

# How not to prioritize

A high-level reprimand to US astronomers highlights the need for the objectives of 'big science' to be openly debated.

On 8 January, NASA's administrator Mike Griffin upbraided the annual meeting of the American Astronomical Society in Austin, Texas, for a lack of team spirit. Not only were some of the scientists less than enthusiastic about the human-exploration goals that have been this administration's top priority in space, they were also messing up the agency's astrophysics programme with special pleading to Congress. Such interventions, Griffin warned, would thwart the community's stated goals and blight its future projects.

The meatiest bone of contention was the Space Interferometry Mission (SIM). SIM offers a way of discovering planets by observing slight jitters in the position of their parent stars. NASA has spent nearly \$600 million on it already. On the basis that finishing it might easily cost a further \$1.85 billion (see page 228), the agency had planned to assign it \$22 million in this year's budget, keeping it far from any prospects of flight. Congress gave it three times that much, apparently wishing to see it move into full development. Such development would, warned Griffin, leave NASA no room for any other astrophysics missions of any size, and force delays or cancellations on those already in development.

Griffin sought to portray the boost for SIM as a fratricidal move to circumvent the settled result of the astronomers' 'decadal survey' process. Under the auspices of the National Academy of Sciences, the community gives its funding agencies, and the lawmakers who provide their budgets, regular surveys of its priorities. This process has been much praised, but is marred by shortcomings. In particular, the most recent survey was undercut by a self-deluding ineptitude on matters of cost. The James Webb Space Telescope was just one of the seven 'major initiatives' prioritized by that survey, yet by the time of its launch it will on its own have cost far more than the total that the survey envisaged for all of them. As that most recent wishlist also included spending on as-yet unfinished projects from the previous survey (of which an early form of SIM was one), it is not clear what help it offers decision-makers today.

Another problem is that there are legitimate interests in the future of American space research that such surveys may not capture. SIM is a project based at the Jet Propulsion Laboratory (JPL) in Pasadena, California. The advent of full-cost accounting at NASA, which means that money follows specific projects to a greater extent than ever before, has heightened the importance of flagship projects for the institutions that host them. A healthy budget for SIM could serve to maintain a pool of talent at JPL that might otherwise be eroded; if you want a reason for the lobbying, this is a pretty good starting point. The beauty of such a power-house is no doubt in the eye of the beholder, but everyone should recognize that the benefits of a healthy JPL are felt beyond the precincts of Pasadena.

The lesson for the astronomical community is that the decadal survey should provide a range of more and less capable missions, thus making it easier for policy-makers simultaneously to satisfy the community's goals and the constraints of the public purse. It should also agree that the imprimatur of priority bestowed by a decadal survey has a use-by date — after a certain time, perhaps as little as five years, it is reasonable to ask whether a given mission is still the best way to achieve its stated goal. Anyone setting priorities needs to scrutinize SIM in this spirit.

Meanwhile, NASA's administrator needs to accept that Congress has a legitimate role in setting goals for his agency. He should also consider that portraying the astrophysics budget as a zero-sum game is a tactic that could backfire: if astronomers thus threatened successfully lobby for a significant transfer of funds from human spaceflight to science, his position will be weakened. And Congress should, when exercising its powers, open up a public debate on all the issues involved — which may often go beyond the merits of a single mission. ■

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## Deserting the hungry?

Monsanto and Syngenta are wrong to withdraw from an international assessment on agriculture.

"This is a most reluctant decision." These are the words of a spokesman for the agriculture-industry body CropLife International speaking to *Nature* this week. The decision in question is that by two CropLife member corporations, Monsanto and Syngenta, to pull out of the International Assessment of Agricultural Science and Technology. This is an ambitious, 4-year, US\$10-million project that aims to do for hunger and poverty what

the Intergovernmental Panel on Climate Change has done for another global challenge.

The scale of the ambition is clear both in the project's promised outcome, as well as in its internal workings. When published later this year, its reports promise to map how science, technology and accumulated good-farming practice can be used to reduce hunger and improve quality of life for rural people in developing countries (drafts can be accessed from [www.agassessment.org](http://www.agassessment.org)). At the same time, the writing and review teams (some 4,000 experts in all) comprise a grand coalition including scientists, government officials, representatives from seven UN agencies, farmers' groups, a rainbow of non-governmental organizations (NGOs) and industry, including chemicals manufacturer BASF and agri-biotech giants Monsanto and Syngenta.

But these last two, part of the assessment from the beginning, have now decided to quit. No public statements have been offered, but the spokesman for CropLife told *Nature* that the decision was prompted by the inability of its members to get industry perspectives reflected in the draft reports. One of these perspectives is the view that biotechnology is key to reducing poverty and hunger, and it is based in part on high (and rising) levels of demand for biotech crops from farmers across the developing world.

Insiders agree that the current draft is decidedly lukewarm about the technology's potential in developing-world agriculture. The summary report, for example, devotes more space to biotechnology's risks than to its benefits. The report says that evidence that biotech crops produce high yields is not conclusive. And it claims that if policy-makers give more prominence to biotechnology, this could consolidate the biotech industry's dominance of agricultural R&D in developing countries. This would affect graduate education and training, and provide fewer opportunities for scientists to train in other agricultural sciences.

CropLife says that it does not take a "dogmatic" position and remains open to rejoining the assessment if the other team members are willing to be more even-handed. But the views outlined in the draft chapter on biotechnology, although undoubtedly over-cautious and unbalanced, nonetheless do not represent the rantings of a fringe minority. The idea that biotechnology cannot by itself reduce hunger and poverty is mainstream opinion among agricultural scientists and policy-makers. For example, biotechnology expansion was not among the seven main recommendations in *Halving Hunger: It Can Be Done*,

a report commissioned by former UN secretary-general Kofi Annan. The writing team for this report included Kenya's Florence Wambugu, perhaps the strongest proponent for biotechnology in Africa.

The assessment's secretariat and chairs, too, need to ask themselves some searching questions. For starters: how come these founding members of the assessment got to the point of walking out? This is not the first time an initiative has sought to find common ground between

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NGOs and industry on a major issue involving science and public policy. There are many lessons that can be learned by talking to, for example, the organizers of the Mining, Minerals and Sustainable Development project, or the World Commission on Dams, both of which produced consensus reports that have had far-reaching impacts.

Whatever happens next, the status quo is not an option. A meeting to agree the final text is expected to take place in April. Monsanto and Syngenta must get back to the table before then. If they maintain their current position, it will be a blow to the credibility of an important scientific assessment. In addition, public confidence in the biotech industry and in its ability to engage with its critics will have been undermined.

Perhaps most important of all, believing as they do that biotechnology is an essential response to hunger, the two companies will be letting down those that they most want to help. ■

## Philanthropy needed...

... to save a historic home of scientific stimulation.

**T**he Ciba Foundation's biomedical symposia, which began in 1950, were scientifically influential but were special in other ways too. Thirty or so scientists would gather at 41 Portland Place, part of a beautiful eighteenth-century mews in central London, to spend three intense days discussing a cutting-edge theme, eating formally together in the antique-strewn dining room, and sleeping in overheated bedrooms that cannot be locked from the outside — gentlefolk, after all, don't steal.

Non-British delegates would be bemused by the crazy plumbing and disconcerted by how loudly the undulating floorboards creaked. But all were charmed. Many Nobel laureates have acknowledged the intellectual stimulation of the meetings. Ulf von Euler, for example, said that his ideas of how neurotransmitters are stored and released were stimulated by the foundation's meeting on adrenergic mechanisms in 1960. In contrast, Arvid Carlsson was devastated when the same colleagues rejected his notion — which won him the 2000 Nobel prize — that dopamine was a neurotransmitter in the brain. A reputation could stand or fall on the consensus of a Ciba Foundation meeting.

Time moves on. The Swiss pharmaceutical company Ciba-Geigy was merged into Novartis in the mid-1990s, and the foundation was duly renamed. In 2002, when the foundation's sponsor moved its R&D centre from Basel to Boston, it decided that this old-fashioned, eccentric elegance did not fit its style of conference support (see page 233).

That was a blinkered decision, given the strong links that the pharmaceutical industry needs with the academic community. True, corporate sponsorship has got tougher, with shareholders demanding much greater, and more immediate, accountability. Nevertheless, Novartis should have negotiated more sympathetically with the foundation to explore new approaches. It is easier to destroy an organization with a strong reputation and institutional knowledge than to build one up from scratch.

But the foundation must shoulder blame too. Although it maintained the quality of its meetings, it made no noticeable acknowledgement that the conference game has changed. Its paper-based approach to publication seemed increasingly quaint, for example. Furthermore, the foundation's trustees and directors should have put up a stronger and more public fight for its life. Their decision to work discreetly on the basis of contacts rather than embark on an 'undignified' campaign to find a new sponsor was almost certainly wrong. Be that as it may, the Novartis Foundation is set to be dissolved at the end of next month, having had 15 months to wind things up.

But it is not necessarily curtains for 41 Portland Place. The likely new tenant, the Academy of Medical Sciences, may still be persuaded to continue the international meetings if the right sponsor were to emerge. Such a rescuer might be institutional, as was happily found last year by the similarly small and intense Berlin-based Dahlem Conferences, saved by the newly created Frankfurt Institute for Advanced Studies. Or there may be an enlightened wealthy individual willing to foster top-level scientific brainstorming and debate.

Any offers? ■