On The Frontier

Views From The Leading Edge

THE FUNDING DILEMMA:

GARAGE INVENTION VERSES LABORATORY RESEARCH

Richard G. Parks

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 appears 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 companies 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 projects, especially those 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.

Such cognitive bias is the result of convictions held by an individual that results in a situation where personal subjective factors take precedence over factual elements of the technology.

The personal beliefs of evaluators often overrides pertinent factual considerations and typically leads to highly negative evaluation outcomes, sometimes even unwarranted personal attacks on the innovator. Condemning the innovator rather than actually evaluating the technology is one of the oldest tactics used for dismissal of new ideas.

Under such circumstances, the evaluator substitutes personal opinions or widely held industry viewpoints for scientific facts and actual conditions, while ignoring reasonable potential outcomes of the technical program under consideration.

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 concept are cast as insurmountable, typically reinforced by observation by the evaluator that success in the area of the proposal has never before been achieved.

The fallacy here is that at one time every technology we have today was cast as unattainable. As Carl Sagan once quipped, absence of evidence is not evidence of absence.

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. As Sacherman observes:

> 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 primary 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 its fundamentals or potential.

Such individuals are unable to properly evaluate a new invention because most are simply unqualified for the job. The 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 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, a widely held popular myth among many professionals holds that nothing of value can ever be developed by an underfunded innovator using inadequate facilities. This nonsense applies with peculiar force when it comes to evaluating projects of a micro-business or garage start-up.

The notion that such inventors are all just a bunch of cranks with nothing important to contribute beyond minor gadgets of questionable value is a patently unwarrented stereotype. Inventive people operating under less than ideal circumstances have developed many enormously successful concepts.

Such individuals are typically underfunded, sometimes ridiculed and usually criticized on the basis of inadequate scientific and technical evidence. Unpredictability of technical progress, especially when it is originated by amateurs, 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 knee-jerk response of practically every organization or professional 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.

Unfortunately, there is a lack of recognized documentation of past errors in evaluating new inventions available to help organizations avoid expert bias. Unlike nearly every other area of human endeavor, in technical evaluations of innovation for funding, 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. Nor are these mistakes taught in technical or business schools.

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, avoiding questions about limits to expertise or the presence of personal subjectivism. Thus it is that warnings against risky new technology are loudly trumpeted while the abject failures of such prophecies are quietly filed away and forgotten.

Many professionals will hotly dispute any notion that they may carry the baggage of such cognitive bias 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. Many textbooks and lectures often portray inventors as either fools or charlatans to be proven wrong in educational academic exercises.

And let’s not forget the widely held popular characterization of inventors as wild-eyed kooks in books, movies and human culture at large. These tropes actively promote negative stereotypes of innovative people.

Against this cultural background, is it any wonder then that objectivity 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.

Much of this is a result of the way these grant programs are developed. Bureaucrats administering technology-funding programs typically call on academics and other professionals to assist in establishing requirements for evaluating new technology.

The problem with this approach is that many of these individuals are highly conservative individuals with long experience in the rigid and archaic peer review process. In recent times peer review has come under criticism for personal bias, character assassination, subjectivism and other terrible shortcomings. While some dispute the existence of such cognitive bias, there is growing evidence for it.

More and more people are beginning to notice the discrepancy between what evaluators of new technology say they are doing and what is actually happening. Indeed, papers published by Sacherman and Armstrong are damning in this regard.

Armstrong, for example, 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.)

According to Armstrong, 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 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 safe from an investment standpoint. But large-scale capitalization of established technology actually 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. (Technological Innovation, Its Environment and Management, U.S. Dept. of Commerce, 1967, Pg 17)

Throughout the history of western civilization, people who did not have a “proper” academic background, “adequate” means, or both have made important discoveries.

Most of these firms and individuals would today be considered hopelessly ill suited for the tasks they set for themselves. In many of these cases, principal investigators 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.

It is instructive to briefly examine the qualifications and available facilities of the aforementioned individuals. None of these people would be judged to have possessed the track record, facilities, qualifications or means necessary to accomplish what they undeniably did.

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, in this instance, no reliance on large organizations or teams of highly trained individuals to accomplish the fundamental technological innovations that made practical steam engines possible.

To the contrary, the resources brought to bear were remarkably 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.

And 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 be quick to 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. Even worse, 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 much more 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 other scientific authorities that were widely accepted as the basis for all attempts to fly were hopelessly incorrect.

They experimentally discovered the actual values needed to construct a working wing surface, and on December 17, 1903, successfully flew the world's first heavier-than-air powered 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 subjective 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 for a powered flyer, much less actually build a working device?

There is no need to speculate upon the outcome of such 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. Moreover, one can see in the words of this historical rejection the same rationale often used today when dismissing 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 powered 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 project because of their subjective 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 failure was to cost the nation dearly in the first and second world wars not long afterwards.

An interesting counterpoint to this history involves another, more “respectable” research program aimed at producing a powered flying machine. This saga has been thoroughly documented and stands as a stark refutation to critics who dispute the occurrence of scientific bias.

A former secretary of the Smithsonian Institute, Doctor Samuel P. Langley, proposed a project to construct a “flying machine”. This proposal did receive funding, mainly because the principal had the "proper" credentials and was expected to carry on this type of research.

On the basis of controversial evidence, which was later proven incorrect, the Smithsonian Institute credited the invention of the airplane to Doctor Langley. This occurred even though Dr. Langley never successfully flew any of his man-carrying prototypes in contrast to the successful flights of the Wrights.

Wilbur Wright exiled the original Wright aircraft to England from the United States in 1928 as a result of a continuing dispute with the Smithsonian Institute concerning Doctor Langley's work.

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 large 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 investigator in a project to develop a new method of imaging and reproduction of documents.

Engineers and technicians in twenty-one major companies reviewed Carlson’s invention and all of them denied funding for the infant electrostatic copying technology.

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, even if successful by the standards of the day, be very complex and quite large. Further, evaluating experts all concluded that, even assuming that the technical requirements could be worked out, there would be no market for the machine.

This latter criticism was justified on the basis of the fact that Carlson’s technique made copies inferior to those produced by established offset printing and photostat cameras.

Moreover, the existing offset printing process was already beginning to dominate the duplication market at the time Mr. Carlson proposed his concept. The infant Xerox process was thus deemed completely worthless.

Even the Battelle Memorial Institute, which was ultimately persuaded to support development of the new concept, was initially quite skeptical.

But in an altogether unexpected turn of events, 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 most other experts had applied.

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.

It is certain that prior negative appraisals made by respected 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.

The leading laboratories of the day like General Electric, IBM or RCA, staffed by researchers with prior discoveries and academic accolades to their credit would have been seen as the only venue for development of this kind of technology.

Today all these firms are either gone or are greatly shriveled due to an inability to recognize radical innovation of the kind represented by Chester Carlson's invention.

Carlson's first crude prototype of a Xerox copier, a black wooden box, now sits in the Smithsonian Institution's Hall of Photography. A bronze plaque marked the location in the little room where the first electrostatic copier was built.

This memorial not only commemorated the birthplace of a new duplication 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 copiers based on the Xerox process could scarcely conduct business without them. Few print shops today have equipment other than electrostatic copiers.

Methods like offset duplicator printing and photostatic cameras that seemed so superior to the process invented by Chester Carlson have become historical curiosities. They couldn't compete against the technology which experts of the day considered worthless.

Conclusion

Anyone who knows the history of innovation would have little difficulty in recognizing the danger of basing a funding 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.

The former examples demonstrate that badly needed technical advances can either be delayed or lost entirely.

Individuals or organizations involved in early development of breakthrough technology often lack the expected credentials and facilities needed to convince the funding agency to provide money.

In the case of the Hubble Telescope debacle, practically unlimited funding was provided for development. The contractors involved had the credentials and facilities needed to give the appearance that they were capable of successfully accomplishing a proposed project, even if they ultimately couldn'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 many funding organizations and practically guarantees that innovators outside the mainstream research population 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 evaluators applying biased review principles previously noted 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 they do not recommend funding. On the other hand, if they support such funding, their credibility is "on the line".

Consider what will occur if a funded proposal fails 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.

But if such 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 micro-businesses and garage inventors will never be funded.

The scale of 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.

Even worse, those instrumental in preventing funding of such concepts point to the prevalence of failure to obtain funding as “proof” that most new concepts are flawed to begin with.

This notion is not only wrong, but ignores the dynamics of innovation. Presently accepted concepts and technologies today would not ultimately have been successful were it not for the lucky failure of flawed 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.

One is far more likely to secure funding for a new invention by buying a lottery ticket than by convincing a funding organization to back the new idea. Success for new innovation comes in spite of what society does rather than because of it. This is a searing indictment of the way our present civilization deals with innovation from unexpected quarters.

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.