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## Rejoinder

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We are grateful to all discussants for their comments and to an editor for initiating this discussion. Rather than addressing each discussion separately, we identify several themes of interest and contention among the discussants that we now develop separately.

## **1** Foundations of DIC

A theme common to all discussions is that DIC is so far more of a plausible measure of complexity than a well-grounded criterion. We completely agree with this perspective and even share the more radical prognosis of Meng and Vaida that DIC may simply lack a theoretical foundation. Indeed, there are deeper concerns with DIC than just that of a definition in the missing data case. In this regard, we do agree with Carlin that our "casework" analysis cannot solve the problem of defining a proper DIC for missing data and even less in general. Therefore, Carlin's point that "authors do not refer at all to any derivation, nor to any subsequent interpretation of model complexity" is both true and meaningless: if DIC as originally defined is a universal way of evaluating model fit or model complexity, it should also apply in the missing data setting and we showed here that it clearly does not. The main conclusion of our paper is thus that DIC lacks a natural generalisation outside exponential families or, alternatively, that it happened to work within exponential families while lacking a true theoretical foundation. Similarly, regarding Meng and Vaida's criticisms about our proposal of an almost tautological emphasis, we (obviously!) cannot agree: in the paper, we are considering models that can be *fruitfully* regarded as missing data models, that is models for which there is a many to one mapping linking the complete data and the observed data.

Some discussants attempt to provide alternatives that could establish theoretical foundations for DIC. For instance, van der Linde focusses on DIC as an approximate estimated loss, in the same way that BIC is an approximate log Bayes factor, even though she is obviously less critical of DIC in exponential families. She seems to envisage our developments as the result of various approximations. In that perspective, we could wonder what is the whole point of producing such criteria. If the approximation (of a posterior loss?) cannot be evaluated, we should then consider other models in which no approximation is required and then check the appropriateness of each approximation. Further, while using true loss functions is usually sensible (Celeux et al. 2000), it remains to be seen which loss functions correspond to each of the DIC<sub>i</sub>'s, if any. (In this regard, DIC<sub>2</sub> could be described in a sense as being a more robust version of the basic DIC<sub>1</sub>.) This obviously does not relate to the hair(y) loss mentioned by Meng and Vaida!

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