



Feist, G.J. & Barron, F. (2003). Predicting creativity from early to late adulthood: Intellect, potential, and personality. *Journal of Research in Personality*, 37, 62-88.

Our Karma Ran Over His Dogma

Gregory J Feist

Department of Psychology

San Jose State University

greg.feist@sjsu.edu

As a young intellectual, I was fascinated by the mystery of human creativity and the creative process. As early as my undergraduate days, I came upon the seminal work on creativity conducted at UC Berkeley's Institute of Personality Assessment and Research (IPAR). At its core, the group consisted of Donald MacKinnon, Frank Barron, Harrison Gough, and Ravenna Helson. But one book and one quote stood out immediately as one of the more powerful insights concerning the simultaneous power and danger of creative talent: "Thus the creative genius may be at once naive and knowledgeable, being at home equally to primitive symbolism and to rigorous logic. He is both more primitive and more cultured, more destructive and more constructive, occasionally crazier and yet adamantly saner, than the average person." This quote was from Frank Barron's 1963 *Creativity and Psychological Health*. After reading that book, I knew I wanted to work with him. I applied to graduate study at both UC Berkeley (IPAR) and UC Santa Cruz, where Barron had moved in the late 1960s. As fate would have it I was rejected at UC Santa Cruz and accepted at UC Berkeley. So I never got the chance to work with Frank Barron in graduate school.

But much to my bewilderment and pleasure, after I completed my PhD on the psychology of scientific eminence and creativity, Barron approached me about a long-lost project and desire of his: to follow up the very first full IPAR assessment of graduate students from 1950. Eighty male graduate students had gone through the intensive three-day weekend assessments (with Erik Erikson being one of the psychological staff members). When Barron approached me, it was the early 1990s and time was running out. These men, who averaged 27 years old in the original assessment, were now past 70. Frank had tried to get funding for a complete follow-up but couldn't succeed in getting the money. We then wrote two grant proposals to NSF and ultimately

we failed as well. With a small grant from my university (San Jose State), we got enough money to conduct a scaled down follow-up study.

Upon completing data collection and writing the results up, we had a manuscript I thought we could proud of. But I never would have predicted what happened when we submitted it for publication. After 4 months of not hearing anything from the journal editor, I finally contacted him and he told me he had farmed it out to an associate editor. When I contacted the associate editor, he said he had never received it. It must have gotten lost in the mail! As a sign of goodwill and as an attempt to make reparations, however, he offered me a choice: let him review it and make a unilateral decision or send it out for full review. I was pre-tenure and couldn't wait another 3 or 4 months. I also thought he would be inclined to review it positively, given the fact that it had gone unreviewed for almost 4 months.

Well, he tore it to shreds and flat out rejected the paper—mostly for sample size and power reasons. After all, we only had complete follow-up data on 53 of the original 80 participants. Given that 44 years had transpired between first and second assessments, I thought a sample size of 53 was not bad. So I was flabbergasted by a flat out rejection. To be fair, I felt some of the criticisms were valid, but not all. I was convinced I could address most of the concerns. So I did something I had never done before and haven't done since: I sent a revised manuscript back to the editor, even though he had not invited resubmission. I was so convinced it was a good paper and that I had addressed his concerns that surely he would see it my way. He apparently was not amused and did not feel that way. Sure enough, he rejected it a second time. This was turning in to the worst publication (or non publication!) experience of my career.

I then submitted the manuscript to the *Journal of Research in Personality*. At this point, I was thrilled to go through the “revise and resubmit” and then “accept” sequence. I always

thought the paper was a unique contribution—one of the few studies of creative individuals to have extremely rich data at the beginning of a group’s career and then rich follow-up data at the end of their career. How accurate were grad advisors’ ratings of their graduate students’ potential, originality and success? Were they more or less accurate than the same ratings made by psychological staff who only knew the participants for 3 days (rather than the 3 years or so of grad advisors)? We had the data to answer these questions: both were somewhat predictive of lifetime achievements, but the insights of the psychologists were even more accurate than the grad advisors!

Yet I never expected to get the e-mail from Lynne Cooper, editor of *JRP*, that the paper had been awarded “article of the year award” for 2003. Did I have the impulse to send a copy of the award to the original journal editor who had twice rejected the paper outright? Yes! But my wiser side restrained that impulse. This is one of those times where one could actually say, our karma ran over his editorial dogma.

The award, however, was somewhat bitter-sweet. Frank Barron passed away in October 2002, six months after the paper was accepted, and so he never knew that his last empirical paper garnered the “article of the year” award. I gain deep satisfaction, however, in knowing that we were able to bookend Frank’s career with a study that fought so many battles just to exist, but in the end was duly appreciated.