Jeff's View

The risks of playing safe

When I was science advisor to the Swiss government, my friend Walter sent me a bulky document that looked like the manuscript of a book. I wondered where Walter had found the time to write his opus, but then noticed with dismay that it was the application for a network grant which Walter had put together for himself and a dozen other colleagues. Most of it was financial charts, time-tables, CVs, organizational details, and the usual statements from the institutional officials; less than half of it was about science. Walter, a stellar biologist and highly creative mind, must have spent untold hours putting together this monster. Why on earth would our National Science Foundation waste supercomputers on adding up grocery bills? Something was out of whack.

Walter's case is not unusual. Wherever I look, the public systems for science funding are drifting off course. They should stimulate novel discoveries, but increasingly encourage short-term pedestrian research. If you apply for your first research grant and dare to venture into new territory, you probably will not get funded because you *lack prior experience*. (Apparently you are supposed to continue what you did as a postdoc.) If you do have the experience and propose to tackle an ambitious and risky problem, they will damn you for *going on a fishing expedition* and advise you to *be realistic* – to do experiments that are bound to work. And if you get funded, you will probably have to apply for renewal within less than two years. This means that you will have to beef up your Progress Report with results you already had before you applied for the first round – or you will be labeled *unproductive*. And you will have to lie. You will have to promise practical benefits that you do not really believe in; you will have to present detailed research plans, time tables and sometimes even *milestones* that violate everything you know about the uncertainty of innovative basic research. If you want to survive, you must play along with the subtle corruption of the system. Your price will be disillusionment and cynicism – and ours less scientific innovation. As you probably have your best ideas while you are young, your first years of independent research are particularly powerful engines for scientific innovation. Our systems for funding science make this engine stutter and also undermine your honesty and enthusiasm. They force you to divert too much of your time from research to writing grant applications, particularly if you are still young and lack secretarial help and collaborators. To paraphrase Albert Einstein, our granting agencies have perfected the means while confusing the goals.

How did matters get that far? A major culprit is the aversion to risk. We Europeans are probably world leaders in zero risk mentality, but we no longer hold universal patent rights on it. This mentality rears its ugly head whenever organization or administration balloons out of control. In trying to prevent waste of public funds, many of our research funding systems are obsessed with preventing failures, unexpected problems, surprises and exceptions. But failures, unexpected problems, surprises and exceptions are at the very heart of scientific research. Research is an expedition into the unknown – that is why it is so exciting. A funding system that over-regulates research in the name of efficiency saps creative potential and collective wealth. It wastes the very resources it should protect.

The urge to control and predict research can grow bizarre flowers. When completing the final report for one of my grants, the form sheet asked me *Did you obtain the results you expected?* What a cheek! I was tempted to answer *Of course not!*, but then thought better of it and resignedly typed in *Yes*. Let somebody else be the hero.

Other innovation systems are not as timid – look at the biotechnology sector. In spite of all its hype and flops, it is one of the most dynamic and innovative activities in today's life sciences. But it is not for the faint of heart, because the time from discovery to market is long and most start-up companies go belly-up within the first few years. Yet the system prospers because the few winners more than make up for the many losers. Venture capitalists know that investing in the early phase of a start-up company increases not only their potential pay-off, but also their risk. They take it for granted that profit and risk are Siamese twins. Nature knows it, too; if it had been bent on avoiding risks during the evolution of life, we would all still be bacteria.

Every decision to hire a young scientist or to fund a research project is a calculated risk. But taking risks is not the same as being reckless. Anyone who takes risks without professional know-how, experience and good judgment will have to pay a steep price. Our present *Peer Review* system – in which a group of experts judges the risks and the potential of individuals or research projects – is essentially a device for scientific risk control. But this device has started to fail because we force it to make unreasonably stringent selections. When there is only money to fund one out of ten research projects, the "no" of a single committee member is usually deadly and even a competent and fair-minded group of peers will hand down erratic judgments. Like any risk control device, *Peer Review* also selects against the exceptional. It is a *Great Equalizer.* It encourages blandness and selects against novel ideas that challenge accepted dogma.

The leveling influence of *Peer Review* is not limited to science, but also impoverishes the performing arts. Today's young singers or instrumentalists who aspire to international fame must win international competitions in which a jury of experts picks the winners. *Peer Review* again. A brilliant young pianist confessed to me that in these competitions he never played the way he felt, because a highly individualistic interpretation was bound to rub one of the jury members the wrong way. Peer review was forcing him to play up to a generic taste and to adopt a bland style that was least likely to displease. No wonder that concert performances around the world have become so stereotyped. If creative activity is subjected to Darwinian selection by groups of experts, the result is often timidity, standardization and mediocrity.

Zero risk mentality reflects a lack of courage, the key ingredient of scientific success. Success in science depends on many factors – intelligence, perseverance, talent for leading and inspiring others, organizational skills – but none of them outranks courage. It takes courage to face the grueling selections of academia,

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to choose a difficult research problem, or to challenge an idea everybody takes as dogma. Anyone who wants to discover new springs must dare to swim against the current. Because many of our research funding systems lack courage, they hide behind numbers and try to quantify the risk of a basic research project, or of a scientific career, as precisely as possible. They are hooked on *Citation Frequencies*, *Impact Factors*, *Grant Scores* and *University Rankings*. More and more, these phony numbers now decide hirings, promotions, and the flow of research funds. Their ''precision'' satisfies everybody's longing for *objectivity* and *transparency*, but nobody seems to care a hoot about the questionable methods by which these numbers are concocted. And once these numbers have escaped from Pandora's Box, nobody can put them back in.

University rankings are particularly obnoxious, because newspapers love them even more than administrators and politicians do. ''WORLD-WIDE RANKING OF UNIVERSITIES PLACES UNIVERSITY A AS *NUMBER 1* AND UNIVERSITY B AS *NUMBER 2*''. It is all so neat, everyone can understand that. And if it rankles you that your own university is only NUMBER 67, it feels good to know that the university nearby is only NUMBER 69. But how can one possibly rank institutions as complex as universities? Adding up Nobel prizes, impact factors, and outside grant money may make some sense with the natural sciences, but what about the humanities? Their research grants are usually small and they tend to publish their best work as books for which impact factors and citation frequencies are not available. Who cares? Most rankings simply ignore the humanities. It always amazes me that those ranking gurus get away with it. In their intellectual universe, Friedrich Nietzsche, Baruch Spinoza, Bertrand Russell or Ludwig Wittgenstein would just be invisible black holes. And is a university with twenty departments of average quality better or worse than one with eighteen bad and two truly outstanding ones? The choice cannot possibly be ''objective''. But the ranking bandwagon keeps on rolling and is seriously distorting our universities. Many of these now try to attract professors who bring with them the most Brownie points for the next round of ranking. And the humanities are scrambling for the electronic limelight by publishing their work piecemeal in highly specialized journals. The result is predictable: the number of journals explodes and the importance of papers erodes. It may rhyme, but makes no reason.

Our science enterprise is caught in a seemingly insoluble dilemma. On the one hand, too many scientists are chasing after too little money, and deciding who should get what is becoming ever more crucial – and difficult. On the other hand, the unprecedented mandate and power of the decision machinery have made this machinery too complex, too costly, too conservative – and often too arrogant. There is no simple way out and it would be naïve to hope for *The Solution*. Each country does – and should do - things a little differently, and forcing all funding systems to work the same way would do more harm than good. Moreover, I do not see any credible alternatives to *Peer Review* of individuals, research projects, and institutions. For better or worse, we are stuck with this instrument, but we should use it more cautiously and with a keener awareness of its problems and limitations. Here are four simple suggestions. First, funding agencies should keep in mind that more organization and control do not necessarily mean better science, and that every page researchers must write or fill in cuts into scientific productivity. We biologists want to get the Nobel Prize for Physiology or Medicine, not that for Literature. Second, each country should make sure that its researchers can apply to several different funding agencies, because monopolies are as harmful for research funding as they are for the general economy. Third, evaluation committees should rely more on scientific expertise and intuition than on ''objective'' indicators and should aim for a healthy balance between the true and tried, and the innovative and risky. Fourth, we scientists should rid ourselves of the arrogant notion that managing science is best left to the dim-witted. Unless we all roll up our sleeves and put more thought and effort into shaping funding policies, our fragile *SS Science Enterprise* will get crushed between the Scylla of dwindling funds and the Charybdis of computerized evaluations.

I have probably spent as much time on review panels, prize committees and advisory boards as most of my colleagues and have profited a lot in the process. Evaluating the research of others has widened my scientific horizon and made me aware of the aleatory forces that can shape a scientific career. But usually I learned more about my colleagues on the committee than about those I was supposed to judge. The last committee I served on was truly superb and motivated by the best of intentions, yet in retrospect I realize that with time we started to overrate ourselves and became subconsciously arrogant. Because we respected and liked one another, we also slipped into the habit of avoiding disagreement. *Where all men think alike, no one thinks very much* noted Walter Lippmann, and I suspect that he scribbled it during a committee meeting. I no longer serve on evaluation committees, but if I did, I would urge all committee members to read some of the grossly erroneous judgments which artistic and scientific giants such as Robert Schumann, George Bernard Shaw, Eduard Hanslick, Robert Virchow, Otto Warburg, or most of the official art critics of around 1900 passed on the ideas or achievements of some of their contemporaries. My favorite example is how Ernest Rutherford, one of the greatest physicists of all time, dismissed the possibility of nuclear power generation as ''pure moonshine''. Peers peer into the future with nearly as much error as everybody else. And there is no obvious reason why they should be especially qualified to judge human creativity. Perhaps nobody can. But as long as we cannot avoid judging others, let us do so with caution and a healthy dose of modesty.

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