THE FUNDING DILEMMA:

GARAGE INVENTION VERSES LABORATORY RESEARCH

Evaluation Bias

A proposal for new technical innovation stands more than an even chance of being dismissed if the either the concept or the principal investigator appear too unorthodox. Evaluators typically review such proposals against a background of their own experience and prejudice. Since most experts called upon to review technical inventions are professionals who are affiliated with large laboratories or academic institutions, the background of the proposer and setting for the work are predisposing factors in the mind of the evaluator.

Too often evaluators place a premium on expensive facilities, academic achievements, engineering degrees and published papers. Another frequent mistake made by evaluators is the presumption that technological criteria can be separated from business and economic considerations when evaluating the projects of individual inventors or small firms. No consideration whatsoever is given to the historically documented meager means and backgrounds of those who produced much of the proven technological innovation we possess today.

Experimental funding proposals are typically judged more on the basis of who the principle investigators are and where the work is being conducted rather than the technology being proposed and the actual potential for successful conclusion. Little or no consideration is given to positive aspects of the technology, preexisting experimentation or promising alternative developmental paths.

Subjective evaluation substitutes the personal opinions of the evaluating expert for actual conditions of the technical program. A negative evaluation under such conditions is often more often than not based on the differences of the innovator's background from the norms typically expected of individuals engaged in similar technical development. Worse still, problems which must be solved to achieve success with the proposed concept are often cast as insurmountable, typically reinforced by the observation of the evaluator that success in the area of the proposal has never before been achieved.

Overstatement of the difficulties involved in development of new technology is normally not malicious. (Historically documented cases of malice do exist, however.) Those who evaluate innovation do not always have a thorough understanding of the problems and promise of new concepts:

> "American and British history is riddled with examples of valid research and inventions which have been suppressed and derogated by the conventional science community. This has been of great cost to society and to individual scientists." (Cognitive Processes and the Suppression of Sound Scientific Ideas, J. Sacherman 1997)

One of the major U.S. Patent Office requirements for patenting an invention is that it not be obvious to one "ordinarily skilled in the art". Those ordinarily skilled in the art often include engineers, scientists, business leaders and other professionals active in the field where the particular invention occurs. Consequently, those who are most likely to be relied upon to evaluate new innovation are the very people who are least likely to grasp it's fundamentals or potential.

Such individuals are unable to properly evaluate a new invention because most are simply unqualified for the job. The very essence of invention is projection of technical art beyond the confines of traditional knowledge where ordinary skill and intuition cannot see. Too often experts will fail to comprehend critical elements of a new technology or are too pessimistic in assessing its potential. Prophecies of unattainability can, unfortunately, have a self fulfilling effect. Young technology developed by amateurs who venture into commercial research and development nearly always looks weak and risky.

Progress is slowed by lack of capital and access to facilities. Consulting services are unavailable and the innovator must often learn everything needed to perfect the invention. In no other endeavor is one person expected to master the entire range of skills, from scientific investigation to the construction trades, in order to develop a new technology. To make matters worse, popular myth holds that nothing of value can be developed by an underfunded innovator using inadequate facilities.

The notion that garage inventors are all just a bunch of cranks with nothing important to contribute beyond minor gadgets of questionable value is an unwarrented stereotype. Many enormously successful concepts have been developed by inventive people who were typically underfunded, sometimes ridiculed and usually criticized on the basis of inadequate scientific and technical evidence. Unpredictability of technical progress has historically resulted in delay or even outright loss of useful, innovative and practical devices.

Some of the more subjective aspects of the methods used to evaluate technical inventions bear scrutiny here. The response of practically every organization, when confronted by requests for funding, is to invest only in predictable incremental technology that appears in places where it is expected to appear. There is a demonstrated human propensity to ignore new and innovative concepts, particularly if those concepts do not emerge in a fashion that fulfills orthodox expectations and especially when success cannot be predicted from the existing technological base.

There is no well-documented experience base to help organizations avoid expert bias against new innovation. Unlike nearly every other area of human endeavor, in the area of funding new innovation, little or nothing is learned from past mistakes. The long history of improper and incorrect evaluations of past innovation is not helpful in raising warning flags precisely because it is entirely ignored by most mainstream professionals.

The history of such past failures has largely been suppressed because it is an embarrassing reminder of how poor the track record in this area has been. No one wants to be reminded of a job poorly performed or face a widely available archival record of such repeated failure. The tendency among professionals is to carefully cultivate an air of competence and objectivity while avoiding anything that raises questions of limits to expertise or subjectivism. Thus it is that warnings against risky new technology are loudly trumpeted while the abject failures of such prophesies are quietly filed away and forgotten.

Many professionals will hotly dispute any notion that they may carry the baggage of subjectivism in pursuit of their work. But such protestations ring hollow in light of the track record of some of the more egregious assessments of well-known inventions by experts in the past. Indeed, the attitudes of many technical individuals are shaped by early encounters with negative examples of invention while they were students. Inventors are often portrayed either as fools or charlatans to be proven wrong in textbook exercises. And the popular myth of inventors as wild-eyed kooks in books, movies and the culture at large actively promotes negative stereotypes. Against this background, is it any wonder that objectivity often goes out the window when the time comes to evaluate an invention from an individual innovator or his small company?

To make matters worse, research proposals from inventors for federal, state, institutional and commercial grants are typically evaluated using methodologies that specifically discriminate against them. Armstrong has provided evidence for an "author's formula," a set of rules that those writing proposals can use to increase the possibility of acceptance. (“Barriers to Scientific Contributions: The Author's Formula”, J. Scott Armstrong The Wharton School, University of Pennsylvania Philadelphia, Pa.)

Authors should:

(1) not pick an important problem
(2) not challenge existing beliefs
(3) not obtain surprising results
(4) not use simple methods
(5) not provide full disclosure
(6) not write clearly

Historical Examples of Innovators Unlikely to Succeed

Massive investment in a new technology generally happens only when a concept matures, once refinements have been made and after skeptics have either been convinced or discredited. This is usually very late in the technological development cycle. In the United States, characterized as it is today by short term thinking and investment, new innovation does not usually appear compelling enough to warrant capitalization in the pre-commercial stages of development. Mature products are typically considered safer, and large scale capitalization of established technology occurs just at the point when products based on it are in fact becoming obsolete.

The majority of important inventions and innovations of the Twentieth Century have come from independent inventors and small businesses. (The Sources Of Invention, Jewkes, J. et al., London: Macmillan, 1958) Throughout the history of western civilization, important discoveries have been made by people who did not have a "proper" academic background, adequate means, or both.

Most of these firms and individuals would today be considered hopelessly ill suited for the tasks they set for themselves. All either lacked a track record, acceptable academic background, were chronically underfunded, had no access to suitable facilities or were handicapped by all of these factors.

James Watt, inventor of the first practical steam engine, Pierre and Marie Curie, discoverers of Radium, Albert Einstein, discoverer of the Special and General theories of Relativity, Wilbur and Orville Wright, inventors of the airplane and Chester Carlson, inventor of the Xerox process are among those who would probably have been denied funding today. None of these individuals would be judged to have possessed the track record, facilities, qualifications or means necessary to accomplish what they undeniably did. It is instructive to briefly examine the qualifications and available facilities of the aforementioned individuals.

James Watt, an instrument maker, had no prior experience with engines or any other large machines when he made critically important improvements to the steam engine. As a direct consequence of his work, practical mechanical power sources became available which did not depend upon the location of running water or the use of animals with their inherent limitations. This fundamental change in the availability of reliable mechanical power helped trigger the industrial revolution and reshaped the world.

There was no reliance on large organizations or teams of highly trained individuals to accomplish the fundamental technological innovations which made practical steam engines possible. To the contrary, the resources brought to bear were remarkbly limited for an achievement of such magnitude. Watt's work was in fact based entirely on his own research and technological insight. (“From Watt to Clausis”, D.S.L. Cardwell, Cornell University Press, 1971, pp. 41-50) No responsible evaluator in any current government program or commercial corporation would consider a recommendation for funding a proposal submitted by someone so lacking in experience.

Pierre Curie was a physics teacher at the School of Physics and Chemistry in Paris. After about 15 years of pursuing his career, he earned about the same salary as a skilled factory worker. Not the sort of financial backing that would be considered adequate for a major research effort to discover new natural elements. Marie Curie was a student who had just completed her master's degree. Her training was entirely academic and she had no experience in industrial chemical processing. Any modern assessment of her qualifications would completely rule out any possibility of successfully isolating a scarce and elusive chemical element.

Yet together, these two individuals isolated Polonium and Radium from tons of pitchblende ore in a small abandoned shed. Today we know that this kind of chemical separation requires the services of highly trained specialists in a well run and well financed chemical factory. ("Men and Discovery", Milton A. Rothman, W.W. Norton & Co., 1964) Any knowledgeable evaluator would certainly point out the impossibility of such a research program conducted by two individuals who were undercapitalized, inexperienced and lacking even the most basic facilities.

Albert Einstein was not a child prodigy and further compounded this lack of an auspicious beginning by dropping out of high school at the age of sixteen. He went on to fail the entrance examination to Zurich Polytechnic University. Most educators today would have branded young Einstein as an intellectual failure, and indeed, his German high school teachers told him that he would never amount to anything. He also earned the enmity of his instructors because his questions "disrupted" classes.

While he did finally succeed in getting into the Zurich institution after a stint in a Swiss cantonal school, one of his instructors famously referred to him as "a lazy dog". He could not even qualify for an assistant position, the lowest ranking post-graduate job. After graduating, Einstein was essentially unemployable and barely survived, a marginalized individual if ever there was one. He only managed to obtain work at the Swiss patent office because a friend interceded to have him recommended by a successful Swiss industrialist. ("A Variety of Men", C.P. Snow, Charles Scribner's Sons, 1967, pp. 91-96 and COSMOS, Carl Sagen, Random House, 1980, p. 199)

This then, was the man who revolutionized physics with his ideas of time, space and gravitation. With no "track record", poor academic credentials and limited means, young Einstein would have had absolutely no chance whatever of getting a federal, state or university research grant today. It is very likely that a research proposal from such an individual would have been savagely ridiculed by any "responsible" evaluator since the concepts being proposed challenged the work of Isaac Newton, a well known and highly respected scientist.

Wilbur and Orville Wright never formally graduated from high school and neither attended college. They started the Wright Cycle Company in 1892 and became seriously interested in the problem of heavier-than-air flight in 1899. During the period between 1901 and 1902, the Wrights determined that air tables compiled by Otto Lilienthal and widely accepted as the basis for all attempts to fly were hopelessly incorrect. They experimentally determined the actual values needed to construct a working wing surface, and on December 17, 1903, successfully flew the first heavier-than-air vehicle.

It is virtually certain that the owners of a small, low technology business would never have been seriously considered by any modern technology evaluators for a contract to develop an advanced flight technology requiring extensive knowledge of cutting-edge aerodynamic engineering of the day. It is easy to see the reasoning that would have been applied. How could two "high school dropouts" with no "track record", working out of a bicycle shop, even develop a viable concept much less actually build a working device?

There is no need to speculate upon the outcome of a proposal evaluation, however. The federal government actually had an opportunity to contract with the Wrights at a time, early in their work, when they could have used some assistance. In 1905, two years after they first flew successfully, the Wrights offered an unsolicited proposal for an aircraft to be delivered to the government without a contract. There was to have been no obligation to initiate a contract until after the vehicle had passed a successful flight trial.

A major on the general staff in charge of the Board of Ordnance and Fortifications at the United States War Department, sent correspondence to the Wrights wherein it was stated that the government found it necessary;

> "... to decline to make allotments for the experimental development of devices for mechanical flight."

He went on to state:

> "... the device must have been brought to a stage of practical operation, without expense to the United States. It appears from the letter of Messrs. Wilbur and Orville Wright, that their machine has not yet been brought to the required stage of practical operation."

The federal government responded in 1905 exactly as it would have today. It rejected the proposal. One can see in the words of this historical rejection the rationale presently used to dismiss proposals presented by individuals without “adequate” credentials working under unfavorable conditions.

The United States had a golden opportunity to support development of the only practical flying machine technology in existence at the time. Instead, evaluators were blind to the opportunities the proposal represented and could not even properly consider the proposed research because of their personal and professional bias. In the face of such incompetence, the Wrights licensed their airplane to the French and the United States tragically lost a virtual monopoly on aircraft technology that was to cost the nation dearly in the Second World War.

An interesting counterpoint to this history involves another research program aimed at producing a flying machine which was proposed by a former secretary of the Smithsonian Institute, Doctor Samuel P. Langley. This proposal did receive funding, probably because the proposer had the "proper" credentials and was expected to carry on this type of research.

The original Wright aircraft was exiled from the United States in 1928 by Wilbur Wright as a result of a continuing dispute with the Smithsonian Institute concerning Doctor Langley's work. On the basis of controversial evidence, which was later proven incorrect, the Smithsonian Institute credited the invention of the airplane to Doctor Langley. In 1948, after a war in which the airplane played such a pivotal role, the Wright Flyer was returned to the land where it first flew. ("How We Invented the Airplane", by Orville Wright, copyright 1953 by Fred C. Kelly, David McKay Company, Inc., pp. 3, 10, 12, 13, 42, 64-68, 74)

The federal government is not the only large organization to fail ignominiously when confronted with the task of properly evaluating a watershed concept. The case of the Xerox process and the modern copying machine should give pause to those who believe that corporations are the best judge of the practically, utility and commercial potential of new ideas.

Chester Carlson, inventor of the modern electrostatic copier, had managed with difficulty to work his way through college, attending the California Institute of Technology, earning a B.S. degree in physics. By today's reckoning, he might possibly have met the standards for a low-level entry position in one of the major technology companies. It is doubtful, however, that he would have been considered qualified to receive a contract as the primary director of a project to develop a new method of imaging and reproduction of documents.

Mr. Carlson's invention was reviewed by engineers and technicians in <u>twenty one major companies</u>. All those who bothered to take the time to evaluate the contrivance decided it was unlikely that the process could ever be reduced to an automatic machine suitable for use in an ordinary print shop. Such a device would, by the standards of the day, be very complex and quite large.

Assuming that the technical requirements could be worked out at all, experts further concluded that there would be no market for the machine, since the technique made copies that were inferior to those produced by the existing technologies of offset printing and photostat cameras. The established offset printing process was already beginning to dominate the duplication market at the time Mr. Carlson proposed his concept and the infant Xerox process was thus deemed completely worthless.

Even the Battelle Memorial Institute, which was ultimately pursuaded to support development of the new concept was initially quite skeptical. But in an altogether unexpected turn of events, the skepticism of the many experts who previously reviewed the technology was ultimately disregarded. The concept was finally given an objective appraisal by a member of Battelle's staff who was focused on the technology issues themselves rather than more subjective factors. Unfortunately, this appears to be attributable more to luck than any fundamental organizational competence. Battelle has since proven as lackluster as other major organizations in identifying and funding new innovation.

One has to wonder what modern consultants at Battelle would make of a funding request from Mr. Carlson, had his proposal been made today. Prior negative appraisals by so many industrial corporations and the inventor's lack of a "track record" would have played a big role in any evaluation of a technology review team. Mr. Carlson's facilities would also have been rejected as unacceptable. A "... grubby little room behind an Astoria, Long Island, beauty parlor..." in New York would simply have been seen as completely inadequate for a task that clearly required a first class research facility, such as the laboratories of General Electric, IBM or RCA, staffed by researchers with prior discoveries and academic accolades to their credit.

Carlson's first crude prototype of a Xerox copier, a black wooden box, sits in the Smithsonian Institution's Hall of Photography. A bronze plaque marks the location in the little room where the first copier was built. It is not only a commemoration of the birthplace of a new process, but of a new industry as well. Both the wooden box and the bronze plaque also testify to the existence of something less tangible, but perhaps even more important. They stand as a monument to the fact that the opportunity for individual invention as opposed to team engineering has not become extinct in our era. ("My Years With Xerox, The Billions Nobody Wanted", by John H. Dessauer, Doubleday & Company, 1971, pp. xii, 21, 22, 26, 34, 222, 223)

Countless Xerox copiers operate today in practically every office and print shop around the world, each mutely attesting to the failure of our institutions to recognize innovation. Skeptical professional engineers and marketing specialists were completely unrealistic in their assertions that the Xerox technology would not work and that it would be useless if it did.

Indeed, owners of electrostatic copiers could scarcely conduct business without them. Few print shops today have any equipment other than machines based on the Xerox process. Printing methods like offset printing, which seemed so superior to the process invented by Chester Carlson, have become historical curiosities, unable to compete against the technology experts of the day considered useless.

Conclusion

Anyone who knows the history of innovation would have little difficulty in recognizing the danger of basing a qualification assessment on academic achievement, financial means, track record and facilities. Not only does such a screening process prevent funding of micro-businesses and individual innovators like Chester Carlson and the Wright brothers, but it also results in spectacular failures like the Hubble space telescope, whose optics were initially impaired by a fabrication mistake (spherical aberration) that every amateur who ever built a telescope knows about.

In the former examples, badly needed technical advances are delayed or lost because the individuals or organizations involved in the early development lack the expected credentials and facilities needed to convince the funding agency to provide money. In the latter example, practically unlimited funding is provided because the contractors involved have the credentials and facilities needed to give the appearance that they are capable of successfully accomplishing a proposed project, even if they can't.

The most important criteria for success in innovative pursuits are conspicuous by their absence in modern technology evaluation methodologies. Insight and the will to succeed are more important than academic prowess, prior success or large laboratories. To quote Einstein, "Imagination is more important than knowledge." A flawed technological review system is presently being applied by funding organizations and practically guarantees that innovators outside the mainstream research population will never receive any funding.

Those who conduct evaluations of innovative proposals are often accomplished individuals who possess business, scientific and engineering backgrounds placing a premium on everything the small innovator typically lacks. An evaluator with such qualifications will tend to support safe, predictable incremental research conducted by individuals who have backgrounds similar to their own.

Revolutionary research characterized by radical and "risky" concepts proposed by micro-companies and maverick innovators will continue to be ignored until such flawed evaluation practices are changed. Historical facts tend to suggest that the innovator with a successful breakthrough invention will not meet the expectations of an evaluator applying biased review principles and will therefore not receive a truly impartial review.

An evaluator who opposes funding for a radical concept presented by an individual or small organization having limited resources and an unusual background takes no risk. If he supports such funding, his credibility is "on the line". Consider what will occur, should a funded proposal fail to yield the intended results. If the funding was awarded to an individual with the "proper" background, or a "sophisticated" organization with "adequate" resources meeting the usual expectations for conducting such research, the choice will draw little comment. If a contract was awarded to an atypical individual or organization, a chorus of voices will be heard demanding an investigation into an "obvious" misuse of funds. These factors all but guarantee that proposals submitted by small businesses and garage inventors will never be funded.

The scale of the loss of innovative concepts as a result of the problems just noted is staggering. Many innovators simply give up in disgust or fail to continue pursuing their innovations due to a lack of resources. Those instrumental in preventing funding of such concepts point to the prevalence of such ultimate failure as “proof” that most new concepts are flawed to begin with.

This notion is not only wrong, but ignores the dynamics of innovation. Many presently accepted concepts and technologies in existence today would not ultimately have been successful were it not for the lucky failure of flawed early evaluations to terminate their development. These inventions are monuments to the perseverance of innovators working against substantial odds who won through to the final triumph, often at great personal cost.

Alas, the only way an inventor can ensure the success of an innovative concept is to develop it entirely without any help. Luck is, unfortunately, far more important in raising development capital than any other single factor. The success of new innovation comes in spite of what society does rather than because of it. This is a searing indictment of our present civilization. Those who inhabit the future will wonder in amazement at our profound ignorance in these matters, just as we look in askance at those who opposed progress in the ages before our own. History, it seems, is not without its ironies.