Response to Tony O'Hagan's and Simon French' comments on `Expert Judgement Combination using Moment Methods'

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We would like to thank Tony O'Hagan and Simon French for pointing out such relevant issues regarding our paper.

In our response, let us first clarify that we do not suggest the direct elicitation of moments of uncertain quantities. There are well known approximations that allow one to pass between quantile assessments and mean/variance assessments which may be used to pass from one parameterisation to the other. To the theoretician such approximations may appear crude. In applications however, one should be carrying out some qualitative assessment about the uncertainties involved (for example to check whether there are important variables on which one should condition first, whether the uncertainties are roughly symmetric or highly skewed, etc). If the expert questions are structured appropriately then the possible theoretical problems of using quantiles to estimate moments can be mitigated in practice. The main advantages of the moment approach are the mathematical properties of the scoring rule, and the ease with which it can be explained.

In our paper we have defined the moment analogue of well-known properties of expert judgement combination in a probabilistic setting. We agree with French that the zero correlation preservation property is not a desirable property in the context of performance based expert judgement combination. We favour the marginalization property and find in the moment context that no combination function can satisfy both the marginalisation and the zero correlation preservation property, nor both the marginalisation and the external Bayesian property. Regarding the persuasiveness of the zero preservation property in the expectation context, we can add that a statement of zero expectation, in analogy with a statement of impossibility for the zero probability preservation, can be regarded as a statement of non-existence.

Both reviewers say they prefer to see expert assessments as data and, as Bayesians, do not agree with the approach taken here. There are of course various ways of taking account of expert data. For example if there is only one expert then the analyst could just adopt this expert's view as his or her own. When there are multiple experts who could provide different assessments this is not possible. The formal Bayesian approach then essentially involves the analyst constructing a prior and adjusting this via Bayesian updating on the basis of the expert information. This contrasts with the approach adopted by us and in the Classical model of constructing a linear pool of the expert assessments where the combined assessment represents rational consensus. There is a fundamental issue at stake here. The usual Bayesian approach is intellectually pure and "coherent". However, it is at heart an individualistic subjective theory and provides no good answer as to why any two people should adopt the same viewpoint. In particular if an analyst puts together a Bayesian model that incorporates expert views as data as above then two fundamental choices are being made which place the analyst in some sense above the actual experts. First, the analyst is defining a prior distribution for the variables of interest. Second, the analyst is defining - through the likelihood function - the way in which expert judgements are going to be allowed to influence the analysts own opinion! This may be a mathematically coherent approach, but why should it be acceptable to a real-world decision maker? ore acceptable is an approach in which different viewpoints are treated fairly and indeed equally a-priori, in accordance with Cooke's expert judgement principles. Our approach only demands that the analyst specify the weights for the different calibration variables. These are declared beforehand so that the experts know exactly how their assessments are to be judged. Expert weights are defined purely by expert performance and not by a judgement of the analyst or any other.

Finally we should deal with two technical points raised by O'Hagan. He is right to point out that there is no extension to Bayes Linear that encompasses higher moments than the second. We should have made it clearer that we were discussing the inclusion of higher powers of the uncertain quantities in the collection of quantities under consideration. O'Hagan also suggests an invariance property for the moments derived from combining experts' quantiles and moments obtained from combining the derived moments for each expert. In the light of partial belief specification we believe this to be less important then one might initially think.In contrast to the moments of an uncertain quantity, the marginalization property does not entail a linear pool of the experts' quantiles. Determining the combined quantiles involves a full specification of the probability distribution for quantity of interest. Since the experts only provide a partial specification of this distribution, further assumptions will be needed to determine the combined quantiles. Whether the invariance property holds or not will strongly depend on these assumptions.