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RESPONSES TO QUESTIONS RELATING TO STATISTICAL THEORY AND BUSINESS AND ECONOMIC STATISTICS

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1. What do you consider to be the major role or roles of statistical theory in business and economic statistics.

[Gallant] In my opinion, the major role of statistical theory should be to allow ideas from economics, business, finance, etc. to be brought directly to bear in the analysis of data without distortion. For instance, if financial theory suggests that the evolution of a time series should be constrained by a stochastic Euler equation derived from a utility function, one should not be obligated to use a limited class of utility functions in order to make that constraint mesh with conventional time series methods. One should be able to ascribe arbitrary utility to agents and let statistical theory do the job of relating the model to data.

[Pratt] The role of statistics is to provide a foundation and tools for thinking usefully about the aspects of real-world problems associated with uncertainty, especially about inference from data. The role of statistical theory is to help in developing such a foundation and tools. I don't insist on a single foundation, but other than the Bayesian foundation there is none in existence or prospect that I find at all convincing and truly a foundation.

2. What do you consider to be some of the most fundamental principles of statistical theory.

[Gallant] Since I view the major role of theory as bringing subject matter considerations directly to bear on data, I judge principles by their value in achieving this objective. From this perspective, there are two great ideas in statistics: maximum likelihood estimation and associated inference principles; and, Bayesian estimation and associated inference principles.

Given a story derived from subject matter considerations about how data ought to behave, either can deliver estimates of model parameters and assess the validity of the story almost regardless of its complexity. Exceptions are usually due to inadequate computing resources or numerical instabilities rather than failure of the theory per se.

Method of moments, especially when couched in the form leading to two-stage least-squares, three-stage least-squares, and generalized method of moments, shares some of the features of maximum likelihood and Bayesian methods in that method of moments can deliver estimates and associated inference procedures in very complicated situations. However, method of moments is somewhat more art than rote and the general, unified theory (Gallant, 1987) is more recent and less well known than maximum likelihood and Bayesian methods.

[Pratt] One of the most fundamental principles of statistical theory is the likelihood principle - as a principle; applicability is another matter. I do not mean the likelihood principle based on a model inadequate for the situation, or applied unrestrictedly to a model adequate only for restricted purposes in the situation.

The practical reflection of another fundamental principle is that inference from part of the evidence is usually misleading, because of natural or unnatural selectivity. This makes most of what one sees cited as evidence in newspapers useless and enormously complicates legal and other adversarial processes. We haven't begun to face up to this as far as I can see.

3. How can principles of statistical theory be taught most effectively?

[Gallant] I think that the major error that we make is that we usually separate theory from applications in instruction. What I think should be done instead, and what I do myself to the extent that time and energy permit, is to begin a topic with an application, teach the theory that relates to the application, apply it, perform the computations, and make the relevant inference. The problem with this approach is that it is a lot of work. It is hard to find good applications that relate directly to the theory and, when found, class time has to be spent on the underlying subject matter considerations in order to breathe life into the application.

[Pratt] I wish I knew how they could be taught effectively at all to either ordinary people or those with an important influence on practical affairs. Sound thinking about uncertainty doesn't come naturally - even about indisputable probabilities, let alone about inference - as Tversky et al. have demonstrated abundantly. Foundational arguments, abstract concepts, toy problems don't help. Promoting statistics principally as holy oil to pour over data is unprincipled. Recipes aren't principles, even if they can be taught, made tasty, learned, or followed. Effectiveness that counts requires demonstrating a big bottom line. Yet a real demonstration would depend on an understanding of the very principles we want to teach.

Even though the Neyman-Pearson mirror has reflected a lot of light on statistical theory, we might at least stop trying to teach reading reversed images after 50 years of fruitless struggle, and give informal Bayesian inference another go. Bayes may not come naturally, but it comes a lot more naturally than Neyman-Pearson, as chronology suggests. This applies also to many questions below.

4. What are some major unsolved problems in statistical theory which are particularly important for the field of business and economic statistics?

[Gallant] It is becoming increasingly apparent that standard methods of times series analysis such as Box and Jenkins (1970) methods are inadequate for the analysis of macro economic and financial data. These data exhibit substantial nonlinearity that seem qualitatively like switching regimes, random coefficients, ARCH, etc. I expect that contributions in the area of nonlinear time series analysis, including nonparametric time series analysis, will have the most practical impact over the next five to ten years.

[Pratt] How ideas from economics, business, finance, etc. can be brought directly to bear in the analysis of data without distortion. Developing tractable, interpretable prior distributions in spaces of high or infinite dimension - such as responses of over 100 million U.S. adults to a governmental policy, or an unknown distribution on the real line. How to make valid inferences from insufficient statistics, as we must in practice. How to know when conventional or convenient priors are adequate. How to report inferences when they aren't.

5. Since probability theory is central in statistical theory, what concept of probability do you regard as most fruitful for use in business and economic statistics?

[Gallant] I personally find frequentist notions most useful in interpreting a likelihood and Jeffreys' (1961) notion of degree of reasonable belief most useful in interpreting a prior and a posterior. This entails mixing the two notions in a Bayesian application, which some regard as heresy.

[Pratt] Subjective. Especially but not only in business and economics. Why try to fool ourselves and others? This points up the importance of the problem of identifying as well as possible those subjective probabilities that would be reasonably widely shared to a good approximation if they and the evidence on which they are based were understood.

6. What are your views on the Bayes-non-Bayes controversy in statistical theory and how do they relate to applied statistical work?

[Gallant] By and large, I regard the controversy as a waste of time and pay but minimal attention to it myself. I think that one can extract useful information from data using either or both methods.

[Pratt] Non-Bayesian methods should not be used if they make no sense from a Bayesian point of view or are strongly contradicted by "reasonable" Bayesian methods in the situation at hand or in general. Distinctions among methods that are no greater than differences among "reasonable" priors are not worth worrying about in applied work, and the case for worrying about them in theory is not one of principle, though they may have tactical relevance, e.g. in arriving at "objective" (agreed on) methods.

Since even the most devout Bayesians rarely if ever use - except illustratively -formally assessed, truly subjective (personal) prior probabilities in major or minor inference or decision problems, it is silly to expect or ask others to.

7. Traditional hypothesis testing procedures, e.g. use of p-values have come under attack in recent works. What are your views on hypothesis testing.

[Gallant] I would like to be precise as to my frame of reference and attitude toward p-values and significance tests. My frame of reference is: Fisher's (1947) story about the lady tasting tea; three recent articles on the topic by Casella and Berger (1987), Berger and Sellke (1987), and Berger and Delampady (1987); a 1987 seminar at North Carolina State University by Dennis Lindley; and related conversations with Dennis Lindley. My attitude toward p-values is that they are a compact, efficient, and standardized procedure for recording the outcome of a test; better, for example, than writing statistics with asterisks. To me, a p-value is not some sort of proxy for the area under a posterior distribution or a measure of the weight of the evidence for or against some proposition.

By and large I find the theoretical arguments in support of p-values persuasive. One aspect of the literature cited above is that for the one-sided alternative the Bayesian and p-value approach are much the same so there is not much to argue about. The difference comes with the sharp null. On this I agree with Cox (1987) that much of the attack consists of: assuming the Bayesian view is correct, noticing a difference between the frequentist approach and the Bayesian approach, then claiming the the frequentist approach must be wrong. The outcome of reading the articles above and listening to Dennis Lindley is to make me extremely leery of Bayesian results acquired by putting positive prior mass on a sharp null.

The problem with p-values, as I see it, is in practice. The model is never exactly correct in applications, there is always specification error. Usually specification error biases p-values downward. If the specification is held fixed and sample size is increased, the problem worsens. What partially mitigates against this effect is the fact that practitioners complicate the model as sample size increases; for instance, Roger Koenker (1988) searched the literature and found that labor economists add parameters to wage equations at about the rate of the fourth root of the sample size. Unfortunately, the mitigating effect of model complication eventually breaks down because present-day computing equipment and algorithms accommodate additional observations far more gracefully than additional parameters.

[Pratt] My views haven't changed noticeably from those I expressed in JRSSB 1965. One-tailed p-values interpretable as approximate posterior probabilities are useful. For the rest, the real problem is usually so far from the nominal problem solved by hypothesis testing that we might do better at this stage of understanding of statistical inference to discard other kinds of hypothesis tests and avow our ad hockery. Yours and mine, anyway - Dr. Never-mind's we may want to disavow. What could be the meaning of all those tests of sharp null hypotheses known to be false - or their p-values - even were there no Edwards-Lindman-Savage (Berger-Sellke) problem?

8. Some regard "data analysis" to be an art. How can statistical theory contribute to making data analysis more effective.

[Gallant] I think that the objective of data analysis is too unstructured for it to be much other than an art. I take the definition of data analysis to be the act of puzzling over a set of data without much guidance from some substantive body of knowledge in the hopes of deducing interesting facts and structure. All the better if they come as a surprise. With this definition, I don't see how the field can be anything but a collection of imaginative tools that have been useful in similar situations. I would expect that if an objective were set forth that was sufficiently narrow for theory to progress and if it were accepted as defining data analysis, the practical value of field would be substantially lessened.

It is probably quite useful to sit back and take an unstructured, uninhibited look at data from time to time using a collection of tools acquired haphazardly. However, I would not think that one would try to substitute this activity for structured inquiry guided by knowledge of the relevant subject matter.

[Pratt] By distinguishing artistry, ad hockery, and sophistry. By remembering that the whole purpose of statistical theory is to make data analysis more effective. About "data analysis" nothing smart or "smart" to say comes to mind.

9. What are your views on the theory and use of "boot-strap" techniques in statistics?

[Gallant] The bootstrap seems to me to be just a mechanization of procedures that statisticians have always used. It has always been routine to fit a model and then examine residuals and various other statistics and diagnostics in order to find a parametric model that accurately describes the data generating mechanism. Having found it, it is accepted as the model, and the sampling distribution of estimators and tests are worked out. What the bootstrap should offer is protection against serious blunders in identifying the data generating mechanism. This protection is only partial. Some traditional methods of model identification are usually used in conjunction with it and the theory of the bootstrap, the last time that I looked, was not adequate to guarantee that bootstrap sampling distributions would lead to correct inferences in many relevant circumstances.

The bootstrap has not had much impact on applied economic research. My guess as to why is that it is a one-off proposition so that one cannot learn much about the data generating mechanism from it. With the traditional approach, an area of inquiry can build up a sequence of parametric models, evolving over time, that compactly and succinctly summarize past experience.

[Pratt] When "bootstrap" techniques are applicable to the problems I see and available in standard computer packages, maybe they will be a help. They won't be a panacea. They won't eliminate the need for or difficulty of hard thought about opaque assumptions, and indeed appear to require some of it themselves in the kinds of complex situations where one most needs help. They won't eliminate the need for prior distributions or equivalent judgments about compromises among extreme models - to pool or not to pool and such questions. They won't solve all problems of too many parameters. But I don't mean to undervalue them just because they have been oversold.

10. How can statistical theory be used to improve the art of statistical graphics?

[Gallant] Most of my views on statistical graphics, for better or worse, have been formed by reading Tufte's (1983) first book on the subject. It seems to me to be just applied common sense with some imagination and ingenuity tossed in. My views on this topic are the same as with respect to data analysis above: an attempt to mathematize graphics in order to attack it with statistical theory would probably be counter productive.

There are issues in statistical graphics to which these comments do not refer. I classify the topic of efficient generation of graphics as within the domain of computer science and the perception of graphics as within the domain of psychology.

[Pratt] By helping identify what aspects of data we should seek to present - the impacts of statistical variability and (non)robustness, for example. What is comparable across subsets of the data. Which kinds of time-series plots bring out real effects and which produce mirages.

11. Is formal decision theory theoretically sound or does it need improvement?

[Gallant] Decision theory strikes me as a reasonable story for describing how an individual ought to make decisions in the face of uncertainty. It leads to useful models of individual and aggregate behavior in the social and biological sciences.

As it relates to Bayesian statistics, it seems to be a sensible way to examine the posterior distribution using the loss function to structure queries. Personally, I much prefer a graphical examination of the posterior distribution of the object of interest including a graphical comparison with its prior distribution. For instance, in a demand study I would rather over-plot the prior and posterior density of an elasticity instead of recording the optimal point estimate corresponding to some loss function.

As it relates to finding optimal statistical procedures from a frequentist's viewpoint, it often leads to difficult mathematics. This, I think, is a serious problem: it is too time consuming. By the time an optimal procedure can be found to address a new substantive issue, the discipline has moved on to other issues. This confines much such activity to statistical problems that once were interesting or are routine.

[Pratt] Formal decision theory is absolutely sound theoretically, and a great help to logical thinking. Proposed alternative theories of normative, rational, logical, and/or coherent thinking, inference, and/or decision-making are on nothing like the same footing in either respect. It is reasonable that those really interested in them should continue to work on them, but others should not be bothered, in any sense. I am speaking from the point of view of theory, logic, scientific inference, philosophy, not empirical psychology. Descriptive decision theory is another ballgame in another field altogether.

In practice we need informal frameworks too, recognizing the cost of thinking and computing. But see 6.

12. Some recommend the use of randomization in experimental design while others recommend that it not be used. What are your views on this issue?

[Gallant] I have always thought since my first days as a graduate student that randomization is one of the few bright spots in statistics. I still think that Kempthorne's (1951) book on the design of experiments is one of the best books that I have read. The approach seems so sensible. Randomization guards against modeling errors, unknown or unforeseen factors, etc. But what is equally important, it induces the sampling distribution of statistics and allows the assessment of significance using the randomization test. Results are clean, exact in small samples, and about as uncontaminated by unverifiable assumptions as it is possible to get in statistics.

[Pratt] In the contexts where the question would normally arise, randomized assignment of treatments makes an enormous difference to the credibility of the results, even if the full explanation hasn't been spelled out. It is true that the hypothesis test directly justified by randomized assignment of treatments is often not really an appropriate form of inference (see 7) and usually not the only or most important type of inference we wish to make, while "units" are rarely sampled from the "population" of real interest, even if "units" and a "population" can be defined. I believe the latter applies even in medicine and agriculture when treatments have actually been randomly assigned to a fixed set of units, and applies in spades in economics when the "units" are merely observational occasions and neither kind of randomization has taken place.

13. Some adhere to the likelihood principle in making inferences while others do not. Is this issue an important one and what are your views regarding it?

[Gallant] Berger and Wolpert (1984) come reasonably close to summarizing my views and behavior:

> First, the consequences of the LP seem so absurd to many classical statisticians that they feel it a waste of time to even study the issue. Second, a cursory investigation of the LP reveals certain oft-stated objections, foremost of which is the apparent dependence of the principle on assuming exact knowledge of the (parametric) model of the experiment (so that an exact likelihood function exists).

[Pratt] The likelihood principle should not be a hot issue in practice, but failure to understand it or agree to its implications can lead to misplaced effort and misunderstanding in both theory and practice. At the same time, even worse consequences can result from combining a correct likelihood principle with an incorrect model, or adhering to a misunderstood version of the likelihood principle. The likelihood principle says correctly that optional stopping doesn't matter. But a frequentist approach that disobeys the likelihood principle can identify problems that are not so easy to understand in other ways. For example, selection effects are really nonrobustness of flat priors in many dimensions, but not as easily identified Bayesianly as frequentistically. I wish more people today understood this even half as well as Mosteller and Wallace did in 1964. It should be emphasized that the appropriateness and force of the likelihood principle are not limited to parametric models, but there is much valuable work on robustness that would be very hard to bring into line with the likelihood principle.

14. Non-parametric approaches are often suggested as being superior to parametric approaches in analyses of applied problems. What does statistical theory have to say about this issue?

[Gallant] Nothing. Depending on the assumptions regarding the data generating mechanism and the choice of an optimality criterion, one or the other approach is superior.

To illustrate, suppose one takes the frequentist view, has a regression situation with one independent variable, normally distributed errors, etc. If one assumes that the response function is a linear function of the independent variable then least squares estimates provide the best estimate of the value of the unknown function at some point within the range of the observed independent variables. If one is only willing to assume that the unknown function is three times differentiable then non-parametric approaches avoid catastrophically biased estimates. I don't see that statistical theory can have much to say about the reasonableness of assumptions on an *a priori* basis.

Addressing the question from a practical point of view, I think that both parametric and nonparametric approaches have their place.

Simple parametric models are fine if they are correct or at least an adequate approximation to the true data generating mechanism. The problem is to know when they are and are not an adequate approximation. Diagnostic testing helps. The trouble with diagnostic testing is that one must envisage every possible alternative story as to what could have generated the data and then devise some statistical method to detect that alternative. Even were this possible, one runs out of patience, time, and energy long before anything like an adequate battery of tests is exhausted.

Nonparametrics seems to me to be a very efficient way to get an idea as to what generates the data when one does not have enough confidence in a simple, parametric model to use it with but minimal diagnostic testing. Also, nonparametric density and regression methods force the use of graphical methods which I think is quite useful and helpful in these circumstances.

As an illustration, there is evidence acquired from non-parametric estimates that time series from financial markets have an innovation density with side lobes (Gallant and Tauchen, 1988; Gallant, Hsieh, and Tauchen, 1988; Pagan and Hong, 1988). This feature, when severe enough, seems to cause parametric fits to exhibit disturbing anomalies (Gallant, Hsieh, and Tauchen, 1988). Yet I have never seen a parametric time series analysis that did not assume, *a priori*, that these side lobes were not present and stick with that assumption from start to finish.

[Pratt] Nonparametric thinking has been a great help, and some data come in a form requiring methods based on ranks or comparisons, but most of the problems really solved by nonparametric methods unfortunately either are tests of hypotheses (see 7) or in fact require very strong assumptions such as strict shift (translation) and homoscedasticity. The important problem is robustness in complex situations, such as multiple regression, and neither nonparametric nor robust theory has really handled or even closely approached the real problems here. In practice, it is easier to visualize how robust methods might be developed to do the job, and they may be doing better than I realize, though one could wish that they were less ad hoc, complex to understand, manipulable, potentially faddish, etc.

15. Do you find a need for a concept of causality in statistics? If so, which concept would you recommend?

[Gallant] I agree with Pratt and Schlaifer (1988) and see no practical reason to debate or worry about the true meaning of the word cause or its derivatives. My views are probably in line with the majority of statisticians in that it is my opinion that without the ability to experiment, statistics cannot be used to unequivocally establish a relationship among variables under acceptably general, explicitly stated conditions. In the analysis of observational data, additional assumptions and subject matter considerations must be brought into the analysis before statistics can be of much help and conclusions are always, and must always be, tenuous. A relation among variables cannot be definitively established from observational data. There are many circumstances in science, government, and enterprise where observational data is all that is to be had and one must do the best with it that one can. I do believe that there are rules for proper and sensible behavior in these circumstances. Zellner's (1988) normative prescriptions are a reasonable approximation to what I think proper behavior is.

[Pratt] I see a great need for the concept of causality that experiments with randomized treatment assignment are aimed at. Other concepts of causality are confusing matters in practice and should be relegated to back if not smoke-filled rooms along with non-Bayesian theories of normative decision-making. Even in deterministic contexts and in casual speech, I conjecture that where causality means anything, it implies visualization of at least two alternative treatments or analogues thereof and at least implicit assumptions about what else would have stayed fixed and, where relevant, what would have been allowed to vary as it "naturally" does. (All else fixed is logically impossible.) There are indeed deterministic laws, and "stochastic laws," which must hold under at least some somewhat statable circumstances if they are to be regarded as demonstrated and meaningful. Physics is mostly about deterministic laws, and doesn't need causality. A set of one or more deterministic laws does not determine causality without assumptions specifying the variables to be controlled and the variables staying fixed, enough of each so that the remaining variables are determined by the laws.

16. Some assert that simple methods and models will probably work better than complicated methods and models in analyzing statistical problems. What are your thoughts on this issue?

[Gallant] Almost every one accepts the idea that if two models agree equally well with past data and predict as well then the simpler is to be preferred if they can agree what simpler means. Some would take it to mean fewer parameters, lower degree of a differential equation, fewer lags, linear in the parameters or some weighted combination of these factors. Others might think that simpler means that the substantive notion that underlies the model is simpler even though the model derived from it is quite complex.

When simplicity is used as an excuse for compromise, I take exception. As I said above, the major role of statistical theory should be to allow substantive considerations to be brought directly to bear in a statistical analysis without distortion. Simplicity is a common excuse for not doing so. To this, I object.

[Pratt] Simple models may be easier to understand than complex ones, and may forecast better in some circumstances, especially when regimes are not changing. But the regime may be changing at the level of the simple model and unchanging only at a deeper level requiring a more complex model. And simple models are almost never adequate to analyze causality in the sense I see as useful except when treatments have been randomly assigned. Part of the problem is that, Bayesianly speaking, flat priors may be more adequate in analyzing simple models than complex ones, so we are usually comparing a fairly adequate analysis of a simple model with a very inadequate analysis of a complex model. A similar statement applies to non-Bayesian analyses with the additional difficulty that it is much less clear, at least theoretically, what an adequate analysis of the complex model would be. Practically, the complex model may need an analysis cooked with both Bayesian and non-Bayesian ingredients. And there is the ever-present reality that in business practice almost all analyses are and will be limited to what statistical computing packages provide.

Small standard errors of parameters or estimated forecast standard deviations prove nothing. Consider, for example, extrapolating a one-variable regression. If you assume linearity, all statistical measures will look better - as will parsimony - than if you allow a (statistically insignificant - what a misnomer!) quadratic term, but you will just be fooling yourself. Even if you are engaging in the most passive of forecasting. In studying causality, it is far worse.

17. How can statistical theory be used to improve the quality of data, say survey data, census population counts, etc.?

[Gallant] I have not studied government data collection in any detail and so cannot offer any specific suggestions. One can offer the general suggestion that developments in sampling techniques, imputation, time series analysis, etc. no doubt could be usefully incorporated into the process as these developments occur. One also could suggest that more attention be paid to the appropriateness of the statistical treatment in relation to the purposes of the end users of compiled series. For instance, many economic series can be properly regarded as measurements on a controlled process. Such series are routinely seasonally adjusted by the collecting agencies which is clearly ill-advised for most end uses, especially policy formation (Ghysels, 1988)

[Pratt] I haven't the perspective or experience that leads me to a useful generalization here. Census population counts seem to me a very special case and far too complicated to discuss. I do have a sense that if we want to know what is really out there, we should do a lot more to calibrate what we measure and to correct our estimates - by regression methods or the like, perhaps. The fact that we are often more interested in changes than levels complicates the issue, to be sure.

18. If you had to recommend one or two works dealing with statistical theory and its implications for applied work, which would you recommend? Why?

[Gallant] I would recommend: David R. Cox and David V. Hinkley (1974), *Theoretical Statistics*, because it contains a reasonably complete and balanced treatment of the competing points of view and at the conclusion of a thorough reading one would have a good working knowledge of statistical theory.

[Pratt] I don't know what I could recommend in the spirit of the question. Since 1961 I have taught statistics only sporadically and I haven't kept up with the appropriate books. Still, I am not optimistic that there is or can be, for example, a book about statistics for any but exceptional applied workers that discusses the concepts of statistics and their limitations truly honestly and not superficially or uncritically. I have had great trouble suggesting anything that would tell mathematically minded colleagues what statistics is all about really. I still come back to Savage, Fisher, and, hesitantly, Neyman. Jimmie Savage's Foundations of Statistics is terrific if you don't get hung up on the mathematics. I have had occasion to look at it repeatedly and recently. It holds up wonderfully and hasn't been significantly superseded. R. A. Fisher's books are fascinating if you don't get hung up on the computational methods, though they are not for the unwary or naive. (Some might say that about Jimmie's too, although I wouldn't.) I don't know how to get from those two to some idea of where the field of statistics is now, conceptually. (I do think that says something about where we are now.) So I suggest giving Neyman's First Course a try. Maybe it would provide the missing balance, though it doesn't aspire to the depth or breadth of the other two - in fact, it is more in the spirit of the question asked than of the question I am answering. Unfortunately for the purposes I have in mind, I think it provides just too little too late, although I haven't looked at it recently.

19. Currently, what do you view as the most significant trends and developments in statistical theory and how might they affect practice in business and economic statistics?

[Gallant] My guess is that developments in nonparametric regression, nonlinear time series analysis, and deterministic chaos will have the greatest impact in the near future. My reasons are as follows.

A nonparametric analysis takes formally into account the fact that a model is an approximation and is, therefore, in my opinion, a more honest statistical analysis than than an analysis that conditions probability statements on an assumed parametric model. I expect that a larger fraction of applied work will at least partially adopt a nonparametric approach for this reason.

Much recent economic theory and applied work suggests that nonlinear time series are the rule rather than the exception with economic data. I expect that statistical methods that accommodate nonlinearity will be rapidly adopted in applied work.

Deterministic chaos is a fascinating and plausible alternative view as to the origin of stochastic phenomenon. I expect that statistical methods that can admit of the possibility of deterministic chaos as the data generating mechanism, without depending on model identification (which is hopeless) for their validity, will become very important in the future. It would not at all surprise me if scientific opinion coalesces around this view of stochastic phenomena and sweeps the Bayes/frequentist controversy into the dustbin in the process.

[Pratt] What I see in statistical theory is such a forest of developments of which the most significant are so removed from practice in business and economic statistics that I would feel silly speculating on their effects or trends. Maybe it is just me, maybe it is the state of the field, maybe it is normal. I can't even meaningfully trace the effects today of the most significant developments of the 40s and 50s, let alone see a similar distance into the future.

20. Overall, how do you view current practice in business and economic statistics with respect to the use or misuse of principles of statistical theory?

[Gallant] I cannot, offhand, recall any flagrant misuses of statistical principles that I have encountered within, say, the last year in seminar presentations, papers I've read, or papers that have been submitted to the *Journal of Business and Economic Statistics*. Nor have there been minor occurrences that are so repetitive that I can bring them to mind. Most of what I see that is wrong, uninteresting, or noninformative owes more to an unfamiliarity with the economic and business literature or a failure to get principles from economics, finance, etc., to mesh properly with statistical principles rather than a misuse of statistical principles per se.

[Pratt] A few statistical groups in a few places are doing fine. Maybe some individuals you (or I) never heard of are doing fine. For instance, I have been surprised once or twice by the soundness of company statisticians in industries with special requirements, such as FDA approval. But mostly we are just nowhere. There's lots of data. Businesses are drowning in accounting and marketing data. But the principles of statistics are not to be seen at a managerial level. The reason is not that they would be misused if they were, though perhaps they would. They just aren't part of managers' thinking.

Harvard Business School, a pretty major player, teaches almost nothing you or I would call statistics to MBAs. It would be very difficult to teach a statistics course they would tolerate in the current local and national environment, and even more difficult to teach one they would find relevant enough to internalize. Elsewhere it may look different - certainly it is in some ways - but if statistics teaching is having a real effect, why is there no feedback or even sign of it in the reactions of employers to Harvard MBAs? The annual conferences on Teaching Statistics in Business Schools are addressing the problem, but maybe attitudes will have to change as far back as elementary school. Unfortunately teaching statistics even in high school has its problems, such as who will do it, and should we teach running before walking?

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