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Effective funding for research

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Here is an anecdote going around in scientific circles: Once God applied to the National Science Foundation for a research grant to do some more work on creation. His application was rejected for the following three reasons. One, it was a long time since He had last worked on this subject. Two, nobody else was able to repeat His work, and three, His work had been published not in a reputed referred journal but only in a book.

This anecdote highlights the extremes to which sometimes peer reviews go in killing a promising proposal. Some or all of the above three reasons are given. The first reason points out that the author of the proposal had been inactive for a long time and, hence, had lost his competence to do research. The second reason iterates the repeatability of a scientific experiment. Credibility in an experimental result grows if many independent experiments lead to the same result. The third reason stresses the need to publish one's results in a journal that subjects all its articles to rigorous vetting by referees.

By and large these criteria are useful to committees allotting research funds to scientific proposals. These days, research costs money, sometimes enormous quantities of money. In the 1930s, Earnest Rutherford did pioneering experiments to probe the hitherto untouchable nucleus of an atom. The cost of his equipment made in the lab was no more than a hundred pounds.

Today's particle accelerators trying to probe particles run into billions of dollars. Particle physics, astronomy, nanotechnology, biology and biochemistry, name any field of science, the frontier level research costs money. And if the amount to be disbursed is limited and insufficient to meet all demands, some selection process must come into play. This is where panels made of peers sit in judgement to decide. And the same criteria apply when a research paper is submitted to a reputed journal. Peers decide if it is worth publishing.

While all this seems reasonable and logical, the system has its weaknesses, with the result that committees may end up throwing away the baby with the bathwater. For, one consideration that weighs heavily with committees is that the proposal should be reasonably
consistent with what we already know. By imposing this requirement, they try to filter out all cranky stuff. Again, this seems reasonable, but look what it would have done to a few ideas that were new at the time they were proposed.

Einstein's special theory of relativity would have been rejected by a research journal today as cranky. Hubble's discovery that the universe is expanding would also have been

viewed with suspicion. Works by Heisenberg, Schrodinger and Dirac on quantum mechanics would have been considered crazy. For, all these researches were out of tune with the physics and astronomy known at the time. Yet, they got published and eventually recognised by the community, largely because the criteria for publication were not as restrictive in the first half of the twentieth century, as they are today.

One reason for this is that money did not enter the picture in as critical a way in the olden times. After World War II, scientific research went into a high gear and its growing cost made money important. The European nations, which had hitherto led the scientific quest, now fell behind as the land of hope and glory — the United States — could attract the cream of research talent to its shores. As a result, scientists have landed themselves in a situation where only the 'safe and proven' areas would be supported and anything outlandish and crazy-looking will be shown the door.

While one may justify this approach as making an effective use of public money, one may question whether it is the best way of doing so. The ultra caution rules out discovering something unusual that nature has hidden so far from the scientist. The spirit of curiosity that has led science to the present level of progress is being stifled with this extra caution. That is why I would like to see something by way of venture funding for really new ideas, if scientists of demonstrated ability propose them. This fund may be no more than 10 per cent of the total kitty, but it will still be effective.

Today, when a group of scientists proposes to build a big observing facility, it has to specify what it expects to observe with it. Contrast this need with the episode narrated by the Nobel Laureate astrophysicist Chandrasekhar. Two great scientists of 1930s, Edwin Hubble and Arthur Eddington were briefing the press on the new proposal to have a 200-inch aperture telescope for studying the universe. When asked, as to what they expected to find with it, the scientists replied: "If we knew the answer there was no need to build the telescope."

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