# Interactive comment on "Technical Note: Approximate Bayesian Computation to improve long-return flood estimates using historical data" by Adam Griffin et al.

**Anonymous Referee #1**

Received and published: 2 October 2018

The paper is a valuable and interesting brief contribution to the literature on statistical techniques for incorporating irregular data in flood frequency estimation. It needs some more explanation and justification of assumptions, tidying up, minor revisions and a thorough proof read. I have not attempted to review all the mathematical details, or the description of the sufficient summary statistic.

Main comments 1. The discussion of post-FSR developments appears to end in 2001. It would be helpful to include more recent work or guidance, particularly since the Dixon et al paper is already referenced. 2. The reference to MacDonald and Sangster appears rather out of place and somewhat selective. 3. P2 line 9 mentions that

FEH methods improve long return period flood estimates: in the context, some readers might take this to mean that the FEH pooling method is a way of including historical data. 4. The discussion of current methods appears to be split between sections 1 and 2. Some re-structuring would help. 5. The introduction needs to explain why the research is needed, for example defining an objective, specifying a hypothesis to be tested or describing a gap to be filled. 6. It would help if the paper justified why the analysis is focusing on annual maxima. 7. The paper could usefully make clear how the annual maxima for the two upstream gauges were calculated, presumably by summing the two sub-daily flow series and then extracting the AMAX? 8. A significant assumption is made that the channel and floodplain hydraulics have not changed over the historical period, so that a current rating can be applied over the full period. The paper should explain how this assumption was checked: what investigations were carried out? For example, there is evidence that a weir was built in Shrewsbury in the early 20th century, and that earlier flood levels may have been elevated due to the presence of a mill wheel in one bridge arch. 9. The assumption that the historical period starts at the earliest event is weak. There are better ways to estimate the length of the period, for example as described in the Dixon et al (2017) reference already included. 10. The paper would be improved if it commented on whether the ABC method could be applied in the case where the magnitudes of the historical threshold exceedances are not known. 11. P.7 line 23: does the statement that high flows are underestimated rely on the assumption that the plotting positions of the observed floods are correct? 12. P. 7 line 26 refers to section 2.2 but in that section there does not appear to be a mention of how the rating treats out of bank flow. 13. P. 7 line 27: why test the method on a data series that is thought to be in error? 14. P. 7 line 28-30: might another explanation for the poor fit be that the chosen distribution is not an appropriate model, perhaps due to the effect of floodplain storage features (locally known as "argae") on the Severn upstream of Shrewsbury? 15. P. 8 line 3-4 is not entirely clear. Why can likelihood methods not be applied in some of the situations mentioned, such as using sedimentological data? Elsewhere the paper seems to imply that the true strength of

the ABC method is that it always converges.

Minor issues and presentation 1. The meanings of the phrases "new data points" and " point estimates" in the abstract are not clear. Similarly in the caption of Figure 4. Is "point" being used in some specialised sense here? 2. Some acroynms need spelling out in full when they first appear, such as GEV and AMAX. 3. There is repetition in the first few lines of page 3, and contradiction between 15 seconds and 15 minutes. 4. There are several uses of the phrase "as such" in a way that is grammatically questionable.

C3