

Discussion of the Best, Waller, Thomas, Conlon and Arnold paper

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I would like to point out to the authors of this stimulating and well-presented paper that their world does not exist.

I am not so much concerned here with the implied assumption that individuals live their lives inside the region defined by their address (although this is also probably an underdiscussed aspect), nor am I questioning the perhaps overly idealistic probabilistic assumptions about Poisson distributions and log-normal priors for their mean parameters. What disturbs me is a seeming lack of coherence, or inner consistency, of the class of spatial Poisson models used here.

Best et al. build a Poisson model with spatial interaction for incidence counts for 56 districts of Scotland. If one takes the motivation and arguments favouring these models seriously, they would a fortiori lead to the same type of models also if for example two of Scotland's districts were taken together, or if some of her parts were to be partitioned more finely. But the proposed class of models is not 'closed' under such operations; the Poisson aggregates agreeably but not when coupled with log-normal priors.

Taking the partitioning of Scotland to its logical fine limit, the (perhaps implicit) motivation behind the models used leads to a continuous-space random field $Y(x)$ which conditional on a mean parameter field $\lambda(x)$ is a Poisson process, and where the $\lambda(x)$ is a log-Gaussian process. This is precisely the class of log-Gaussian Cox processes; see recent work of Møller (1998) and Møller, Syversveen and Waagepetersen (1998). Therefore, one possibility would be to analyse demographic incidence counts data by viewing these as aggregated versions of such continuous-space models, with appropriately modelled covariance functions, priors on the relevant parameters, and so on. The benefit is a more coherent and less ad-hoc based class of models, which would be conceptually and statistically more satisfying also regarding interpretation of parameters. The difficulty is the need for more work in order to make analysis possible for aggregated log-Gaussian Cox processes. Such might be possible through suitable stochastic process Monte Carlo regimes. One should also explore log-double-exponential Cox processes.

Another class of models where aggregation from a continuous model is more immediately manageable is the Poisson field type with a smoothed Gamma process as its random mean, as introduced in Lo (1982) and recently extended and made more tractable by Ickstadt and Wolpert (see e.g. their paper at this conference).

One might argue that strict statistical models are always fictitious, and fitting an incorrect model, via estimation of its 'least false' parameters, often produces useful results. But good fiction with strong internal logic is often better than stories with weaknesses in their fundamentals. And the present story seems to be forced into a situation where priors have to be placed on and posteriors have to be computed for least false parameters of not-quite-existing models and whose interpretation changes too much by small alterations of the underlying map.

I realise that this paper is part of a certain tradition within spatial modelling of count data. I assume that if we assume, for the sake of the argument, that my comments are of interest, then they are of relevance to a long list of other papers in this area. Of course many of these efforts, including the present article, are useful and provide insights into the phenomena modelled, but I think other approaches in due time will prove more useful.

References

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