

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

R.E. Benestad

rasmus.benestad@met.no

Received and published: 26 June 2013

Response to the comment by Dr. Jaap Hanekamp

I would like to thank Hanekamp for his interest in our paper and efforts he has put into discussion and comments. It is fairly obvious he has a very different view on the matter than ours. I think one reason is because he takes the experience he has from his own field and thinks this also is valid for another. In his comment, he presents himself as an authority, on line with that he accuses us of, with little consideration for the risk of having misread or misinterpreted the objective of our paper. It is difficult to grasp from what he writes what he thinks our objective is, but it must be something he thinks he can read between the lines.

To state our objective again, the paper tries to make sense out of the controversial issues that have plagued climate sciences in the recent years, and presents a number

C296

of cases which suggest that we can learn from mistakes. The set of cases was selected because they have received some attention in the public debate about climate change and because they are clear-cut examples where it's easy to demonstrate mistakes.

Hanekamp accuses our paper for having “*rudimentary approximate of the philosophy of science*” which “*is only reserved for those authors that do not share ‘the view presented by the mainstream climate research community’*”. I think this statement is false, and I will challenge him or anybody else to find other similar (flawed) cases which are in line with the view of the climate science community. There have been some, 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), but these have been acknowledged and dealt with. We have learned.

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.

Hanekamp furthermore poses some rhetorical questions:

“*The central question that is raised immediately by the paper is whether agnotology exclusively is the playing field of those who criticize the ostensible consensus position on human-induced climate change or whether it is found in the latter as well, e.g. as to manufacture some kind of consensus.*” My answer is “no” - agnotology is not exclusively the playing field of one side. This is an insinuation and an attempt of Hanekamp to read between the lines. I think the second suggestion about manufacturing some kind of consensus is a red herring. Science is about discussions, discourse, and replication, and a scientific consensus emerges when one explanation is far more persua-

C297

sive than alternative ones.

"Thus, on what grounds do the authors think that human-induced climate change is scientifically well established and thus beyond any scrutiny? (Indeed, which position in any scientific discourse is beyond scrutiny?)" The consensus view is certainly not beyond scrutiny, but we will expect that such scrutiny should be solid and persuasive – not papers where data have been removed, based on mere 'curve fits', or involve logical flaws. I think we agree that no scientific discourse is beyond scrutiny, but the critical thinking should not end at the first stage of scrutiny. Indeed, we argue in our paper that "A continuous replication of published results and dissemination through scientific fora can nevertheless contribute towards a convergence towards the most convincing explanations".

We also think that human-induced climate change is fairly well-established.

"Consensus seems the only reference ('evidence') in the paper whereby the whole exercise consequently becomes question begging." This sentence does not make sense to me. Please explain more carefully and be more specific.

"The basic aspect of science, however, is proof (either empirically or logically), not assumption". Here we are in complete agreement, and I'd like to point out that we also have provided proof in terms of our replication of the cases we refer to, and we provide the data and source code for our analysis. Alternatively, the results are conditional upon a set of stated assumptions. In his comment, Hanekamp himself makes a set of assumptions, which are not clearly stated. I think it would help the discussion to make these explicit (e.g. about symmetry).

"Thus a simple referral to the consensus of some view on human-induced climate change simply cannot do". I agree completely, and I think Hanekamp must have mis-read our article. Our position is the question why are there differences between the consensus and a number of scholars with different views, and can we understand the reasons for so?

C298

"As a result, the argument as a whole collapses into circularity. This is the critical and unacceptable asymmetry the paper introduces devoid of any clarification or evidential basis." This does not make sense to me. Please explain more carefully the reasoning. Can the criticism be more specific?

"Logically, the approach the authors chose must by default capture the symmetry of the debate they themselves initiate. More to the point, they need to prove as carefully as possible that their approach can only apply to those that criticise the alleged consensus view on human-induced climate change rather than simply assume it with reference only to some kind of consensus. Consequently, no amount of examples they might bring forward in support of their claims can be regarded as proof of their argument whatsoever." I think Hanekamp has misunderstood our objective. Agnotology, learning from mistakes tries to make more sense out of some of the central controversies that have emerged around climate sciences in the past years. We want to show that there is a number of influential papers which have influenced the public perception about climate change, and that many of these papers fail the test of scrutiny.

"Worse, the corollary of the authors' argument seems to be that one requires only a single basic agnotology example in the discourse the authors assume to be free thereof in order to refute the paper in its entirety." This argument does not make sense to me. Please explain more carefully the reasoning. Can the criticism be more specific?

"No attempt thereto, in line with some form of falsification, has been made. So, the very reproducibility they require from others does not apply to their own work, which introduces another kind of unacceptable asymmetry." This argument does not make sense. Is this a different form for asymmetry to above? Indeed, our paper discusses falsification (the cases provided) and we provide data and open source code so that others can test our analysis. Hence, we expect that others will be able to test our methods – it is important to require the same from ourselves as from others.

"Furthermore, the very a priori (ex cathedra might be a more accurate descriptor) ex-

C299

clusion of the consensus view on human-induced climate change from the analysis the authors bring forward can result in nothing more than fallacious appeals to authority and popularity (of which the term 'consensus' is but a variation)." Hanekamp should provide some counter examples. So far, he only assumes that a symmetry exists, and assuming is not a valid proposition (as he too observes).

"Again, why would the consensus view as defined by the authors be exonerated from the very enquiry the authors initiate other than by an appeal to authority and popularity?" This interpretation is strange. In our paper, we argue "The merit of replication, by re-examining old publications in order to assess their veracity, is obvious. Published results in particular should be replicable, and access to open source codes and data should be regarded as a scientific virtue that facilitates more reliable knowledge. Results are far more persuasive if one can reproduce them oneself, although replication of published results requires scientific training, numerical skill, and mastering of statistics. One concern is that modern research is veering away from the scientific virtue of replication and transparency." We do not want to limit this to one side, but it must be a continuous process for any view. We will make this point clearer in the revised version of our paper.

"Besides, why would scientific peer-review within the sphere of the purported consensus-view on human-induced climate change be any guarantee for scientific quality, which the authors think is lacking in critical literature? Scientific majority views could afford quite proficient mechanisms to keep other views at bay precisely through peer-review, also known in this case as pal-review." We have not taken any position about the quality in the consensus-view, and not made a general statement about the opposing view (to call the cases we chose 'critical' may be inappropriate, as they contain severe flaws as exposed in our demonstrations). The point about keeping other views at bay is relevant to our agnotology paper, and a case emerges through the comment provided by McKittrick. I agree that this is a concern, and we need to rely on the editor's digression to judge the various referees' reports. Having said this, Hanekamp

C300

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. Here he presents his own ex cathedra – there seems to be no room for doubt. For this reason, discussion papers like this are valuable.

"The dominant epistemic community within climate science could well hinder freedom of research and publication, which could result in impeding certain research themes that are not regarded as in line with the dominating paradigm and thereby ignored for less than charitable reasons" This would be unfortunate, but 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-server (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 (McKittrick). 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.

"It simply poses the problem of peer-review and its quality safeguard." This is a fair point, and we do discuss this in our paper. One issue is obstacles to new and controversial views (assuming persuasive evidence, logic, and valid conclusions) and the other issue is sloppy reviews. I hope our paper can trigger more discussion about the peer-review.

"The authors, again, make no attempt to support their asymmetric assumption: what seems to be wrong with those who criticise the professed consensus view on human-induced climate change is assumed to be right in the consensus discourse." We are making no such assumptions. Hanekamp is trying to "read between the lines" and makes a set of assumptions about asymmetry and about veracity. We will try to make

C301

it clear in the text that we are not making the kinds of assumptions that Hanekamp makes.

“... the purported well-established scientific suppositions on climate change. This is an odd position to take (to say the least) from the perspective of the history and philosophy of science. One can be entirely indifferent to this subject to nevertheless be completely astonished by such a view. Chemistry is one of the oldest scientific fields in the history of science, and chemists have seen many theories come and go during centuries of research. How can a young field such as climate science be so sure of itself, as the authors in their paper seem to be, other than to be totally ignorant of the history and philosophy of science?” Hanekamp presumes that the lessons made in chemistry must be true for other disciplines, and he interprets our paper in a surprising way. 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 (comparing their age is a bit of a red herring – climatology does not start from scratch, as chemistry did).

“With the presence of multiple kinds of asymmetry as highlighted above, this paper essentially is unpublishable. Why would any reader be persuaded by the claims made in the paper other than being forced to accept fallacious arguments from popularity (consensus), authority and some examples of supposed failures of those who criticise the alleged consensus view on human-induced climate change, whereas the consensus view is ex cathedra exempt form any kind of agnotological scrutiny?” I think that Hanekamp’s assumption of symmetry needs to be established before he can say that his arguments are valid. I would also say that demonstrations of how science work

C302

(through openness/transparency, replication, and testing, as we have done here) normally is the most persuasive strategy. I disagree that the consensus view is ex cathedra exempt form any kind of agnotological scrutiny, and we are not taking this position in our paper. This is a strawman argument from Hanekamps, devoid of logic and evidence.

“Inductively, such an argument simply fails and is an affront to scientific logic.” Here we have different views. I will say the same about Hanekamp’s comment, which to me is a subjective opinion, lacking evidence and logic. He is presuming a symmetry and complains about asymmetric presentation of cases. I challenge him to find this symmetry. Without it, his whole line of reasoning collapses. In any case, he has mistaken the objective of the paper when he gets hung up on the question of asymmetry.

“In terms of education I will definitely use this manuscript in my philosophy of science classes, whether published or not, but not in the way the authors might have envisioned it. It can only be used as an example of how some scientists maltreat very elementary aspects of understanding and doing science. It shows that universities should require far more of their students with respect to arguments, reasoning (logic), and evidence. This paper fails utterly on all three counts.” This is great, and I hope the students will be presented my response to his comment too. Otherwise, Hanekamp would himself fall into the very same trap about which he has expressed concerns regarding ‘pal-review’ (see the text above). It would also be great if he and his student learned R and data analysis and would be able to understand the reasons for why the cases are clear-cut examples of flawed conclusions – not just “examples of supposed failures”.

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

C303