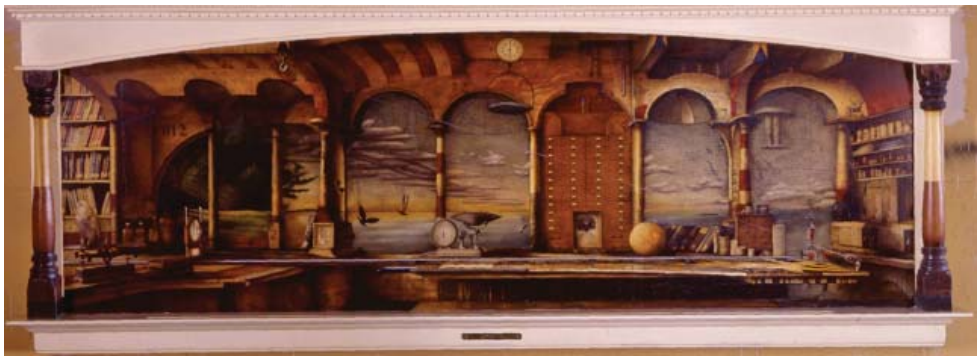
The Psychology of Science and the Origins of the Scientific Mind by Gregory J. Feist. New Haven: Yale University Press, 2008. 336 pp. $25 (paperback). ISBN 9760300143270.

On the second floor of the National Academy of Sciences headquarters in Washington, D.C., there is a most impressive painting by Robert Van Vranken, *Untitled (Where Do Thoughts Come From, Where Do They Go?*). In my mind this amazing picture, a huge panorama of a scientific laboratory, encapsulates everything the psychology of science is about. The book under review could well be seen doing the same task.



It is very difficult to disagree with the ambitious aim that Gregory Feist has set for this book, and that is to provide a foundation and a *Tour d'Horizon* for the psychology of science. One might certainly think it is just that, since this book received the William James Prize of the General Psychology Division of the American Psychological Association in 2007. Yet it is an uneven book, which in spite of its considerable contributions needs substantial improvements.

About the title. Obviously, there is not one "scientific mind" any more than there is one "scientific method." Scientific brains and scientific thinking take many forms. One thinks, for instance, of Ian Mitroff's book *The Subjective Side of Science*, whose examination of thought and research of the Apollo moon scientists shows very different patterns for the theorists and empiricists in the sample. Yet though Mitroff is not mentioned in this book, I am sure that Feist would see this sort of diversity as being basic to understanding how science works. "The scientific mind" is thus a metaphor rather than an assertion of fact.

Having set an ambitious aim for himself, can Feist be said to have accomplished it in this book? I believe the fair answer is, "not yet." But, as J. Alfred Prufrock says in Eliot's poem, "Let us go and make our visit."

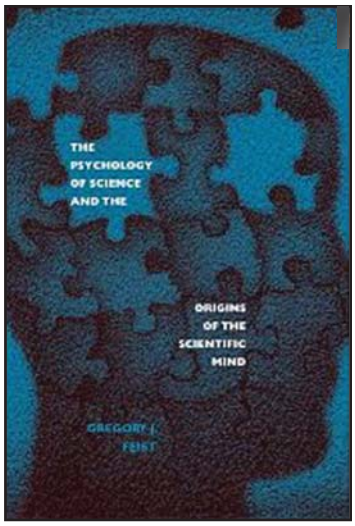
Our first visit is to Chapter One, in which Feist looks at the "meta-disciplines" associated with studying science: The Philosophy of Science, The History of Science, the Sociology of Science, and the Psychology of Science. Having once been a practitioner of the sociology of science, I will put the heaviest emphasis on it. To begin with, one would have thought that his timeline for sociology of science would include Alphonse de Candolle's *Histoire des Sciences et des Savants depuis Deux Siecles* (Geneva, 1873), a truly sociological work in spite of its title. But this is a minor quibble.

More serious is his perception that the heavy hitters in the sociology of science were Robert Merton's students Jonathan and Stephen Cole and Harriet Zuckerman. I believe that one could argue, instead, that along with Bernard Barber, the Mertonians were in fact a prologue to the serious sociology of science. That sociology was centered in a journal called *Social Studies of Science*, and strongly influenced by the Center for Science Studies of the University of Edinburgh. Eventually many scholars founded The Society for Social Studies of Science (4-S) (which later included "and Technology"—SSSST), which of course was not restricted to sociology, but included people from the other meta-disciplines as well. Whereas the Mertonians studied things like promotions and reward patterns, the next generation of sociologists of science spent much more time on the actual processes by which science is produced. Among the many insights this later work produced, and in no particular order, we find the following:

1. The scientific paper is often partly fictional (Knorr)
2. Laboratories engage in the social construction of scientific facts (Latour and Woolgar)
3. Science is heavily political (Latour, Nelkin, Boffey)
4. The Mertonian norms are often honored "in the breach" (Mulkay)
5. Division between science and non-science is arbitrary (Collins, Pinch, Barnes)
6. The peer review process is often biased and flawed (?)

Now both Social Studies of Science and the 4-S (or SSSST) appear on Feist's timeline, but he does not appear to appreciate their importance, or make many citations to them. In addition there was also Science, Technology and Human Values, and Science Studies. What was particularly important about the 4-S is that it put the various meta-disciplines together.

With regard to the psychology of science, he neglects the role of the Subgroup on Social Psychology of Science, whose newsletter *Social Psychology of Science* I edited (later with help from Robert Rosenwein) for at least twelve years. This was a Subgroup of the 4-S. The newsletter did not start "in the 1990s" (p. 155) but in 1982, more or less in synchrony with



the Subgroup itself, and did not last "a few years," but until 1994. One can scan the newsletters themselves to see the large number of psychologists, sociologists, historians, etc. (There were dozens of subscribers early on, and 125 subscribers in 1994.) Panels at 4-S meetings on psychology of science were often to draw from those on the subscription list, as did conferences. The newsletter included book reviews and the addresses of subscribers. It helped to showcase the work of such eminent science historians as Stephen Brush, whose work on theory acceptance has proved so important (it is not mentioned in Feist's book at all, even in the section on Planck's Principle). This newsletter was ephemeral, indeed, but the fan mail I got showed how many people appreciated it.

A final remark on this chapter. With regard to the history of science, multi-volume histories of astronomy (e.g., by Bailly and Delambre) were in full bloom by the 18th century, and I suspect earlier as well.

Chapters Two and Three deal with neurological and developmental issues. Here I believe Feist does better. His discussion of Sulloway's findings on birth order and on Planck's Principle (ageing and openness) are good. His introduction of the findings (his own work) from various Westinghouse Science Competitions are particularly interesting. At the same time, however, I feel he spends too much space on "scientific" abilities in children, and too little on the peculiarities of adult scientists.

For instance, there is the curious matter of high concentration. Quite a few inventors and scientists have shown an uncanny ability to shut out the world around them as they concentrate on their projects. Inventor Bill McLean was quite capable of sitting on a couch, concentrating on some weapon system, being called to dinner, and eating quietly, then returning to the couch, and suddenly jumping up and saying, "OK, I've got it, when do we eat?" This concentration is often referred to as, from its side effect, absent-mindedness, but as William James said, absent-minded people are often "present-minded somewhere else."

In his discussion of Keith Simonton's findings about creativity and age, I believe that Feist underplays another feature of Simonton's data, which is that the "early peaking" of creativity is most marked in the most theoretical sciences, and least marked in those that heavily depend on facts and skills. In

particular, inventors tend to peak later, and often keep inventing into periods often thought to be affected by senility. Jack Rabinow, for instance, came up with the world's first pickproof lock when he was about 80. It is also interesting to note that Rabinow, who came up with about 2,000 inventions (and patented 230 of them), kept a timeline on his lifetime creativity, noting the number of ideas per year. He found that the highest level was reached when he was running his own company, and could rapidly turn his ideas into prototypes because of his skilled technicians. Rabinow's chart in his *Inventing for Fun and Profit*, might find a place in Feist's book, since it is a far more differentiated and sensitive indicator than the simple division of "young" and "old" that Feist employs.

Chapter Four, on cognitive approaches to the psychology of science, is the most satisfactory chapter in the book. Feist does a good job of reviewing experiments, historical case studies (though Darwin is covered in the most detail), and materials on modeling, clearly an extremely important aspect of how scientists think. It is here that one finally gets to mechanical intuition or ingenuity, so important in physics, chemistry, and zoology. He has read widely, and he takes pains to consider most of the issues. He leaves out, however, Morris Stein's work on animistic metaphors, which I have found particularly useful. I have certainly observed that inventors and scientists alike tend to make what they study "come to life" by endowing atoms, molecules, etc., with feelings and even thoughts. For instance, I was sitting one day with Paul MacCready, aeronautical inventor, and he described the Bernoulli effect to me as if the molecules of air were actually alive. Similar comments were made by inventors Jacob Rabinow and Raymond Damadian. The latter insisted that "all scientists" use such metaphors to think. Bill McLean, inventor, was described by a colleague as "grunting and sweating and trying to get the third derivative" as he felt his way into the manner in which the Sidewinder missile responded to the airflow. Karen Knorr has also discussed the physicality of scientific thinking. In discussing Sara Mednick and remote associations, Feist fails to note that Mednick's Remote Associates Test, for instance, correlates with patent disclosures. The RAT test figures in Gerald Gordon's study showed research groups with Hi-RAT members, paired with high differentiating leaders, to be the most productive. It also figures in the classic study by Pelz and Andrews, *Scientists in Organizations*, in assessing the intellectual quality of members of R&D groups.

In Chapter Five, the author tackles the relationship of personality to science achievement. There are some very striking findings here, both about the personalities of those who choose science and those who are good at it. Some of the more valuable findings in the chapter come again from Feist's own research. His meta-analysis of scientists' personalities

shows, for instance, that scientists tend to be more dominant, arrogant, hostile, self-confident, autonomous, and introverted. This is even true of women scientists. Yet the personalities of high achievers are different from low achievers. For instance, while conscientiousness is strong for the average scientist, it is much less strong for the highly creative scientist. I was surprised, however, that when Feist discusses the Myers-Briggs test, he doesn't point out the most typical personality profile for productive scientists: INTJ, introverted intuitive thinkers with judgment (mentioned by Mary MacCauly, a professional tester). I remember giving the MBTI to a group of radar engineers and scientists. There was only one member of the group who wasn't essentially INTJ!

Chapter Six on the social psychology of science is relatively good with regard to "how future scientists are influenced to go into science." Gender issues are handled in some detail. One would like to see a little bit more on the dynamics of science graduate schools. Furthermore, it is surprising that he does not mention the work of Joseph Ben-David and similar scholars on the growth of disciplines. As far as the dynamics of scientists in groups are concerned, one really needs to go somewhere else. His statement that Industrial/Occupational social psychology (in the psychology of science) is undeveloped is a joke. About half of the "scientists" in the United States, I believe, are in the private sector. Feist pays little attention to them, but the literature is enormous. My library has several books on scientists in private organizations from the *1950s*, that is, before Pelz and Andrews wrote their classic *Scientists in Organizations*. As for the dynamics of R&D groups, I would guess that there are at least a half-dozen journals that focus on this, to say nothing of individual case studies, in book form or otherwise. The literature on groups in creativity (including scientists) is also extensive.

Chapter Seven is about the psychology of science and what might be done with it. He gives a very knowledgeable discussion about various forms of aptitude testing, and competitions such as the Westinghouse one. He does mention Howard Gardner's "Multiple Intelligences" without noting that C. W. Taylor had earlier proposed the same thing (with a cute chart to boot). This then leads into a discussion on the future of the psychology of science, which I found unconvincing. The fact that Feist and several other scholars have created a journal and an association to study psychology of science, however, is very germane to this question. Feist has been a leader in pushing the psychology of science recently, and its future success may well depend on his ability to bear this mantle.

Chapters Eight and Nine are about "the evolution of the human mind" and "the origins of the scientific thinking." I am not expert in either of these areas, but was unimpressed with both chapters, especially the latter. I believe

that as a popular treatment of the issues in question, it is not bad. Yet for the more discerning mind, it is not satisfactory. His knowledge of the specialist literature seems modest, at best. He cites Will Durant rather than Lynn Thorndike, for instance. I have no reason to doubt his anthropological sources (in Chapter 8), but if one is looking for history of science (Chapter 9), or even the prehistory of science, these chapters may not be the place to start. I looked in vain for any number of standard sources that were simply not there (e.g., Marrou, Partington). In earlier chapters he quotes Howard Gruber on Darwin, but does not seem to have read Janet Browne's definitive biography.

Chapter Ten is on pseudoscience, anti-science, and postmodernism. Although I am inclined to agree with his negative judgments on postmodernism, the part on pseudoscience is simply incompetent. Imagine writing a chapter on this subject without knowing about the SSE, about *Zetetic Scholar*, about the Mars Effect debate (and the Starbaby scandal), etc. Feist has read the skeptical literature and apparently nothing else. He has done no original research in this area, and is obviously unfamiliar with the literatures of UFOlogy, paranormal research, near-death experiences, and so on. I found particularly disturbing his statement that along with flat-earth theory and alchemy, ESP and UFOlogy are not more fundamentally advanced than they were thirty or forty years ago. His sweeping statement is not correct. He has simply not done his homework. This might be acceptable if he were a bench scientist who wanted to blow off steam on this subject. It is not acceptable from a social scientist who should be better informed.

Bottom line. So what has Feist accomplished with this book? I believe that he has brought together a large amount of literature and arranged it in a logical way. Given his knowledge of a good deal of the psychological literature, he has performed a service in bringing it together. As one gets into the details, however, there are many respects in which this book does not measure up, and especially in the ambitious goal of founding a psychology of science. He has not read everything he needs to have read. Furthermore he does not seem overly familiar with actual scientists. I don't recall any materials from interviews he did, or any anecdotes from biographical studies which he himself carried out. I grew up with a father who was an experimental scientist, and even as an outsider to his world learned a substantial amount of lore about how scientists operate. It is not clear to me how familiar Professor Feist is with such lore.

Many of the problems might be corrected with a second edition. But between the first and second editions, there is a lot of work to be done.

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