



SP Forum

To the Editor:

It is hard for me to decide whether or not Mr. Gerr's letter in the Forum section of the October 1994 issue of this magazine deserves a response. He does not seem to address the basic issue of whether or not fraction-of-time probability is a useful concept. This is the issue being debated, isn't it? In fact, I cannot find one technical point in his letter that is both valid and clearly stated. But, because Mr. Gerr has clearly stated in his letter that, regarding philosophical issues in science and engineering, he prefers "New York" style vicious attacks like Hinich's to carefully worded slyly mocking replies, like mine, it has occurred to me that I might get through a little better to the Mr. Gerr's out there if I tried my hand at being just a little vicious. I hope the readers will understand that I am new at this; I give them my apologies now in case I fail to overcome my propensity for writing carefully and, when appropriate, slyly. But, before proceeding, I would like to set the record straight regarding what is and what is not slyly mocking. My reply to Hinich's review is nothing but plain fact without embellishment of any sort. It is my fictional article "Ensembles in Wonderland" which was obviously written to be humorous, that one could classify as slyly mocking.

Mr. Gerr's letter reveals a lot of misunderstanding and this provides us with some insight into what may motivate vicious attacks on attempts to educate people about alternative ways to conceptualize problem solving. It is hard for me to imagine how Mr. Gerr could have missed the main point of my response to Hinich's review. This point, which is clearly stated in both the book [1] under attack and the unappreciated

response to this attack, is that, and I quote from my response,

"There is really no basis for controversy. The only real issue is one of judgement—judgement in choosing for each particular time-series analysis problem the most appropriate of two alternative approaches."

To argue against this point is to be a zealot in the truest sense of the word, fanatically fighting for the One True Religion in statistics.

Sociologists and psychologists tell us that vicious behavior is often the result of paranoia born out of ignorance. In the example before us, both Hinich and Gerr demonstrate substantial ignorance regarding nonstochastic statistical concepts and methods, including fraction-of-time (FOT) probability. This case has already been made for Hinich in the Forum section of the April 1994 issue of this magazine. So let us consider Gerr's letter. First off, Gerr admits to the kind of behavior that is supposed to have no place in science and engineering, by identifying himself as a "partisan spectator." Webster's Ninth New Collegiate Dictionary defines partisan as "a firm adherent to a party, faction, or cause, or person, esp: one exhibiting blind, prejudiced, and unreasoning allegiance." On the basis of this admission alone one has to wonder whether to continue reading Gerr's letter or flip the page. (It's interesting that Gerr is into partisanship and Hinich's university appointment is in the Government Department.) But what the heck, let's see if we can find some technical content in his letter.

Mr. Gerr's first of three technical remarks is quoted here:

"For me, the statistical approach to signal analysis begins with a probabilistic model (e.g., ARMA) for the signal. The signal time series is viewed as a single realization and as data arising from the model. The time series data is used in conjunction with statistical techniques (e.g., maximum likelihood) to infer parameters, order, appropriateness, etc. of the model. The abstract notion of an infinite population plays no role."

Not too surprisingly, it is difficult to tell what point Mr. Gerr is trying to make here. He starts with a probabilistic model and ends with a denial of the notion of a population. Would Mr. Gerr care to tell us how he interprets "probability" in "probabilistic model" if he denies the notion of population? My guess is that his thinking does not go this deep. But let's try to extract some meaning by reading between the lines. In spite of his sympathy with Hinich, Mr. Gerr seems to be agreeing that the problem-solving machinery of probability theory (e.g., ARMA modeling and maximum likelihood estimation) can be used regardless of whether one conceptualizes its use in terms of stochastic probability (with its associated ensembles or populations) or in terms of fraction-of-time (FOT) probability. This is the point that is made by the book [1] under attack: This book does include ARMA models and the maximum likelihood method as parts of the nonstochastic theory. True to the "blind allegiance" definition of partisanship, Mr. Gerr is apparently agreeing with the book while sympathizing with the attack on the book. Either Mr. Gerr has not read the book at all, or he may

simply not have thought hard enough and long enough about these things. This is important to point out because I suspect it is the primary reason that there is any controversy at all.

Mr. Gerr then goes on to admit that the FOT approach may be required for chaotic time series. But again, true to form, he then makes a remark that is difficult to interpret:

"The fraction-of-time approach may be required, though not necessarily: in [1], it is shown that statistical model-fitting techniques developed for stochastic time series models can also be useful in fitting chaotic time series models."

This sounds like Mr. Gerr is again confused about the fact that many probabilistic models can be interpreted or conceptualized in terms of either stochastic probability or FOT probability. Thus, regardless of the fact that a model was originally derived in the stochastic probability framework, it can—depending on the particular model—still be used (and/or rederived) in the FOT framework. In fact, AR models were originally derived within the FOT framework, not the stochastic framework [2]–[3]. This will probably surprise Mr. Gerr. And if he is not confused about this, then he is again agreeing with the book [1] whose attack he supports.

On the assumption that people working with stochastic processes would have enough of an understanding of the subject to compare it with the nonstochastic theory presented in [1], this comparison was not made very explicit in [1]. Responses to [1], such as those of Messrs. Hinich and Gerr, suggest that this assumption is false more often than it is true. To make up for this, an explicit comparison and contrast between the theories of stochastic processes and nonstochastic time-series is made in Chapter 1 of [4].

Mr. Gerr concludes his letter by considering transient time-series and erroneously concluding that time averaging

a biperiodogram over successive blocks of data (which he identifies with FOT methodology) is inappropriate, whereas spectrally smoothing a biperiodogram is appropriate. Obviously, he does not realize that the infamous book [1] that proposes FOT concepts and methods shows that when the data block, over which spectral smoothing of the biperiodogram is performed, is partitioned into subblocks over which time averaging of the biperiodogram is performed instead, the results from these two methods can closely approximate each other if the subblock length and window shape are chosen properly. In other words, it is very clearly explained in [1] that the FOT framework for spectral analysis includes frequency smoothing as well as time-averaging methods. This again brings up the question, did Mr. Gerr read the book [1], and if so, did he comprehend anything?

It is my recommendation to Mr. Gerr, and others who would entertain joining this discussion of the merit of considering alternatives to stochastic thinking, that the book [1] that started the furor so nicely exemplified by Hinich's review, and Chapter 1 of [4], be read carefully, the way they were written. This should be a prerequisite to criticism, vicious or otherwise.

Before closing this letter, I should point out that the so-called controversy that statisticians like Hinich and Gerr are promoting is about as productive as the statisticians' endless debate between the "Bayesians" and the "frequentists" over whether or not prior probabilities ("prior" meaning "before data collection") should be included in the One True Religion of statistics [5]. The debate is endless, because it is based on the faulty premise that there is One True Religion. In fact, the subject of our "controversy" is not unrelated to the Bayesian/frequentist debate. This debate dates back to the 1920s, and involves many well-known statisticians, some 40 of whom are referenced in [5] for their contributions to this debate. The conclusion in [5], published just

last month, is, I am happy to report:

"The Bayesians have been right all along! And so have the frequentists! Both schools are correct (and better than the other) under specific (and complementary) circumstances ... Neither approach will uniformly dominate the other... knowing when to [use] one or the other remains a tricky question. It is nonetheless helpful to know that neither approach can be ignored."

This is very encouraging! These pragmatic statisticians are attempting to dispell belief in the concept of One True Religion.

I conclude this reply with a little dialogue that I find both amusing and supportive of my response to vicious attacks:

Can old dogs be taught new tricks?

Maybe, but the teacher might get barked at for trying.

Should the teacher accept the barking graciously?

Maybe, but if the old dogs band together into a pack, the teacher better bark back.

— William A. Gardner

References

1. *Statistical Spectral Analysis: A Nonprobabilistic Theory*. W. A. Gardner, Prentice-Hall, 1987.
2. "On a method of investigating periodicities in disturbed series, with special reference to Wolfer's sunspot numbers," G. U. Yule, *Phil. Trans. Royal Soc. London, A*, Vol. 226, pp. 267-298, 1927.
3. "On periodicity in series of related terms," G. Walker, *Proc. Royal Soc.*, Vol. 131, pp. 518-532, 1931.
4. *Cyclostationarity in Communications and Signal Processing*, edited by W. A. Gardner, IEEE Press, 1994.
5. "Toward a reconciliation of the Bayesian and frequentist approaches to point estimation," F.J. Samaniego and D.M. Reneau, *Journal of the American Statistical Association*, Vol. 89, pp. 947-957, 1994.