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In this paper the authors have made a convincing case for the need to modify Hampel's definition of influence curve before using it in time series analysis. The basic intuition is simply stated. Time series have "memory," so a definition using the concept of the influence of observations one-at-a-time must be inadequate. It is helpful to have this intuition reinforced by the analysis provided by the authors.

We like the model form $y_i^\gamma = (1 - z_i^\gamma)x_i + z_i^\gamma w_i$ as a generalization of the usual contamination model, as it appears to us to be a more realistic model for outliers. Concerning the technical aspects of the paper we have several questions, however. In their general replacement model (2.1) the authors require the contaminated process to be stationary and ergodic. Is it not furthermore necessary to require *joint* stationarity of the (x, w, z) process when the components are dependent?

While in some settings it is possible to consider estimates of the form $T_n = T(F_n)$ (see Huber (1981) and Künsch (1984)), the authors require a more sophisticated definition which defines T as a limit of sequences of functionals T_n (see Hampel (1971)). They seem to require very weak conditions on the T_n's, but we wonder if the stronger one of equicontinuity may be needed. Without this condition, how can we be sure that for some fixed n, $T_n(X_1, \ldots, X_n; F)$ is not very far from $T(\mu_F) = \theta$, even for large n? We also wonder if more attention needs to be paid to the domain of definition of T. Suppose, for example, we take the domain to be the space of stationary and ergodic processes. Then we note that the IC is derived from the ICH, which is defined for some measures that are *not* stationary and ergodic.

Finally, we note that the IC defined by the authors is process dependent but not data dependent because the IC is essentially obtained by "expecting" the data out of the ICH (cf. Hampel (1974) and Künsch (1984)). Thus this IC is appropriate for studying questions of "gross error sensitivity" but not questions of a pointwise nature. Gross error sensitivity is probably not a sufficient basis for evaluating robustness, so we feel additional criteria will need to be introduced to complete treatment of robustness in time series.

We now wish to raise concerns of a practical nature. We write quite frankly wondering how important psi functions and influence curves will prove to be in time series modelling. As the authors note, experienced time series analysts are quite familiar with outliers, and perhaps it should not go without saying that these analysts have some pretty good ideas on what to do about them. The time indexing and the memory that make the theoretical treatment of outliers difficult provide some resources to guide the practical handling of outliers. Moreover, in practice, we have recourse to much richer models than those contemplated in the paper under discussion.

As examples, we refer to pages 67–70 of Jenkins (1979) and to Miller (1986). In the first reference, a change in policy creates an "isolated" outlying observation followed by a gradual return to equilibrium. This effect is evident in the residuals

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