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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 $T(\mu_F) = \boldsymbol{\theta}$, 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 and is modelled by intervention analysis. (See Box and Tiao (1975).) In the second reference, a bivariate time series model of two fertility measures is constructed. The data analysis reveals the years of World War II as a "patch" of outlying observations. Intervention terms are added to the model to handle them, and the parameter estimates show that their amplitude is not constant. The main point is that outliers often have "assignable causes," and if so they can be incorporated into the model, rather than downweighted. A secondary point is that outliers that do not have assignable causes may deserve to have full weight, as their downweighting may result in the underestimation of the variance of future observation. Jenkins (1979), in fact, handles a second outlier in his series by doing nothing to it because he can find no cause for it.

We are very concerned about our ability to recognize data that have been generated from the models presented by the authors. We know, for example, that an AO model with a core AR(1) and contaminating white noise is, theoretically, an ARMA(1, 1) process. Yet there is evidence that

(a) this situation is very difficult, if not impossible, to identify from the usual data analyses involving correlation functions or spectra; and

(b) even when we try to fit an ARMA(1, 1) model to the data using nonlinear least squares, the parameter estimates have very unattractive properties. (See Miller (1980).)

The authors have shown that once a contamination model is properly identified, their estimation techniques are attractive. Do they have any suggestions for model identification? If not, do they have any notion of the effect of model misspecification on their estimation techniques? While confrontation of these questions may not be appropriate in a paper on influence curves for time series, we feel we need answers to them before the authors' work can be applied.

In closing we wish to express the spirit in which we hope research on robust methods in time series will be done. Time series are usually analyzed in an environment in which collateral information in available, whether it be knowledge of economic or social upheavals, of policy changes, or of malfunctioning equipment that monitors a stream or a machine. Moreover, time series analysts are trained to recognize patterns in correlation functions, spectra, and sequence plots that suggest fundamental modifications of simple, basic models, such as ARMA models. If robust techniques can help us do these things better, then they will be welcome additions to theory and practice. If robust techniques can only promise estimates of parameters of "core processes" without due regard for other events that impact the series of interest, and without suggestions for model identification, then we fear they will be of limited practical use.

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1. General remarks. I would like to begin by saying that I enjoyed this paper very much. As with their previous works, both individual and joint, Martin and Yohai have achieved in this paper a nice combination of analytical rigor and practical significance driven by clearly presented intuition. I congratulate the authors on this contribution.

Despite their central role in many areas of robust statistics, the traditional influence curves proposed by Hampel have played a somewhat limited role in the study of robustness properties of statistical signal processing procedures for applications such as communications and control, primarily because of the restriction of their applicability to static models. Other approaches, such as minimax robustness, have proven to be much more useful in this context (see, for example, the recent surveys by Kassam and Poor (1985) and Poor (1986)). However, by allowing for the treatment of dynamic models, the notion of influence functionals as proposed by Martin and Yohai eliminates this principal disadvantage. The introduction of a heuristic tool of this type is thus a major advance from the viewpoint of robust statistical signal processing, and I can foresee a wide range of applications of Martin and Yohai's ideas in this area.

2. System identification. System identification is among the many applications that can be examined in the context of the Martin–Yohai influence functional. For example, consider the simple problem of identifying a first-order time-invariant linear system from measurements of inputs and noisy outputs. This problem corresponds to the model

$$\begin{aligned} s_i &= \theta s_{i-1} + u_i, \qquad i \in \mathbb{Z}, \\ q_i &= s_i + n_i, \qquad i \in \mathbb{Z}, \end{aligned} \tag{1}$$

in which we assume that $\{u_i\}_{i \in \mathbb{Z}}$ and $\{n_i\}_{i \in \mathbb{Z}}$ are independent i.i.d. $\mathcal{N}(0,1)$ sequences and $|\theta| < 1$. The nominal observation process $\{x_i\}_{i \in \mathbb{Z}}$ consists of the inputs and noisy outputs $\left(i.e., x_i = \binom{u_i}{q_i}\right)$, and so we can think of actual