Response to: *Interactive comment* on "Understanding groundwater/surface-water interactions through hydropedological interpretation of soil distribution patterns" by I. Nalbantis (Referee).

I would like to express my gratitude towards I. Nalbantis for the thorough review of the manuscript and the constructive criticism supplied. Below a short response to some of the key issues; a more detailed response will follow with a revised manuscript, if the feedback from additional reviewer/s is positive.

I've used *italics* to indicate original comments by the referee; followed by a bulleted response:

1) Methodology section

"The fact that the presented work relies heavily on previous works makes it difficult to clearly discern the real contribution of the former. For this reason it is suggested to divide the aforesaid section into three sections."

"Reference to the Pearson correlation coefficient together with a test of its significance is required…"

"Using more mathematics…,help in enhancing traceability"

- Excellent suggestions; will divide into methodology section into:
    - Site description – description of general characteristics of the individual basins.
    - Background information – this will include the data and information from Ebrahim and Vilholth (2016)
    - Methodology – this will be my methodological contribution, more detailed description of the statistical procedures followed as well as including more mathematics to improve the traceability of the methods.

2) Statistics

"…author employs the Pearson correlation coefficient, r. It is known that….explained variance (EV)…is equal to the square of r. The correlation coefficients between various attributes are therefore low"

"…the qualifier "significant" will be interpreted as "statistically significant". It is therefore necessary to provide information on the statistical significance of r."

- Although I argue that EV values between BFI and selected soil attributes (e.g. 0.52 – depth, 0.25 – clay content and 0.61 – recharge soils) are relatively low, it is noteworthy that these values are considerably higher than EV values of attributes which normally explain BFI (e.g. stream length, drainage density, rainfall, aridity index etc.). I did however neglect to contextualise these values in relation to other similar studies and will include this in updated manuscript.
- Indeed, when the qualifier "significant" was used it referred to "statistically significant". Will include the significance level in updated version of manuscript.

"The choice of the Pearson correlation coefficient as measure of statistical dependence implies that the relationship linear. …nonlinearities in hydrological processes can lead to nonlinear relationships and low values of r even though variables are strongly related…suggest use of another measure of dependence which avoids implying linearity (e.g.

Spearman's rank correlation coefficient). …aforementioned results with low values of explained variance will be improve…

- Excellent suggestion; I selected Pearson correlation coefficients in order to compare results with that of Ebhrahim and Vilholth (2016) and other studies. I did however suspect that the data was not normally distributed and the assumption of linearity not correct. A very good suggestion to include additional measures of dependence.

3) Specific comments and technical corrections

- The referee proposed more than 30 corrections and suggestions. I went through them all, and all of them contributes to improved quality of the manuscript. These corrections and suggestions will be included in a revised version of the manuscript.

4) General

"Very often, some parts of the studied system are modelled in detail (in space-time) while for other parts simplified models are employed; in that way essential interactions among system components may be poorly represented or even omitted…Nalbantis et al. (2011)

- Thank you for the reference to this paper; I've only read through the abstract but already, some of the key sentiments which I would like to express are highlighted there.
- I do believe that the inequity in terms of dealing with some parts of the hydrological system in models results in a skewed picture of how the system functions. Also, in calibrating the models, the modeller often use/tweaks that parameters representing parts of the system which he/she least understand in order to improve the outputs of the model. This results in models which 'works' but for the wrong process reasons (Weiler and McDonnell, 2004). These 'calibrated' models are not suitable to predict impacts of land-use change (especially in ungauged basins). In my opinion, soils and soil properties are very often used as a 'tuning knob' to calibrate models.
- I am looking forward to give the paper a thorough read in the near future.