humlum et al. (2011a) and the grey part shows the section of the data they discarded. The figure can be reproduced from "replicationDemos" (Table 1).

BHDCN2013 argued that Humlum et al. (2011a) is “wrong” because it does not fit perfectly well the data from -10000 to -4000 BP. However, as any expert in harmonic forecast knows, e.g. tide experts, to forecast accurately a record with a subset of statistical periodic harmonics, the records needs to contain all harmonics covering the investigated scale. BHDCN2013 may at most argue that Humlum et al. (2011a) model was “partial” (e.g. did not include the Milankovic cycles responsible for the Holocene curvature or other low or high frequency cycles) and perhaps not fully optimized because based on just statistics instead of on astronomical considerations as done in Scafetta (2012b). However, the Humlum et al. model is not misleading in the sense that the claimed oscillations such as the quasi millennial oscillation do not exist at all, and it is not misleading in the limits of the authors who never claimed that their model could accurately forecast 6000 years before -4000 BP, but at most a few hundred years after 2000 AD.

In any case, in the figure above I add some blue lines highlighting that Humlum et al. (2011a) model did hindcast with sufficient accuracy the maxima around -9000, -8000, -7000, -5000 BP. In fact, it is well known that Holocene records contain a quasi millennial cycle and other oscillations close to the three oscillations used by Humlum et al. (2011a) which are well correlated to equivalent solar cycles (Bond, 2001; Kerr, 2001; Scafetta, 2012b and references therein).

Therefore, BHDCN2013 first “agnotological” cases is nothing but a Straw-Man argument. BHDCN2013 also do not propose any alternative model explaining the GISP2 record, nor do they explain why the IPCC models are failing to reconstruct the standstill temperature since 2000.

Case 2: unclear physics and non-objective analytical design (!)

Here BHDCN2013 criticized Scafetta (2012a). I have already rebutted some of the claims above.

This entire “agnotological” case is nothing but gross misinterpretations of Scafetta’s works and a long list of accusations based on poor mathematical and physical understanding.

From a physical point of view, Scafetta (2012a) did not discuss the physical mechanics but simply stated that the mechanisms needed to be searched for in the solar/climate forcings and their oscillations. Moreover, contrary to what is claimed many times by BHDCN2013, I am talking mostly about a planetarian influence on the sun and of this on the climate, and the influence may be regulated by gravitational or electromagnetic forcings.

In any case, because the exact physical mechanism was not disclosed, BHDCN2013’s arguments on resonances, damped oscillations, time responses, responses to noisy forcing etc are only a product of their personal vivid but poor imagination and handwaving interpretations of how the things should work in their opinions.

For example they claim than any resonance climatic effect would also respond in an equivalent way to GHG radiative forcing. However, many times in my papers I am talking of a direct solar/astronomical cloud modulation by means of cosmic rays, solar wind etc, that are alternative to the GHG radiative forcing. Moreover, some of my oscillations are supposed to be tidal oscillations etc. BHDCN2013 seem to believe that because the IPCC models use only radiative forcings, these are necessarily the only forcing of the climate system that might exist.

In accusing Humlum et al. (2011a) BHDCN2013 even seem to question the existence of a lunar tidal forcing!

Moreover, as very clearly stated in Scafetta (2010) I am not talking about simple resonance but mostly of collective synchronization in coupled oscillators, which is not exactly the same thing.

About some other mathematical claims about the free-phase climate model about one standard deviation, about the chi-squared test, etc.), are non-senses as explained above.

Case 3: unclear physics and misappropriate curve-fitting (!)

BHDCN2013 criticize Loehle and Scafetta (2011) by claiming again a lack of clear physical basis and the analytical setup. They again repeat several misconceptions including their insistence that we focus on resonance mechanisms, while Scafetta (2010) assumed synchronization of coupled oscillators. In any case, to explain the exact physical mechanisms was not the topic of the paper, which only focused on applying the adopted harmonics.

Again, our model is not a simple curve fitting exercise because the harmonics are chosen from astronomical considerations. Better understanding of this harmonic is provided in following papers (e.g., Scafetta 2012c; Safetta and Willson, 2013, etc).

BHDCN2013 systematically fail to understand that scientific theories start with the modeling of the observations, not with a full analytical explanation of the physical details, which occurs gradually in time.

I discussed BHDCN2013 errors about their claimed optimized 65.75 year and 21.5 yr curves above in section 3. Finally their proposed random model exercises are meaningless because the purpose in science is interpreting the data.

Essentially, a physical theory can be challenged by demonstrating that: (1) it does not interpret correctly the data; (2) by proposing an alternative theory that interprets the data better. BHDCN2013 provide none of the two cases because they have not demonstrated that our model does not agree with the data and they have not proposed an alternative theory that better reconstructs the data. I remind again that the IPCC models preferred by BHDCN2013 perform far worse in reconstructing the data even by ridiculously keeping their phases as free parameters as BHDCN2013 claim they need to be used.

Essentially, BHDCN2013 do not provide any better interpretation of the data, they just talk about "noise".

Case 4: ignoring negative tests (!)

Here BHDCN2013 question Solheim et al. (2011, 2012) claiming among other things: *"In particular, SSH2011's estimate of the SCL for cycle 23 (12.2 yr) was substantially longer than the estimate of 10.5 yr reported by the Danish Meteorological Institute (based on Friis-Christensen and Lassen (1991) and follow-up studies) and 10.8 yr estimated by Benestad (2005) (Table 1)."*

BHDCN2013 statement is quite surprising and further demonstrates a state of confusion in which Benestad and colleagues find themselves. First, one wonders how Friis-Christensen and Lassen, and Benestad could accurately determine the length of solar cycle 23 in 1991 or 2005, respectively, given the fact that solar cycle 23 lasted from May 1996 to January 2008. Second, contrary to what stated by BHDCN2013 the official solar cycle 23 length was estimated to be 12.6 years as shown here: http://en.wikipedia.org/wiki/Solar_cycle_23

I observe that the official 12.6 year value is far closer to that estimated by Solheim et al. (12.2 year) than to that estimated by Benestad in 2005 (10.8 year) as reported in "Table 1" of his paper: Benestad table is, however, nonexistent in Benestad (2005)!

Thus, the 10.8 year length estimated in Benestad (2005) for solar cycle 23 is a clear artifact due to the fact that Benestad calculated it in 2005 when the solar cycle 23 was not ended yet. Friis-Christensen and Lassen simply conjectured the length of cycle 23, did not measured it!

Case 5: presumed dependencies and no model evaluation (!)

BHDCN2013 criticize Scafetta and West (2007, 2006a, b, 2005) using Benestad and Schmidt (2009). I discuss the case above in Section 5.

The other "agnotological" cases.

It is too long to detail the issues but the other comments by the other criticized authors highlight numerous other shortcomings in BHDCN2013.

10. Comments on the "inadequate reviews" provided by the anonymous reviewers

In my opinion the reviews provided by the anonymous reviewers are quite inadequate and mostly demonstrate their biases. In fact, they have provided no evidence that they truly checked the numerous claims made by BHDCN2013 against a large number of papers already published in the peer reviewed scientific literature and, therefore, already carefully checked by numerous other scientists. Reviewer #1 even says so explicitly.

As a consequence, it is correct to state that the reviewers simply "believed" in BHDCN2013 without truly scientifically checking anything.

It is evident to me that the reviewers' behavior manifests severe "bias" against the critiqued authors up to the point that both reviewers have not realized not only the scientific errors but also the severe inappropriateness of BHDCN2013. Anonymous reviewer 2 explicitly cheers on the purpose of the paper to debunk "deniers", in so many words, which is hardly a demonstration of objectivity.

Indeed, if BHDCN2013 think that the criticized works contain severe scientific flaws they just needed to write proper comments and submit them to the original journals. However, both reviewers were not able to acknowledge that the critiqued authors have a right to reply to

BHDCN2013 accusations and that the readers too have a right to read both sides of the debate, not just one side.

By doing so, the anonymous reviewers have demonstrated an inadequate professional attitude and poor respect toward the critiqued authors, the scientific journals that published their papers, the editors of those journals, the referees chosen by those editors, and the society and the public in general. In any case, even if clearly biased against the critiqued authors, both anonymous reviewers disliked BHDCN2013 by realizing its overall weakness and did not suggest its publication in the present form.

The behavior of these referees is nevertheless important. It clearly demonstrates the existence of strong "confirmation biases" and personal "opportunism" in the climate science debate instead of a sincere search for the truth. This problem is just strongly emphasized in particular when reviewers do that "anonymously", that is, without accountability.

This demonstrates the urgent need of drastically reforming the "peer review" process. In fact, scientific papers need to be evaluated for their scientific merits alone. As it is now, the anonymous peer review process gives "infinite" freedom to the reviewers to accept or reject a paper for any (valid or invalid) reason, which on average unfairly favors the "politically correct" theories of the time despite their flaws.

I believe that using open comments, such as at Earth System Dynamics, might be a step toward the right strategy for improving the "peer review" process. However, additional progress needs to be made in particular to make the referees accountable for what they write.

Confirmation biases and personal opportunism clearly weaken, from a purely scientific point of view, the "political correct" IPCC AGW theory advocated in BHDCN2013. Benestad and colleagues should not try to use "shortcuts." They should not attempt to publish just a critique against other authors and seek to defame scientists that they dislike by trying to prevent the simultaneous publication of the responses from the critiqued authors by taking advantage of the flaws and the inadequacy of the current anonymous peer review process.

11. Conclusion

I believe that BHDCN2013 have written a very poor and weak work under any point of view: philosophical, mathematical and physical. Above I have demonstrated a number of different errors, shortcomings and misinterpretations. Other critiqued authors have highlighted other shortcomings. Therefore, I need to suggest the rejection of this work or that this work is published as it is together with the rebuttals of the criticized authors (free of charge).

I need to conclude that BHDCN2013 reminded me the pamphlet "Hundert Autoren gegen Einstein" (A Hundred authors against Einstein) published in 1931, which today, according to Goenner, is considered a mixture of mathematical–physical incompetence, hubris, and the feelings of the critics of being suppressed by modern physicists
(from http://en.wikipedia.org/wiki/Criticism_of_the_theory_of_relativity).

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