fact that only finite observations are available. The class of models I have in mind includes various mixtures of deterministic and stochastic models. An example of such mixtures is the one mentioned by Berliner and studied in Chatterjee and Yilmaz (1991) in which a deterministic system serves as an input to a second system. I also like to mention that, similar to other statistical problems, there are cases in which statistics alone cannot determine the most appropriate model. In these cases, subject matters of the problem under study, such as the implications of a model, should play a more important role in the analysis. All of the above discussions are familiar to statisticians and show that important problems in chaos are no different from those in statistics. The only difference between chaos and statistics is that traditional statistics begins with linearity, whereas chaos is necessarily nonlinear.

In sum, chaos is fascinating because of its mathematical simplicity. It is important, especially to statisticians, because of its nonlinear nature. Theory of chaos and analysis of chaotic data are parts of statistical theory and modeling. Statisticians should be interested in chaos and can make significant contributions in chaos because it is statistics, although not in the traditional and linear sense.

Finally, I like to list some areas in chaos that statisticians and probabilists are well equipped to make significant contributions:

1. Ergodicity conditions of nonlinear dynamical systems, deterministic as well as stochastic.
2. The invariant density of a given dynamical system.
3. Methods (statistical and graphical) for uncovering lower dimensional systems based on noisy data.
4. Nonparametric statistical methods for dynamical system analysis, both for prediction and for structure recovery.
5. Complexity measures of a nonlinear dynamical system based on finite and noisy realizations.

Rejoinder (part 1)

Sangit Chatterjee and Mustafa R. Yilmaz

We are indebted to all six researchers for their stimulating and thoughtful comments on the two surveys. They make it abundantly clear that the theory of nonlinear deterministic chaos is still in its formative stage, and its relationship with statistics is just beginning to be explored by statisticians. We are especially pleased that each comment provides a somewhat different perspective concerning the emerging theory. Collectively, these comments help clarify and sharpen the important issues that will keep researchers busy for a long time to come.

The main motivation for our survey was our belief that the theory of nonlinear deterministic systems provides a different and potentially useful perspective from which statisticians can look at complex processes. We are pleased to observe that this opinion is shared by all but one of the commentators. The basic reason for the recent explosion of interest in this perspective is valid if not yet real: it may enable us to understand and explain the sources of randomness in some processes. No statistician can be indifferent to the exploration of this possibility, no matter how far from reality it might seem presently.

Within the confines of a rejoinder, it is neither possible nor appropriate for us to respond to all comments. With a conscious effort to avoid repetition, we shall briefly touch on some of the issues and points raised, especially in those cases where there is an apparent conflict in viewpoints, a contribution to be recognized or an error to be corrected. For this purpose, we have divided our comments into three sections. First, we briefly respond to each author in alphabetical order, next we provide a brief update of the literature and then conclude with some final thoughts and comments.

DISCUSSION ON COMMENTS

Professor Cutler provides expert discussions of singular and absolutely continuous probability distributions on attractors and their implications for dimension estimation [also see Hunt and Miller (1990) in this context]. Her discussion goes far beyond our review, but contrary to her statement, we do briefly mention lacunarity and nonuniformity in Section 1.2. Professor Cutler also discusses, as does Professor Smith, two basic ways noise enter a dynamic system. First, the errors can be observational or measurement errors. Here, the observation is simply corrupted by an additive random term as given by Cutler's (7). In the classical time series literature, such errors are discussed under the umbrella of outlier analysis. Time series analysts will immediately recognize (7) as an additive (type I) outlier model (Peña, 1990). The second type of noise in a chaotic system is "when the system under evolution undergoes perturbations or changes." In the time series literature, this type of outlier is called an innovational (type II) outlier, and it is distinguished from an additive outlier in that the innovational outlier continues to influence the time series indefinitely for future observations. As Professor Cutler elaborates, both these situations present considerable difficulty, although different in nature, in parameter estimation as well as the interpretations of results.

Professor Granger is the only discussant who does not see much value in the study of deterministic models of chaos. His clear preference for stochastic models is apparent in his willingness to assume that truly stochastic processes exists in reality, while he believes that the existence of chaos remains to be established outside computer simulations or laboratory experiments. Surely, to assume that truly stochastic processes exist in reality is tantamount to assuming that abstract mathematical notions of probability and randomness actually exist in reality. It seems fair to ask him if and when these conclusions have been established as facts. What happened to the subjective versus frequentist controversy concerning probability in the real world? We certainly believe that abstraction is a useful tool for studying real processes, but how can one justify the assumption that abstract *is* real?

One way Professor Granger distinguishes white chaos from an iid series is that white chaos is "singular," whereas an iid series is not. He defines singularity to mean that there is a function g such that $\text{prob}(x_{t+1} - g(x_t) = 0) = 1$. Professor Granger fails to observe that such a function g can exist for an iid series also; it certainly does for a series of finite length. Thus, his notion of singularity (which, incidentally, is quite different than that used by Professor Cutler) is not a valid way of distinguishing between chaotic and iid series.

Professor Granger then introduces measurement noise and discusses the problems of forecastability in presence of white chaos. In the end, he seems to dismiss chaotic models and opts for a classical parametric model of the type $y_t = f(x_t) + \varepsilon_t$, with iid ε_t and nonlinear f, and advocates methods based on nonlinear time series models. We have no disagreement with the classical approach, but it cannot be used to dismiss deterministic models in all cases. Using his own reasoning, one should not conclude that chaos does not exist if its existence has not yet been fully demonstrated. Chaotic models and self-similarity could indeed be ubiquitous, and no branch of science can be regarded as fully investigated until these concepts are explored. One of the surprising consequences of the modern version of Darwinian theory is that apparently tiny influences on the gene pool can have a major impact on evolution. Schroeder (1990) gives many practical (and not laboratory produced) examples of fractals and chaos in the universe. The criteria for the choice of a model should be its ability to describe and predict a complex process. We maintain that chaotic models are intrinsically appealing in this regard, even though identification may remain to be a serious problem.

Finally, in spite of Professor Granger's insistence of predictability of a chaotic series and the lack thereof of iid series, we remain unconvinced that such a distinction can be made. We refer to Casti (1990, 1991) for two excellent discussions on randomness, chaos, predictability and their relationships to universal computer, Turing–Church hypothesis and Gödel undecidability. For a more technical discussion along these lines, including references to Martinlöf, Kolmogorov, vonMisses, Chaitin, Solomonoff and Solovay and others, see Uspenski, Semenov and Shen (1990).

Professor Griffeath contributes insightful discussions of random number generation, cellular automata and complexity that he prefers as a more general term than chaos. His call for researchers to focus on connecting the statistics of trajectories of nonlinear dynamics and the statistics of sample paths is well-taken. This also applies to his suggestion for a hybrid area of deterministic dynamics from random initial states. Professor Geweke apparently feels the same way, for he discusses this idea at much greater length.

We appreciate Professor Griffeath's reminder concerning commonly used congruential random number generators and the pathological performance of some of them. Of course, the history of such algorithms go back to the pioneering work of Von Neumann. We apologize for this oversight on our part, but we did briefly refer to random number generation in Section 4.6. In his discussion, Professor Griffeath asks the provocative question, "What if all the Monte Carlo simulations during the next century are based on (2) and then someone uncovers a flaw?" A response to such a query may go like this: the results of a simulation have to agree with theory, if there is one, or with intuition. At the very minimum, the results of a simulation must make sense in some space, be it a picture or a numerical output. If such checks do not detect any flaw in the random number generating algorithm, then either there is nothing seriously wrong with the algorithm or real (?) randomness is not a prerequisite for the verification of the theory or the law under consideration.

Professor Griffeath elaborates on the many advances in the field of cellular automata, including his specialties, such as interacting particle systems and coalescing random walks. We are pleased to read about these advances and look forward to studying aspects of these works. Recent press reports (Amato, 1991) indicate advancements of cellular automaton hardware for analog-type simulation of physical events (such as experiments in wind tunnels, genetics, polymers and particle physics). Such computing tools have been called computronium or programmable matter.

Finally, Professor Griffeath reminds us of the possibility of seeing the Mandelbrot set on toilet paper. In the same vein, we hope the genetic engineers do not synthesize a tiny Mandelbrot set (bug?) and release it in the environment with their self-similarity genes intact!

Professor Geweke addresses the implications of determinism for the fundamental role that randomness plays in statistics. He presents an approach for inference and prediction using deterministic models that incorporate uncertainty in a particular way. Specifically, he assumes that the generating rule f is known and uncertainty pertains only to the initial condition x_1 and a parameter vector θ (using his notation). Professor Geweke takes the Bayesian viewpoint to construct the likelihood and the predictive probability distribution functions for the tent and the logistic maps. This contribution clearly elaborates on Berliner's work as well as Professor Griffeath's suggestion of hybrid deterministic models.

Using prior distributions of the initial condition and the parameter θ, he illustrates the value of the Bayesian methodology, even when there is no randomness in the process once it begins. Two obvious questions come to mind concerning this approach: first, how practical is it to assume that the deterministic generating function is known (or could be known)? How robust are these results with regard to the functional form of f? Second, how practical is it to assume that priors can be ascertained, and how sensitive are predictive distributions to the priors? Undoubtedly, more work needs to be done before we can expect reasonable answers to these questions.

Professor Smith expands and elucidates on many statistical aspects of chaos theory, including dimension estimation, the impact of measurement error and Bayesian versus frequentist views of parametric inference for chaotic systems. On the important question of operational definitions of chaos and randomness, he reaches the conclusion that the distinction is only meaningful if we can measure it, which we cannot if the correlation dimension is too large. It appears that most real processes may be high-dimensional, and the issue of distinction remains unresolved.

Professor Smith further discusses the correlation dimension and its estimation using the asymptotic power law. His discussion of the independent distance hypothesis is illuminating. He provides us with a maximum likelihood estimate of the correlation dimension, but we are unsure about its properties. For example (using his notation), $\hat{\nu}$ is clearly a function of ε, but how are we to decide on it for actual estimation? Finally, how does $\hat{\nu}$ behave for processes of known values for the correlation dimension. Professor Smith addresses these two queries by providing us with sample size calculations for achieving known bounds of root mean squared values. His conclusion that $\hat{\nu}$ cannot be estimated with precision for reasonable sample size is disheartening, but this is not sufficient reason for statistical scientists to give up their search for relatively simple models for nonlinear systems.

Professor Smith expresses disappointment that our paper did not go into more details about statistical issues raised by the vast literature. We are surely guilty of that, but our objective was to review this emerging field as broadly as we could for the benefit of the statistical science community. So, perhaps we could be forgiven after all. Professor Smith comments on our discussion of the relationship between fractional Brownian motion, fractional differencing, chaos and fractals. We did not imply that chaos is closely connected with fractional models, but merely that the latter have some relevance as modeling tools for fractal generating processes. We do think that the book is not yet closed on the extent of their relevance to chaos. Finally, we admit that our comment about the importance of chaos and nonlinear science in general was meant to be a bit provocative. Niels Bohr once said that a great truth is a truth whose opposite is also a great truth!

We are heartened that Professor Tsay agrees with us regarding the importance of the study of chaos and nonlinear dynamical systems, particularly as an area for statisticians to be involved in. We agree with his view that, at a conceptual level, the dichotomy of deterministic and stochastic models is arbitrary. The real aim of study is to look for appropriate models that are also parsimonious.

Professor Tsay emphasizes the need to keep the objectives of the analysis and the constraints of the environment in mind. We are happy to see that Professor Tsay recommends a deeper study of a mixed strategy approach, where the system is modeled by a mixture of a deterministic and a stochastic component. He states that the basic difference between chaos and statistics is that traditional statistics begins with linearity, whereas chaos is necessarily nonlinear. As a point of emphasis, we tend to agree.

Finally, Professor Tsay presents us with a list of areas in which statisticians can make significant contributions. Some of these areas were also mentioned in our paper. If we could add one more item of general nature to this list it would be this: in traditional statistics, we have a hierarchy of models beginning with a simple linear model and Gaussian iid for the error distribution and proceeding to generalized linear model GLIM, with an error distribution from the exponential family (McCullagh and Nelder, 1986). The study of nonlinear dynamical systems would be facilitated if we could develop a similar structure where the models and estimation techniques span the simple as well as the complex.

A BRIEF UPDATE ON THE LITERATURE

Since our survey was done, a number of papers have come to our attention, including many of the additional references provided by the commentators. We mention a few others below that span theory as well as applications. This is not an attempt to ensure completeness of the literature review, which would be futile, but to avoid obvious omissions.

Ornstein and Weiss (1991) give a mathematical review of statistical properties of various dynamical systems. Their review is broad and gives a historical development of ergodic theory, its interactions with other fields of mathematics over time and an up-to-date mathematical synthesis of chaotic systems. Lalley (1989, 1991) provides discussions of counting problems of the orbit of a point under the action of a discrete group and of orbits of hyperbolic flows and various renewal theorems. Lalley (1988, 1990) also analyzes various topics in operations research dealing with traveling salesmen problems with a self-similar route, packing and set covering functions of self-similar fractals. Finally, a new volume from the Santa Fe Institute, edited by Zurek (1991) has many interesting and revealing discussions of complexity, entropy and Bayesian estimation of nonlinear dynamical systems.

In the discussion of long-range memory and long-range correlations, we failed to mention the work of Graf (1983) and Hampel (1987). These authors provide useful information for analyzing data with possible long-term memory. In the more applied domain, Wolff (1991) studies correlation integrals in the presence of autoregressive and moving average processes. In the biomedical area, Bassingthwaighte, King and Roger (1989), Van Beek, Roger and Bassingthwaighte (1989), and King, Weissman and Bassingthwaighte (1990) provide fractal descriptions of spatial statistics with regard to myocardial blood flow. In particular, these authors use fractal dimension as a description of heterogeneity in two-dimensional space, a measure of space fillingness. Fractal models of vascular networks are used to study the distribution of blood flow and to derive scale-independent measures of variance. Finally, to show the chaos-mania is even sweeping the popular press in the financial world, we cite the writings of Crowell and Peters (1991), Laing (1991) and Peters (1991). These essays discuss the implications of nonlinear systems on the Efficient Market Hypothesis and the Capital Asset Pricing Model as workable theories of portfolio management.

CONCLUSIONS

We believe that future years will witness a great deal of activity in researching the many issues raised in these discussions. It is clear that the theory of chaos is capable of significantly changing the way statisticians think about time series and dynamical systems in general. It is in this sense that the emerging theory is of potential importance, and ultimately, we can expect a richer set of modeling tools to be available to statisticians. We can only hope that these surveys and discussions will have made a contribution toward those goals. With no intention of hyping or overselling it, we conclude with an ode to chaos from Nietzche: "... One must still have chaos in oneself to be able to give birth to a dancing star. I say unto you: you still have chaos in yourselves."

[See "Rejoinder (part 2)" for additional references.]