A guide to increased creativity in research - inspiration or perspiration?

by Craig Loehle

There are four requirements for a successful career in science: knowledge, technical skill, communication, and originality or creativity. Many succeed with largely the first three. Those who are meticulous and skilled can make a considerable name by doing the critical experiments that test someone else's ideas or by measuring something more accurately than anyone else. But in such areas of science as biology, anthropology, medicine, and theoretical physics, more creativity is needed because phenomena are complex and multivariate.

Innovative scientists are held in high regard, but the means by which they achieve innovation are not spelled out in any manual for graduate students. Courses on the scientific method (which few biology students take anyway) do not mention the subject. Philosophers of science are more concerned with formal theory structure, proof, logic, and epistemology. Karl Popper (1963), for example, invokes the generation of alternative hypotheses but says nothing about where one is to get them.

The purpose of this article is to present certain strategies that may promote scientific creativity. The pressures on scientists today oppose truly creative thinking. Pressures to write grants, teach, and publish leave little time for undirected thinking. Industrial laboratories today are far more directed than in the past, particularly where costs per experiment are high. I also want to counter the widely held view that creativity is something one is either born with or lacks, with no hope of training.

Choosing a problem

Perhaps the most important single step in the research process is choosing a question to investigate. What most distinguishes those scientists noted by posterity is not their technical skill, but that they chose interesting problems. There is some guidance that may be given.

Picking fights. Science is supposed to be an objective, dispassionate business. Students are advised to write in the third person. Editors cut out comments that are too personal. All this is appropriate for the public face of science, rather like stiff turn-of-the-century photographs.

But let's say you read a paper that makes you furious. Your anger is an indication that at some level you recognize that here is a problem that needs resolution. The gut feeling that the other person is wrong, or that there is a better way to do it, is a good guide to choosing an interesting topic for yourself.

Setting out with irrational determination to prove the author wrong provides a drive that can allow you to break out of your preconceptions. Such base emotions can be a strong creative force, causing you to dig deep and work intensely. After you have finished writing your paper, you can go back and remove the comments about what an imbecile the other person is. The effort to refute someone can even lead to evidence supporting them or to a different topic altogether. Intensive rivalries, as in the race to discover DNA (Watson 1968), can also provide this essential intensity. Thus whereas the finished product may appear dispassionate, truly creative work is often driven by strong passions.

Where there's smoke. A good strategy for finding an interesting problem is to follow the fire trucks, because "Where there's smoke there's fire." When there is intense debate on a topic, inconclusive or contradictory experiments, or terminological confusion, then things are probably ripe for a creative redefinition of the problem or application of a new method. If, however, your tendency is just to choose sides, then you are merely more kindling and should stay away from the fire.

The Medawar zone. There is a general parabolic relationship between the difficulty of a problem and its likely payoff (Figure 1). Solving an easy problem has a low payoff, because it was well within reach and does not represent a real advance. Solving a very difficult problem may have a high payoff, but frequently will not pay at all. Many problems are difficult because the associated tools and technology are not advanced enough. For example, one may do a brilliant experiment but current theory may not be able to explain it. Or, conversely, a theory may remain untestable for many years. Thus, the region of optimal benefit lies at an intermediate level of complexity, what I call the Medawar zone in reference to Sir Peter Medawar's (1967) characterization of science as the "art of the soluble." These intermediate problems have the highest benefit per unit of effort because they are neither too simple to be useful nor too difficult to be solvable.

Robert H. MacArthur was a prominent ecologist, active in the 1960s and 1970s, who dies young. MacArthur was known not for being right all the time, but for having an unerring creative instinct for discovering interesting problems that were solvable and for extracting the essence of complex problems so that they became solvable. What is notable about such people is that even when they were wrong, they are wrong in an interesting way and on an interesting topic.

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The issue of what is interesting and what is solvable lies at the heart of great discoveries and what we calll genius. Some who choose to grapple with the big questions fail because they address problems not ripe for solution. The more common problem afflicts the average scientists who shies away from really interesting problems in favor of easier ones. Such intellectually timid scientists produce the bulk of T.S. Kuhn's "normal science" (Kuhn 1970).

Working on too-easy problems is disadvantageous both because no one may notice your results (yawn!) and because easy problems often turn out to be merely pieces of a larger puzzle and only soluble in that context. For example, in the first x-ray pictures of DNA (Watson 1968) two forms (A and B, differing by water content of the sample) of diffraction pattern were evident. James Watson and Francis Crick did not focus on explaining or interpreting this difference, but rather they focused on the more difficult problem of the DNA structure. When that puzzle was solved, the A and B patterns were easily interpreted.

When someone succeeds in frequently hitting the target (the Medawar zone), that person will often appear to be more intelligent than a pure IQ test would indicate. (1) To an extent, the feel for interesting problems can be transmitted by contact, which justifies the graduate-student-as-apprentise practice and explains the fact that certain laboratories ferment with new ideas. Such labs are often observed to fade away or return to what is considered normal after the death or departure of the person or persons who provided the creative spark.

The creative spark is not easily obtainable through the formal textbook portion of scientific training, and it may not arise spontaneously. For example, Richard Feynman (1984) recounts his experience as a visiting faculty member in Brazil in the 1960s. Physics in Brazil was just getting started. To outward appearances, the faculty knew the facts. Library and laboratory facilities were adequate. Yet there was almost a complete lack of comprehension of the process of innovation and discovery. Science was a textbook exercise of learning definitions rather than one of discovery. Even in the United States today, entire departments or disciplines sometimes get stuck in such a listless state.

Releasing creativity

Most people can learn to be far more creative than they are. Our school system emphasizes single correct answers and provides few opportunities for exploratory learning, problem solving, or innovation. Suddenly, when one becomes a graduate student, however, it is expected that one is automatically an independent thinker and a creative problem solver. I thus next focus on ways of encouraging creative approaches and reducting blocks to creativity.

Barriers to navigation. In the early fifteenth century, Prince Henry the Navigator of Portugal set out to explore Africa and open it to Portuguese trade (account in Boorstin 1983). Portuguese expeditions began to work thei way down the western coast, always within sight of land. Upon reaching Cape Bojador, the Portuguese sailors would inevitably turn back, convinced that this was the end of land and that no ship would ever pass it. Prince Henry sent out 15 expeditions between 1424 and 1434 until finally one succeeded by sailing a few miles out to sea and going south for a few miles.

As a navigation feat, this maneuver was trivial. The barrier was not a physicla one byt a mental one. Many barriers are of this type. An itme becomes fixed in the mental landscape, immutable. What lies beyond the barrier becomes not merely unknown, but unimaginable. Major enhancements in creativity can be achieved by developing the courage to recognize and overcome mental barriers, just as the Portuguese sailors did.

A simpel test for creativity involves giving test subjects a set of objects and a goal, to see if they can use ordinary objects in unusual ways (e.g., a rock as a hammer). Noncreative individuals are often stumped by these tests. In science, too, objects become fixed in meaning. In many cases, an assumption comes to have the rock hardness and permanence of a fact.

My children had been playing with some yarn for months, calling it spaghetti for their toy kitchen. When my four-year-old daughter started twirling it around to the music, one piece in each hand like the Olympic gymnasts, my five-year-old daughter became upset because you do not twirl spaghetti around and dance with it. Therefore, young scientists or those venturing in from other fields often make the most revolutionary breaks with tradition: they are able to ask, "Is this really spaghetti?"

Those whom we note as outstandingly creative have often been described as possessing a childlike innocence or sense of wonder, and they ask seemingly naive questions. This attitude contributes to creativity by keeping the mind flexible. Ambiguity and the unknown make many people nervous, however. It was not until the late fifteenth century that European mapmakers would leave sections of their maps empty. Before that, they had filled the empty spaces of their maps with the Garden of Eden, the kingdoms of God and Magog, and imaginary peoples and geography (Boorstin 1983). We do not easily suffer blank spaces on our mental maps, either.

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A major obstacle in science is not ignorance but knowledge. Because Aristotle was so comprehensive, logical, and brilliant, his writings became the ultimate standard of truth. Galen's works provided a similar barrier in anatomy and medicine. Incremental improvements to such a subject are difficult to incorporate into the mainstream of thought, because people keep returning to the original. New facts become like little pieces of clay stuck onto a large statue: they tend to fall off or not show.

Another type of barrier of the mind is the definition by the community of scientists of what is a serious problem and what is not. Until the late 1970s, physicians regarded turbulence as largely beyond the terra firma of well-behaved phenomena subject to "real" scientific study. The discovery of the mathematics and physics of chaos (chaotic attractors, universality, relations to fractals, and all the rest) is rightly called a revolution (Gleick 1987) because it brought within the realm of orderly study an entire class of phenomena previously classified as "void, and without form."

In the case of chaos, there was a well-defined phenomenon, turbulence, that was deemed intractable. A more common situation is when a topic is not even recognized as such. When Darwin wrote his book on the origin of coral reefs (Darwin 1842), other scientists did not even recognize that there was a problem to be solved. When Darwin found earthworms interesting enough to write a book about them (Darwin 1881), the world of science was quite surprised. Recognizing problems that others do not even seen can be considered a prime characteristic of the truly innovative.

Barriers to recognizing a phenomenon or problem are many, including concreteness, visualizability, and complexity. Before Riemann, the geometry of Euclid was identified with the three dimensions and properties of our sensory world. The axioms therefore were too concrete for anyone to conceive of altering them. Breaking this concreteness barrier led to many forms of non-euclidean geometry.

Visualizability can also be a limiting factor. Once Poincare sections of the orbits of strange attractors were published, it became evident to everyone that they was some kind of regularity to turbulent phenomena. Formal proofs of this fact were far less influential to the general scientific community because they are much less accessible (Gleick 1987).

Complexity and heterogeneity are also major barriers to recognizing problems. The genius of Newtorn was in recognizing that a ball thrown in the air and a planet circling the sun are "the same" with respect to gravity. He made the further crucial abstraction of treating his objects as point masses, reducing the complexity to a minimum. These abstractions and simplifications of Newton are, in reality, simple, but only after the fact.

It is characteristic of mental barriers that once overcome they are never given a second thought. THe Portuguese navigators never considered Cape Bojador a serious problem once it was passed. Of course, many scientific achievements really are complex. The mathematics necessary to grasp quantum mechanics is quite difficult and is not just a mental barrier. Nevertheless, a scientist must always be alert for barriers that can be circumvented.

A significant barrier to navigation is the set of structures we have erected to facilitate our work: namely, academic departments. The current system seeks to fill all the square holes with square pegs. The biology department wants one geneticist, one physiologist, and one ecologist, but they don't wnat three generalists who work in all three areas. In what department would one put Darwin: genetics, geology, taxonomy, or ecology? Darwin considered himself a geologist, but the worl remembers largely his biology. Should Goethe be in the literature, biology, physics, or philosophy department? He actually was most proud of his work on optics, though that work was largely flawed. Would Newton or Fisher find comfortable academic niches today? The current rigid departmental system is confining to the truly creative person and discourages the vitally important cross-fertilization of models, data, techniques, and concepts between disciplines.

Don't be an expert. All graduate students are taught that it is essential to become an expert. As a short-term goal it is, of course, valid. Academic search committees are also looking for experts. As a lifestyle, however, becoming an expert can inhibit creativity.

Why is this? After all, it seems that an expert has more tools at his or her disposal for solving problems. The problem revolves around or mental constructs. In learning a subject, we create a network of facts, assumptions, and models. Once we think we understand something, it is linked up to an explanation and supporting ideas. this construct may not be true, but it comes to seem real nevertheless. As one becomes more of an expert, a larger and more complex network of facts and explanations accumulates and solidifies, making in difficult to entertain radical alternative ideas or to recognize new problems.

The expert is in danger of developing the small cage habit. Zoo animals, when moved to a larger cage, may continue to pace about an area the size and shape of their old smaller cage (Biondi 1980). An Aristotle or Freud may create a set of bars within which most people pace rigidly, never noticing clues from outside the cage. The danger in becoming an expert is that one tends to build one's own cage out of the certainties and facts which one gradually comes to know. Dogmatism builds cages in which the dogmatic then live and expect everyone else to live also.

How does one not become an expert? Astrophysicist S. Chandrasekhar gave a remarkable television interview a few years ago. He has led a scientific career notable for a rate of productivity that has not slowed down at all into his 70s. When asked how he has avoided the drop in creativity and productivity that plagues many scientists, he replied that approximately every seven years he takes up a new topic. He found that he would run out of new ideas after working in an area for too long. This pattern led him to tackle such topics as the dynamics of stellar systems, white dwarfs, relativity, and radiative transfer. Although all these subjects are in astrophysics, they are different enough to present unique problems.

We need only turn to Darwin to find a truly remarkable example of the value of changing topics. He wrote books on the origin of coral atolls, the geology of South America, pollination of orchids, ecology of earthworms, evolution, human emotions, the taxonomy of the world's barnacles, and movement in plants. When he decided that a topic was interesting, he would delve into it in depth for a period of years, write up his results, and move on. After his early books on geology, he only returned to the topic a few times during the remainder of his career. In today's atmosphere, he would have been encouraged to follow up on his early study of corals or geology for the rest of his career. Imagine him in a modern geology department telling his department head that he planned to spend the next 20 years working on evolution, earthworms, and orchids (see Figure 2).

It is easy to protest that learning a new subject is too hard and takes too long. I am not suggesting that everyone can or should strive for the diversity of Charles Darwin. Taking up new subjects within a discipline or linking up with related disciplines appears more difficult, however, than in fact it is. It is much less difficult than the original graduate school experience, because the mature scientist has an arsenal of tools, terms, and techniques that are transferable between topics. I assert that the value of cross-fertilization far outweighs the cost of learning new skills and facts. Studies have shown that a wide spectrum of interests is typical of highly creative scientists and helps account for their creativity (Simonton 1988).

Practical problems beset the brave soul who eschews the expert label. Getting grants for research in a new area will be difficult. Department heads will frown. Exploring new territory inevitably evokes the Columbus response: shaking of heads and muttering as you disappear over the horizon and a hero's welcome when (if) you return. A strategy some researchers employ is to maintain a home base of expertise in a narrow area to keep department heads and deans happy, with frequent excursions to diverse topics to stay fresh.

Don't read the literature. When students ask how to get started in science, they are inevitably told to read the literature. This advice is fine for students, because they are used to looking up the answers in the back of the book anyway and repeating the examples they have seen. For the practicing scientist this first step is destructive, however. First, it channels your thoughts too much into well-worn grooves. Second, a germ of an idea can easily seem insignificant in comparison to finished studies. Third, the sheer volume of material to read may intimidate you into abandoning any work in a new area. Medawar (1979) also advises against reading too much, arguing that study can be a substitute for research.

My recommendation for the first step (after getting the germ) is to put you feet up on the desk and stare out the window. Try to elaborate the idea as much as possible. Do some calculations or quick lab experiments. Write a few pages. Only after the idea has incubated and developed will it be robust enough to compare it to existing literature. Given a certain level of knowledge in a subject, you know generally what is going on, so you are not likely to be reinventing the wheel. When you go to the literature, you may find that someone has preempted you or that you idea is invalid, but at the risk of only a few days or weeks of work. The cost of good ideas killed off too soon is much higher than the cost of some wasted effort.

Work habits

Let's get bored. Boredom or inactivity is a seriously underrated part of being creative. I do not, of course, mean that being creative is boring, or that boring people are creative, but that slack time, quiet time, is a valuable part of the total creative process. Consider an artist. If he walked into the studio and immediately began to dab on paint and did so for eight straight hours, I would not anticipate seeing anything of real beauty. Novelists may go for months or years collecting facts, traveling, and searching for inspiration. Poets are notorious for working only when inspired.

Yet because a scientist's time is valuable, we seem to expect an eight-hour day. This day is fine if you are doing routine science (e.g., screening 100 chemicals in mice for cancer risks using standard methods), but not good for science that requires deep thought. To quote James D.

Watson (1968), "much of our success was due to the long uneventful periods when we walked the colleges or read the new books," not exactly the factory style of doing science. As John Cairns stated (1988) on reviewing Frances Crick's autobiography, "Many readers will be struck by the thought that Crick belongs to a bygone age, when biologists were given time to think. What granting agency today would give several years of support to a young scientist who just wanted to build models? What 30 year old would now dare to embark on such a perilous pursuit?"

In comparisons of student problem solving (Whimbey and Whimbey 1975), it was thought that the better students would be found to read a difficult problem faster and solve it faster. In fact, the good students took much longer to read the problem, because they were thinking about it, but then took less time to answer the questions or do the math. The poor students often were jumping ahead and solving the wrong problem. On simple problems, there was little difference in performance.

This habit of jumping ahead leads too often in science also to solving the wrong problem. The pace of academic life and research has become so frenetic that activity and motion have come to replace thought. The need for careful thought and planning is particularly acute for studies on complex systems where laboratory technique does not dominate, such as epidemiology, ecology, and psychology. There is a simple test for freneticism; merely ask someone, "Why are you collecting this data?" If they are too busy to answer or cannot explain it, the ratio of thought to activity is too low.

There are some research techniques that have fallen out of favor in recent decades as being inefficient but which should be reintroduced. One of these is the highly sophisticated pipe-smoking technique. This instrument has its utility in the almost incessant and highly ritualized care it demands, which keeps the hands busy while the mind contemplates some problem, while at the same time leading a passerby into believing that the smoker is actually doing something (for detailed instructions, see McManus 1979). In contrast, an unfocused gaze with hands behind the head is immediately interpreted as goofing off. Of course, I do not recommend smoking, but some substitute for the pipe is sorely needed.

An equally effective technique, good for deeper contemplation, is the walk. This technique is looked down on today as being too low-tech. Besides, someone walking is obviously not working. Darwin used to take an hour walk every day around a course he had laid out (Figure 3). He would become engrossed in his thoughts; therefore he put some small stones at the start, kicking one off at each round so that he did not have to keep track of how many circuits he had made or worry about time. It was during these walks that he wrestled with the deepest questions.

The practice of taking long walks as an active part of intellectual activity used to be a common part of academic life in Europe. Professors would take their graduate students on walks to debate, discuss, and question. These days graduate students are lucky to even see their professor in the halls. Our idea of a walk is going to the copy machine. Some psychologists have found that taking patients for a walk is very effective in getting them to open up and express themselves. With our short attention spans these days, it would no doubt require practice to be able to come to conclusions or formulate complex thoughts while walking and remember them back in the office, but it can be done and would be beneficial.

If you can't walk, try running. I have been a recreational jogger for 15 years. I sometimes find that a pain in my ankle that I feel when walking or jogging will go away if I switch to a sprint. This cure suggests a strategy to overcome writer's block, which afflicts many scientists. The scenario I often observe is that someone finishes an experiment or field study and then sits down to "write up the results." It reminds me of the Peanuts comic strip in which Snoopy is trying to write a great novel and keeps getting stuck on "It was a dark and stormy night."

Starting at the first word to write up the entire study is rather intimidating. The walking writer, like Snoopy, is noticing the pain in his ankle at every sentence and is likely to stop and massage each sore spot, thus repeatedly getting stuck. Such jerky motion is also anathema to creative thought. Sprinting can sometimes cure both problems. Sit down with a cup of coffee (optional) and define a short piece to be written in a defined interval, say the methods section in one hour. Then sprint without worrying about grammar or style, which can be corrected later. Leave blanks where the references should go. Often this plan will get one off the mark and writing may continue for several hours. If it turns out not to be a good day, the sprinting technique at least allows for an hour or two of solid work. The utility of this approach depends on the style of the researcher and is most useful for hyperactive individuals who do not like to sit still and for perfectionists like Snoopy who get stuck on the first sentence.

Be unrealistic. It is a fatal mistake to have a realistic estimation of your mental capacities. Someone who is realistic will never attempt problems that seem hard, because few of us are Newtons. On the other hand, creativity is only marginally related to IQ. That is, above a certain point such as 120 or so, IQ is not predictive of either productivity or innovation (2) (Simonton 1988).

As we look back on great scientific discoveries, many of them seem childishly simple to us. The great innovation of Galileo was to avoid trying to explain why objects fall (as Aristotle had) in favor of quantifying how they fall. When Newton treated objects as point masses it was brilliant, but in retrospect it is a simple concept. The great innovation of Vesalius was to do dissections himself and base his anatomy book on what he actually saw rather than on the authority of Galen (Boorstin 1983). His further innovation was to use medical diagrams in his book. All of these are elementary ideas.

Some may despair that all the easy ideas have been found, but this assessment is far from true. In the last two decades, fractals and chaos have transformed the foundations of science, yet the basic concepts and even some of the formal math are intuitively obvious and simple once learned. Often the solution we seek will turn out to be simple and well within the reach of our intelligence. It is puzzling why scientific discovery is so hard when the final result can often be demonstrated to an eighth grade class.

Inverse procrastination. The first priority of the innovator is procrastination. Only by putting off routine duties and avoiding committee assignments can one find time to daydream and browse in the literature. I do not believe it is fair to call this procrastination and avoidance irresponsible behavior. Rather, it has to do with lead times being more important than deadlines. The gestation time for ideas, methods, and models is often quite long. The Eureka! phenomenon is usually the tail end of a long process of puzzling over a problem, reading about it, and discussing it with colleagues.

For example, ever since my teens I have been fascinated with the ability of some trees to live for thousands of years. I read accounts of tree lifespans and counted rings on stumps without any goal in mind for many years. But eventually this information led me to a new approach to the problem of the energetic costs of achieving great age (Loehle 1988).

I believe that most creative scientists have a long list, or zoo, if you will, of perhaps only partially articulated questions and puzzles that they mull over and that guide them. The need to feed the inmates of his zoo at regular intervals is strong, because these ideas will blossom into the next set of research problems. This drive leads to what I call "The First Law of Inverse Procrastination": always put off some of what you should be doing today so you can do something that might be relevant later.

Surfing. If I say that creative work is like surfing, you will think I am from California. By this analogy, however, I mean that good ideas come sporadically and unpredictably and should be pursued as they pass by, just as the surfer pursues the wave. Some waves are small, some large. Some days the surf is up, and some days it is not. For the really big waves, it can take real effort to stay on the crest. The little waves can be caught by jotting down notes wherever you are. When the surf is up, it is crucial to recognize it, and, like the California hot-dogger, cut classes if necessary to hang ten. At such times, one should shut the door and disconnect the phone. In such a creative wave, sometimes entire first drafts of papers can be ridden in a continuous burst of writing. Such work is often of the highest quality even though hurriedly done.

Does such an approach mean one should be a prima donna, only working when the mood strikes? Certainly not. On days that are not good for surfing, there are articles to read, manuscripts to revise, equipment to order, papers to review, phone calls, meetings, and so on and on. The point is not to be moody but to be receptive to the creative muse (to be musey, if you will). Designating a time of day for research or following too rigid a pattern of work is detrimental to creative thought.

Surfing applies to topics popping up, as well as to being inspired in general. To cite B. F. Skinner (1959), "a first principle not formally recognized by scientific methodologists: when you run onto something interesting, drop everything else and study it."

This principle points out two fundamental problems with the current peer review grant-giving process. First, reviewers may not concur with your assessment of what is interesting. Second, the current review system requires one to lay out, in some detail, the steps and procedures one is going to following through several years and what the expected outcome is going to be. Except for observational or very expensive studies, this demand is completely unrealistic, because research is a contingent process. It also precludes following up interesting leads. Examining Faraday’s notebooks, one sees that he did several experiments per day in an iterative, tinkering type of research. How could he have planned this research in advance or presented it to a review panel?

To ensure survival, many researchers practice a form of deception by squeezing interesting projects between the cracks of other grants. My argument is that funding, except for very large studies, should be in larger amounts over longer periods than it is today, and funding should be directed more toward broad lines of inquiry rather than the current narrow focus.

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Today's highly competitive climate has led to the misconception that the quality of proposed work and its outcome is predictable from a detailed grant proposal. Few if any really surprising discoveries get explicitly funded this way. As Koestler (1964) noted, "The history of discovery is full of arrivals at unexpected destinations, and arrivals at the right destination by the wrong boat." A much better practice is to fund investigators, as does the Howard Hughes Medical Foundation, for three- to five-year periods based on the individual's track record rather than to fund a detailed proposal. This practice frees up the truly productive from the huge overhead of chasing grants (as much as 30% of one's time) and from making overly rigid research plans. One cannot predict or control what the creative person will do, but he or she can be encouraged by adequate support.

Conclusions

The path of creativity is strewn with the bones of those consumed by the vultures of mediocrity, accountability, and responsibility. One cannot schedule creactive breakthroughs, budget for them, or prove them in advance to a review panel. An entirely different, flexible approach to science is necessary to encourage creativity. The concept that time is too valuable for staring out the window or reading for pleasure is equivalent to doing lab work while standing on one's head. Free and undirected thought and research are essential. Scientists of the world, throw off your chains! You have nothing to lose but your "normal science"!

(1) C. Loehle, 1990, manuscript submitted.

(2) See footnote 1.

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