The History of Intuitive Statistics according to Lee R. Beach

The notion of Man as an Intuitive Statistician had its roots in the 1940’s and 1950’s. The idea was that human learning resembles the process that scientists go through when learning about some aspect of the physical world, so in some sense every human is an intuitive scientist. This was a hot topic when Cam Peterson and I got our Ph.D. degrees (1961) in experimental psychology at the University of Colorado. We both studied under Ken Hammond, who was a disciple of Egon Brunswik, who was a major proponent of the man as an intuitive scientist idea. In fact, ‘intuitive statistician’ is Brunswik’s term.

In 1964, Cam and I independently decided to do post-doctoral work at Michigan with Ward Edwards, who with Larry Phillips was investigating the use of Bayesian statistics to design man-machine systems for helping people integrate information in order to make decisions. Ward had written a very good introduction to Bayesian ideas, which was what brought me to work with him. Cam and I each did research somewhat related to Ward and Larry’s work and then wrote our Man as an Intuitive Statistician paper, melding Ward’s thinking with Brunswik’s. The basic idea was that humans live in a variable and unpredictable world, that they had developed statistics to deal with it, and that statistics therefore must resemble in some way their subjective way of dealing with the uncertainty. We felt somewhat like anthropologists, only we were using the products of the mind to make inferences about the mind. The general strategy was to use deviations from prescriptive (normative) theory to adjust the theory and make it descriptive. [The idea was explored in greater detail in Barclay, Beach & Braithwaite (1971), Normative models in the study of cognition. Organizational Behavior and Human Performance, 1971, 6, 389-413, which to my knowledge was read by nobody.]

While Cam, Larry, and I were at Michigan, Amos Tversky also was there, getting his Ph.D. (with Clyde Coombs) and, I think, Ward was on his committee. Soon, Danny Kahneman arrived and he and Amos began to collaborate (apparently they had known each other in Israel). We all attended seminars at Clyde’s home on ‘mathematical psychology,’ and Amos and Larry were the stars; they could talk math-talk so very well. A few years later, Tversky and Kahneman wrote prospect theory which was exactly what the aforementioned research strategy called for; a descriptive theory based on altering the prescriptive theory in light of experimental data.

But then things began to go wrong. The bad seed had been planted earlier by Ward and Larry’s Bayesian research, which found that people did not revise their probability judgments as much as Bayes Theorem prescribed—i.e., that data had less impact on people’s uncertainty than on Bayes’s—which was labeled ‘conservatism’. Unfortunately, there is nothing psychologists like more than showing that people are flawed (You can’t write a book, publish a paper, teach a class, or get paid to consult if all you have to say is that people are really good at something) and the idea of flawed judgment caught fire. Then Amos and Danny came up with the idea that the flaws (biases) are because of shortcuts (heuristics)—which never made much sense to me; can you have a short-cut if you don’t have already know about the long-way-around against which to compare it?

The market place for ideas isn’t much different from the market place for anything else. You’ve got to work hard, advertise, and push the product. Academics who succeed big are good at all this; they eat a lot of overcooked chicken at conferences and put up with a lot of boring small talk before giving talks at any gathering that asks them. But the payoff is big, sometimes a Pulitzer Prize. (Let me say here that I like Danny very much and he deserves all the credit and fame he has received; he earned it through a lot of hard work. But, I think the product he’s selling is not very helpful for psychology or cognitive science—why I think that will become clear in a moment.) And it was no different for heuristics and biases and the notion that humans are cognitive cripples. Its proponents were good salesmen and its audience was enthusiastic—both inside psychology and, maybe even more so, in other disciplines and the popular press.

In the meantime, many of us continued doing our research both in the laboratory (which was largely comparing performance with normative models) and in the ‘real world.’ And it wasn’t going well. Cam and I eventually (and independently) came to the conclusion that the notion of subjective probability (and thus intuitive statistics) is pretty useless. Cam dropped probability from decision analysis (he owned a decision consulting business) and I dropped it from my decision aids (I was doing research on birth planning, decisions about surgery, and a whole lot of other stuff). We both found that when it came to non-laboratory, real decisions, people didn’t know what we were talking about when we asked about probabilities. Things worked better without any reference to probability—if we had to, we talked about degree of belief instead and a simple scale (e.g., 1-5) worked just fine.

In 1987 or 88, a conference was held in the Netherlands about the viability of expected utility (EU), and its variant subjective expected utility (SEU), as a model of decision making. The gist of the conclusion was that it isn’t a good model because it requires what is called ‘the gamble analogy.’ That is, the decision maker is seen as a gambler making a risky decision, in which case it can be showed that maximization of expected value is the optimal strategy—but lots of research showed that people don’t maximize EU or SEU. Turns out that this is because, from the decision maker’s viewpoint, most decisions, even risky ones, aren’t really gambles. In gambling you place your bet and wait passively to see if you won or lost. In real life, you chose an option and work hard to make it work. This would be cheating in gambling but it makes perfect sense in real decisions.

Working hard to ensure success makes probability irrelevant. Work is based on causality—you do what you think will cause the future to become what you want it to be (all decisions are about the future). Probability, in the sense used in probability theory—especially relative frequency—is utterly beside the point. All the research we’d done on subjective probability was largely a waste of time. People are not intuitive statisticians, they are actors in a world in which they have acquired a set of cause and effect rules that they can adapt to the circumstances to ensure that the future meets their needs. In short, human cognition is about causality not probability.

Danny Kahneman acknowledges the role of causality in cognition, but he seems to regard it as a flaw. But, evolutionarily, causal thinking is precisely the right fit for the world we inhabited. And, when the world got so complex that uncertainty could be overwhelming, humans invented probability theory as a **tool** to help them extend their cognitive capacity to deal with it (not unlike hammers and pencils and computers, etc.). But it is cumbersome and artificial; causal thinking is our ‘natural’ way of thinking. Of course, uncertainty exists, but its primary role in everyday life is to temper our actions, to make us take greater care as we try to shape the oncoming future, to be quick to recognize and adjust our actions to accommodate the unexpected.

***You may quote any of the above but I would appreciate seeing what you write before you do anything with it.***

Lee R. Beach 22/8/18