Rejoinder

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My thanks to all six discussants for their comments, which have focused attention on a number of issues raised by the paper. In my reply I have attempted to classify what the major issues are, dealing with the individual discussants' points under those general headings.

1. IS THERE REALLY A TREND?

Given the emphasis of the paper and the practical importance of the question, it is not surprising that several discussants have focused on this issue, and Raftery has provided an enlightening alternative analysis.

Let me first remind the reader of my own conclusions on this question. The evidence for a downwards trend is by no means clear-cut. Fitting model (4.1) with estimated β_j yielded nothing at all. A likelihood ratio test based on split data (Section 5) also failed to produce a significant result, though, as Raftery points out, it would have been worth trying some more parsimonious forms of the alternative hypothesis. Only the calculation of exceedance rates in Table 4 produced any solid evidence for a trend, and even there it is hard to be sure about their significance. This may indeed be rather a weak conclusion but I think it fairly reflects the evidence in the data.

Raftery has proposed an alternative analysis based on the point process of exceedances of a fixed level. By concentrating on cluster means and employing a time transformation to take account of both seasonal effects and the missing data, he creates a data set for which the null hypothesis of a homogeneous Poisson process would be reasonable, and he then tests this against the alternatives of: (1) a log-linear decay in the intensity, and (2) a change-point model. The evidence against homogeneity is stronger using the change-point alternative than a log-linear decay, but still "not worth more than a bare mention."

I think this is consistent with my own conclusions. Indeed, merely from the data in Table 5 it is possible to carry out a likelihood ratio test of whether the Poisson rate is the same over the two halves of the data. For exceedances of level 16, I obtain a deviance statistic (nominally χ_1^2) of 3.36, while, based on exceedances of level 20, the corresponding value is 1.74. Again, this is some evidence but hardly very strong.

As a technical aside, a full development of Raftery's analysis would presumably take account of the fact that the seasonal variation itself depends on estimated parameters. This would be tedious but straightforward to incorporate into the analysis of model (1), but model (2), with its nonregular features, may pose more problems.

The real import of these conclusions, however, can only be assessed in comparison with similar analyses carried out at other sites. My understanding is that ozone analyses at other sites in Texas have yielded far more clear-cut evidence of downward trend than this one. There is also an argument that the absence of a clear *upward* trend is evidence in itself that air quality regulations are having an effect. Thus it may be better to focus on *estimation* of a trend rather testing for its existence. In that case, the main message of the paper would be not to look for a simple additive trend but to measure it in terms of estimated exceedance rates of high levels, in which case the evidence for a trend may well depend on the level chosen. Fairley's comments reinforce this point; standard statistics such as mean or median may not give an accurate indication of what is happening at extreme levels.

2. FORMATION OF CLUSTERS

The other aspect which all the discussants mention in some way is the local behavior of the series near high exceedances. This can be subdivided into several individual points.

Identification of clusters. The paper used a very crude rule to identify clusters, and both Joe and Singpurwalla queried the appropriateness of this. To answer a point of Singpurwalla's, if the clustering rule failed to produce approximately independent clusters then equation (3.6) would indeed not be valid. In Smith (1984) and Davison and Smith (1989), an alternative method has been proposed based on assuming the full point process of exceedances to be a simple doubly stochastic process. However, the clustering procedure proposed is very similar to the one in this paper. One could also consider methods based on, say, the Neyman–Scott or Bartlett–Lewis models for a clustered point process. There is scope for more work here. In the present study I do place considerable weight on Table 3, but it is reasonable to ask what we would do if the results were not so good, as might indeed happen if there were longer clusters than there appear to be in the present data set. In such cases, a more careful treatment of the clustering problem might well be required.