From: Donna & Stephen [donnastephen@optonline.net]
Sent: Friday, October 03, 2003 5:01 PM
To: NSTC_RBM
Subject: NSTC - support for research

hi people,

I write as a member of the public - and not even a citizen, just a legal permanent resident. I don't wish to address any general policy issues, practices, etc, but would make the following set of observations.

1. Any normal, plausible or feasible research field will have funding priority over what one might call "weird science" or over science whose applications seem very remote if practical at all.

2. It is true that the vast majority of "weird science" should not be funded, and the proponents thereof are not necessarily effective in ordinary society either.

3. However, there are some speculative, apparently purely hypothetical ideas, or apparently irrelevant research that originate from mainstream scientists and mainstream science that turn out to be valuable either to advance theory or in much later applications. Instances where the ideas ran far ahead of the practical applications: holography - 1947, well before lasers; research into oxygen-halogen reactions - which reactions occurred so quickly and seemed so irrelevant to ordinary chemistry that almost no-one cared - except that work formed the basis of understanding anthropogenic effects on the ozone layer, many years later, etc etc. I'm sure that you all can think of things along these lines.

But ask yourself - of all these now useful or relevant ideas, how many would have been granted funding under the current environment, or indeed under many of the submissions you will received ?

4. Accordingly, I suggest the following:

5% (or some other appropriate figure) of the total research budget should go on projects that either appear probably unfeasible or lack any observable practical applications.

5. The committee to oversee and approve these projects should be established mainstream scientists who are known for their multi-disciplinary contributions - generalists tend to be better at identifying and assessing the long-term potential, I think, and tend to be less orthodox in their approach.

6. Obviously, no perpetual motion machines or Casimir-effect drives and the like get funding, but any decent scientific committee can sniff such projects out immediately.

7. The greatest young scientists tended to have unconventional ideas, by definition - such a proposal would allow them the luxury of thought, and that in turn would maintain or restore (you decide) the highest intellectual prestige of US scientific research.

regards

Stephen R Gould