NIH's Strategic Planning Rorschach Blot

Back in 1975, National Institutes of Health (NIH) molecular biologist Martin Gellert wasn't looking for a commercial payoff when he and colleagues Howard Nash and Kiyoshi Mizuuchi began to study the ways certain viruses integrate themselves into their host bacteria. Soon after beginning work with *E. coli*, however, the team made an unexpected discovery—that of DNA gyrase, an enzyme responsible for twisting the bacterial genome into its normal "supercoiled" state. The team found that, lacking DNA gyrase, the bacterium simply died. Within a year, the researchers had not only defined the mechanism of supercoiling, but also discovered a family of DNA gyrase antagonists. Last year, one such antagonist—norfloxacin—racked up $410 million in sales for Merck, which markets it as a broad-spectrum antibiotic.

Gellert thinks his story has an inescapable moral—particularly today, when, like the National Science Foundation, NIH is under increasing pressure to emphasize directed, applied research. "If someone had told me in 1975, 'Go forth and find a new antibiotic,' I probably wouldn't have had any better ideas than to go scrounging through soil samples looking for new compounds," he says. "If you say you want to cure disease X and limit yourself to exploring the things you can see in the near future, you're likely to have bad luck."

But Gellert, like many scientists working for and funded by NIH, believes that NIH may be falling into exactly this trap. The focus of their concern is a year-long strategic planning exercise kicked off by Director Bernadine Healy, which has drawn fire for emphasizing applied, disease-oriented research—including work with potential commercial applications—at the possible expense of basic biology (*Science*, 14 February, p. 788; 3 July, p. 20; 24 July, p. 476). Those sentiments are not universally shared, however. Instead, the current draft of the strategic plan—a 204-page, double-sided, loose-leaf draft that outlines NIH goals, a philosophy, a mission, a statement of means, and six "trans-NIH objectives"—is something of a Rorschach blot for the research community, whose members mostly seem to be reading into the plan either their greatest fears or fondest hopes about the way research should be managed.

Since last December, Healy has been promising a strategic plan aimed at justifying NIH's $10 billion budget in terms of its contribution to the nation's welfare. Without such a plan, Healy has said, NIH faces the prospect of declining public and political support, shrinking budgets, and an inevitable cutback in basic research. This year, Congress may have helped make her case by failing to increase NIH's budget much over the rate of inflation for the first time in recent memory—a move that NIH officials take as an indication that their agency can't afford to be without a strategy much longer.

To the extent that this strategy implies that NIH will focus more intently on research with commercial applications, however, it has created sharp divisions within the research community. Consider, for instance, the views of just two scientists who responded to Healy's outline of the strategic plan in the 17 July issue of *Science*. "A lot of work in NIH grants is on basic [biological] mechanisms and so forth," says Abulkalam Shamsuddin, a University of Maryland pathologist who thinks that a new emphasis on applied work would be welcome. "Now, if you come up with a practical application, there's not any interest in it. Instead, we're all in a rat race for the Nobel Prize." On the other hand, Joaquin Fuster, a University of California, Los Angeles, neuroscientist, complains that the NIH strategy amounts to little more than "a reflection of a desire to capitalize whatever we've got, whatever its sources, for short-term profit, and to satisfy [near-term] political aims."

The NIH officials in charge of the plan are eager to reassure the research community that Fuster's fears are unfounded. NIH associate director for science policy and legislation Jay Moskowitz casts the strategic plan not as a series of top-down directives, but as a process for bringing the research community together with NIH officials to determine when important research areas that touch on the plan's overarching goals aren't being met by the R01 investigator-initiated grant program. Once officials and institute research councils, study sections, and outside researchers hammer out a consensus about the best way to fill such research gaps, Moskowitz says, the problem may well be solved by a flurry of R01 proposals spurred by the discussions themselves. "The point is to shape the future, not let the future shape you," he says.

But researchers like Gellert remain unconvinced that the consensus approach to research management will produce the results promised for it. "In my view, modern biology is not yet ready for that kind of engineering approach—not in the sense of 1960, of saying, 'Let's send a man to the moon,'" he says. "We know a lot, but it's all in bits and pieces. What we know is far outstripped by what we don't know."

–David P. Hamilton

with research management experience like Edward Frieman, director of the Scripps Institution of Oceanography. In his letter, Frieman urges NSF to "seize the opportunity to play a key and fundamental role in helping to forge the nation's new overall R&D posture," arguing that it is the agency best suited to take on the task. He makes several suggestions for orchestrating the "new order," predicting "an enormous amount of community support."

Assembling all those divergent opinions into a consensus statement will be a tough job—all the more so because the NSF's special commission has just the next 2 weeks for the task. Indeed, even members of the NSB, such as Charles Hosler, senior vice president for research at Pennsylvania State University, have grumbled openly that the strategy is being cobbled together with undue haste.

For NSF traditionalists, it will only get worse next year. The House Science Committee is planning a 12-hearing review of the NSF's programs and objectives, and the Senate, too, will be taking a close look because the NSF's 5-year reauthorization comes due in 1993. And this scrutiny of federal science is likely to be more intense in coming years because R&D will be supported by a weak economy, predicts Edward David Jr., retired chief of research for Exxon and former White House science adviser. He believes the cold war's end will bring a period of deflation, with a depressing 25% to 30% drop in funding for R&D. "It's been 50 years since we've operated a peacetime economy and we have no idea how to do it—no idea," David says. In the private sector, big companies like Chrysler have already closed central labs, and others will follow suit, he expects. David doesn't expect NSF-supported research at universities to be immune. "Downsizing," David warns, may be the theme of the decade. "I'm not advocating any of these terrible things," he says. "It's just the way things are going."

And Massey says he has done his best to elicit reaction from the community. Though he concedes that he was "a little surprised" by the number of critical comments and by the overwhelming concern "that we might do something to damage the foundation," he notes that, "I was deliberately provocative" in presenting the issues to the public and the science board last summer. "I wanted to make sure the issues were addressed sharply," Massey says, "and I must say I have been very successful in that."

–Eliot Marshall