

From: Dr. Fred Sachs [sachs@buffalo.edu]
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To: Holland, Michael J.
Subject: Research evaluation

If we knew what was to pay off, we would only do that kiind of work. To paraphrase Einstein, "If we knew what we were doing, it wouldn't be science". There is a terrible hazard of coming up with criteria to evaluate the significance of research. Nearly all important ideas have been off-the-wall findings. These are approaches and ideas that no one would fund. It is generally agreed among scientists that their most important results are those obtained without explicit funding; results obtained by using the mooney from funded projects to try things that were not included.

Successful grant applications, particularly those to NIH, require that the proposed experiments can't fail. Since 3/4 of all submissions must be rejected according to current funding standards, the review panels look for any excuse to turn down an application. A possible failure is a sure fire trigger to lose. This of course makes applicants into liars, because they must say that the most important and interesting things to do are those that can't fail. These are inherently the least interesting experiments. That is not to say that these boring measurements are flawed, but any exeriment that can't fail is not an experiment at all, just a measurement.

There are too many grants being funded because applicants fear a gap in funding, and hence become application witters instead of full time scientists. If the number of grants/PI was limited (perhaps to two), then no one would spend time writing additional proposals, and hence would get down to work. If the funding rate was higher then scientists would not feel the necessity to keep writing applications to avoid academic death due to a lack of research funds. The government advisory groups should realize that most schools have turned over tenure decisions to granting agencies: those that are funded get tenure, the rest don't. This leads to ageing of the scientific community as young scientists are driven off by the pressure. The average age of applicants obtaining their very first grants has risen to about 36. Loss of a single grant can lead to the untenured scientist being fired. This doesn't make for creative science. It rewards the least interesting and hence the safest research. Young people are the ones with the new ideas, and they need to be supported.

The conflicts of funding "powerful" NAS members, etc, with large laboratories costs the access of young people to research. To my knowledge, there has never been a study of the value of research/\$ in large and small labs. My prediction is that small labs where the PIs are intimately involved with the ongoing research are much more efficient. I suggest such a study of efficiney of labs size vs. productivity/\$ is in order before changing priorities.

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