

REMEMBERING LEO BREIMAN

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I published an interview of Leo Breiman in *Statistical Science* [Olshen (2001)], and also the solution to a problem concerning almost sure convergence of binary tree-structured estimators in regression [Olshen (2007)]. The former summarized much of my thinking about Leo up to five years before his death. I discussed the latter with Leo and dedicated that paper to his memory. Therefore, this note is on other topics. In preparing it I am reminded how much I miss this man of so many talents and interests. I miss him not because I always agreed with him, but instead because his comments about statistics in particular and life in general always elicited my substantial reflection.

Technical comments here are in part my responses to Leo's 2001 paper in *Statistical Science* [Breiman (2001)]. The paper is interesting and provocative, but it demonstrates an attitude that seemed somewhat unfortunate in 2001 when it was published and remained so in 2005 when Leo died. It is even less fortunate today. D. R. Cox may have stated the obvious when he noted in his discussion [Breiman (2001), page 216] that, "Like all good caricatures, it contains enough truth and exposes enough weaknesses to be thought-provoking."

In his discussion of the paper, Bradley Efron states (page 219) that, "Prediction is certainly an interesting subject. Leo's paper overstates both its role and our profession's lack of interest in it. . . the whole point of science is to open up black boxes, understand their insides, and build better boxes for the purposes of mankind. . . we can hope that the present paper was written more as an advocacy device than as the confessions of a born-again black boxist."

For years I have preferred Cox's approach [Breiman (2001), page 216]. "Professor Breiman takes data as his starting point. I would prefer to start with an issue, a question or a scientific hypothesis." Also, I believe strongly that crisp mathematical formulations of statistical problems can clarify rather than obscure them; likely, if pressed Leo would have agreed. The paper and Bruce Hoadley's discussion of it focus on the importance of predictors. A predictor might be "important" if it predicts whatever outcome is in question accurately by itself. Alternatively, it might be called "important" if the performance of other predictors is harmed by its absence. More generally, a variable might be deemed important if it is approximately mutually predictable with a set of predictors, and the entire set is important or not by either criterion. These notions permit easy expression in mathematical

terms, though space precludes precise statements here. Both the paper and much discussion of it are about selecting predictive features, in particular about tracking the behavior of features as time, age, or some other dimension varies. This may amount to choosing a parsimonious set of basis functions for a linear space of functions that describes the sample paths of that feature. Coefficients of the feature in the carefully selected basis then become features themselves in whatever classification or prediction is required. See, for example, Sutherland et al. (1988), Chapter 10.

Suppose that an “outcome” y might be predicted from input x , and that the mechanism by which the outcome is determined involves not only the input x , but also noise. The conditional distribution of y given x , $f_{\theta}(y|x)$ might depend also on unknown parameters; denote them by theta (θ). There are, then, three obvious sources of randomness. Leo argues—I think correctly—that the principal issue facing the scientist who might make inferences from data is to predict some future y^* drawn from the distribution described by f , but not necessarily to make statements about f itself. Leo scoffed, probably unfairly, at the substantial energy spent by members of the statistical community quantifying information about θ available from data. He was slightly unfair when he spoke derisively (page 204) of “Bayesian methods combined with Markov chain Monte Carlo.” The strict, frequentist approach to inference has as sources of randomness the noise, and x itself. Only in “random effects” models, though conventional for a Bayesian, does the distinction become blurred; see Hill (1965). The frequentist does not impute randomness to θ ; for that person it is a leap to impute randomness of any origin, subjective or frequentistic, to it. I am surprised at the pejorative lumping together of the different approaches to inference about (f, x, θ) , ignoring, as it were, upon what inference is conditioned. More or less, frequentistic inference is conditioned on θ and Bayesian on x . Though Leo’s paper was published nearly 10 years ago, he speaks (Section 11.3, page 213) of microarray data, which hardly permit analyses without at least an implicit Bayesian/random effects model for the distribution of p -values in rows of a large rectangular array. For careful discussion of how Bayesian formulations, empirical Bayes solutions, and predictions regarding future data combine to yield much of interest regarding expression arrays and brain imaging, to cite two areas of application, see Efron’s papers on local false discovery rates (*locfdr*), available through <http://stat.stanford.edu/~ckirby/brad/papers/>.

Section 11.1 and following of Leo’s obviously provocative 2001 paper is about survival analysis. His acknowledgments (page 215) make explicit mention of his and my discussion of the celebrated Cox model in particular and of biostatistics in general. In this arena I believe that Leo’s critique is right on. For starters, survival itself, rather than the more difficult concept of hazard, especially relative hazard, is of paramount interest. Predicting survival for the next patient matters far more than does testing any hypothesis about past patients. Leo’s various complaints about the practices and orientation of some leading statistical journals would be difficult to report diplomatically when it comes to testing the parameters of a particular

model for survival versus predicting survival for the next patient. His were not merely the ruminations of a senior professor at a famous university who may have had a paper rejected. "Testing" parameters of a model is fraught with difficulties that owe in part to sample size, even when the model is correct and censoring is not an issue. This critique is applicable no matter the philosophical underpinnings of scientific inference. Difficulties include but are not limited to the celebrated "Lindley Paradox." See, for example, Lindley (1957) and Lehmann (1958).

I was fortunate to be able to discuss with Leo work by Piette, Nazari and Olshen (1998). It ran afoul of the editor of a major statistical journal and, perhaps unwisely, was never revised for submission elsewhere. What separates this paper from many others in survival analysis is its prediction of readmission for future substance abuse patients rather than inference about parameters of models that describe past patients. It uses a parametric bootstrap applied for the most part to Weibull models that are shown to fit the data. The bootstrap was adapted to prediction in our context. To be precise, the paper is a case study of readmission patterns following 42,648 discharges of patients treated for substance abuse in U.S. Department of Veterans Affairs hospitals for a year straddling 1990–1991. We learned that substance abuse inpatients are at extremely high risk of readmission, particularly if they are more than 65 years of age and have chronic medical problems. Risk of readmission is highest immediately following discharge and declines subsequently. We learned that intrinsic difficulties in predicting readmission rather than limitations of our model, account for its varying accuracy.

While Leo Breiman was certainly an important statistician, probabalist, and colleague, he was also a good friend. Leo's notion of compromise was clear enough to all who knew him, but it was not the modal approach. Think of a couple, one of whom wishes to live in New York, while the other wants Los Angeles. Though living in St. Louis might be a compromise of sorts, it would please neither party. So it was with Leo: your way or my way, but necessarily one of the two. If it's not always your way or always mine, it might be said that the party is willing to compromise. That Leo was willing to "compromise" was illustrated in bringing our four-author book [Breiman et al. (1984)] to completion.

Leo and Chuck Stone had neither spoken for awhile nor did either have an algorithm whereby the book could be completed, no matter that each author in his own way had spent much effort furthering CART. One day Leo and Jerry Friedman were having lunch in a restaurant on Hearst in Berkeley. Per chance, Chuck Stone, my wife Susan Olshen, and I came to the same restaurant. To ignore one another would have been to be publicly rude, something to which Leo was quite allergic. On Leo's urging, the four coauthors agreed to meet after lunch in his office. Susan said that she wouldn't attend, instead would go to a library to read a book. I said, "Nothing doing. You come to the meeting." As the meeting began, Leo took a chair in back of his desk, with Jerry seated to one side. Chuck and I were to be seated facing Leo from the far side of the desk. Deliberately, I took a chair with an elevated seat and placed it adjacent to the desk, between Leo and Jerry on

one side and Chuck and me on the other. I asked Susan to sit in the carefully situated chair. I guessed that the ever gallant Leo would be as accommodating in that scenario as in any other, and I hoped that he and Chuck could agree on whatever needed agreeing. Surely Jerry and I would go along with anything to which Leo and Chuck agreed. *Mirabile dictu!* Leo and Chuck came quickly to at least superficially amicable agreement, in the Breiman style. All of us did what we decided to do, and the book was born. The rest was downhill.

Even when dealing CART itself, occasionally Leo may not have been sufficiently generous to prior contributions by others. He stated [Breiman (2001), page 207] that, “While trees rate an A+ on interpretability, they are good, but not great, predictors. Give them, say, a B on prediction.” Of course, “boosting” and other technologies by which to enhance trees, many discussed by Leo in his paper, were well known before 2001. The very early reference by Morgan and Sonquist (1963) to CART-like algorithms advertised them as, “automatic interaction detectors.” See the discussion on pages 181 and 216 of Breiman et al. (1984). In work not reported here, others and I have found that if splits on successive nodes of a binary decision tree are taken to suggest two-factor interactions, while splits on individual nodes have suggested main effects, then plugging the entire list of candidates into “the lasso” [Tibshirani (1996)] and plugging output into most any reasonable classifier can lead to accurate prediction. Admittedly, validating the entire process is difficult.

In closing this remembrance I am reminded of a story told me by a former Berkeley colleague, now sadly also deceased. It concerned appointments of Leo and Chuck Stone to the UC Berkeley faculty. He said to the dean who was considering the files. “Look. You know me. I’m against almost everything. But I think that these would be great appointments.” Fortunately, as it turned out, Berkeley got this decision right and appointed two remarkable individuals to its esteemed faculty. One of the two, Leo, has been gone now for five years. It understates the case to say that I miss him very much!

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