The Rush to Publish

As the pace of discovery quickens, molecular biologists scramble for rapid publication—a trend exacerbated by the heightened competition among the top three journals

ON 12 JUNE 1990 FRANCIS COLLINS OF the University of Michigan rushed off a paper to *Science*, where the editors were already alerted that it was on the way. But no sooner had Collins submitted the paper, announcing his discovery of the neurofibromatosis gene, than Raymond White, his competitor and former collaborator at the University of Utah, got wind of it. Dismayed that he would be scooped after his own massive, 3-year hunt for the gene, White called Benjamin Lewin, editor of the rival journal *Cell*, to see how quickly he could publish the paper White was furiously writing, describing his discovery of the same gene. The answer was fast: White's paper appeared a mere 17 days after submission.

Meanwhile, by the same remarkably efficient and somewhat mysterious grapevine, Collins had learned that White's paper, submitted 2 weeks after his own, would beat his into print by a week. Although Collins' paper was already on the fast track at *Science* when editor Daniel Koshland was alerted to the problem, he went to the extraordinary measure of remaking the last few pages of the magazine to squeeze the Collins paper in a week ahead of schedule. The two papers came out on 13 July, and both groups shared the limelight at a well-attended press conference and on the front page of the *New York Times*.

The White-Collins saga is surely unusual in terms of the lengths that the editors and authors were willing to go to beat each other into print, but it is by no means an aberration. In fact, the rush to publish goes back to the 17th century when, in an effort to force scientists to divulge their data, an obscure secretary of the Royal Society of London came up with the rule that priority goes to whoever publishes first—not to who discovers first. Researchers have been vying to publish first ever since.

Watson and Crick's seminal 1953 paper describing the structure of DNA, for example, was published in just 3 weeks. And when high energy particle physics exploded in the 1960s, that field was gripped with a mad scramble for scientific precedence—and for instant publication—as it has been several times since (see box). But while the fast track was once largely reserved for extraordinary discoveries like the double helix, it is now becoming almost commonplace, especially for advances in the molecular biology of human disease.

This trend is fueled partly by the quickening pace of discovery in that field—the tools to fish out disease genes have been available for less than a decade—and by the technological advances that make rapid publication feasible. It is aggravated by scientists' increasing perception that mass media publicity can bring in badly sought grant money—as well as by pressure from the charitable foundations that want publicity for "their disease." And it is also driven, in no small part, by the relatively new and often bitter competition among journals, which exacerbates existing tensions among investigators and enables them to play one journal against the other. Everyone knows that the way to get fast service at *Science* or *Cell* or *Nature*, says Frank McCormick, a molecular biologist at Cetus Corp. in Emeryville, California, is to tell the editor that a competing paper is coming out in one of the other journals.

While most investigators applaud the faster turnaround journals are offering, some are wondering if perhaps a good thing has gone too far. Two or 3 months from receipt to publication is one matter, they say; 2 or 3 weeks is another.

"This is getting out of hand," grumbles one investigator, insisting on anonymity, who was recently beaten out in one of these last-minute scrambles. Richard Roberts of Cold Spring Harbor Laboratory agrees: "I deplore [the trend]." And immunologist Jack Strominger of Harvard University declares: "The vogue journals do not do the cause of science a service by rushing things into print."

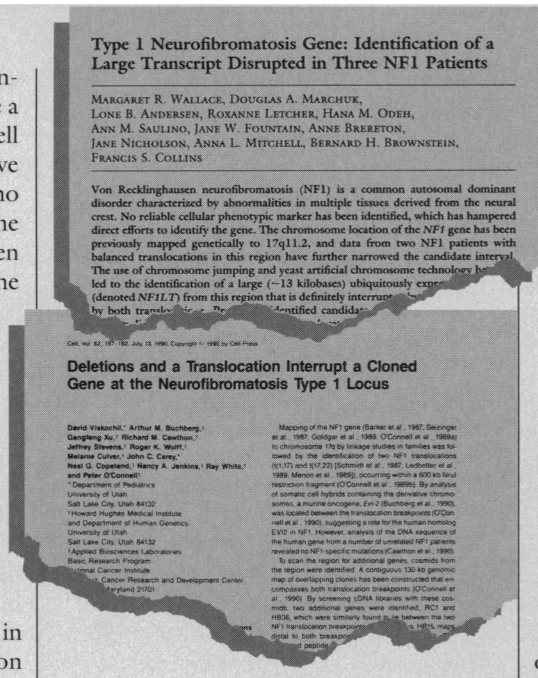
The critics have several gripes about this ultrafast publication. They talk of cronyism at the journals and speculate that the fast track is available just to insiders, which makes it difficult if not impossible for any but the chosen few to compete. Still other critics, like Strominger, are galled by the power of the editors to decide, according to their whims, which papers are "hot" enough to warrant the fast track in the first place. "What is important or not is taste, and taste today may not be taste tomorrow."

But the biggest worry is that rapid publication may, in some cases at least, be premature publication; that in their rush to publish, scientists may cut corners and the review process may be compromised, leading to incorrect or incomplete work. Stanley Pons and Martin Fleischmann's cold fusion paper, published by the *Journal of Electroanalytical Chemistry* in just 4 weeks, is a precedent no one wants to repeat.

But by and large, critics of the ultrafast track are hard pressed to find major problems with these papers, just a slight sloppiness creeping in. Editors at *Physical Review* acknowledge that during the 1987 frenzy over high-temperature superconductivity, they published some papers that ordinarily wouldn't have passed muster. And more recently, both the White and Collins neurofibro-

SELECTED FAST TRACK PAPERS

GROUP	TOPIC	JOURNAL	DATE	TURNAROUND
Watson, Crick	DOUBLE HELIX	NATURE	4/25/53	3 WKS
Cherwick	NOBLE GASES	SCIENCE	10/12/62	10 DAYS
Aaronson	V-SIS ONCOGENE	SCIENCE	7/15/83	6 WKS
Waterfield	V-SIS ONCOGENE	NATURE	7/7/83	3 WKS
Gallo	AIDS VIRUS	SCIENCE	5/4/83	5 WKS
Fry	CHERNOBYL	NATURE	5/15/86	4 DAYS
Chu	SUPERCONDUCTIVITY.	PHYS. REV. LETT.	3/2/87	3 WKS
Sleight	SUPERCONDUCTIVITY	SCIENCE	2/26/88	17 DAYS
Fermi Group	Z PARTICLE	PHYS. REV. LETT.	8/14/89	4 WKS
SLAC Group	Z PARTICLE	PHYS. REV. LETT.	8/14/89	3 WKS
Pons, Fleischman	COLD FUSION	J. ELECT. CHEM.	4/10/89	4 WKS
Tsui	CF GENE	SCIENCE	9/8/89	4 WKS
Collins	NF GENE	SCIENCE	7/13/90	4 WKS
White	NF GENE	CELL	7/13/90	17 DAYS
White	NF-GAP	CELL	8/10/90	18 DAYS
Welsh	CF GENE TRANSFER	NATURE	9/27/90	3 MO.
Wilson	CF GENE TRANSFER	CELL	9/21/90	15 DAYS
Hendrickson	AIDS RECEPTOR	NATURE	11/29/90	6 WKS
Harrison	AIDS RECEPTOR	NATURE	11/29/90	6 WKS
Tamanoi	NF-GAP	CELL	11/16/90	7 WKS
McCormick	NF-GAP	CELL	11/16/90	6 WKS
Collins	NF-GAP	CELL	11/16/90	3 WKS



Type 1 Neurofibromatosis Gene: Identification of a Large Transcript Disrupted in Three NF1 Patients

Margaret R. Wallace, Douglas A. Marchuk, Lone B. Andersen, Roxanne Letcher, Hana M. Odeh, Ann M. Saulino, Jane W. Fountain, Anne Brereton, Jane Nicholson, Anna L. Mitchell, Bernard H. Brownstein, Francis S. Collins

Von Recklinghausen neurofibromatosis (NF1) is a common autosomal dominant disorder characterized by abnormalities in multiple tissues derived from the neural crest. No reliable cellular phenotypic marker has been identified, which has hampered direct efforts to identify the gene. The chromosome location of the *NF1* gene has been previously mapped genetically to 17q11.2, and data from two NF1 patients with balanced translocations in this region have further narrowed the candidate interval. The use of chromosome jumping and yeast artificial chromosome technology h… led to the identification of a large (~13 kilobases) ubiquitously expr… (denoted *NF1LT*) from this region that is definitely interrupt… by both transl… …dentified candida…

Cell, Vol. 62, 187–192, July 13, 1990, Copyright © 1990 by Cell Press

Deletions and a Translocation Interrupt a Cloned Gene at the Neurofibromatosis Type 1 Locus

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Mapping of the NF1 gene (Barker et al., 1987; Seizinger et al., 1987; Goldgar et al., 1989; O'Connell et al., 1989a) to chromosome 17q by linkage studies in families was followed by the identification of two NF1 translocations (t(1;17) and t(17;22) [Schmidt et al., 1987; Ledbetter et al., 1989; Menon et al., 1989]), occurring within a 600 kb NruI restriction fragment (O'Connell et al., 1989b). By analysis of somatic cell hybrids containing the derivative chromosomes, a murine oncogene, *Evi-2* (Buchberg et al., 1990), was located between the translocation breakpoints (O'Connell et al., 1990), suggesting a role for the human homolog EVI2 in NF1. However, analysis of the DNA sequence of the human gene from a number of unrelated NF1 patients revealed no NF1-specific mutations (Cawthon et al., 1990).

To scan the region for additional genes, cosmids from the region were identified. A contiguous 130 kb genomic map of overlapping clones has been constructed that encompasses both translocation breakpoints (O'Connell et al., 1990). By screening cDNA libraries with these cosmids, two additional genes were identified, RC1 and HB36, which were similarly found to lie between the two NF1 translocation breakpoint… …s, HB15, maps distal to both breakpo… …d peptide…

Priority Fight. *Authors and editors scrambled to be first with the neurofibromatosis gene.*

matosis papers were flawed: Collins misinterpreted some mapping data; White made a minor sequencing error that, while it was well within accepted accuracy rates, would have confounded efforts to work out the amino acid sequence of the protein encoded by the gene. "That's the kind of thing you get when you rush," says Koshland, who adds that the errors were not serious and in no way undermine the achievement of either group.

Both the White and Collins papers also suffered from incompleteness, another side effect of the rush. The two teams were in such a hurry that they published when they had sequenced just a tiny piece of the mammoth neurofibromatosis gene. Another several weeks of work and they would have realized that it bears an uncanny resemblance to a gene involved in human cancers—a stunning observation (published in a second article by White a month later, also in *Cell*, this one with an 18-day turnaround) that provides insights into how the neurofibromatosis gene causes its benign tumors and other effects. "We would have preferred waiting to include that in the first paper but felt we couldn't," concedes White.

"That's a problem," notes Maxwell Cowan, chief scientific officer at the Howard Hughes Medical Institute, which employs both Collins and White. "A few years ago there would have been pressure to wait until the whole thing was sequenced."

Even if journal editors agree with such concerns, they are caught in something of a bind—pushed on the one hand to get timely and significant work out quickly, then criticized on the other for short-changing peer review. Koshland and *Nature* editor John Maddox defend the fast track for special papers and deny that peer review is compromised, though Koshland does confess to a slight uneasiness: "Rapid publication is a service journals provide to their authors—up to a point. Where you step over the line is a judgment call." He also worries that, inevitably, all the scurrying diverts attention from papers on the routine track. But such concerns notwithstanding, both Maddox and Koshland concede that, given the new relationship among journals, they have little choice if they are going to keep getting the "hot," competitive papers. "If I don't compete, the fast-track papers will go to *Cell* or *Nature,*" says Koshland, who adds that "to some extent, though, the most exciting papers are those for which there is no competition, because they are novel."

What's behind the rush? One of the biggest factors is the virtual explosion of molecular biology in the past decade, with the advent of new tools for zeroing in on genes, along with an ever-growing number of scientists entering the field. The upshot is that an investigator who has spent years hunting down a particular gene risks having another well-equipped team catch up virtually overnight, or at least within a few months—a possibility that heightens the urgency many already feel about staking their claim fast. "Clearly, with more people doing science and doing it faster, you have to publish soon or you feel you will be scooped," says James Watson, director of Cold Spring Harbor Laboratory.

What's more, in molecular biology, as in particle physics, investigators are often competing for the same discrete and very visible prize—say, finding and sequencing the cystic fibrosis gene—where little glory goes to also-rans. Says neurobiologist Richard Aldrich of Stanford: "Before, science was more interpretive. It was not so much to be first but to be the best. But there is no such thing as a better sequence."

On the journal's side, what's changed is the facility of publishing quickly. What the polymerase chain reaction has done for molecular biology—reducing the time to sequence genes from months to weeks—word processing and electronic typesetting have done for publishing. And with the fax machine, editors can now get same-day review of their hot papers if they find a willing referee. But technological advances aside, it is the competition among journals, more than anything else, that seems to drive the current push toward instant publication.

Nature and *Science* have been competing in a more or less friendly fashion for years. Then in 1974, Benjamin Lewin left *Nature* and started his own journal, *Cell*, where he is both the editor and the owner, raising the competition to new heights. Between *Science* and *Cell,* at least, the competition is far from amicable—in fact, it is more like an outright war. Lewin would not be interviewed for this article, saying only, for the record, that any comment he made "would be twisted and misrepresented in the pages of *Science*."

"Lewin is very competitive," responds Koshland. "Part of his motive is to beat us out." At *Nature* as well, which has more cordial relations with *Cell* than does *Science*, the editors are increasingly worried by what they see as Lewin's attempts to overtake their papers—especially following a recent incident when *Cell* published a paper in 15 days, knowing one by a rival team was soon to come out in *Nature.*

Whatever Lewin's motives, nearly everyone agrees that he has stirred the pot. Lewin has been consistently able to attract top-flight papers and to publish some of them exceptionally fast, although he is not unique in that regard. "All three journals, if pushed, will publish extremely rapidly," says Tom Jessell of Harvard. Phillip Sharp, a molecular biologist at the Massachusetts Institute of Technology, agrees: 'If you have a paper of real timeliness and significance, any one of them will give you a month turnaround." But Lewin is widely perceived to have the edge, which may be something of a mystique: the average turnaround time at *Cell* in the last five issues of 1990 was about 4½ months, roughly comparable to those at *Science* and *Nature.*

In any event, speed is not the only factor scientists consider when choosing a journal. Other, more subjective considerations have always come into play, notes cancer gene hunter Bert Vogelstein of Johns Hopkins University. All three journals have their relative strengths. *Science* and *Nature* offer a broad audience. *Science* can boast of the largest circulation, 150,000. *Nature* offers better European exposure. And *Cell*, which is widely read by molecular and cell biologists, has a format that accommodates longer articles. But beyond that, adds Vogelstein, the choice depends on who the editors and reviewers are, which articles the journal has published recently in your field—and thus how favorably you think your contribution will be viewed.

But now with the heightened competition among journals, speed of turnaround has become a bargaining chip, enabling authors to shop around to find the best deal—a development that leaves editors in something of a quandary. When Hughes investigators Wayne Hendrickson of Columbia and Stephen Harrison of Harvard began writing their papers on the structure of the binding

Lessons from Physics

The rush to publish hit high energy physics with a vengeance in the mid-1950s, during the heyday of the hunts for the new V particles, now known as the strange particles. Researchers were so eager to get their brand-new data into print that they couldn't wait for publication but instead started sending out copied preprints to their colleagues. By the time *Physical Review*, the premier physics journal, could publish the papers, nearly everyone in the field had already seen them; indeed, some of them had been picked up by the *New York Times*.

Sam Goudsmit, the *Physical Review* editor at the time, came up with a solution: *Physical Review Letters*, a new journal devoted exclusively to rapid publication. The idea was that researchers could stake their claim to a discovery with a one-page paper in the new journal, which they would presumably follow up with a more detailed paper in *Physical Review*.

For rapid publication to work, the editors had to abandon hot type and come up with new tricks for "firehouse production," recalls Arthur Herschman, a former editor at both journals who is now at the American Association for the Advancement of Science. They did, typing manuscripts on an IBM typewriter, jury-rigged with a special contraption holding the Greek symbols needed for equations. The typewritten page was their camera-ready copy. "We could process a paper in a week," Herschman says, and many papers came out about 2 months after receipt. Over the past 3 decades, however, the length limit has crept up to four pages and the average turnaround time has expanded to about 4 months, just shy of the 5 or 6 months at the parent journal.

Even during the frenzied days of the 1960s, "we never published without at least the motions of peer review," says Gene Wells, an editor at *Physical Review Letters*, who adds that they could usually do considerably more. The journal office was then located at Brookhaven National Laboratory, "so for a really hot, competitive paper, we would find someone to review it as we stood there," says Herschman. But even that was not always fast enough for the particle hunters, so the journals also instituted a policy that particle hunters could request publication without review, provided they had a letter from their department chair seconding the idea. That policy was broadened in 1976 to include any experimental field. There was a catch, though: the journal would publish a disclaimer on the first page of the article alerting readers that, at the authors' request, it had not been peer reviewed.

That option has been used less than a dozen times, says Wells, who adds that most teams do not want the stigma of that tag line. Indeed, the last time was in August 1989, when competing teams at the Stanford Linear Accelerator Center and Fermi Lab were scrambling to produce large quantities of Z particles before the new detector came on line at the European Laboratory for Particle Physics (CERN). Both U.S. teams submitted papers within days of each other, but only the Fermi group requested special treatment. Shortly thereafter, the journal revoked the policy, deciding that with the fax machine, which more or less guarantees overnight review, there was no longer any excuse for publishing without peer review.

Again, in 1987, with the startling discovery of high-temperature superconductivity, editors at *Physical Review* and *Letters* were forced to come up with yet another strategy to deal with the flurry of papers. "Information was literally changing week by week. It was something I have never seen before," recalls Peter Adams, editor of *Physical Review B: Condensed Matter*, which published many of the papers. "We had people phone in papers and fax them in. And people drove 100 miles to deliver their papers to the office so they could get the Friday evening receipt date," as opposed to a Monday date, 3 days later.

The editors realized that traditional peer review would not suffice, not just because of speed but because "the information was too new," Adams says. "No one could effectively review the work because no one had ever done anything like it." Their solution was a committee of 20 people, all of whom received each paper, although only one or two were designated primary reviewers. Says Adams: "The system was completely outside the norm," but necessary to publish papers within 6 weeks or so of receipt.

But the journal paid a price for that speed, Adams concedes. "I was aware at the time that a lot of it could have been more carefully done. If we had not done it, a huge amount would probably never have been published, but the field would have been slowed tremendously. I believe we helped the field mature." He also admits that if he hadn't published the papers, many would have gone to other journals. ■ **L.R.**

site of the receptor for the AIDS virus last fall, they called editors at both *Science* and *Nature* to see if either would guarantee publication of the two papers together before the end of the year. Both journals promised rapid turnaround, if in fact the two papers warranted it. To the authors, it was a toss up, says Hendrickson: he preferred *Science* because he has published there more but yielded to Harrison who preferred *Nature* for the same reason. To save time *Nature* typeset the two manuscripts before sending them out for review and published them back to back 6 weeks after submission, on 29 November.

Even Collins, who complained about the fast track *Cell* gave to White, called Lewin just 2 months later seeking rapid publication for his paper on cystic fibrosis gene transfer, done in collaboration with James Wilson of the University of Michigan. Lewin delivered, publishing that paper in just 15 days—a week ahead of a competing paper submitted to *Nature* on 27 July.

When he learned about the imminent *Cell* paper, *Nature's* Maddox faced the quandary that Koshland had a couple of months earlier: Do you move up publication of that paper just to match Lewin, thereby opening up the journal to similar requests from other authors? Maddox decided not to budge but did lift *Nature's* press embargo so the two groups could share the limelight. Asserts Maddox: "We won't be pushed by what the other journals are doing."

While the benefits of the fast track, at least to the authors and sometimes to the journals, are clear, what about the costs, especially in terms of the quality of the science? "These 17- or 18-day wonders can't possibly be getting reviewed," is a common complaint. "There's nothing wrong with publishing fast, but the papers had bloody well better be reviewed," grumbles David Cox, a gene hunter at the University of California, San Francisco, who adds that the quality of the review all too often "depends on how hot the merchandise is. Ironically, the hotter a paper is, the more scrupulously you want it reviewed. That is usually what happens. But if the editors disrupt the process, the paper won't be given that scrutiny, and there will be more errors."

Both Maddox and Koshland emphatically deny that they skimp on review. "If we don't get things peer reviewed, it will catch up with us," asserts Koshland. "We hope as much as anyone to publish quickly, but we want to be right, too," adds Maddox, who probably holds the world's record: 4 days from submission to publication of a paper on the nuclear disaster at Chernobyl, which he insists was still reviewed. "Even Ben Lewin, when he pulls a rabbit out of his hat, as he sometimes does, probably goes to great trouble to see they are properly reviewed. Nevertheless, he makes mistakes," Maddox adds, although he declined to identify any. In a recent article in *The New Biologist* Robert Martin of the National Institutes of Health describes four major retractions at *Cell* within the past 2 years, but the fact is that only one of the papers was on the fast track, published in under 2 months.

Is the accelerated review on the ultrafast track as good as that for papers not judged so "hot," whether at *Cell* or *Science* or *Nature*? Probably not quite, concedes Collins, who has experienced it several times over the past few months. "The question is, are there subtle pressures in the atmosphere of rapid publication that lead the editors and reviewers to treat the paper in a less critical way? To say, 'Yes, there are some little problems, but we will let them go'? I expect there are fewer revisions going on in these pressure-charged situations than for the usual paper."

Koshland, too, worries about the minor errors that may creep in if rushed papers are not finetuned—and about the major errors that editors thus far seem to have avoided. "In the long run, if *Cell* publishes too many bad papers as the result of rushing, it will lose its credibility. *Science* has a good reputation, and that is more important than publishing in 2 weeks. But," he adds "we do everything possible to publish fast."

Is there an alternative to the race? Yes, agree researchers, administrators, and at least two editors: for competing teams to arrange simultaneous publication, preferably in the same journal, as Hendrickson and Harrison did last fall for their papers on the AIDS binding site. They decided to publish jointly, Hendrickson says, because they realized that "if we went to two different journals, it would inevitably lead to a race between us and the two journals, and then other elements would enter in than who did it first, like how good was your choice of journal and your relationship to it."

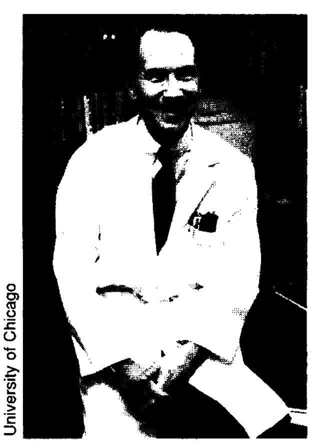
While investigators and editors would prefer publication in the same journal, if two authors have unknowingly submitted their work to different journals, the editors at *Science* and *Nature* will sometimes try to coordinate publication. Simultaneous publication "probably serves everyone's needs best," says Collins, "but it requires people to give a little." It won't work when the two teams are competing and not communicating. Nor will it work when journal editors won't talk to each other. And that means that unless editors come up with some alternative procedures to handle these priority scrambles, as the physics journals have attempted to do, the trend is likely to be with us for some time.

■ LESLIE ROBERTS

Third Strike for Idaho Reactor

Which is mightier, peer review or pork barrel politics? The fate of the Power Burst Facility (PBF), an aging nuclear reactor in Idaho, hangs on the answer to that question. A handful of researchers and legislators hope to turn the facility into a research and cancer treatment center and they have persuaded Congress to stuff money into the Department of Energy's budget to begin modifying the reactor. But, for the third time in recent years, an independent review panel has just advised against spending federal dollars on the project*. Energy Secretary James Watkins is now faced with the choice of siding with his peer reviewers or with powerful members of Congress.

Congressional pressure has already kept the facility going well beyond its planned lifetime. DOE has sought since 1985 to decommission the reactor and tear it down, but Idaho's congressional delegation has managed to insert language in DOE's annual appropriations bills forcing the department to keep the machine on standby at a cost of about $3 million a year. Their ultimate aim is to convert the reactor, which is located at the Idaho National Engineering Laboratory, into a facility for a cancer treatment known as boron neutron capture therapy. This consists of injecting boron compounds into the blood stream and focusing beams of neutrons on a tumor. Boron in the tumor "captures" neutrons, giving the surrounding cells a dose of radiation.


University of Chicago

Not needed. *Hellman's panel gave PBF low marks.*

The price tag for converting the reactor—at least $30 million—spread alarm last year among researchers at Brookhaven National Laboratory and the New England Medical Center, who also are studying this potential cancer therapy. They are concerned that their federal funding would be lost if DOE is forced to fund the PBF conversion and have lobbied hard against the idea (*Science*, 13 April 1990, p. 156). Researchers and engineers affiliated with the Idaho laboratory have responded that the PBF has significant advantages over other medical reactors for treating some types of cancers because it delivers neutrons at a higher rate.

Opponents of the Idaho plan picked up some powerful support last April, when DOE's Health and Environmental Research and Advisory Committee issued a report stating that "there was no evidence to support the conversion of the PBF to a clinical facility." The committee cited the conclusions of a National Cancer Institute group that reported 8 months earlier that adequate boron compounds had not yet been developed.

The Idaho researchers and their congressional delegation were not deterred, however. In June, Senators James McClure and Steve Symms asked Watkins to convene an independent panel to examine once again the merits of converting the PBF. Watkins agreed, and in August he turned to the Institute of Medicine to carry out the task. But Idaho legislators weren't prepared to wait: They used their influence to include $13 million in DOE's 1991 budget for design studies, limited reactor modifications, and maintenance.

The IOM committee, which was chaired by Samuel Hellman of the Pritzker School of Medicine at the University of Chicago, issued its report on 2 January. Its verdict: "There is neither enough information nor is the information currently available sufficiently encouraging to convert the PBF or to maintain it for this purpose." Hellman told *Science* that research on the therapy should continue, but said his committee agreed that the PBF reactor is not needed to carry it out.

If Watkins decides to take the IOM panel's advice, he has two options: ask Congress to rescind the $13 million it appropriated for fiscal year 1991, or spend the money and try to close down the reactor in 1992. Either way, the Idaho delegation would not get what it wants—something it has done with remarkable regularity in the past.

■ MARK CRAWFORD

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