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Discussions

Interactive comment on "*Joint Editorial* "On the future of journal publications in hydrology"" *by* G. Blöschl et al.

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Interactive comment by Hoshin Gupta, Professor, The University of Arizona May 12, 2014 on "Joint Editorial -On the future of journal publications in hydrology" by G. Blöschl et al., Hydrol. Earth Syst. Sci. Discuss., 11, C1243–C1246, 2014

I thank Professor Erwin Zehe for inviting me to comment on this Joint Editorial. I found it very interesting and timely, and agree with much of what is said. However, having recently been an Editor of WRR, and as someone who has published in the Hydrological Literature for the past three decades, I have some perspectives that do not all quite agree (perhaps) with those expressed in this manuscript. I also comment on some of

the other (very interesting) review comments.

1) Productivity: I agree with Axel Bronstert that while there has clearly been an increase in "productivity", if productivity is to be measured by numbers of manuscripts submitted, I think it is less easy to agree that there has been a marked increase in the percentage of high-quality submissionsà→meaning papers that I would categorize as "A-level" papers that: a) Clearly identify and discuss an outstanding problem b) Identify a clear strategy for studying the problem (this includes clear identification of study assumptions and hypotheses) c) Implement a comprehensive plan of study to investigate the problem (this includes exploring the implications of, and sensitivity to, all assumptions made) d) Provide a clear and concise discussion of results that either support or contravene the study assumptions and hypotheses, and e) Provide a clear and concise discussion of the implications of the studynfindings.

2) Least Publishable Unit: In this regard, I also think there has been a tendency towards submitting the "least publishable unit" rather than one (or two properly connected) well-argued and well-presented manuscripts. This tendency is, of course, related to the "pressure to publish" and the fact that there tends to be academic and professional reward for volume at the (partial) expense of quality. This is recognized by the later when the authors say "... incremental publishing of the least publishable unit is a pattern the Editors advise against. Each paper is assessed on its merits and whether it constitutes a significant contribution to the field. It is through substantive, high quality papers that the discipline of hydrology is advanced."

3) Coherence: In regards to the discussion "There have also been changes in the way hydrological science is undertaken. International collaboration has expanded greatly (e.g. via research programmes of the European Union) and typical group sizes have increased. However, this increased coherence of the research process is not fully reflected by the coherence of the community research output" it is not at all clear to me what is the authors mean by "coherence" of the research process.

4) Length of Manuscripts: One major problem is that Hydrology manuscripts tend to be somewhat lengthy/verbose. This is partly related to a culture in which editors and reviewers insist that each paper "stand on its own" in the sense of being ï£ijunderstandable by the reader without having to access previous sources. In particular certain kinds of "facts" – including methods, models and data – are typically re-described to a degree that is much less prevalent in some other fields such as physics and mathematics. There is really no need for this, and simple citation of source publications should be sufficient in many cases (e.g., if a person uses the Leaf River data or the SCE-UA algorithm). In particular, just as it is becoming now more possible to report on "data", we should (as a community) also permit reporting on "models" and "algorithms" so that they can then be cited instead of described simply for the convenience of the reader or for "completeness" of a manuscript. This is especially true now that such sources can easily be "hyper- linked".

5) Citation Practice: In regards to the discussion "All hydrology journals have an impact factor less than four ... this may reflect the relatively small size of the hydrological community, the way the community is organised and, importantly, the common and seemingly well-rooted practice of citing relatively old articles ... the quality of research will be enhanced if authors integrate the most recent findings from the hydrological literature in their papers, in a similar way to other disciplines" I must disagree in some important aspects. Having been a recent editor of WRR, I am very sensitized to the fact that there are some very poor practices in "citing" that have developed. a) For example, it has become common for authors to make a statement about something they did, and then cite a paper, often recent, to indicate that what they did is somehow OK, just because someone else did it, rather than providing a coherent and logical argument in defense of the practice! This is simply lazy and should not be permitted. This is to some extent driven by the journals trying to raise their impact factors (which are based on the past two year citations) and insisting that authors cite papers from the past two years. Citing recent papers should only be encouraged if the recent papers actually make notable contributions that are worthy of mention (are relevant and of

good quality). b) In this regard, I have also noticed a tendency to cite more recent papers to support some fact or argument that was actually advanced much earlier – In other words, many authors do not cite, or are not aware of, original sources, thereby indicating that they have not really studied the literature and are simply responding to some recent work. This is again both lazy and disrespectful, and does not serve our community well.

6) Impact-Factor: I believe it would be more meaningful to compute and report the Impact Factor Curve for a journal rather than the 2-year IF. In other words, the IF can be computed for 1, 2, 3 etc years and published as a curve versus time. For example WRR has a much higher Impact Factor for 10 years, reflecting the quality of submitted publications that stand the test of time.

7) Role of the Review Process: I completely agree with the comment "... the Editors strongly believe in the role of the review process as being not just screening manuscripts but constructively improving them. Critical constructive reviews can be of considerable value to authors." In this regard I have noticed that some reviews tend to provide comments of the kind "I do not agree with the approach" or "this is the ï£ijï£ijï£ijï£ijï£ijï£ijï£ijï£ijï£ij wrong problem" or "the problem should be studied in this other way", etc., without providing significant useful feedback that can enable the authors to improve their work, without turning the manuscript into one that "the reviewer wished he/she had written". As such, it is my opinion that the review process should aim to enable the authors to do better work, even if the eventual result of the Editorial evaluation is to "reject" the paper. Conversely, of course, this presumes that the authors will not take advantage of the review process to submit a sub-standard manuscript in the hopes of gaining ideas to pursue.

8) Open Access Publishing: (See also comments by Axel Bronstert). In regards to the move towards "Open Access" publishing, I am gratified that the authors remark "... care needs to be taken to render the open access system affordable to authors from financially disadvantaged countries". I am not completely convinced by the move from

"reader pays" to "author pays" as this turns the financially disadvantaged scientist into more of a consumer (easy access to read) rather than a producer (easy access to publish). The move by WRR to make all papers open access after two years is a positive step. However, if we are to move to "author pays" for all journals, then submission costs should be tied to "academic salaries/cost of living" or some such factor related to each country, rather than the author having to "beg" for special consideration. Another related problem that must be addressed is the fact that all submissions are charged fees whether or not the paper is accepted for publication – if we are not careful, this can distort the entire publication process (see the problem of the huge surge in Open Access publications of very poor quality, as has been discussed in the literature).

9) Quantity and Quality: In regards to Quality and Quantity (again see comments by Axel Bronstert and Erwin Zehe), in some countries the quality of a researchers productivity is measured by something like a) the 5 to 10 most highly cited papers, and b) the 5 to 10 "best" papers published within the past 5 years. If something like this were to become more common practice it might serve to reduce the tendency to judge productivity by H-Index and by total numbers of publications, and encourage people to work towards submitting fewer but better quality manuscripts. Also, I completely agree with Erwin Zehe that to expect one or more major contributions per year (in a sustained way) from a researcher is absurd. I note that between 1985 and 1991 (six years) I did not publish a single paper (try doing that today) while investigating the problem of robust optimization for hydrological models that was perceived at that time to be "difficult". Then in 1992 and 1993 Duan, Sorooshian and I published the Shuffled Complex Evolution papers that have since been highly cited. Similarly, a professor recruited to be Chair of Statistics at a prominent University in the 1980's made sure that the University Administration would not seek from him "more than one significant publication every 5 years" (implying that this was the time required to do meaningful work). Another thing that might help to reduce the flood of publications (although I am not sure about this) would be for people to – after exhaustive study – report on their failures (see also Erwin Zehe's comments) and thereby reduce the tendency

of "reinvention of the wheel". In this regard I note that the 1976 paper in WRR by Johnston & Pilgrim was essentially one such paper that inspired me to investigate the problems of hydrological model identification. ï£ijï£ij 10)Reward for the Reviewer: Finally, in regards to the comment by T Francke and Maik Heistermann on the role of the reviewer, I agree that the reviewer should be encouraged (but not forced) to make themselves known to the authors, as this does (in my opinion) lead to more balanced reviews. However, I am completely AGAINST any form of renumeration being offered for the Editorial and Review process, beyond possibly a token amount. My reasoning is that the Scientific Society system (AGU, EGU etc) provides a "professional" context that we all as a community benefit from, and as professionals we gain a considerable amount from our participation in it. Rewarding service with money can only serve to distort this system and lead to progressive "corporatization". The problem of not enough reviewers is better met by raising standards, and by discouraging the tendency to submit too many papers. However, acknowledgement of AE and reviewer service is desirable, and is not difficult, as has been demonstrated (partially) by WRR, which sends acknowledgement letters to AE's (documenting effort expended) that can be used in their Annual Reviews. I do not have a strong objection to the proposed "credit" system, but just suggest that it should be approached with caution, while keeping my argument regarding professionalism in mind. ï£ij

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/11/C1349/2014/hessd-11-C1349-2014-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 4209, 2014.