

REPLY

Scientific dialogue on climate: is it giving black eyes or opening closed eyes?

Reply to “A black eye for the *Hydrological Sciences Journal*” by D. Huard

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INTRODUCTION

*Τὸ ἀντίξουν συμφέρον καὶ ἐκ τῶν
διαφερόντων καλλίστην ἄρμονίαν καὶ
πάντα κατ' ἔριν γίνεσθαι* (Opposition
unites, the finest harmony springs from
difference, and all comes about by strife;
Heraclitus; ca. 540–480 BC)

We always welcome critiques to our views and methodologies, and in Anagnostopoulos *et al.* (2010) (hereinafter “our paper”) we have responded even to critiques in weblogs. We appreciate even more the formal Discussion paper by D. Huard (2011) (hereinafter simply “Huard”). Huard discusses, on the one hand, several points related to the essence of our research and, on the other hand, issues related to the editorial procedure of its publication, by delving into the latter (specifically, by requesting and receiving the entire file of the review process). In sequel to the critical reviewers, who, as we acknowledge in our paper, “[made] us more confident that we did not err”, Huard, despite implying that our “poor science” has given a “black eye” to *Hydrological Sciences Journal* (*HSJ*), helped us to clarify scientific matters broader than the scope of our paper and strengthened our arguments, as we detail below.

TIME SCALE OF COMPARISON

Huard writes: “The idea of evaluating climate simulations initialized in the 19th century based on temporal correlation with observations at the yearly time scale is incongruous for anyone familiar with climate simulations.” A reply to this argument can already be found in section “Justification of the methodology”, subsection “Scale of comparison”, of our paper. Instead of pointing out what we possibly got wrong there, which could start an interesting discussion, Huard merely repeats the already-replied-to argument in its original form. In repeating it, he focuses on the yearly time scale, thus missing that our comparisons are also made at climatic (30-year) time scale, and that, apart from simulated series alone, we also compared statistical characteristics of climate model outputs *versus* those of the real climate.

NATURAL CLIMATE VARIABILITY vs MODELS

Huard diagnoses that it is “a common misconception, that climate models predict natural climate variability” and thus “a false premise . . . that the selected climate simulations predict (forecast) climate in a

deterministic sense”. We feel that Huard addresses these statements to the wrong audience. We generally agree with his diagnosis, and our results confirm it; but we did not use any premise of this type, nor were we influenced by this misconception. Instead of using any premise (as is typically done), we *tested* whether the model outputs are consistent with reality (which reflects the entire variability, due to combined natural and anthropogenic effects). Our results extend Huard’s statements further. Specifically, we show that, climate models are not only unable to predict the *variability* of climate, but they are also unable to reproduce even the *means* of temperature and rainfall in the past. For example, as we stated in our paper, “*In some [models], the annual mean temperature of the USA is overestimated by about 4–5°C and the annual precipitation by about 300–400 mm*”.

Given our results, an interesting question would be: Under what premise could one, in order to derive meaningful results for the future, use models that fail to reproduce the known past, in terms of *both mean level and variability*? Huard does not ask this straight question. Yet he admits no predictive skill of models for the past. In his own words, “*under constant external forcing, TAR and AR4 simulations have no predictive skill whatsoever on the chronology of events beyond the annual cycle*”, and quotes Smith *et al.* (2007): “*Previous climate model projections of climate change accounted for external forcing from natural and anthropogenic sources but did not attempt to predict internally generated natural variability*”. Thus, he implies a skill for the future, regardless of poor behaviour in the past.

PREDICTIONS vs PROJECTIONS

According to a terminology used by the IPCC and commonly followed in other related literature, climate models do not do *predictions* but *projections*. As formulated by Huard: “*Climate simulations included in IPCC’s TAR and AR4 also make no pretence of predicting/forecasting weather or climate*” and “*A climate projection is thus not a prediction of climate, it is an experiment probing the model’s response to change in GHG [greenhouse gas] concentrations.*”

This is not only an issue of terminology and semantics. It is generally accepted (e.g. Brown 2001, summarizing Karl Popper’s views, Weijs *et al.* 2010) that science makes testable predictions, otherwise it is not science. In this respect, Huard’s strong statements, that climate models cannot do predictions, may undermine their incorporation in science. What

he and the established climate literature call *projections*, we view as *conditional predictions*, where the conditioning lies on GHG concentrations. However, since our tests are made on the past (mostly 20th century), the conditions are known (observed) and the outcomes (real climate) are also known. In this respect, our falsification framework is scientifically rigid.

In fact, it is the IPCC that uses climate model outputs as predictions. Calling these by another name, such as “*credible quantitative estimates of future climate change*” (Randall *et al.*, 2007, p. 591) does not change the essence. For example, in IPCC (2007, Fourth Assessment Report—AR4; *Summary for policymakers*, p. 15), we read (our emphasis): “*It is very likely that hot extremes, heat waves and heavy precipitation events will continue to become more frequent*”. This is one of a total of six occurrences of the word “*will*” in a similar context (in the three next pages of the section “*Projections of future changes in climate*”), the last one being “*... anthropogenic carbon dioxide emissions will continue to contribute to warming and sea level rise for more than a millennium*”—not to mention the over 20 appearances of expressions such as “it is expected”, “it would”, etc. The same style is adopted in other IPCC documents, including the Freshwater Chapter (Kundzewicz *et al.* 2007). The conviction that climate model outputs are credible predictions for the future propagates beyond IPCC texts, often without mentioning their origin (for which we cannot imagine anything else but climate models). Taking as an example the most cited “climate change” document (as seen by a Google Scholar search), the so-called Stern Review, we may see that in a single page (Stern 2006, p. vi of the Executive Summary) the word “*will*” appears ten times. The same is also obvious in many papers and conference talks, where sometimes the “projections” are presented as facts. Therefore, we are not the right recipients of Huard’s warning not to treat climate model outputs as future predictions. Our difference with standard climate literature is that we show that the predictions cannot be credible and, thus, cannot provide a guide for the future and for policymaking.

CHAOTIC BEHAVIOUR OF CLIMATE

Huard writes: “*The natural variability of the climate system is largely chaotic*” and thus “*unpredictable*”. Not only do we endorse this statement, and not only have we presented research results on this issue (Koutsoyiannis 2003, 2006, 2010, Koutsoyiannis *et al.* 2009, Christofides and Koutsoyiannis 2011), but

we have also pointed to this problem in the second paragraph of the conclusions of our paper, the one that begins: “*However, we think that the most important question is not whether GCMs can produce credible estimates of future climate, but whether climate is at all predictable in deterministic terms.*” It is climate modellers who say or imply otherwise; for example Schmidt (2007, our emphasis):

Weather is chaotic; imperceptible differences in the initial state of the atmosphere lead to radically different conditions in a week or so. **Climate is instead a boundary value problem**—a statistical description of the mean state and variability of a system, not an individual path through phase space. **Current climate models yield stable and nonchaotic climates,** which implies that questions regarding the sensitivity of climate to, say, an increase in greenhouse gases are well posed and can be justifiably asked of the models.

Therefore, again we are not the right recipients of Huard’s warning that climate is chaotic.

AVERAGING SIMULATIONS

Huard writes: “[When averaging] *multiple members of an ensemble of simulations ... climate components that are due to natural variability will average out and leave only the response of models to the external forcing ... [we] apparently did not realize this and averaged correlation coefficients computed with individual simulations, instead of computing the correlation from averaged simulations.*” It is not true that we averaged correlation coefficients; instead, while we present (for brevity) the average values of correlation coefficients (Tables 2 and 4), we also present their statistical distribution (Fig. 7), while we cite Anagnostopoulos (2009), who presents all coefficients for each individual station. We do not find a strong basis in averaging simulations, as suggested by Huard, before calculating statistical indices such as correlations and efficiencies. Thus, we will not follow his suggestion, but he may feel free to perform himself what he suggests and hopefully publish his results. We can predict with confidence that these results will not be much more encouraging than ours. We expose our argument related to this prediction in the Appendix, where we explain that, if individual simulations are uncorrelated to reality, there is no way to make an ensemble that would correlate to reality, as we average out.

NUMBER OF POINTS REQUIRED

Huard discusses also a “*comparatively minor problem*”, i.e. “*that the number of independent 30-year samples in the time series used by [us] is rather small to compute a meaningful correlation coefficient*”. Here we wish to mention that we did not focus on correlation coefficients only; for example we also examined coefficients of efficiencies that incorporate the (strong) biases and we compared several observed and simulated statistics (see p. 1096 in our paper). However, we agree that having few points is indeed a problem; perhaps, as Huard suggests, minor, but reflecting a major one, which, again, we do not think is ours.

Whoever proposes a hypothesis, a theory or a model, is responsible for thinking of the testability of the hypothesis, theory or model. One valid solution, for example, would be if the climate modellers provided runs of several hundreds or thousands of years, so as to have more data points for comparisons, perhaps with the help of palaeoclimatic reconstructions. But Huard, admitting that the natural variability cannot be captured by climate models, annuls this solution. What remains is to wait for some hundreds of years, until we have enough data points to ensure testability and, thus, upgrade of popular climate hypotheses into a theory. Until then, we can call them simple conjectures and make no use of them in practice and particularly in policymaking.

SEPARATION OF NATURAL AND ANTHROPOGENIC VARIABILITY

Huard writes that we “*expected individual models to show some skill in predicting multi-decadal climate variations. They do, but their skill is limited to the small fraction of climate’s variability driven by external forcing*” (our emphasis). Here, he does not provide any citation or argument to support how we know that “*they do*”. (Furthermore, his assertion about what we expected is wrong: we did not expect anything, we just tested.) In our understanding, Huard’s argument includes the following points:

- (a) Climate is varying.
- (b) Climate variability is a mix of background natural variability and changes in external forcing conditions; the two are separable.
- (c) Climate models cannot describe/predict natural variability, which is chaotic and unpredictable.
- (d) Climate models do predict variability due to external forcing; they do have skill in it.

Here, we should point out that, from our observations of natural climate, we only know the total variability and not separate parts, such as natural and forced variability. Any separation is subjective and made by models, which, as Huard admits, are not able to represent the natural variability, but only “*the small fraction of climate’s variability driven by external forcing*”. But how can we know that the separation made by these models has any element of reality? Even if it had, how can we know that a model with no skill in reproducing the large part can describe the small part? How can we trust a model that is admitted to have skill only for a small part of the total variability? How could we reject a hypothetical model (e.g. one in which the climate sensitivity is very small), according to which the entire observed (past) variability is “*internally generated natural variability*”, while the response to change in external forces is negligible?

If one would accept the above logic (points (a)–(d)), then one could make any type of model, with any explanatory variable one wishes, calling the divergence of each model with reality “unexplainable natural variability” and producing a diversity of explanations and predictions of the future. In our view, while different types of forcings of the climate system can be understood, speaking of separable fractions of the variability, driven by the different forcings, is arbitrary (Christofides and Koutsoyiannis 2011). The underlying dichotomous or reductionist logic, while being very common, has been largely harmful to science (Koutsoyiannis *et al.* 2009, Koutsoyiannis 2010).

THE DECISION TO PUBLISH

Near the end of his Discussion, Huard makes an appeal to the “*mutual respect and trust in the professionalism of our peers*”, which makes an interesting contrast with several of his statements referring to us, the editor, the reviewers and other authors, and ultimately the “... *HSJ coming out as lacking the discrimination required to identify poor science*”.

Whether the *HSJ* got “*a black eye*” is for the reader to judge, as is whether “*reviewers A and C rejected the paper on technical and methodological grounds, not philosophy*”, since the entire review file is now public¹. The reader may also assess whether we made “*factual errors obvious to anyone familiar with climate science*” and Z.W. Kundzewicz, the Editor of

the *HSJ* (as well as of the International Association of Hydrological Sciences, for 14 years) failed to see them. It is also possible that Kundzewicz and Stakhiv (2010) share “*the same misguided assumptions about climate simulations*”. However, if Huard thinks that two Coordinating/Principal/Chairing Lead Authors of the IPCC freshwater chapters in IPCC Assessment Reports (Kundzewicz in the Fourth—Kundzewicz *et al.* 2007, 2008, Stakhiv in the First, Second and Third—Lins *et al.* 1990, Stakhiv *et al.* 1992, Kaczmarek *et al.* 1995, Arnell *et al.* 2001) are not familiar with climate science, then he should be more concerned with the IPCC than with the *HSJ*; but this is also for the reader to judge.

CONCLUDING REMARKS

Interpretations, views and opinions, and even methodologies and hypotheses, can be wrong—even the most popular—and the instrument to evaluate them is scientific dialogue. The very content of our paper is all about this, which normally should be regarded as trivially common in science. By showing the poor skill of climate models in reproducing past climate evolution, we think that we are constructive rather than destructive. In particular, we hope to have contributed in showing that current modelling approaches can be dangerous, because, as they are unable to reproduce climatic variability, naturally they hide or underestimate future uncertainty (cf. Koutsoyiannis *et al.* 2007, Koutsoyiannis 2010). This may also contribute to the search for better alternatives, perhaps less algorithmic-intensive, needing less powerful supercomputers (which, despite being also money-intensive, ultimately may not make any difference), and more thought- and knowledge-intensive.

In any case, we would like to thank D. Huard for providing us with the opportunity to clarify these points and strengthen our logic and methodology.

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¹As it has already been available to Huard, it is annexed also to this Reply as a Supplementary Information on the *HSJ* online site.

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APPENDIX

Relationship of average correlation of different simulations to correlation of average simulation

Let \underline{r} denote a random variable representing the real (observed) climate, \underline{s}_i ($i = 1, \dots, n$) represent different simulations and $\underline{a} := (1/n) \Sigma \underline{s}_i$ represent the “averaged simulation” suggested by Huard (2011), where the summation Σ is meant for all i . Then, Huard’s (2011) correlation coefficient will be:

$$\rho_H = \frac{\text{cov}[\underline{r}, \underline{a}]}{\sqrt{\text{var}[\underline{r}] \text{var}[\underline{a}]}} \quad (\text{A1})$$

where $\text{cov}[\]$ and $\text{var}[\]$ denote covariance and variance, respectively. We wish to compare it with the average of individual correlations, i.e.:

$$\rho_A = \frac{1}{n} \Sigma \frac{\text{cov}[\underline{r}, \underline{s}_i]}{\sqrt{\text{var}[\underline{r}] \text{var}[\underline{s}_i]}} \quad (\text{A2})$$

Clearly, $\text{cov}[\underline{r}, \underline{a}] = (1/n) \Sigma \text{cov}[\underline{r}, \underline{s}_i]$. Since all \underline{s}_i represent the same process, we may assume that they have equal variances, i.e. $\text{var}[\underline{s}_i] = \text{var}[\underline{s}]$. Hence we obtain:

$$\rho_H = \rho_A \sqrt{\frac{\text{var}[s]}{\text{var}[a]}} \quad (\text{A3})$$

It is readily understood that the square-root factor in (3) is bounded from below by 1 and, thus, ρ_H

cannot be smaller than ρ_A (assuming that they are positive). Evidently however, when ρ_A is zero, as practically happens in all cases in precipitation simulations (Tables 2 and 4 in Anagnostopoulos *et al.* 2010), and in most cases in temperature (Table 2 in Anagnostopoulos *et al.* 2010), ρ_H will also be zero.