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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Large-sample hydrology: a need to balance depth with breadth" *by* H. V. Gupta et al.

M. Ostrowski

manfred.ostrowski@gmx.net

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The proposal to extend the data base for identifying suitable model structures contains a number of interesting questions. Basically, the implementation of an extended standardised data pool for complex catchments is definitely helpful and could create impulses for new directions and most of the potential positive outcomes are addressed (Chapters 3.1-3.4). However, I have a few points to raise concerning some assumptions, the description of the state of the art, its historical development the kind of sample catchments as well as the proposed concept. First of all, a modified more suitable title could be: Integrated-sample hydrology: a need to combine depth with breadth The present title builds an artificial barrier between large sample and individual detailed

studies, which does and should not exist. At the beginning, I would like to mention that scientific hydrology is not connected with myths, mysteries or sagas. Expressions like holy grail as stated in this manuscript or in other papers are not very helpful. We have knowledge gaps and insufficient information. This has already been critised during the discussion of the paper by Beven (2001) in HESSD. If at all, hydrologic modelling might contain slight aspects of art, as the different choices for simplifications, abstractinons and assumptions are partially subjective and objective. Some approaches are more "beautiful/elegant" than others. If we follow the definition by Hadamard (1902), there is still no approach visible to turn the ill-posed inverse problem of (distributed) hydrological modelling into a well-posed problem, unless we use overly simplifying model structures, which are hardly acceptable in practice. All we can do to reduce the degree of ill-posedness is adding more prior information, see Renard et al (2010), which would be just the opposite of this proposal: going into depth. Such information is not adressed in this paper, on the contrary, data sets with an average, commonly available information density are aimed at. Clearly, if a large sample should be built up, much more information must be contained in their documentation than supplied e.g. by MOPEX. As one example not the runoff data, but the water levels and related stage discharge relationships including upstream and downstream boundary conditions must become available. The detail of documentation must be agreed upon à priori. The elementary rule in my teaching of hydrological modelling approaches was: Never model a catchment which you have not seen personally during an intensive field visit! To obey this rule would be hardly possible for a large sample. However, digital means are available today to distribute far wider information such as online maps, photographs and videos, as partly available for the DMIP-II, but higher density and resolution should be provided. The paper could be more specific, even being an opinion paper. E.g. the DMIP-II results (Smith et al, 2012) and the Strategic Science Plan NOAA, 2007 contain clear information and define directions for future scientific and development strategies; it seems that they were insufficiently discussed. Also, the manuscript hardly indicates the connection between the discussion paper and the new science plan with an emphasis on so-called socio-hydrology and hydrologic systems under change. Again it does not seem helpful to connect philosophy (Panta Rhei) too closely with hydrology. Here I want to cite Roberto Rigon "I like philosophy but if hydrologists play the philosopher, they do not do their job“ (Rigon, 2013). We could similarly define connections between music and hydrology (For rainfall alone: It never rains in Southern California, I am singing in the rain, In the early morning rain) or painting. We must keep in mind that hydrology is a combination of natural and applied (engineering) sciences. Of course water management strategies are closely connected to if not part of social sciences including economics together with environmental sciences. Hydrology including mathematical modelling and systems analysis are the necessary tools to support this in the best reliable way. If the approach wants to contribute to water management, breadth would require to open up mesoscale hydrological modelling towards improved integration of urban and agricultural development and water quality modelling, hydraulic infrastructure and its operation. The most important step, however, would mean to build a wider inter and trans disciplinary modelling community, which is not at all visible from the authorship. Also, the paper does not describe the state of the art and practice in hydrology sufficiently well. The authors ignore the considerable amount of relevant gray literature and the even larger amount of catchment data sets from applied sciences and operational practice, which should at least be considered in a discussion paper, only to mention the NOAA Science plan, 2007. The problem of intentious information loss due to the ignorance of gray literature was recently described in detail by Uhlemann et al (2013). The statistics presented and the conclusions drawn in that paper seem to be representative for the modelling context also. The dominant self-restriction of scientific hydrology to commercial literature leads to a loss of about sixty to seventy percent of the relevant information. Thus, it seems rather questionable whether a concept based on such a limited information base can be relevant for the future of hydrologic research, if it is also aiming at positive practical impacts. However, it can be expected that open access and other initiatives will change the transparency of discussions significantly and might support the idea of large sample hydrology significantly. Also, I have a sin-

cere doubt in the relevance of scientific commercial contributions to the solution of real world hydraulic structural design and water management tasks. A standard task of applied hydrology is the prediction of extreme values at multiple ungauged locations such as the derivation of a 100-year longitudinal section in a catchment, which excludes lumped models from the beginning (See below). The problem is described in the paper by Smith et al. (2012) and was treated to some extent in the PUB initiative. Most of the catchments investigated in practice are increasingly influenced by anthropogenic impacts such as urban/agricultural development and technical infrastructure under changing climatic boundary conditions. They are transient systems and some existing modelling approaches in practice are already able to a limited extent to model such changing systems through the reconstruction of historical states and the definition of likely futures such as demographic, landuse and climate and technological changes. These models can only be distributed models, mostly based on sub catchments along the water courses (node-link models), which are further subdivided into elementary units (HRU's). They do not need to be newly developed, rather further improved and linked to continuously growing data bases and parameter estimation strategies, well knowing as mentioned that these will never turn the inverse problem into a well-posed problem. Increasingly, these models are combined with other models such as specific models for river corridors and urban drainage systems, groundwater and also with quality models to allow integrated watershed management, this is just becoming state of the art. This ongoing development is not at all mentioned in the paper. In fact, this would really mean breadth in the sense of the definition of modern hydrology. It is acknowledged that the reproduction of uncertain measured data with uncertain models and input/calibration data still needs further consideration, the real challenge, however, is the estimation of reliable extreme values for the assessment of the the risk e..g. of a dam failure with a return interval between 1000 and 10000 years or of a dyke failure (200 years) or in the light of critical water quality concentrations and pollution loads, rehabilitation of morphology or integrated basinwide water management. The limitations of hydrological modelling concerning real time forecasting have been visible during the recent flood in May 2013 in central Europe. Many gauges went out of service again long before reaching maximum water levels. The management of the floods included unexpected backwater effects, blasting of dykes as well as the intentional sinking of barks to close unforeseen dyke breaks. The problem of limited practical relevance of scientific hydrology modelling was described by the reviewer for flood aspects (Ostrowski, 2004). Figure 1 demonstates the existing dilemma. A high quality parameter estimation effort is a requitement but does not guarantee reliable predicted extreme values far outside the range of observed values. A system which seemed to be nearly linear becomes highly non linear or even chaotic for low probabilities of occurence. In this light future research should always include a modelled distribution function for extreme peaks flows, volumes and flow duration curves, not only for the gauging station but for all subcatchments and river sections (longitudinal hydrologic sections). The prediction at ungauged locations as performed in the DMIP-II hints into this direction. This will allow a far easier plausibilty assessment of computed distributed hydrographs and their superposition. Science should at least consider models on the practically applied or soon expected sophistication level. From the manuscript it becomes obvious that the authors are fairly pessimistic about the reliability of present model predictions. However, they correctly cite Bergstroem (1991), who confirms that the confidence in specific models is increasing with the number and duration of model applications. I can personally confirm this statement. Which confidence does Bergstroem have after another 25 years of application since the end of the eighties? Personally, I have longterm experience with implementing hydrological modelling techniques in practice. The model I developed (Ostrowski, 1982) is still widely applied nationally. The German Federal State of Northrhine Westfalia implemented a generalised procedure for hydrological modelling, see Ostrowski (1987), based on that model structure and the related spatio-temporal resolution. The model development was building on the knowledge about the Stanford Watershed Model IV (Crawford and Linsley, 1982) and HYSIM by R. Manley (1975), but aimed at the optimum use of the general German data base (hydroclimatic and geographic data). It was also related to the German sub-catchment

inventory with a spatial resolution of a few km^2. This was accompanied by an accreditation procedure, in which models had to prove that they were able to reproduce the measurements of a heavily urbanised catchment including hydraulic infrastructure. This seems to be similar to the crash test mentioned in the manuscript. Interestingly, it might be one of the rare cases when due to obvious data uncertainties initiatives were started to fullfil the modelling data requirements better and not the other way round. New gauges were installed, other analogous information was digitised. Later a code of good modelling practice followed which ist still valid and applied. The State authorities also run a similar programme to DMIP for combined urban drainage quantity/quality models using the same data base (See Russ, 1991). The conclusion was that several models fullfilled the requirement with varying quality. However, it had to be realised that the qualification of the experts applying a model was frequently the bottleneck (see below). The model has been continuously improved e.g. to better represent non linearity of processes with emphasis on soil moisture and flood routing, to connect it to digital data bases (standardised time series and digital maps, GIS) and new prior parameter estimation inventories such as the Van Genuchten parameters for the German soil classification system. These improvements were possible through the transdisciplinary cooperation of state authorities, universities and consulting enterprises. I think that the search for a best suitable model should be related to the regionally available data base. We thought that our approach is such a regional orientation and did not publish commercially. For the question of future orientation it might be helpful to look at the recommendations of the White Paper by Beck, 2009, who evaluated the last 30 years of environmental modelling. This is what is obviously required, integrated environmental modelling of lower end mesoscale catchments. Hydrological quantity modelling of rural catchments is a compulsory, but fairly limited part of such integrated river basin modelling. Quantity modelling has already reached considerable standards for practical application in different regions. Without ignoring the need for continuing efforts to further improve quantity modelling: this is by far not the most urgent problem. It seems timely to combine the existing sections of hydrological modelling (urban, rural, quantity and quality) to answer the actual, not the future questions how to develop and manage intensively used catchments under change (EU Water Framework Directives (2000/60, US Clean Water Act). Bach & Ostrowski, 2013 recently presented a potential approach for such integrated modelling. They also emphasise that a reliable assessment and solution of spatial water management problems including hydraulic structures and water quality problems are best possible and possibly only feasible through the highest spatio-temporal resolution available. As mentioned above, besides data, parameters and model structure a major source of uncertainty is the expert applying the model. There are always application errors (see figure 2). Smith et al (2012) say that they had no information about the individual model setup and parameter estimation strategies of DMIP-2 participants, which would have been very much desirable. Holländer et al (2013) state that potentially 50 % of the overall model uncertainty might be related to the differing qualification of modellers which I consider as realistic. So, in summary, when assessing the uncertainty of model predictions we can by no means ignore the contribution of the expertise (education, experience and intution) of the modeller to uncertainty. Mathematical hydrological models can be helpful tools, but also dangerous toys. It seems very questionable whether reviewers of most commercial publications about hydrological modelling were really able to assess the overall quality of a modelling case study, including the qualification of the authors applying the model. Also, in contrast to the regulations of most commercial publishers including HESS the data sets (raw and modified data) do rarely become publicly available (MOPEX and DMIP are positive exeptions, with some restrictions) nor are they stored in infinite repositories. In this respect, commercial publishing is quite contradictive. This is addressed in the manuscript but clearer proposals for the improvement of the situation are missing. A further key aspect of the manuscipt is the supplement containing a selection of commercially published large sample studies. The data resolution is either one day or one hour. Daily data, however, is completely insufficient to analyse hydrological processes in a physically meaningful way. Daily data is just sufficient to estimate reliable water balances. If a specific model contains physically meaningful parameters and equations,

they degenerate to empirical functions and indices with increasing time step. Again this is well known in practice (obviously less in science) and the general time resolution e.g. in Germany for hydrological modelling is one hour or smaller (30-15 minutes, in urbanised catchments 5 minutes). A basic rule applied is that the time step must be shorter than one third of the time of concentration of the smallest sub-catchment. The problem was demonstrated and explained by Ostrowski et al., 2010. Only for very short time steps and HRU resolution in the range of several hectares realistic and physically meaningful simulations and parameters can be expected. A final and severe concern is the assessment of data quality. Andréssian et al, 2009 try to convince the community of the high quality of the data in large catchment samples- without any prove. With experience in science and teaching, water administration and practice, I come to the conclusion: this is simply not true, it is wishful thinking! Applying a model we have good reasons to mistrust any incoming data and to check it thoroughly. This is not at all meant to criticise organisations supplying this data. They do the best work possible under given resources limitations, but standard methods have their limits. Frequently, data assimilation and their control in practice take more than 60% of project duration; this is expensive. And: The model applied is an important if not the dominant tool itself to test the data for homogeneity and consistency. Again, the manuscript addresses the point but should say more about the required procedures for data quality assurance.

Summary

The NOAA Science Plan, 2007 (Appendix B, conclusions) says: "Although distributed, physically based modeling is clearly the direction that operational hydrologic forecasting is takingâĂŤand should be taking, much research is needed to determine just how distributed and just how physically based such models should be“. Similar conclusions can be drawn from the DMIP-II project (Smith et al, 2012) "The combined results from DMIP 1 and 2 in the Oklahoma region show that spatially distributed hydrologic modeling is advancing. In a practical way, DMIP has confirmed that spatially distributed hydrologic modeling should and will continue to play a major role in NWS river and water resources forecasting.“ These conclusions call for consideration of catchments with the best possible (density and resolution) information on model drivers and multiple measurement locations of different variables (nested runoff gages, soil moisture, snow etc), including water quality parameters. They also make clear that lumped conceptual models are not the choice in practice. There are many suitable data sets in different national data inventories, but it will take time to identify the best suitable ones. After searching for the holy grail in vain for several decades we can afford the time. Also, combined quantity and quality simulation adds to complexity, but can also open new doors for quantity models; can we use pollutants and nutrients as tracers more than we do (although unstable)?. My conclusion is that looking at a multitude of catchments at the wrong detail and without providing proper transparent methodologies for data quality assessment does not achieve the objective of identifying relevant dynamic hydrological processes (including quality). Instead a smaller number of data sets with high spatio-temporal resolution for nested catchments at the lower mesocale with a direct link to measurements on the operational scale should be publicly archived. Degrees of freedom should be reduced by defining spatial resolution (sub-catchments) and time steps (1 h) à priori. This alone is a real challenge (see manuscript), let alone widening the view for combined quantity/quality hydrological modelling of transient anthropogenically deformed catchments. And a key question must be answered from the beginning: how do we identify and assess the different expertise of modellers in comparative studies? Without doubt some modellers have more skills than others.

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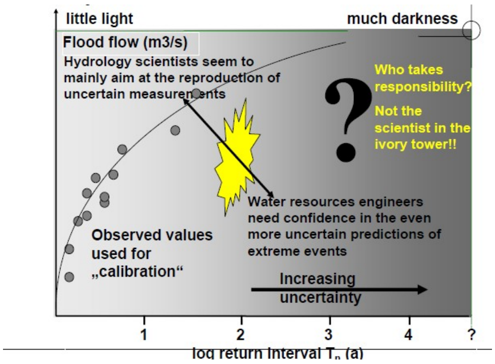

Fig. 1. figure 1 conflicting objectives (from Ostrowski, 2004)

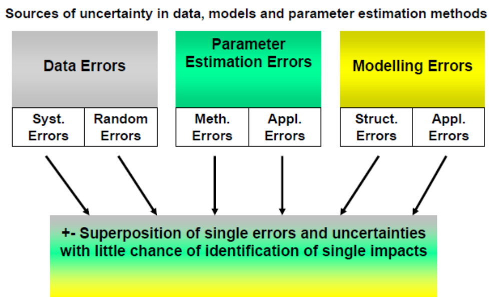

Fig. 2. figure 2 uncertainty impacts including application errors (from Ostrowski, 2004)