# Comment

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A search for new directions to pursue in the Design of Experiments was undertaken at workshops at Berkeley in June of 1984 and January of 1985, and at UCLA in July of 1985, but the thought that design of computational experiments might stand alone as a substantial topic for research can be dated from the January 1986 workshop at UCLA. Subsequent workshops at Urbana in May of 1987 and at Santa Fe in September of 1987 confirmed this promise. The push for (and the early organization behind) this development should be credited to Toby Mitchell.

The paper under discussion does a nice job of capturing the richness and fascination of the subject. It gives a faithful representation of trends in the choice of priors, in the choice of criteria, in the use of cross-validation and maximum likelihood estimation, and in territories for application. It seems appropriate to us that this brief comment concentrate on two general themes: how does the area relate to others which have come before, and what particular contributions might we expect from it in the future? What we take to be the important problems will be mentioned in the course of the discussion.

The philosophical approach of uncertainty measures does indeed go back a long way, as has been well documented in Section 4. Beyond the shared need for a catalog of workable and representative priors, the real problem is that of modification of the prior based on observation and somewhat beyond cross-validation or maximum likelihood over a parametric family. Some clever ideas about modification have been put forward by Mitchell but there is no general method to fall back on in this area.

Monte Carlo is another tool that introduces probabilistic notions for use in a deterministic world. Design can and does play a role in problems of a similar vein. Here we are thinking about settings in which individual evaluation is desired over a vast array of "objects" and, while easy to perform, is still only possible for relatively few of them. Interest might then center on the proper allocation of resources to neighborhoods which are determined by a suitable "distance" between objects, say.

In saturated or super-saturated contexts, Latin hypercube sampling and off-line control are appropriate techniques in the absence of interactions, and some Bayesian methodology seems necessary in any event. Does this paper contain enough evidence, anecdotal or otherwise, to suggest that the present research will establish its own identity, lead to catalogs of useful designs or give real guidance to someone possessed of a like problem? The answer seems to be: not yet.

Our use of the terms object and distance is premeditated. In Johnson, Moore and Ylvisaker (1988) it is shown that, in the absence of good prior knowledge, designs of a geometric type have certain robustness properties. Such robustness is associated with low correlations between observations. Coupled with the thought that few observations mean large separations (surely consistent with low correlations), certain design problems are reduced to more basic geometric ones. In effect, one goes full circle to return to a deterministic question. (Incidently, the IMSE criterion does not show up in a very favorable light in these considerations, while the MMSE criterion surfaces readily and has a natural connection with $D$-optimality.)

Our preference in any event is to remain free of thinking in terms of regular or stylized design spaces, such as the unit cube in $d$ dimensions. This can be aided by limiting consideration to finitely many sites, hence finitely many designs, while not really violating the spirit of what is feasible. Structure might then be imposed with the notion of distance between sites. The problems become: what distance is appropriate and, most importantly of all, how should the distance chosen undergo modification in the face of observed data? Thus, beginning with the collection of data at distant and suitably chosen sites, where ought one to turn now for further experimentation? This point of view seemingly emphasizes design and plays down the role of analysis of posterior uncertainty. However, the answer selected might well come out of such an analysis.

In summary, we find the area of design for computational experimentation is a lively one and the present article attests well to that. Much thought is still required but, since successful applications continue to emerge, this is certainly a worthwhile enterprise.

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