Comment

A piece of the action

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We lost another one last week. A bright young assistant professor in his first few years as an independent faculty member at a university e-mailed me to say that he was leaving academic science. He wasn't leaving because he wanted a bigger salary, or because he hated his job - quite the contrary; this is a man who loved what he was doing passionately. He was leaving because he had been unable to get funding for his research. This happens, of course, to people with bad ideas, or no ideas, but I don't think he fits either of those cases. I've seen his research proposals and I think the ideas were imaginative, even exciting. But they were not particularly fashionable and they were not sparklingly written, and he was not working at a powerhouse research institution. So we have lost a promising scientist and we have also lost the work he might have done, at least until someone else comes up with the same ideas. And even then, given the conservatism and trend-worship of the funding agencies, who knows if that someone else will have better luck? All of us know of cases like this, and many of us harbor the belief that there are many excellent ideas that never see the light of day because they can't get funded.

At the same time, in the USA, Congressional supporters of biomedical research are hoping to improve on President Bush's proposed $2.8 billion increase in the budget of the National Institutes of Health (NIH), the major supporter of such research. They are seeking a 16.5%, or $3.4 billion, increase, which would bring the total NIH budget to $23.4 billion in fiscal year 2002. This would continue the ongoing effort to double the NIH budget over the period 1999 to 2003. Officials of NIH, awash in new funding, are actively seeking ideas for how to spend this largess. Afraid that they will have too little in the way of spectacular results to show for such spectacular funding increases, they are throwing money at genome-wide projects designed to accumulate reams of data, while not increasing the number of individual investigator-initiated projects that are funded, at least not at anything like the same rate.

So on the one hand we have an embarrassment of riches, while on the other we have people who leave science for want of a small amount of initial funding. To complicate the picture further, genomics is changing the scale of funding that investigators need. Driven by genomic discoveries and the cultural change they are creating, biology is becoming Big Science. To do front-line biology research increasingly requires access to expensive technology such as cDNA microarray facilities, mass spectrometry, mouse genetics and so on. New investigators at small universities, whose start-up packages barely cover setting up a modest lab and paying a technician or postdoctoral fellow for a year or two, are having increasing trouble getting their programs off the ground, let alone competing in this climate.

I have an idea that may help address these problems. It offers a radical and, I think, creative use for some - not even all that much, actually - of that additional funding. It helps level the playing field between large institutions that can give generous start-up packages and small ones that can't. And it gives beginning investigators at all institutions a chance to try out their most innovative ideas without the burden of first having to acquire preliminary data, even when their ideas are completely out of fashion.

The idea is this. Last year NIH awarded research grants to principal investigators at 485 degree-granting institutions in the USA. Since some of these were educational grants to non-research universities, let's say there are 400 PhD-awarding institutions in the biomedical sciences (the exact number doesn't really matter for this discussion). I propose that three starting faculty from each of these institutions, each year, be designated NIH Biomedical Beginning Investigators. Every beginning investigator will then be given a research grant of $175,000 each year for the first three years of his or her career. No proposal is needed and no review will be undertaken. Small and large schools will be treated alike. If an institution doesn't have three starting faculty members in one year, they may carry over those slots until they are filled, but no institution may have more than nine such investigators at a time. Thus, at steady state, the program will be funding up to nine people each year at 400 institutions at $175,000 per year, for a total outlay of $630 million. That is only 3% of the total NIH budget, and it is less than 20% of the requested increase in that budget for this coming year. Each year, 1,200 investigators will leave the program and up to 1,200 more will enter it.

A few more important rules must be noted. The money may not be used to pay any portion of the investigator's salary. Not a penny of institutional overhead (affectionately known as 'indirect costs' in the US) maybe taken out of the grant. Apart from these two restrictions, the money can be used for any research-related purpose. If the investigator wishes to save the money for the first two years in an interest-bearing account and then buy a big piece of equipment at the start of year three, that is fine. No yearly accounting will be required. At the end of the third year the investigator must submit a two-page summary of the work that was supported, including a list of any publications and funding that has been garnered because of the results. This information will be collected and summarized, but will never be used in any critique of the investigator by any funding agency.

To those who would charge that the program spends money foolishly, without regard for quality, I would answer that this is exactly the point. Quality judgements remain in research funding, in the regular research grants program, which would still account for more than 95% of the total budget. This proposal is meant to do something very different. It is meant to give the maverick, the geographically-restricted, the late bloomer, or the gremlin from left field, the opportunity to have something they do not have now: a piece of the action. We have long wondered whether, 'out there' somewhere, there may not be scores of great ideas that never see the light of day for want of initial funding. Now we can find out. It hedges our research bets, corrects for possible oversights that we may make in the peer-review system due to ignorance, laziness, rigidity and faddishness.

My proposal is directed at the largest US funding agency but there is no reason that a version of this program cannot be implemented at smaller agencies such as the US National Science Foundation, which funds non-biomedical research, and at equivalent agencies in other developed countries. If so large-scale a program is impossible elsewhere, even a smaller version of it will still help deal with the same problems, which are endemic everywhere that governments support scientific research. The exact number of investigators and institutions is not crucial and can be adjusted to fit the available resources.

If we do this, maybe there won't be so many e-mails like the one I just received. And to those who would charge that throwing money at large numbers of investigators just to see what will turn up is a betrayal of our tradition of peer-reviewed funding, I would answer that it betrays nothing. It merely seeks to remedy a few of the defects of that system. Besides, isn't just seeing what will turn up exactly what the current data-acquisition program triggered by genomics is all about?