

Comment

Sweden has the right idea

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Of the many tasks that I do that are not directly connected with teaching, research or column-writing (including reviewing manuscripts and grant applications, sitting on various advisory boards, miscellaneous administrative chores and so on), the one that has given me the most satisfaction in recent years is one I do for the Swedish Foundation for Strategic Research. It involves reviewing applications for their Senior Individual Grants. I don't know anything quite like this grant program anywhere else, but I know a lot of places, including the USA, that could sure use one.

The first Senior Individual Grants competition was announced in 2003. Its stated purpose was to enable senior scientists with a strong track record to take up new challenges. Applicants had to be working in Sweden, in the research areas supported by the foundation (natural science, engineering and medicine). The goal was to enable these established investigators, in their 50's and 60's, to renew their research and explore new ideas, preferably of a cross-disciplinary, integrative nature. There was also an understanding that the proposed research should be of some clear benefit to industry and society in Sweden.

The competition had two phases. In the first phase, 18 grants, each of approximately €165,000 or \$192,000 would be awarded for a period of one year, to enable the grantee to be relieved from ongoing tasks and assignments and to develop, primarily through time spent working at another institution, an entirely new research direction. In other words, the grant was essentially a fully funded sabbatical with that mission.

Phase two kicked in a year later, when the foundation announced a number of additional grants, each worth approximately €660,000 or \$770,000, for further research activities over three years. Only those 18 grantees from the first stage were eligible to apply. The purpose of the second grant was, of course, to make it possible to turn the ideas

developed during the previous year into a completely new research program. Twelve awards were eventually made at this stage, meaning that a senior scientist who passed the first phase of the competition had a 67% chance of getting the additional three years of support. The combination of a generous amount of money plus excellent odds of receiving it ensured that the program would attract the attention of a large number of senior scientists.

Applications were reviewed by both internal and external referees. I was one of the external group, and the foundation allowed me to follow the process all the way through - that is, the applicants whom I had reviewed in phase 1 and who were awarded one of the 18 planning grants were also sent to me for review for the phase 2 competition (along with a few I hadn't seen before). Thus, I got to see exactly how these scientists used their year of rethinking their research, and what projects they now intended would come out of it.

Talk about fascinating. The subject matter ranged from systems biology to nanotechnology. Nearly all of the applicants I reviewed at the first stage were distinguished scientists with international reputations, but it had to be said that most of them seemed to be on what I would call the downward part of their careers. They were still publishing, but generally doing things very similar to what they had been doing for more than 20 years. They mostly weren't working at the cutting edge any more because the cutting edge had moved away into other areas. In short, they were at the stage of their scientific lives when many researchers find themselves unable to sustain the level of excellence they once displayed, and tend either to keep repeating themselves or slowly wind down into irrelevance. Yet, given the chance to come up with something new, a significant number of them managed to find - through time spent in other labs, usually in other countries - creative and important new ideas to work on. In most cases this amounted to a significant change in scientific direction, and in many it represented a shift to a

whole new field. Some of the ideas were mundane, but most were not, and many were highly imaginative.

Why hadn't these researchers done this before? The answer, I think, lies in the way science is supported. Once you have established yourself as a young scientist, the conservative funding system, which tends to prefer giving money to things that seem likely to work rather than to things that are innovative and therefore risky, rewards those with a track record so long as they continue to do the things they have a track record in. Try working in a new area and you will often be discounted as overly ambitious (read, 'naive') or unfocused (read, 'straying too far from your own turf'). Anyone with a new idea faces these problems, of course, but they're particularly acute for the middle-aged scientist. Science is seen as a young person's game, and there is an unspoken expectation that senior researchers - and often the fields they work in - should slowly be put out to pasture, leaving the racetrack for the colts and fillies.

I think the notion that science belongs to the young has a lot of truth in it, but I also don't think it's the whole story. Studies of the aging brain have shown that, while younger minds consistently trump their elders in situations that call for fast reactions and cleverness, older people do better in tasks that require wisdom and experience. Since creativity often involves many of these skills, it isn't reserved exclusively to one generation. Certain types of creativity (poetry, for example) do seem to be fueled best in the fires of youth, but musical composition doesn't show the same burn-out with age, and neither does philosophy. Mathematics and theoretical physics clearly are the provinces of young scientists (why is not clear), but biologists on average do their best work in their forties and fifties, and many have done very important work well past that. Genomics is too new a science to judge how it will stack up in this regard, but given that at its highest levels it seems to require both imagination and a broad view of biology, I might predict that significant contributions could be made by scientists of a wide range of ages.

It seems particularly silly to make it difficult for senior scientists to change fields when the history of science in general, and biology in particular, is filled with examples of breakthroughs made by researchers who came into a field from outside, bringing with them a different perspective - and sometimes new techniques - without the burden of the prejudices and unchallenged assumptions that often bedevil those who have long labored in it. Senior scientists would seem to be among the best equipped to do just that, but how are they to change research directions when they are forever type-cast to be what they have been? In the USA, some private foundations such as the Ellison Medical Foundation and the McKnight Endowment for Neuroscience do award research funds largely on the basis of the novelty of the idea and the overall track record of the applicant, without requiring a previous history in the specific field, and anyone fortunate enough to

obtain support from the Howard Hughes Medical Institutes can switch directions easily, but these are exceptions. The rule is that once you have established yourself in a field it is hard to break out of it, and the older you are the harder it gets. Many senior scientists are indeed winding down, and it is crucial to make way for bright young talent, but my experience with the Swedish Senior Individual Grants program leads me to believe that we may be wasting a significant number of middle-aged scientists who could be making significant contributions in new areas if only they were given the chance.

If the Swedes do have the right idea, how hard would it be to implement such a program elsewhere? It should be highly competitive, so even in a large country we're not talking about more than a few dozen awards. Let's say, in the USA, 50. I like the Senior Individual Grants model and it has seemed to work, so why not copy it? The first year, the award would be \$200,000 to cover salary and expenses for the year of planning the new research program. Only scientists 50 years of age and older would be eligible. That's \$10 million for the first year of the program, not a large sum. Then out of those, pick the 30 best proposed programs after the year is up, and fund them for three years at \$333,000 per year. Repeat the program every four years. The steady-state cost would be \$10 million yearly. If the results after 12 years suggest that there is indeed a significant untapped resource in the pool of senior scientists, then we could consider expanding the program, but for now, why not start small and see what happens? The amount of money involved is modest enough that a foundation could do it if the government doesn't have the will.

As I said, reviewing the Senior Individual Grants proposals, watching these middle-aged scientists get excited about their new directions, and seeing the clever things they have come up with, has been one of the most gratifying things I've done. To think that it's possible to renew your career at a time when conventional wisdom might doubt that makes me more optimistic about the fate of the aging scientist. All that might be needed for many is the chance to show what they can still do. And oh yes, in case you're wondering: I'm 57.