

Interactive comment on “Acting, predicting and intervening in a socio-hydrological world” by S. N. Lane

S. Lane

Stuart.Lane@unil.ch

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Introduction: wider issues

I would like to begin by thanking this reviewer, Liz Stephens, for the careful and constructive review that she has provided. It is particularly important that a hydrologist has looked at this paper because, in response to the main points made, I do view this paper as primarily something that a hydrologist should read. This means that the points made about the Abstract and the Introduction by Stephens are particularly important and clearly imply a need for their revision. It also suggests that I need to include some tighter definitions of terms, which is easy to do, and I prefer to do this than to use other terms or phrases. If a hydrologist is motivated to read more on this topic, then this

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article will need to bridge into the language used by the STS community.

In making this revision, early on in the paper, I will need to be clearer that my thesis is a subtle one. I don't believe that we can show that all hydrological modeling is a socio-hydrological practice as, of course, none of us have knowledge of all such modeling practices. Even if I did, I would be bound to make a distinction between two observations: (1) that hydrological modeling may be subject to the same kinds of analyses that have been applied to other areas of science, and which cause us to question the extent to which a supposed supremacy can be accorded to knowledge produced by a 'hydrologic' scientific method; and (2) that if the distinction between knowledge of hydrology and knowledge of society becomes blurred, as it has to be in a socio-hydrological account of the world, then we need to be more sensitive to what this blurring might mean. The reason why the article spends considerable time addressing (1), in the first three parts, is related to (2), because it sustains the normative point that in a socio-hydrological account of the world we should not ascribe to hydrological science some kind of privileged position in our studies. The focus of the paper is socio-hydrology, that is (2), and the need to be reflexive when engaged in socio-hydrological research. Although my way into (2) is the analysis presented in (1). This necessitates revision to the introduction, and also, as Stephens very helpfully suggests, a careful introduction to each section to explain its purpose, as well as its relation to the overall thread of the argument. My thesis is, ultimately, that we are not all socio-hydrologists, but if we want to do socio-hydrology, then we have some thinking to do.

Specific issues

Stephens raises a number of specific issues that will need to be addressed during revision. I will not comment on all of these as most of them can be readily addressed. I do want to comment on a number of points.

My aim in Section 2.1 is not so much to provide a brief introduction to STS for the socio-hydrological community. Rather, it is to help socio-hydrologists to understand what STS

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is all about. The field is too diverse and multi-faceted to review the entirety of 'STS' in such a short space. My focus on the early history of STS and the Latour and Woolgar (1979) study is to show that STS has a profoundly empirical element and, as such, is something that ought to comfort those of us hydrologists who believe that, however we relate observation and theory, a hydrology without observation is an incomplete hydrology. This is one sense in which STS parodies a conventional scientific emphasis upon empirical ways of knowing the world. It also is a powerful basis of dismissing some of the more extremely critical views of STS (e.g. Kuntz, 2012). Latour is there because he was dominant in this early work, even if others now have developed his and other ideas in the STS field. He is also there, along with Stengers and Callon later in the paper, because he has been central to doing more than just talking about what science is; he has developed many ideas about what science ought to be, notably in situations where science is concerned with controversial and publically contested topics. Since the work of Latour and Woolgar, there has been a wealth of other studies that have applied STS approaches to environmental questions and the paper does reference some of these, particularly relating to climate change (e.g. Darier et al., 1999; Shackley et al., 1999; Lahsen, 2005; Sundberg, 2009; Guillemot, 2010; Jasanoff, 2010; Wynne, 2010; Brysse et al., 2012). I do, however, welcome the additional references highlighted.

Section 2.2 is meant to represent the STS view of science in decision-making, not a general review of science in decision-making, and so this needs some clarification. But, I think it would be worth bringing in some additional literature at this point, and clarifying, as Stephens notes, that my review refers to a very particular view of the wider analyses of the relationship between science and decision-making. Clearly, I need to get my seconds and thirds (and firsts) across a lot more clearly! There is also a scope, as Stephens usefully suggests to link into how uncertainty is used in hydro-meteorology and I welcome the references provided in this respect. I will certainly add in the example of flood inundation model evaluation, which I agree has not always been undertaken correctly (Yu and Lane, 2006 argue for the need to include correction for

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chance agreement in flood inundation model evaluation, especially if different model realizations are to be analysed statistically in terms of an improvement in model performance). The paper by Stephens et al. (2012) is very interesting in this respect as it suggests that a model that appears to calibrate well against one floodplain configuration (e.g. with smaller topographic gradients) may actually do so because it is biased by that configuration. There are clear parallels here with the references that I make further in the paper to parameters like Manning's n and validation statistics like the Nash Sutcliffe Efficiency.

The concern that Stephens has regarding certified and non-certified experts is a good one. As with any classification there is a risk of over-generalisation and it would be useful to clarify my view of policy-makers in this sense. Policy-makers are all too often described as 'stakeholders' but rarely is their stake in a problem a personal one. If there is a stake, it is a professional one. Those with real 'stakes' are those who live with those problems, sometimes without choice, and not those whose interest in a problem arises because they are employed and salaried to be engaged as such. There is, then, an implicit sense in my focus upon certified and non-certified experts of trying to rebalance the focus away from policy-makers as stakeholders. But, it also comes from my focus in Section 2.3 upon knowledge and this is where I think knowledge of a hydrological problem can legitimately be classified into that which is certified, which may well include policy-makers, and whose knowledge it is not. A focus upon knowledge reflects a wider worry that 'expertise' regarding environmental questions has been replaced by 'experts in managing governance' (Donaldson et al., 2013) and that this has been to the detriment of genuine scientific enquiry, certified or not. A refocus upon knowledge, in my view, necessarily implies the recognition that expertise in acquiring knowledge is more distributed (as I show in Table 1) and I think the labels certified and non-certified bring this into sharp focus.

The point about networks, as made in Section 3, is useful. The HR Wallingford (2001) report that I refer to in the paper itself came out of a 'Network', Uncertainty in flood

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conveyance. So developing the point about networks would be valuable. I could also bring more of the work from the project that is the basis of this paper in, as we specifically, using STS type methods, followed both consultants and academics to compare and contrast their practices. This has now been published (Landström et al., 2013).

I conclude by thanking Stephens for the thorough and constructive review and in particular for the time that completing it must have taken.

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