Rejoinder (part 2)

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So far as the laws of mathematics refer to reality, they are not certain. And so far as they are certain, they do not refer to reality.

Albert Einstein (1921)

I am grateful to all the discussants for their interesting and thought-provoking comments. I am gratified that Professors Cutler, Geweke, Griffeath, Smith and Tsay generally agree with the propositions that the ideas assoicated with chaos should be of interest to statisticians, and that statisticians can make valuable contributions to this area. Cutler and Smith presented valuable expansions, particularly in the context of the estimation and interpretation of "dimension," on some topics for which my review is only cursory. I strongly agree with Professor Griffeath's view that the real issues discussed in various literatures and under various names really all hinge on "complexity." I also appreciate his discussion of random number generation and cellular automata. Indeed, since I agree with so much of what the discussants said, I will only comment on remaining points of contention or additional suggestions.

RESPONSE TO GEWEKE

I enjoyed Geweke's suggested Bayesian analysis of chaotic models, based on symbolic dynamic data, in the presence no traditional randomness. The analysis is exactly the sort of thing I believe statisticians can contribute to problems of chaotic data analysis. I wish to raise a point involving the specific computational algorithm he used, as outlined in the second paragraph of his Section 4. Specifically, for a fixed parameter value a, he suggested that the twentieth literates of many, equally spaced values may yield an approximation to the natural (Bowen–Ruelle) ergodic distribution for that a. This is not necessarily true. In doing a similar analysis, Steve MacEachern and I noticed that this is not true for a corresponding to an attracting periodic attractor. For finite attractor, the invariant distribution must assign equal probabilities on the attractor. However, uniformly spaced points are not attracted to the limit points uniformly. For example, I ran 5000 equally spaced points in the interval (0, 0.5) for 500 iterates each using the logistic map with $a = 3.4$. This a corresponds to a period 2 attractor, consisting of roughly 0.452 and 0.842. Only 44% of the 5000 points were at 0.842, whereas the remaining 56% were at 0.452. Simply stated, Geweke's original suggestion only guarantees, up to numerical complications, that we approximate the support of the desired ergodic distribution. Therefore, he is right in his concern about the validity of the approximation in the periodic case, although I suspect the problem diminishes as the number of periods increases. For a yielding a continuous ergodic distribution, statistical regularity suggests that Geweke's method would indeed yield a good, again up to numerical complications, approximation.

I think these points are crucial to our interpretation of ergodic probabilities. In the period 2 attractor case above, it is true that almost all (Lebesgue measure) points in the unit interval yield paths which, after a transience period, spend 50% of the time near each of the limit points. However, if the initial condition is randomly and uniformly generated from the unit interval, it is clearly not reasonable to claim that, for every very large N, x_N is equally likely to be near each of the limit points. Further, note that statistical regularity generally requires continuous ergodic distributions. In the case of a finite attractor, if the initial condition is generated according to a distribution that assigns all its mass to the limit points, but not with equal probabilities, the initial distibution never washes out.

RESPONSE TO GRIFFEATH

I have lingering doubts concerning the special role Griffeath appears to ascribe to "truly random" processes. He alludes to the powerful tools of probability theory, namely, the central limit theroem and the law of the iterated logarithm. I suggest that the ergodic theorem may well be included in such a list, and that the relationship between ergodic processes and stationary stochastic processes can be viewed as establishing a link between "truly random" and other complex processes.

Griffeath, in the role of purist, suggests that he debunks my probability statement given in Section 2. Perhaps my Bayesian tint is too strong to appreciate the impact of his argument. In particular, I readily model my uncertainty via probability statements. To clarify, consider the following two points.