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Discussions

Interactive comment on "A comprehensive evaluation of input data-induced uncertainty in nonpoint source pollution modeling" *by* L. Chen et al.

Anonymous Referee #2

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General Comments

The study titled "A comprehensive evaluation of input data-induced uncertainty in nonpoint source pollution (NPS) modeling" by L. Chen, Y. Gong, and Z. Shen is a contribution to better understand how the uncertainty of model inputs – quantities such as rainfall, landuse, topography, and fertilizer amount, which are typically not adjusted during calibration – affect the uncertainty of nonpoint source pollutant model predictions. While there has been a tremendous amount of research into how model parameters affect predictive uncertainty, and some into how model structural error affects model uncertainty, less work has focused on the impact model input uncertainty has on model

predictions in the context of NPS modelling (though see Chaplot, 2005 and Chaplot et al., 2005 for the influence of input uncertainty on NPS modelling, and Vrugt et al., 2008 and Balin et al., 2010 for an examination of input errors impacting discharge predictions). This paper thus makes an interesting, though not totally unique, contribution to the watershed modelling literature.

The Soil-Water Assessment Tool (SWAT), a widely used, spatially distributed NPS model, was calibrated to a total phosphorus (TP) loading dataset using standard tools (SWAT-CUP) and good quality model inputs. These inputs were then perturbed using a Monte Carlo approach, and the perturbed inputs were used to run the model. Inputs were perturbed one at a time (e.g. just rainfall, just topography), then the entire ensemble of perturbed inputs was used to estimate total uncertainty. The variability of model outputs across the set of perturbed inputs, quantified using the coefficient of variability, was taken as an indicator of the uncertainty due to the inputs. The results suggest that for the case study, the primary source of uncertainty is due to the location and number of rain gauges, followed by topographic data source and resolution, with fertilizer amount and land use data contributing the least uncertainty.

Overall, the study was reasonably well structured and informative. The authors' methods were fairly straightforward and reasonably appropriate, though some questions did arise during my review and are detailed below. While the results need to be interpreted carefully so that readers understand how the attributes of the study site, the authors typically guide this interpretation. For instance, the authors attribute the low uncertainty due to fertilizer inputs to the low levels of fertilizers applied in the study site.

I did have comments for the authors regarding their literature review and methods, which are detailed below. The general synopsis is that more work is needed to situate this study in the literature, and that the methods needs to be better described. However, I do not believe any of these criticisms are fatal to the paper itself. I believe they can all be addressed with a major revision.

Specific Comments

I did have a number of questions for the authors regarding their methods, which I believe are not described in sufficient detail. The primary conceptual issue I had with their approach lies with the empirical nature of many of the parameters of SWAT. For instance, earlier studies have found that much of the parametric uncertainty of SWAT lies with the curve number parameters (Cibin et al., 2010). The curve numbers are very empirical, and their optimal values probably serve to compensate somewhat for the input uncertainties. If the model had been re-calibrated to each perturbed input set, the calibrated parameters would likely have compensated somewhat for the perturbed inputs in an effort to reproduce the observed data. By perturbing inputs but not re-calibrating the model to them, the authors may be overestimating the uncertainty due to the inputs. I understand that recalibrating to each perturbed input set would be quite computationally intensive, and beyond the scope of this study. I do not know if the overall results would not change significantly if the model were re-calibrated to the perturbed inputs, though the authors should mention this possible shortcoming in the methods or the discussion.

The authors also mention that when they calculated the uncertainty due to all of the inputs (presented in Figure 3), they retained only behavioral inputs, which they defined as those leading to a Nash-Sutcliffe Efficiency of greater than 0.5. This is a reasonable calibration approach for model parameters, and is used by the GLUE methodology. However, I don't see how this approach translates well to model inputs. If a (perturbed) model input gives a poor fit, but is within the uncertainty envelope of the inputs, doesn't a poor fit suggest that the model is sensitive to that input? The authors need to explain their rationale and approach better in the methodology section.

Regarding the introduction and discussion, more work is needed to situate this study in the relevant literature. A number of key statements are made with no attribution at all. For instance, on page 4: "there is relatively more uncertainty research about hydrological processes but less on NPS pollution" I think most readers would agree

with this statement, but please include some citations of hydrological (e.g., Beven, 2006; Balin et al., 2010) and NPS (e.g., Gassman et al., 2007; Wellen et al., 2015) literature to establish this and many other statements. Also, a pair of papers examining the effect of input uncertainty on the SWAT model in a very different study site should also be cited in the introduction (Chaplot, 2005; Chaplot et al., 2005). One of the interesting results from Chaplot's (2005) work is that there exists a spatial resolution saturation level, beyond which further refinements to resolution do not improve model performance. This result echoed in this work with respect to rain gauges.

Minor comments

P.5 Can you provide a reference where readers can find documentation of the study area's soil types? Many readers will not be familiar with these soils.

P.5. The authors should clarify in the methodology whether they refer to total phosphorus load, concentration, or flow-weighted concentration.

P.7 I think most readers won't understand exactly how the authors perturbed the land use data to simulate their contribution to model uncertainty. More clarity is required here.

P.8. It sounds like the standard deviation is the variability in the amount of fertilizer applied. This is not necessarily the same as the uncertainty. The value of the standard deviation is not given – it would help to situate this study in the literature. Further, can the values of the mean and standard deviation be given in amounts of phosphorus applied?

P.9, line 13. There are also calibration data uncertainty and parameter uncertainty. A citation would strengthen this line.

P.10 The coefficient of variability (CV) as expressed in Eq. 2. assumes a normal distribution. However, the distributions used to estimate the predictive uncertainty may be highly skewed, in which case the CV would need to be calculated with a different equation.

P.11 The number of gauges beyond which improvements to the model predictions are not found should be normalized to the area of the study site.

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Please also note the supplement to this comment:

http://www.hydrol-earth-syst-sci-discuss.net/12/C6191/2016/hessd-12-C6191-2016-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 11421, 2015.