This Week's Citation Classic

Dawes R M & Corrigan B. Linear models in decision making.
Psychol. Bull. **81**:95-106, 1974.
[University of Oregon and Oregon Research Institute, Eugene, OR]

Optimal linear prediction outperforms intuitive prediction of skilled clinicians—e.g., in psychology, medicine, and business. Nonoptimal linear models based on such judges' own predictions also outdo them. We demonstrated that linear models with *random* weights—correctly oriented—do as well as clinicians and unit weighted models outperform them. [The *Science Citation Index®* (*SCI®*) and the *Social Sciences Citation Index®* (*SSCI®*) indicate that this paper has been cited in over 245 publications since 1974.]

Robyn M. Dawes
Department of Psychology
University of Oregon
Eugene, OR 97403

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"As a first-year graduate student in the (neo-analytic) clinical psychology program at Michigan, I read Paul Meehl's book on statistical versus clinical prediction.[1] I was impressed that—in numerous studies involving the prediction of human outcomes—optimal linear (multiple regression) models of relevant input variables outperform clinicians.

"Moving to the University of Oregon and the (old) Oregon Research Institute in 1967, I became involved with the work of Lew Goldberg, Paul Hoffman, Sarah Lichtenstein, Len Rorer, and Paul Slovic on linear modeling of clinical experts. They had discovered an apparent paradox—that linear models not only outperform clinicians at predicting actual outcomes, but that they predict these clinicians' own predictions as well; see, for example, Goldberg's *Citation Classic*.[2] That made sense when clinicians were viewed as imperfect mediators between input and output, introducing unre-

liability by their inconsistency, and invalidity by the degree to which the linear model predicting their judgments was not optimal for predicting the output. Unreliability could be removed by replacing the clinician's actual judgments with a (necessarily consistent) linear model of those judgments, a replacement termed 'bootstrapping.' It worked. Prediction improved. See Goldberg[3] or Dawes.[4]

"But were clinicians necessary at all? The efficacy of bootstrapping was demonstrated by comparing the model of the clinician to the clinician, but what would happen if we compared that model to some other model with appropriately oriented weights? (If such a model did as well, then the clinicians were good for *nothing but* choosing the variables, and their orientation.)

"I occasionally had a crazy idea—that some cynic might take one of our data sets, standardize the variables, add them together in some arbitrary way, and outperform our 'bootstrap judges.' One day my programmer, Corrigan, had slack time and asked for something to do. 'Well, for the last couple of years I've had a crazy idea that....' In four data sets (involving such outcomes as final psychiatric diagnosis and graduate success), we (Corrigan) chose tens of thousands of random weights in the right direction (determined *a priori*); linear composites based on these weights performed as well as did those with weights based on clinicians' judgments. Unit weights did better.

"Talking about our results before they were published was fun. (There was time to talk because *Psychological Review* rejected our paper before *Psychological Bulletin* made it a lead article.) Many people didn't believe random weights would work until they tested them out on their own data sets (phone calls and letters, 'My god, you were right!'). The reason random and unit weights work is that they can be expected to yield results highly correlated with those based on optimal weights—whatever those happened to be. (Weight optimization involves a 'flat maximum.') That can be demonstrated mathematically—as was later done by John Castellan, Ward Edwards, Hillel Einhorn, Robin Hogarth, Dettof von Winterfeldt, and Howard Wainer—and as early as 1938 by S.S. Wilkes.[5] Had we not been dumb enough to use data sets, however, the results would probably have gotten little publicity. Our naïveté made this work a 'classic.'"

1. **Meehl P E.** *Clinical versus statistical prediction: a theoretical analysis and a review of the evidence.*
Minneapolis, MN: University of Minnesota Press, 1954. 149 p.
2. **Goldberg L R.** Simple models or simple processes? Some research on clinical judgments.
Amer. Psychol. **23**:483-96, 1968.
[Citation Classic. *Current Contents/Social & Behavioral Sciences* **13**(10):18, 9 March 1981.]
3. ------------------. Man versus model of man: a rationale, plus some evidence for a method of improving on clinical inferences. *Psychol. Bull.* **73**:422-32, 1970.
4. **Dawes R M.** A case study of graduate admissions: application of three principles of human decision making.
Amer. Psychol. **26**:180-8, 1971.
5. **Wilkes S S.** Weighted systems for linear functions of correlated variables when there is no dependent variable. *Psychometrika* **8**:23-40, 1938.