Review of IPCC WGII AR5 Chapter 3 – Freshwater Resources

FIRST-ORDER DRAFT

Draft date: 11 June 2012

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Review date: 6 August 2012

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1. Preliminary notes

I am thankful to IPPC for the invitation to review the IPCC WGII AR5. I had planned to review a few chapters but unfortunately other commitments did not allow me to work for the time I had planned. I was able to review merely Chapter 3 – Freshwater Resources (hereinafter referred to Ch3). I believe that my comments can be useful because they refer to essential elements related to the logic of the report and the underlining philosophy, and provide key information currently missing in Ch3.

I have not made a review for IPCC before. I am somewhat puzzled to read in the relevant "Charge to Reviewers" (hereinafter: CTR), on the one hand,

> *"The IPCC procedures state that the Expert Review process shall be open, objective, and transparent, with a wide circulation…"*

and, on the other hand,

> *"Please remember that all draft chapters are works in progress and are not to be cited or quoted, or further disseminated".*

The latter statement is made more emphatic in the "Expert Review Certification", in the phrase (emphasis in the original):

> *"… all review materials are considered **CONFIDENTIAL. Do not share the draft chapters with colleagues…**".*

I am certainly respecting this term, but on the other hand it is self-evident that I have to quote phrases from the draft chapter, in order to make my review. In due time, with full respect to the principle of transparency (which I strongly endorse), this review may be made public, if necessary, and, thus, these quotations will become public, as well.

2. Completeness and balance

According to the CTR,

> *"We ask that expert reviewers evaluate chapters for accuracy, attention to key issues, completeness of intellectual coverage and of the literature assessed, clarity, and balance."*

In the Ch3 Draft I can see neither "completeness of intellectual coverage and of the literature assessed", nor "balance." But I hope my comments will help to achieve these. My comments are in full agreement with the purpose of the review process, since, according to the CTR,

> *"The purpose of this review is to help ensure that the report provides a balanced and comprehensive assessment of the latest scientific findings associated with the approved outline."*

I will only present some examples of missing references to the recent literature, most of which stem from my experience as a Co-Editor of the *Hydrological Sciences Journal* (*HSJ*). In that capacity, I was happy to receive and approve a summary of the corresponding Freshwater Chapter of the IPCC AR4 (Kundzewicz *et al.*, 2008). There was a formal Discussion of this article (Koutsoyiannis *et al.*, 2009b) and a formal Reply to this Discussion (Kundzewicz *et al.*, 2009). I find it self-evident that all three documents should have been read and quoted in Ch3 of the AR5, because they represent formal scientific publications related to AR4 and constitute discussions and suggestions made to be included in AR5. Currently, none is quoted.

In addition, *HSJ* has hosted a series of papers and discussions about the suitability of climate model projections for hydrological and freshwater management purposes. These include: (a) the original papers by Koutsoyiannis *et al.* (2008) and Anagnostopoulos *et al.* (2010); (b) the Editorial article by Kundzewicz and Stakhiv (2010); (c) the Opinion article by Wilby (2010); and (d) the Discussion paper by Huard (2011) and its Reply by Koutsoyiannis *et al.* (2011). All of these are perfectly relevant and absolutely essential for Ch3, but unfortunately none of these is currently cited.

From other HSJ publications (and in addition to a few already cited in Ch3), I can certainly propose the following which are related to climate and freshwater of specific key basins: Di Baldassarre *et al.* (2011; future hydrology and climate in the River Nile basin); Wu (2010; Great Miami River); Hänggi and Weingartner (2011; Upper Rhine River); and Okruszko (2011; European wetlands). Last, but not least, I strongly propose for inclusion the work by Hirsch and Ryberg (2012), which is absolutely related to the theme of Ch3, as it is related to the investigation of possible changes in magnitude of floods across the USA (note that this, along with the ones mentioned in the previous paragraphs, have been the most read *HSJ* articles as listed in our web site, and were also heavily discussed informally). Relevant to the same issue, but from another journal, is the work by Lins and Cohn (2011).

Some additional key references from other journals , which are related to the suitability of climate models, climate uncertainty, climate change impacts with special emphasis on freshwater, and climate adaptation issues, include the following: Whitfield (2012); Fildes and Kourentzes (2011); Ward *et al.* (2011); Kiem and Verdon-Kidd (2011); Burke (2011; note, this is newer than two Burke *et al.* papers already cited); Kundzewicz (2011); Stakhiv (2011); Stephens *et al.* (2010); Blöschl and Montanari (2010); Kundzewicz *et al.* (2010); Matthews and Wickel (2009); and Pittock (2009). The authors could find many more by inspecting the reference lists in each of the above.

3. "Will"

Searching Ch3, I found **79** instances of the word "**will**", the first one being in the statement (in p. 2, l. 45):

> *"Evapotranspiration will increase".*

This statement makes an interesting contrast with what is mentioned in following sections, i.e. (p. 5, l. 32-40):

> *"Trend estimations for global evapotranspiration are still not compelling due to high uncertainties in global research results. … On a global scale, evaporation increased from the early 1980s up to the late 1990s but not thereafter … [S]o far no fundamental physical-based explanation has been provided for the so called 'evaporation paradox'".*

This example could make the authors think that "will" may not be a proper expression from a scientific point of view; it implies a naïve and oversimplified view on complex hydroclimatic processes. Even from a literal, rather than philosophical, point of view, I guess the authors do not mean "will" but rather "is projected to". Thus, I suggest locating all appearances of "will" and replacing them accordingly. Likewise, the authors may also wish to inspect and replace 24 and 18 additional appearances of "would" and "expected to", respectively.

There have been several discussions about this issue. We read in IPCC reports and sometimes in journal papers (see e.g. Huard, 2011 and Koutsoyiannis *et al.*, 2011), and we hear very often in conferences that the IPCC and, more specifically the climate models used by IPCC, do not provide predictions (let alone certainties about the future) but merely projections. Whatever the difference in semantics of these terms may be, clarity, consistency, precision and honesty, which are desirable or even necessary in the scientific language, demand not to use "will".

The importance of this remark becomes higher, as it has been questioned that climate models are "ready for prime time" in water resources management applications (Kundzewicz and Stakhiv, 2010; Koutsoyiannis *et al.*, 2008, 2011; Anagnostopoulos *et al.*, 2010). Furthermore, since the authors of Ch3 rely on results given by others, i.e. climate modellers, who admit that their models do not simulate well processes related to hydrology (cf. Koutsoyiannis *et al.*, 2008), it would be unfair for the Ch3 authors to assume more responsibility for possible wrong "projections" than they really have. By using "will" they actually proceed further than climate modellers, assigning the projections a degree of credibility which they certainly lack.

4. "Evidence" and "confidence"

Another related issue that needs to be clarified is the use of the term "evidence" when the authors speak about future states, for example, in their statement (p. 2, l. 44-45):

> *"Climate models project both increases and decreases of available water at the regional scale (high agreement,* ***robust evidence)****."*

In my view, it is unclear what "robust evidence" refers to. I can imagine three different cases:

1. It refers to what the models predict, i.e., there is evidence that the models make these projections. In this case, the word evidence is unnecessary or even wrong: we cannot call

"evidence" the information about what those models project; models provide just their outputs.

2. It refers to the future water availability. This case is even worse: there cannot be evidence about the future for the complex hydroclimatic system. Only the future, when it becomes present, will offer the evidence.
3. It refers to something that has already occurred, in a degree. In this case the word "evidence" is relevant, but the first part of the statement, "Climate models project both increases…" is irrelevant.

Similar problems I had with other parts of the Executive Summary, which I do not list here (because they are of the same type), except one (p. 3, l. 14-15):

> *"Hydrological impacts of climate change on humans and freshwater ecosystems increase with increasing greenhouse-gas emissions (limited evidence, high confidence)."*

Here I was not able to understand how limited evidence can result in high confidence. To clarify this, I consulted the relevant IPCC Guidance Note (Mastrandrea, 2010) which, *inter alia*, says (p. 1):

> *"The AR5 will rely on two metrics for communicating the degree of certainty in key findings:*
>
> - *Confidence in the validity of a finding, based on the type, amount, quality, and consistency of evidence (e.g., mechanistic understanding, theory, data, models, expert judgment) and the degree of agreement. Confidence is expressed qualitatively.*
> - *Quantified measures of uncertainty in a finding expressed probabilistically (based on statistical analysis of observations or model results, or expert judgment)."*

On the other hand it says (p. 2):

> - *"For findings with high agreement and robust evidence, present a level of confidence or a quantified measure of uncertainty.*
> - *For findings with high agreement or robust evidence, but not both, assign confidence or quantify uncertainty when possible. Otherwise, assign the appropriate combination of summary terms for your evaluation of evidence and agreement (e.g., robust evidence, medium agreement)."*

Based on these (in particular, the last point) I do not think that, in IPCC terms, limited evidence can be consistent with high confidence. Furthermore, it is remarkable that none of the Executive Summary points contain any quantification of uncertainty.

5. Bias

I always had the impression that IPCC texts are characterized by bias: they present any evidence in a manner as catastrophe-friendly as possible. I found the following quotation (p. 3 l. 1-3; my emphasis) to be a characteristic example of this type:

> *"Intense precipitation events will become more frequent (high confidence) and droughts will become more frequent (low to medium confidence, medium evidence). The observed intensification of heavy precipitation events is very likely to be anthropogenic.* ***Climate models,***

however, ***do not simulate the observed intensification correctly, so that projections may be biased low.*** *Simulated changes in the incidence of droughts are regionally very variable".*

If climate models do not simulate something correctly, then their results are **incorrect**, not **biased**. Even if one wants to use the more polite expression "bias" instead of "error" or "incorrect result", then what is the reason to assume that they are "biased **low**" rather than "biased" or "biased **high**"?

Looking at the supporting analysis in [3.3.1.4], the only relevant statement is:

"For example the GCMs do not simulate the observed intensification adequately."

This does not give any justification for the phrase "biased low". Actually, even the phrase "do not simulate the observed intensification" in both quotations is biased *per se*. A more neutral and accurate phrase would be "do not simulate the observed precipitation extremes". An even better formulation can be taken from the work by Stephens *et al.* (2010), whose abstract includes the following statements (my emphasis):

*"The character of liquid precipitation (defined as a combination of accumulation, frequency, and intensity) over the global oceans is significantly different from the char****acter of liquid precipitation produced by global weather and climate models. … However, these models produce precipitation approximately twice as often as that observed and make rainfall far too lightly****. This finding reinforces similar findings from other studies based on surface accumulated rainfall measurements. The implications of* ***this dreary state of model depiction of the real world*** *are discussed."*

Such "dreary state" undermines also other statements in Ch3, which should be modified accordingly; for example (p. 1 l. 46-48; my emphasis):

"Reliable surface water supply is likely to ***decrease*** *in many regions because of decreases in snow/ice storage and groundwater recharge, degradation of water quality, and more variable streamflow due to more* ***variable precipitation****."*

In addition to the reference to precipitation, which is poorly modelled, the above statement is another example of a biased statement. I guess water supply is also likely to increase in many other regions, so why mention only the case where it is likely to decrease?

6. Can climate "cease to change"?

There is another point in the Executive Summary worth commenting for a different reason (p. 3, l. 8-9):

"Glaciers would continue to lose mass even if the climate were to cease to change…"

The statement is misleading. Climate never ceases to change. Furthermore, melting glaciers constitute climate changing conditions themselves, and in turn would induce other changes. Perhaps the authors imply here that climate change is only anthropogenic, but this is not consistent even with the AR5 FOD Glossary, according to which (p. 3; my emphasis):

"Climate change may be due to ***natural internal processes*** *or external forcings such as* ***modulations of the solar cycles****,* ***volcanic eruptions*** *and persistent anthropogenic changes in the composition of the atmosphere or in* ***land use****."*

7. Society vs. infrastructure

I believe that statements like the following (p. 3, l. 37-39):

> *"storm surges, floods, debris flows, and droughts … demand the changes for human society in the way how to manage water resources"*

are inaccurate as, by putting the emphasis on the society, they hide the fact that such changes are mainly dealt with by engineering means, i.e. by infrastructure development (Koutsoyiannis, 2011a). Such changes are not necessarily dictated by projected climate changes, but are necessary to resolve existing problems (e.g. Sivakumar, 2011), which have their reasons in demographic and environmental changes, rather than climatic changes (Koutsoyiannis *et al.*, 2009a).

On the other hand, I must commend the authors for not exclusively promoting the so-called 'soft path' (Gleick, 2002) which is certainly insufficient to resolve existing problems; for example, I agree with their statement (p. 33; l. 15-16):

> *"Adaptation measures, which involve a combination of 'hard' infrastructural and 'soft' institutional actions, can be helpful in reducing the vulnerability."*

8. Hydropower

I think that several aspects related to hydropower are not presented objectively. First of all, I do not believe that the following extract is correct (p. 38, l. 50-52):

> *"It was estimated that ethanol from corn and from switch grass requires much more water than other renewable energy sources for the same amount of energy produced, except for hydropower where water is lost from reservoirs [by] evaporation (Jacobson, 2009)."*

Hydropower, which does not consume water for the production of energy but only transforms its dynamic energy into electric energy, cannot consume more water than the production of ethanol for the same amount of energy produced. I have seen the original source (Jacobson, 2009), as well as the one referenced in it (Torcellini, 2003). My impression is that they have counted the entire amount of evaporation. In fact, however, part of this amount, or in some places, depending on the local climate, all of it, is replenished by rainfall over the reservoir area. In such cases, the correct practice is to use the net evaporation, i.e. the difference of evaporation minus precipitation, over the reservoir area. An alternative approach would be to use the difference of evaporation from the reservoir minus the natural evapotranspiration (if the reservoir was not there). Certainly, neglecting the subtrahend in either of the cases results in overestimation of the actual losses. Interestingly, for the case of corn and switch grass, Jacobson (2009) considers in the calculation only the irrigation water (plus ethanol-factory water) requirement, thus subtracting from the total amount the water from rainfall, which, according to the data he gives, is the vast majority. Thus I am afraid the comparison here is biased against hydropower.

Such bias may be present in other cases too, e.g. (p. 39, l. 17-18)

> *"In particular, hydropower operation often leads to fast sub-daily discharge changes that are detrimental to the downstream river ecosystem ."*

This is true only in part, for two reasons. First, the natural flows are also characterized by substantial sub-daily (and over-daily) changes and ecosystems are familiar with changes. Second, modern hydropower operation rules have been adapted so as to take into account ecosystem health. The concept of ecological flow has already been implemented in most dams and the deliberate flooding of downstream areas is among the measures being studied for future implementation. The authors may wish to consult Koutsoyiannis (2011a) about this.

But the most important bias is related to hydropower features missing from Ch3 (i.e. not being mentioned at all in it). In particular, the development of renewable energy sources cannot be developed if they are not accompanied by hydropower. Renewables, such as wind and solar energy, are highly variable and unpredictable, and thus their exploitation requires energy storage. Hydropower with reversible turbines (implementing pumped storage schemes) is the only available and proved technology for large-scale energy storage (Koutsoyiannis, 2011a; Koutsoyiannis *et al.*, 2009a).

9. Trends and Hurst

However, among the information missing from Ch3, the most important, in my view, is the complete absence of any reference to the Hurst-Kolmogorov (HK) behaviour (Hurst, 1951; Kolmogorov, 1940), also termed the Hurst phenomenon, long-term persistence, or the scaling behaviour, which is quite relevant to hydroclimate (Koutsoyiannis, 2003, 2006, 2011b).

Instead, the authors discuss extensively "trends" in several phenomena, e.g.:

> *"Changes in global precipitation are observed and simulated by multiple General Circulation Models GCM …, but global trends cannot be determined. Linear trends for global averages from different datasets (e.g. GHCN, GPCP, GPCC, PREC/L, CRU, etc) during 1901–2005 are statistically insignificant"* (p. 5, l. 17-20).

> *"Certain trends in total precipitation and precipitation extremes are observed, for example in South China where increases in dry days and a prolongation of dry periods have been detected"* (p. 5, l. 24-26).

> *"Trend estimations for global evapotranspiration are still not compelling due to high uncertainties in global research results. There is still little literature on observed trends in evapotranspiration, whether actual or potential … On a global scale, evaporation increased from the early 1980s up to the late 1990s but not thereafter … Fu* et al. *(2009) point out that the magnitude of changes and importance of each of the causes varies from region to region. They conclude that so far no fundamental physical-based explanation has been provided for the so called 'evaporation paradox'"* (p. 5, l. 32-40).

> *"Robock* et al. *(2005) observed an increasing long-term trend in soil moisture content during summer for stations with the longest records"* (p. 5, l. 47-48).

> *"[R]egional down and upward trends in soil moisture have been calculated for China, where the trend to more severe soil moisture droughts has been experienced ... Such findings in drought trends and severity need to be taken carefully, as … PDSI and other methods give diverging results for droughts"* (p. 5, l. 51-53).

"Stahl et al. *(2010) investigated streamflow data across Europe reporting a decreasing trend in stream flow for southern and eastern regions, and generally an increasing runoff trend elsewhere, particularly in northern latitudes. In the Nordic countries, the overall picture shows a trend towards increased streamflow annual values"* (p. 6, l. 33-36).

"In the USA, a significant statistical increasing trend of streamflow was detected for the Mississippi and Missouri regions, whereas a decreasing trend in total runoff was found for the Pacific Northwest and South Atlantic-Gulf regions" (p. 6, l. 33-35-38).

"Analysis of global discharges based on model-simulated runoff ratio during 1948-2004 … revealed that only about one third of the top 200 rivers… showed statistically significant trends, namely rivers recording downward runoff trends … and only 19 having an upward discharge trend" (p. 6, l. 38-42).

"in terms of heavy precipitation there are more locations and studies that show an increasing trend over the late 20th century than those recording a decrease" (p. 7, l. 14-16).

*"Cunderlik and Ouarda, (2009) reported a change on flood frequency on snowmelt floods … over the last three decades with significant trends at 20% of stations in SE Canada towards decreasing magnitudes, whereas increasing peak flows were recorded in NW Canada. In contrast, there is no evidence of widespread trends in extreme floods based on daily river discharge of 139 Russian gauge stations" (*p. 7, l. 18-22)

"Similarly, statistical analysis of annual maximum stream flows in the USA at 30-yr (1959-1988) and 50-yr (1939-1988) timeframes do not prove any significant trend…, probably showing the inability to detect any trend based on short term flow series" (p. 7, l. 22-24).

"In Northern-Western Europe, there is a higher number of gauge stations showing a significant upward trends in flood magnitude and frequency, covering W, S and central Germany…, the Meuse river and its tributaries (except Geul River…). In contrast, in E and NE Germany and in the Czech Republic …, a slight decrease in winter flood occurrence and no change in summer flooding was reported ... In South-western Europe, there is no evidence on generalized trend on annual flow maxima… although regional discrimination shows a decreasing trend on flood frequency in the Pyrenees, a flood magnitude decreases in the Alps region, in relation with earlier snowmelt processes. British rivers showed significant positive trends in high-flow indicators primarily in maritime-influenced, upland catchments in the north and west of the UK … although in previous studies those changes were not so obvious" (p. 7, l. 26-37).

"In Asia, flood discharge of the lower Yangtze region shows an upward trend in the last 40 years …, and both upward and downward trends were identified in a 40-yr record of four selected river basins of the northwestern Himalaya ... In the Amazon region, large floods have been registered in the main channel of the Amazon river and its tributaries, including the July 2009 flood considered one of the highest in 106 years of record of the Rio Negro at Manaus… In Africa, there is no evidence of flood magnitude changes during the 20th Century" (p. 7, l. 39-46).

"In the U.S., droughts are becoming more severe in some regions, but there are no clear trends for North America as a whole" (p. 8, l. 9-10).

> *"In general terms, the SREX Chapter report (2012) concluded that there is medium confidence that since the 1950s some regions of the world have experienced more intense and longer droughts … but also opposite trends exist in other regions"* (p. 8, l. 21-25).

While this information is, no doubt, useful, there are also missing key references, some of which have been already mentioned in section 2 above. For example, in terms of the trends in floods in the USA, the missing reference to the more comprehensive USGS studies for USA by Hirsch and Ryberg (2012) and Lins and Cohn (2011) is characteristic.

Even more important is the missing big picture. The big picture is consistent with the Hurst-Kolmogrov behaviour. More specifically, trends, like the above quoted, which differ in sign in different time periods or in different locations adjacent in space, are none other than manifestations of the HK behaviour (Hamed, 2008).

The Hurst-Kolmogorov behaviour has now been recognized even in the physics literature (e.g. Halley, 2009; Eichner *et al.*, 2003) as well as in the climate literature (e.g. Halley and Kugiumtzis, 2011; Mann, 2011). Given that the discovery of this behaviour has been pioneered by a hydrologist (Hurst, 1951) it would be a pity if it is not referred to in Ch3 (cf. also Koutsoyiannis *et al.*, 2009b, who criticized AR4 for not incorporating HK in the AR4 Freshwater chapter).

Once the HK behaviour is recognized, then the significance of trends is dramatically affected (Koutsoyiannis, 2003; Cohn and Lins, 2005; Hamed, 2008). Unfortunately, in the standard literature the statistical significance of trends is mostly assessed by typical statistical tests that assume independence it time. This is a wrong assumption and the results are equally wrong. Therefore, I suggest that reference to statistical significance in Ch3 should be made with extreme care and only mentioned if the original source has used proper methodology, recognizing the temporal dependence in hydroclimatic processes.

10. Some notes on figures

Hydrology, the science of water on earth, is a quantitative science. Most figures comply with this fact. An exception is Fig. 3-2; what it depicts is too far from quantitative information. Perhaps even the qualitative information cannot be correct: When there is low degree of confidence in detection, how is it possible that the degree of confidence in attribution is high?

On the other hand, Fig. 3.5 wants to depict decomposition of variance into contributions from three sources of uncertainty (internal uncertainty, model uncertainty, scenario uncertainty). Such decompositions rely on a linear view of science which is incorrect. (A Mexican colleague gives the following example—well-understood in Mexico—to illustrate the fallacy of linear decomposition into different contributions: If someone is being machine-gunned by two people at the same time, it is objectively impossible to quantify the contribution of each killer to that person's death, since the result would be the same, even in the absence of one of the two killers).

References

Anagnostopoulos, G. G., D. Koutsoyiannis, A. Christofides, A. Efstratiadis, and N. Mamassis, A comparison of local and aggregated climate model outputs with observed data, *Hydrological Sciences Journal*, 55 (7), 1094–1110, 2010.

Blöschl, G., and A. Montanari, Climate change impacts - throwing the dice?, *Hydrological Processes*, 24(3), 374-381, 2010.

Burke, E. J., Understanding the sensitivity of different drought metrics to the drivers of drought under increased atmospheric CO2, *Journal of Hydrometeorology*, 12(6), 1378-1394, 2011.

Cohn, T. A., and H. F. Lins, Nature's style: Naturally trendy, *Geophysical Research Letters*, 32(23), art. no. L23402, 2005.

Cunderlik, J. M., and T. B. M. J. Ouarda, Trends in the timing and magnitude of floods in Canada. *Journal of Hydrology*, 375 (3-4), 471-480, 2009.

Di Baldassarre, G., M. Elshamy, A. van Griensven, E. Soliman, M. Kigobe, P. Ndomba, J. Mutemi, F. Mutua, S. Moges, J.-Q. Xuan, D. Solomatine, and S. Uhlenbrook, Future hydrology and climate in the River Nile basin: a review, *Hydrological Sciences Journal*, 56(2), 199-211, 2011.

Eichner J. F., E, Koscielny-Bunde, A. Bunde, S. Havlin and H.-J. Schellnhuber, Power-law persistence and trends in the atmosphere: A detailed study of long temperature records, *Phys Rev E*, 68, 046133, 2003.

Fildes, R., and N. Kourentzes, Validation and forecasting accuracy in models of climate change, *International Journal of Forecasting*, 27(4), 968-995, 2011.

Fu, G., S. P. Charles and J. Yu: A critical overview of pan evaporation trends over the last 50 years, *Clim. Change*, 97, 193–214, 2009.

Gleick, P. H., Soft water paths, *Nature*, 418, 373, 2002.

Halley, J. M., Using models with long-term persistence to interpret the rapid increase of earth’s temperature, *Physica A: Statistical Mechanics and its Applications*, 388 (12), 2492-2502, 2009.

Halley, J. M., and D. Kugiumtzis, Nonparametric testing of variability and trend in some climatic records, *Climatic Change*, 107 (3-4), 267-276, 2011.

Hamed, K. H., Trend detection in hydrologic data: The Mann-Kendall trend test under the scaling hypothesis, *Journal of Hydrology*, 349(3-4), 350-363, 2008.

Hänggi, P., and R. Weingartner, Inter-annual variability of runoff and climate within the Upper Rhine River basin, 1808–2007, *Hydrological Sciences Journal*, 56(1), 34–50, 2011.

Hirsch, R. M., and K. R. Ryberg, Has the magnitude of floods across the USA changed with global CO2 levels?, *Hydrological Sciences Journal*, 57 (1), 1-9, 2012.

Huard, D., A black eye for the Hydrological Sciences Journal, Discussion of “A comparison of local and aggregated climate model outputs with observed data”, by G. G. Anagnostopoulos *et al.*, *Hydrological Sciences Journal*, 56(7), 1330–1333, 2011.

Hurst, H. E., Long term storage capacities of reservoirs, *Trans. Am. Soc. Civil Engrs.*, 116, 776-808, 1951.

Jacobson, M.Z., Review of solutions to global warming, air pollution, and energy security, *Energy and Environmental Science*, 2 (2), 148-173, 2009.

Kiem, A. S., and D. C. Verdon-Kidd, Steps toward “useful” hydroclimatic scenarios for water resource management in the Murray-Darling Basin, *Water Resources Research*, 47, W00G06, doi: 10.1029/2010WR009803, 2011.

Kolmogorov, A. N., Wienersche Spiralen und einige andere interessante Kurven in Hilbertschen Raum, *Dokl. Akad. Nauk URSS*, 26, 115-118, 1940.

Koutsoyiannis, D., Climate change, the Hurst phenomenon, and hydrological statistics, *Hydrological Sciences Journal*, 48 (1), 3–24, 2003.

Koutsoyiannis, D., Nonstationarity versus scaling in hydrology, *Journal of Hydrology*, 324, 239–254, 2006.

Koutsoyiannis, D., Scale of water resources development and sustainability: Small is beautiful, large is great, *Hydrological Sciences Journal*, 56 (4), 553–575, 2011a.

Koutsoyiannis, D., Hurst-Kolmogorov dynamics and uncertainty, *Journal of the American Water Resources Association*, 47 (3), 481–495, 2011b.

Koutsoyiannis, D., A. Christofides, A. Efstratiadis, G. G. Anagnostopoulos, and N. Mamassis, 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, *Hydrological Sciences Journal*, 56 (7), 1334–1339, 2011.

Koutsoyiannis, D., A. Efstratiadis, N. Mamassis, and A. Christofides, On the credibility of climate predictions, *Hydrological Sciences Journal*, 53 (4), 671–684, 2008.

Koutsoyiannis, D., C. Makropoulos, A. Langousis, S. Baki, A. Efstratiadis, A. Christofides, G. Karavokiros, and N. Mamassis, Climate, hydrology, energy, water: recognizing uncertainty and seeking sustainability, *Hydrology and Earth System Sciences*, 13, 247–257, 2009a.

Koutsoyiannis, D., A. Montanari, H. F. Lins, and T.A. Cohn, Climate, hydrology and freshwater: towards an interactive incorporation of hydrological experience into climate research—DISCUSSION of “The implications of projected climate change for freshwater resources and their management”, *Hydrological Sciences Journal*, 54 (2), 394–405, 2009b.

Kundzewicz, Z. W., Nonstationarity in water resources – Central European perspective, Journal of the American Water Resources Association, 47(3), 550-562, 2011.

Kundzewicz, Z. W., Y. Hirabayashi and S. Kanae, River floods in the changing climate—Observations and projections, *Water Resources Management*, 24(11), 2633-2646, 2010.

Kundzewicz, Z. W., L. J. Mata, N. W. Arnell, P. Döll, B. Jimenez, K. Miller, T. Oki, Z. Şen and I. Shiklomanov, The implications of projected climate change for freshwater resources and their management, *Hydrological Sciences Journal*, 53(1), 3–10, 2008.

Kundzewicz, Z. W., L. J. Mata, N. W. Arnell, P. Döll, B. Jimenez, K. Miller, T. Oki and Z. Şen, Water and climate projections—Reply to discussion “Climate, hydrology and freshwater: towards an interactive incorporation of hydrological experience into climate research”, *Hydrological Sciences Journal*, 54(2), 406-415, 2009.

Kundzewicz, Z. W., and E. Z. Stakhiv, Are climate models "ready for prime time" in water resources management applications, or is more research needed? *Hydrological Sciences Journal*, 55(7), 1085–1089, 2010.

Lins, H. F., and T. A. Cohn, Stationarity: wanted dead or alive? *Journal of the American Water Resources Association*, 47(3), 475-480, 2011.

Mann, M. E., On long range dependence in global surface temperature series, *Climatic Change*, 107 (3), 267-276, 2011.

Mastrandrea, M. D., C. B. Field, T. F. Stocker, O. Edenhofer, K. L. Ebi, D. J. Frame, H. Held, E. Kriegler, K.J. Mach, P. R. Matschoss, G.-K. Plattner, G. W. Yohe, and F. W. Zwiers, Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties, Intergovernmental Panel on Climate Change (IPCC), 2010 (<http://www.ipcc.ch>; <http://193.194.138.236/pdf/supporting-material/uncertainty-guidance-note.pdf>).

Matthews, J., and A. J. Wickel, Embracing uncertainty in freshwater climate change adaptation: A natural history approach, *Climate and Development*, 1(3), 269-279, 2009.

Okruszko, T., H. Duel, M. Acreman, M. Grygoruk, M. Flörke, and C. Schneider, Broad-scale ecosystem services of European wetlands — overview of the current situation and future perspectives under different climate and water management scenarios, *Hydrological Sciences Journal*, 56(8), 1501–1517, 2011.

Pittock, J., Lessons for climate change adaptation from better management of rivers, *Climate and Development*, 1(3), 194-211, 2009.

Robock, A., M. Mu, K. Vinnikov, I. V. Trofimova and T. I. Adamenko,: Forty five years of observed soil moisture in the Ukraine: No summer desiccation (yet), *Geophys. Res. Lett.*, 32, L03401, 2005.

Sivakumar, B., Water crisis: From conflict to cooperation – an overview, *Hydrological Sciences Journal*, 56 (4), 531-552, 2011.

Stahl K., H. Hisdal, J. Hannaford, L. Tallaksen, H. Van Lanen, E. Sauquet, S. Demuth, M. Fendekova, J. Jordar, Streamflow trends in Europe: evidence from a dataset of near-natural catchments, *Hydrology and Earth System Sciences*, 14, 2367- 2382, 2010.

Stakhiv, E. Z., Pragmatic approaches for water management under climate change uncertainty, *Journal of the American Water Resources Association*, 47(6), 1183-1196, 2011.

Stephens, G. L., T. L'Ecuyer, R. Forbes, A. Gettlemen, J.-C. Golaz, A. Bodas-Salcedo, K. Suzuki, P. Gabriel and J. Haynes , Dreary state of precipitation in global models, *J. Geophys. Res.*, 115, D24211, doi:10.1029/2010JD014532, 2010.

Torcellini, P., N. Long, and R. Judkoff, Consumptive water use for US power production, National Renewable Energy Laboratory, U.S. Department of Energy, 2003 (www.nrel.gov/docs/fy04osti/33905.pdf).

Ward, J. D., A. D. Werner, W. P. Nel, and S. Beecham, The influence of constrained fossil fuel emissions scenarios on climate and water resource projections, *Hydrology and Earth System Sciences*, 15, 1879-1893, 2011.

Whitfield, P. H., Floods in future climates: a review, *Journal of Flood Risk Management*, 2012.

Wilby, R. L. () Evaluating climate model outputs for hydrological applications–Opinion, *Hydrological Sciences Journal*, 55 (7), 1090–1093, 2010.

Wu, S.-Y., Potential impact of climate change on flooding in the Upper Great Miami River Watershed, Ohio, USA: a simulation-based approach, *Hydrological Sciences Journal*, 55(8), 1251-1263, 2010.