Reply to R. E. Benestad, H. O. Hygen, R. van Dorland, J. Cook and D. Nuccitelli

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On a 'personal' note - introduction

Benestad *et al.* take us further into the debate on their paper with their reply on my comments. I am happy that they did, as it will give us a chance to delve deeper into their reasoning and logic, or lack thereof. But before we begin, some clarification on a number of issues seems obligatory. These issues are of a 'personal nature'.

First, Benestad *et al.* mention that I take 'the experience [I] have from [my] own field and thinks this also is valid for another.' The absurdity of this statement can only hit home if Benestad *et al.* seriously think that logic and reasoning is some speciality of mine (they use the term 'authority', which could only be a fallacious referral on any level of logic; I thus reject this personal characterisation) that seemingly lies *outside* the field of climate science. Exactly which field of science is free or beyond or outside the basic tenets of logic and reasoning? Here, the unwarranted compartmentalisation of science in different fields of expertise, as Benestad *et al.* clearly and openly profess, obfuscates the fact that a PhD title stands for *Philosophiæ Doctor*, meaning that the overarching capacity to do science, *no matter what field*, is defined by logic and reasoning, which is an independent and external authority above and beyond any scientific field.[1]

Consequently, I do not bring any kind of expertise (or authority) to the table other than what is implicit in the entire field of science as such (and beyond). I am thus not merely disagreeing with them as a matter of opinion, but pointing at major flaws in their reasoning and logic I will explicate on further below. Reiterating, my disagreement lies not primarily in the empirical but in the logical. Again, the latter is by definition part of any scientific discourse *including climate science*. And it seems clear that Benestad *et al.* agree with me here, making the personal reference to my purported authority all the more baffling and contradictory.

That being said, I do not at any point in my comment 'read between the lines', as Benestad *et al.* remark. If anyone makes certain statements about any number of issues, the logical inferences of those statements can be drawn freely and categorically. That is the very *raison d'être* of logic and reasoning in science (or anywhere else for that matter): making others and ourselves aware of the logical consequences of our reasoning. Those logical inferences are obviously not beyond the realm of error. Reading 'between the lines', however, is something else entirely, namely making allusions *not* derived logically from the text. At no point do Benestad *et al.* make clear where exactly I 'read between the lines' in the sense just defined (in *very* wide-ranging and generous terms). They simply assume it or confuse the one with the other.

Furthermore, the comments that I 'would not like this paper to be published (according to his own comment) because it opposes his own view about how science should be conducted' *and* that I am ostensibly presenting my own 'ex cathedra' can only be typified as nonsensical. I am emphatically not presenting my own view about 'how science should be conducted'. Worse, I have no idea what that means. What is more: since when are logic and reasoning private matters? Or more accurately, since when can anyone (me) impose a private notion of what science is (in terms of logic and reasoning) on anyone else within the scientific community (*viz.* Benestad *et al.*)? Once again, Benestad *et al.* openly try to force the issue by fallaciously shielding off –the compartmentalisation mentioned above- their own field from general logic and reasoning. That is what they clearly infer with their comments cited here.

Additionally, Benestad *et al.*'s ambiguity in reading and reasoning is exemplified by the fact I did not state or infer that I 'would not like this paper to be published', even 'according to [my] own comment' (C301). What I did assert *as a statement of fact*, as opposed to some private opinion, is that the paper as it stands *is* unpublishable because of critical errors in reasoning and logic. If Benestad *et al.* seriously think that the 'experience [I] have from [my] own field' simply cannot be valid for another (which the statements on page C296 *entail*), then by inference Benestad *et al.* raise the specter of relativism as they, consequently, seem to 'abandon any ontic ground of an objectively and independently existing world for mediating differences of opinion and distinguishing truth from falsity'.[2]

The unwarranted compartmentalisation Benestad *et al.* accentuate is exacerbated by the statement made on page C302 in which I '[presume] that the lessons made in chemistry must be true for other disciplines, ….' Of course they do, and that should be blindingly obvious by now. More to the point, the referral to the field of chemistry is *not* about *that* field *per se*, but about the much wider point on changing paradigms, or in Larry Laudan's more accurate term 'research traditions'.[3] In other words, I am talking about the history and philosophy of science with an *analogous* reference to chemistry, not about the specific empirical intricacies of chemistry or climate science.[4] Once more, some unjustifiable compartmentalisation of science is invoked.

Reply – some core issues

Now let's focus on the meat of the argument, and that primarily concerns the asymmetry I regard as the core of the fallacious structure of the paper, which essentially collapses into circularity. That circularity of the whole exercise is made quite clear in the parenthetical remark made in Benestad et al.'s reply to my comment (my italics):

> 'Sure, we are aware about the history and philosophy of science (e.g. Thomas Kuhn, Karl Popper, and Emmanuel Kant) and that history has overturned previous paradigms, but we are not saying that the consensus is right (*however, we can assume that one explanation has swayed most of the scholars in the field of climate science because it seems to be the most persuasive view – a fair definition of consensus*). We are discussing whether we can learn from controversial papers by replicating the work and check the details. We are trying to show how science can be utilized to reduce uncertainty – this even works for a "young" field as climatology ….' (C302)

Let's rework the parenthetical statement (in such a way that no one can find fault in the basic premises and conclusion):

1. Amongst the different explanations on human-induced climate change, one has swayed most scholars;
2. Consequently, the view that swayed most scholars is the most persuasive view;
3. The persuasiveness of the view that swayed most scholars has resulted in consensus amongst scholars;
4. The observed consensus is a result of the persuasiveness of the view that swayed most scholars.

Statement 1 seems straightforward. One problem with it is whether or not it is true that one explanation has indeed swayed most scholars. That needs to be settled empirically, and that does not seem easy to do. But let that pass.

The second premise seems to be the core of the argument, and it is decidedly problematical. Is it the case that the statement that 'the view that swayed most scholars' *entails* that it is the most 'persuasive' one? Charitably, I take the term 'persuasive' to be epistemic in nature (as Benestad *et al.* certainly will agree on). But of course, there are reasons that sway scholars that are decidedly *non*-epistemic in nature: money (grants), academic and political power and recognition and the like, resulting in for instance categorical falsehoods (Diederik Stapel), repression of competing ideas (Alfred Wegener, Nikolai Kondratiev, Ignaz Semmelweis), coercion amongst colleagues (Trofim Lysenko),[5] and etcetera.[6]

Now, this is what I already mentioned in my previous comments (with some extension here). What is Benestad *et al.*'s response? More opinion: '… there is difference between valid conclusions and stifling of opposing views. I don't think that Hanekamp sees the differences. In the physics community, there is a preprint-sever (arXiv), with no review. There is also a wide range of journals, and often one needs to move onto another journal because of one difficult referee. This happened in this case (McKitrick). Furthermore, *scientists are often very independent and pigheaded, and I do not think that one can regard them as being a one-minded entity, hence the assumption that a dominant epistemic community will act in a uniform way must be established before one can say for sure that this is a real problem. Hanekamp is accusing our paper of making assumptions, which he himself follows by his own set of assumptions about the scientific community.*' (C301; my italics)

The sentences in italics are just opinions devoid of any evidence (an academic failing), whereas in my comments, I presented some well-known and documented cases of epistemic (ideological) communities hindering developing scientific insights to which I added some more examples here. Those cases are archetypal features of academic history and thoroughly studied and published on. It seems that Benestad *et al.* find it adequate to counter my empirical examples with some handwaving, ironically faulting me for making unfounded assumptions in the process. To put it bluntly, I do not even have to contend with such sub-standard comments from my students.

But that's not all. Scientists might well be 'independent and pigheaded', but it seems that Benestad *et al.*, with reference to Cook *et al.* (2013), would very much like to have a uniform climate scientist community, which voices an 'overwhelmingly agree[ment] that the earth is warming due to human activity'.[7] So, that seems to make for an epistemic community that could well 'act in a uniform way' (C301), whereby the risk of non-epistemic behaviour might well increase. That such a risk is far from imaginary *is* backed up by the historical examples I referred to.

And, if one thinks that non-epistemic drivers in theory-choices will be in plain sight and thereby easily avoidable, particularly by those who by their own admission are swayed towards the ostensibly most persuasive view (of the epistemic kind), will be sorely disappointed. The difference between 'valid conclusions and stifling of opposing views' is not as straightforwardly discernable as Benestad *et al.*, yet again, imagine. For *that* there is abundant empirical evidence, which is discussed in the philosophical literature.[8] (And when the existential import of the subject matter is substantial, as it seems to the case with the climate change discourse, the 'Law of Inverse Rationality' comes most prominently to the fore: the ability of human reasoning to be undistorted by corrupting aspirations (the non-epistemic drivers such as power, money, recognition) is inversely proportional to the existential weight of the subject matter.)

Statement 2, thus, is false: the former –the view that swayed most scholars …- *does not entail* the latter –… is the most persuasive view. Statement 3 and 4, as a result, collapse: the specificity and substance of the persuasiveness of the view, ostensibly swaying most scholars (if at all), requires supporting evidence on multiple levels that would break circularity. However, nothing of the sort is done. Thereby, Benestad *et al.*'s reply simply re-establishes the circularity found at the core of their paper. The proposition that requires proof is in the final analysis assumed without proof.

Benestad *et al.* furthermore introduce a skewed outlook on the issue of peer-review by misrepresenting my statements thereon: 'Hanekamp presumes that there must be some kind of "symmetry" in the peer-reviewed publications, but he has not provided any evidence for such a symmetry: "This introduces asymmetry in the paper which will be the focus in the subsequent comments." His objection to our paper rests heavily on this asymmetry, and I'd like to ask if he expects symmetry of similar kind, as that he expects in climate sciences, regarding the question of tobacco and cancer, continental drift, quantum physics, or general relativity.' (C297)

Now, by introducing 'the question of tobacco and cancer, continental drift, quantum physics, or general relativity' they firstly imply that climate science is academically on par with the aforementioned academic fields. A perilous assumption, as if the fields they refer to have such well-established theories that scholars are perhaps only left with some fine-tuning, as some climate scientist would like to think of human-induced climate change.

Nothing could be further from the truth. With respect to for instance carcinogens-exposure (such as found in tobacco smoke *but also* foods in terms of natural carcinogens)[9] the global academic community is increasingly aware of the process of biological adaption *due to* carcinogens-exposure rather than assuming that any exposure, however small (apart from zero), is deleterious. This latter concept, also known as the linear non-threshold (LNT-) model, has been around for more than half a century and regarded by many scholars as a secure academic model, which is now slowly eroding under the pressure of increasing empirical knowledge and novel theoretical insights diverging from that LNT-model. So, even theories that for decades haven been academically 'carved in stone' are in effect *radically* mendable.[10] Accordingly, the argument, as clearly implied by Benestad *et al.*, that some theories in some scientific fields surely must be beyond serious dispute is simply and demonstrably false. With respect to theory-revision, climate science as a 'young' field should learn from its 'older' siblings, specifically on the subject of the supposed sturdiest theoretical constructs.

But of course, I did not presume 'that there must be some kind of "symmetry" in the peer-reviewed publications' at all. I am not even sure what that means. What I did specifically ask for is symmetry *in the paper* by Benestad *et al.* Why? That is what the method they propose *entails*. The discerning reader cannot simply be satisfied by a cursory remark on that there 'have been some [flawed cases], such as a bug in the climateprediction.net simulations, a corrigendum in Nature (doi:10.1038/nature02478), error in the GISTEMP data, and glaciers in the Himalayas (2nd work group report, 2007), ….' (C297) Benestad *et al.* haven't really tried at all to test their agnatology method to the whole. The 'continuous replication of published results and dissemination through scientific fora can … contribute towards a convergence towards the most convincing explanations' (p. 455, l. 17 – 19) is laudable, yet is not adhered to by Benestad *et al.* whatsoever. That is foreseeable as Benestad *et al.* are avid proponents of the human-induced climate change premise and as such hardly expected to really sound the depth of their own methodology.

References

[1] I will not tire the authors with an exposé on the realism – anti-realism debate, which, however, has a very close bearing on this subject.
[2] Boger, G. 2005. Subordinating Truth – Is *Acceptability* Acceptable? *Argumentation* 19: 187 – 238.
[3] Laudan, L. 1977. *Progress and Its Problems. Towards a Theory of Scientific Growth.* University of California Press, Berkeley.
[4] The age reference is not about seniority but merely to highlight that chemistry went through a lot more in terms of research traditions than most scientific fields simply because of the age of the field.
[5] Jovarsky, D. 1970. *The Lysenko Affair.* The University of Chicago Press, Chicago.
[6] See further Polanyi, M. 1963. The Potential Theory of Adsorption. Authority in Science has its Uses and its Dangers. *Science* **141**: 1010 – 1013.
[7] Cook, J., Nuccitelli, D., Green, S. A., Richardson, M., Winkler, B., Painting, R., Way, R., Jacobs, P., and Skuce, A. 2013. Quantifying the consensus on anthropogenic global warming in the scientific literature. *Environmental Research Letters*, doi:10.1088/1748-9326/8/2/024024.
[8] When one holds a belief, it must be seen as the possession of a truth, the primary aim of believing. Even if one is conscious of the fact that some commitment would have certain non-epistemic benefits, and even if those benefits lead to a certain belief, one could not think that this was done so *because* of the actual appreciation of those benefits. Practical arguments cannot leave any traces and they must lead to belief (if at all) without the believer being aware thereof.
See Jones, W.E. 2003. Is Scientific Theory-Commitment Doxastic or Practical. *Synthese* **137**: 325 – 344.
See further: Jones, W.E. 2002. Explaining Our Own Beliefs: Non-Epistemic Believing and Doxastic Instability. *Philosophical Studies* **111**: 217 – 249.
[9] Ames, B.N., Profet, M., and Gold, L.S. 1990a. Dietary pesticides (99.99% all natural). *PNAS USA* **87**: 7777 – 7781.
Ames, B.N., Profet, M., and Gold, L.S. 1990b. Nature's chemicals and synthetic chemicals: Comparative toxicology. *PNAS USA* **87**: 7782 – 7786.
Ames, B.N., Gold, L.S. 1990c. Chemical carcinogenesis: Too many rodent carcinogens*. *PNAS USA* **87**: 7772 – 7776.
[10] See e.g. Calabrese, E.J., Baldwin, L.A. 2003. Toxicology rethinks its central belief. Hormesis demands a reappraisal of the way risks are assessed. *Nature* **421**: 691 – 692.
Tubiana, M., Aurengo, A. 2005. Dose–effect relationship and estimation of the carcinogenic effects of low doses of ionising radiation: the Joint Report of the Académie des Sciences (Paris) and of the Académie Nationale de Médecine. *International Journal of Low Radiation* **2(3/4)**: 1 – 19.
Tubiana, M., Aurengo, A., Averbeck, D., Masse, R. 2006. Recent reports on the effect of low doses of ionizing radiation and its dose–effect relationship. *Radiation and Environmental Biophysics* **44**: 245 – 251.
Ricci, P.F., Straja, S.R., Cox. A.L. (2012) Changing the Risk Paradigms can be Good for Your Health: *J*-Shaped, Linear and Threshold Dose-Response Models. *Dose-Response* **10(2)**: 177 – 189.