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# *Interactive comment on* "Eliciting geologists' tacit model of the uncertainty of mapped geological boundaries" *by* R. M. Lark et al.

**Anonymous Referee #2**

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This manuscript addresses the use of statistical expert elicitation to characterize the uncertainty in mapped geological boundaries. The methodology and application are explained clearly and in great detail. The text is well-structured and reads very well. I am not an expert in statistical expert elicitation theory but as far as I can judge the methodology was properly applied. Even though a simplified case was addressed which nonetheless took a lot of work, I do see the added value of expert elicitation in these type of applications.

In summary, I am very positive about this work and have only a few comments

Detailed comments:

(Section 1.1) I am not convinced that there is a true difference between scale-



dependent uncertainty and cartographic uncertainty. In both cases a line is smoothed or generalized.

(Section 1.2) The literature review should include reference to work on statistical modelling of positional uncertainty in spatial objects, as published in the Geo-Information Science literature.

(Page 13 line 16) Why were the upper and lower absolute bounds only assessed by the group as a whole and not first individually, as was done for the three quartiles?

(Page 13 line 17) Why were the upper and lower absolute bounds of the error variable assessed instead of other measures of the tail of the distribution, such as the 5 and 95 quantiles? It seems to me to be much easier to come up with realistic estimates of these quantiles than of the minimum and maximum. How can experts be certain that the error variable is not greater than the maximum or smaller than the minimum? It also looks as if the minimum was too large and the maximum too small. For instance, in Scenario 4 Expert D decides for Q1=-40 and Q3=40, while lower limit = -75 and upper limit =75. This implies that the average probability density between minimum and Q1 and between Q3 and the maximum is greater than between Q1 and Q3, which does not make sense. Also, some of the densities in Figure 4 are discontinuous at the minimum or maximum, which is not realistic either. I am not asking that the expert elicitation is repeated, but these issues may be noted in the Discussion.

(page 23 line 28) I could only access the abstract of Whitaker et al. (2013) but from that it was not obvious to me that this is a sensible approach to extending the uncertainty modelling to 2D polygons. Why not use existing approaches from GI Science, such as Heuvelink et al. (2007, IJGIS 21, 497-513)? Under stationarity assumptions, the uncertainty about the position of a spatial object can be fully characterized by a finite-dimensional probability distribution – which includes spatial dependence – without resorting to a large number of number of parameters.

(Figure 2) The distribution of Expert C is missing because it overlaps with that of Expert

B. Perhaps make a note of this in the figure caption.

Interactive comment on Solid Earth Discuss., 7, 147, 2015.