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To Grant or Not

One of the paradoxes of our time is that we live in an era in which the potential for making exciting scientific breakthroughs in the plant sciences has never been so great. Yet, the ability to fund and maintain a productive research group has never been so difficult. As I visit laboratories around the globe, I see dedicated, committed, and excited scientists of all ages working long hours to carry out cutting edge research that is yielding fundamental new information about applied and basic plant processes. In every major journal, there are exciting papers from research groups worldwide that describe surprising new results about how plants work. Nevertheless, many scientists, particularly those at American universities, worry about whether they will be able to continue their research efforts, and scientific careers, in the face of uncertain funding and relatively low funding levels.

The pressure on academics of all ranks is enormous. It is no longer "publish or perish." It is now "find grant money or perish even if you do publish." In this age of rapid technical advances, and warp-speed scientific breakthroughs, the inability to obtain a grant or renew a grant can have devastating consequences. There is simply no safety net. Furthermore, funding levels are at the point where panels must choose between the "most outstanding," "outstanding," and "excellent" grants to be funded. Clearly, it is difficult to distinguish between these grants using objective criteria. Thus, the process by which grants are chosen for funding under present conditions appears to be more subjective than it has been in the past. Then, even "very good" grants had a strong chance of being funded. Now, they don't stand a chance!

One obvious solution to this problem is to increase the levels of funding so that productive research programs that are contributing new and important information about how plants work will be assured reasonable levels of continued funding. After all, if you complete the goals of your grant, have published timely papers, and have proposed an excellent set of new experiments, why shouldn't you obtain funding? In these difficult times, however, we also need to reassess how we as scientists evaluate grants to ensure that the process is as objective as possible for both young and established investigators. Is it possible that we are applying criteria that are too arbitrary and harsh in the grant review process?

During the 1980s, I served twice as program director for the USDA genetic mechanisms panel. Even then we didn't have a large amount of funds to give out; however, in my judgment, the process was not as onerous. Certainly, the panels were critical. Certainly, the panels chose to fund the best grants and the most promising young scientists. However, the panels were more positive and gave more benefit of doubt than their counterparts appear to do today. I recall one panel, in particular, in which a young scientist just off of a postdoctoral fellowship submitted a proposal to obtain an RFLP map of an obscure weed called Arabidopsis. This scientist had never worked with plants before and had proposed a project that was so new that most of the panel members had not even heard of the concept before, let alone the plant! And no preliminary results were presented in the proposal, except that the young scientist had outstanding training, knew how to carry out the procedures, was clearly capable of carrying out the project, and understood the long-term significance of what success would mean to both fundamental and applied plant sciences. Fortunately, the panel awarded this young scientist his first grant to work on plants, and history now shows the contribution this individual has made to plant biology and our understanding of how plants flower.

The critical question we all must ask is whether this individual would be funded in 1996. Is it possible to obtain a grant at the present time with a high risk project, no preliminary data, and only excellent training and a good track record as a graduate student and postdoc? Is it even possible for an established investigator to obtain funding under these circumstances? The grant panels of today appear to want all of the objectives completed as "preliminary data" even before the project has been submitted for funding! Clearly, this means that novel ideas and high-risk efforts might not be funded. Young scientists who are in their postdoctoral years are at a disadvantage because what they want to do as an independent scientist might differ from their postdoctoral research project. And individuals with no funding, or who have lost funding after long periods of productivity, are at a serious disadvantage.

As a former panel director and current member of a grant panel, the major questions for me have always been: (1) Is the question important? (2) Will the project contribute new information about how plants work? (3) Is the PI well trained and does he/she have a good track record? and, finally, for young investigators, (4) Does the PI have the *potential* for carrying out a cutting-edge research program? And I presume that the PI has the ability to deal with problems as they arise and solve these problems in the course of the project. After all, we all know that the unexpected occurs in science no matter how much thought goes into troubleshooting problems before the experiments are carried out. We have been trained to think these problems out critically and to steer in a new direction.

I also assume that a bright, energetic scientist can learn almost any technique

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on his/her own, and/or with the help of others who use it regularly. Why "downgrade" a grant because a productive scientist proposes to use a new plant and/or procedure that is not yet established in his/her lab? Those of us who were trained in the precloning and prekit era can cite numerous examples of procedures that we were forced to learn on our own from the primary literature to keep our research programs up to date—for example, growing bacteria and plasmids, transforming plant cells, doing triparental matings, carrying out DNA and RNA blots, sectioning tissues and performing in situ hybridizations, and countless other procedures. That is part of what we do and are trained for as scientists—continuous renewal and utilization of state-of-the-art technology to solve basic plant problems. And we continue to do it to this day. Therefore, why "punish" a well-trained and productive scientist with an excellent idea for not having prior experience with a procedure that will be utilized in the course of his/her work?

And why rewrite an individual's grant because "we" feel that there are better ways to solve the problem? Clearly, constructive input is desired and wanted. And constructive criticisms are warranted and very helpful. However, because of the subjectivity of the current funding process, "philosophical" differences on how best to solve a problem appear more pervasive today and seem to be taking on greater importance in the funding process. It has become common to hear, "That grant is not fundable, there is no genetics," or "Just another mutant grant, there's no molecular biology," or "The system is new, but the project is too descriptive." Although anecdotal, these statements reflect, in my judgment, a growing trend to insert scientific "bias" into the evaluation process rather than reflecting objectively on the question being addressed and the likelihood that new and important information will emerge from the experiments—irrespective of the approach and/or organism of choice.

I am greatly concerned about the future of science in the United States and around the globe. I benefited tremendously from a system that nurtured its young scientists and encouraged established scientists to believe that a productive research program would be rewarded with continued funding. Although the granting system in the United States has always been very rigorous, it has been fair. It offered the hope that if you come up with good ideas, forge new frontiers, and are productive, you will be funded. I am worried that the current trend is turning young scientists away from science, or at least the plant sciences, and that established scientists with long records of distinguished contributions are being "cut off," or at least "cut back," to the extent that they say, "I have had it."

What can we as scientists do about this negative trend in the face of the exciting science that lies ahead? First, we need to educate the public and put pressure on our politicians regarding funding and the importance of scientific research and training. We need to lobby vigorously for plant training grants and postdoctoral research programs, and the funds to support young scientists who are embarking on their independent research careers. The days of sitting in our labs and ignoring politics are over. After all, the politicians provide the money required by us to sustain our passion for research.

Second, we need to be a little more generous and give more benefit of the doubt in the grant review process—whether we are reviewing a grant ad hoc or as a member of a grant panel. We need to have faith that a young scientist can carry out his/her proposed project in the absence of "preliminary" data. We need to give them a start—turn them loose and see what they can do!

Third, we need to be more objective in considering a variety of approaches and organisms for solving fundamental problems of plant biology. Yes, the genetic approach with Arabidopsis might make a breakthrough in one situation, but a biochemical approach with a lily might make a greater contribution in another situation. The goal is to bring a diversity of approaches into play to solve fundamental plant problems.

Fourth, we need to recognize that anyone can have a "down period," and take into consideration the long-term record of a PI when considering funding. It would be a disaster to lose individuals who have been productive for 10, 15, or even 20 years because a risky project they were carrying out didn't pan out, or because their research program didn't utilize one of the "trendy" approaches that is commonly used in plant biology labs today.

Finally, we need to reconsider how we keep "senior" scientists in the system—scientists who are in their 60s and 70s, or even 80s, and who have made pioneering contributions to the field of plant biology. We need to find a mechanism to keep them in the lab, to keep them around younger scientists, and to provide them with the resources and facilities to continue to make important contributions. Perhaps a counterpart to the young investigator research program should be considered for seniors. Wouldn't this go a long way toward keeping the thread of history and discovery alive? And wouldn't this approach offer an invaluable source of inspiration for all of us who remain committed to carrying out research science?

Those of you who know me will perhaps say that I am "mellowing." To the contrary, I remain as critical as ever—at least as much so as many of you experienced when I was editor of THE PLANT CELL. However, I am dedicated to reviving the optimism, spirit, and hope in the plant sciences that many of us felt as we climbed up the academic and scientific ladder. Perhaps if all of us reconsider how we evaluate and choose grants for funding and apply a more flexible, but no less critical, approach to evaluating grants, we can make a difference in this "downsized" era.

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