LETTER

The Challenge to Science Management

These are difficult times for practicing scientists and managers alike. Of course, the important thing to remember is that we're all in this together—the foot soldiers on the front lines with dirty boots and faces and those of us in the rear with clean boots and faces, with the Roadmaps, whose vision extends over the next ridge.

It is important to recognize this and to maintain mutual understanding and respect. Everyone understands and respects scientists. They bring us new, improved products and equations and cures and so on. Science managers, however, get less respect than they deserve. We too collect data, do quantitative analyses, have meetings and awards, present ideas and evidence, and use PowerPoint extensively. But science managers must take the broad view, see the future, see the opportunities in crises, promote high risk, high impact, high visibility research that is sure to succeed. We must stay ahead of the curve, ride the wave, and latch onto the next big thing—but not too soon and not too late. We must master the art of managing expectations, providing inspiration and guidance, and bringing new hope and new phrases to the arena.

Hungry scientists are an unruly and provincial lot, difficult to manage effectively. One problem is their allegiance to mom-and-pop science. They often focus on attainable rather than desirable goals, preferring to patiently unravel the Gordian knot, rather than to cut to the chase with a big knife. Nonetheless, they are part of our constituency and it is important to bolster their morale whenever possible by pointing them to small pots of money and large, carefully selected committees. At my Institute we even tried to set up a new Department of Idle Curiosity for them, but adequate funding could not be found.

Despite frequent reminders (even President Lyndon Johnson warned against their habit of keeping life-saving discoveries bottled up in the laboratory) many scientists are timid and reluctant to go for the big win, always urging patience and emphasizing our ignorance (see Kornberg, 2007). Some have even resisted the obvious need to put most of our resources into big-time translational research, and to do so now. With these attitudes, and the tradition of letting scientists determine their own priorities, it's a wonder that any translational progress was ever made before the term was coined.

Science managers must frequently make decisions in the face of uncertainty and reconcile conflicting approaches and values. The ingenuity and nuanced thinking required is rarely appreciated. A marvelous example is the analysis published by six of our NIH Institute Directors (Insel et al., 2004) addressing the issue of big versus small science. They start with Bruce Alberts' 1984 commentary, in which he noted that "doing good science is different from baking bread," and point out that "Alberts decried the emergence of large, science manager-driven laboratories, which he viewed as less efficient and less interesting. . . ." "Today," continue Insel et al., "this judgment remains compelling. . . . [W]e need innovative investigators working in small, focused labs to develop and test hypotheses. . . ." They then segue to, "Our field is currently in a discovery phase, where large-scale science is critical for progress." After naming three "areas of need" for large-scale neuroscience and addressing impediments to their pursuit (including "less funding for single-laboratory, hypothesis-driven, R01-type projects"), they deftly conclude, "in fact, small laboratories could be an integral part of large-scale efforts." Thus are the conflicting approaches reconciled.

We're in good hands.

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