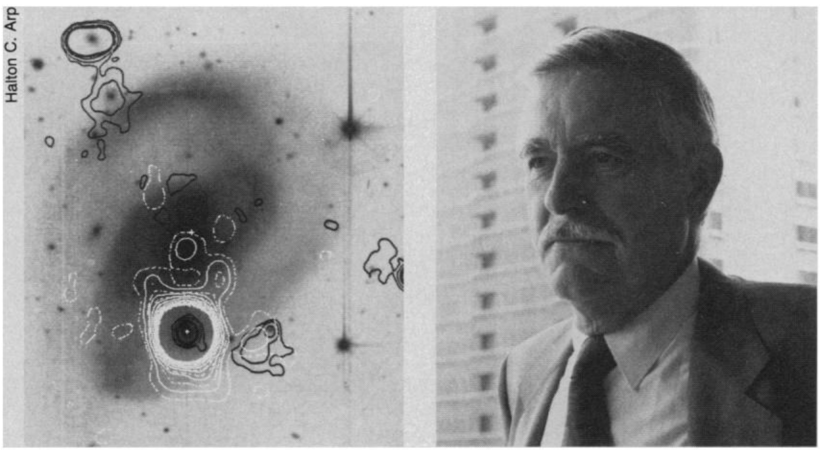
Science Beyond the Pale

Researchers with maverick ideas—particularly in space physics—find themselves fighting an uphill battle for acceptance; should the system be more tolerant of unorthodoxy?

EVERY SO OFTEN A SCIENTIST gets in a scrap with his peers and finds himself (or herself) gradually isolated, ignored, even ostracized for having ideas that aren't accepted by the majority. The community can be harsh when it decides against one of its own.

These cases also pose a dilemma for public agencies. Who should get access to tax-supported facilities? Since the majority can be wrong on occasion, should peers control the gateway all the time? Or should officials now and then let through a theorist too unorthodox to meet with peer approval? And if so, how often? Should as much as 10% of public resources go to research outside the pale? That isn't likely to happen, but it's what one well-known iconoclast astronomer—Halton C. Arp—has been calling for.



Iconoclast. *Astronomer Halton Arp recently found archived satellite data that seem to support his view that the stellar "redshift" theory is wrong. It says the quasar (Markarian 205) and galaxy (NGC 4319) in this image are separated by a vast distance. But x-ray signals (in white) emanating from the quasar appear to link the two.*

The problem of balancing mainstream and outsider interests comes up in every field, but some of the most intense, longest running conflicts seem to appear in space physics and cosmology. This may be so because facts are harder to nail down and ideas tougher to disprove when the subject is millions of light-years away. Then, too, scientists who cannot call upon overwhelming factual evidence to crush an adversary may resort to dogma. That is what several unconventional scientists argue has happened in the competition for scarce telescope time and federal support for research on cosmology.

Arp, as one of the best known of the mavericks in astrophysics, gave a talk this April at NASA's Goddard Space Flight Center, near Baltimore, Maryland, in which he claimed—as he argued in a book published in 1987*—that he is being discriminated against because of his heretical ideas. Arp also claimed he had recently found fresh evidence for his case in NASA's archives.

Another maverick voice in astrophysics comes from not one but a group of astronomers who have taken up the cause of the Swedish physicist Hannes Alfvén. Alfvén is hardly a persecuted renegade; he won the Nobel Prize in 1970 for his theoretical work on space plasma. But many regard his cosmological ideas as belonging to the fringe, and researchers who study his cosmology say they get no public support.

That the science establishment ignores them at its own peril is a point such iconoclasts make whenever they have the opportunity. Often they cite the cautionary example of Alfred Wegener, a turn-of-the-century German meteorologist, to show that there are risks in ignoring the unorthodox.

Wegener published a theory in 1915 suggesting that Earth's continents had once been locked together like the pieces of a huge jigsaw puzzle. He supported it with extensive geological research, but heard it ridiculed as "impossible" by colleagues in Britain and the United States. Wegener died in 1930 an intellectual outcast. Now, 60 years later, his idea is accepted as common wisdom. Could such an event happen even today? Has the system become more open to unorthodox views than in Wegener's day?

Arp, for one, doesn't think so. Educated at Harvard and at California Institute of Technology, he takes issue with the standard "redshift" theory of light and distance in the universe. This is the assumption that the more interstellar light is shifted into the red end of the spectrum, the more distant and faster traveling is its source. Arp's refusal to accept this idea puts him, one astronomer says, at odds with 99.9% of his peers.

The heresy goes further: Arp also doubts aspects of the "big bang" theory, the widely accepted idea that the universe began 10 to 20 billion years ago in a single, explosive burst.

Is Arp mad? Hardly, unless clinging to an idea that just about everyone else has abandoned makes you mad. But even those who feel he is totally wrong see him as a skilled astronomer.

Twenty years ago Arp began finding objects in the sky that challenge conventional wisdom. Some have made national headlines. The photographs *are* odd—many showing highly redshifted quasars in the same patch of sky with low-redshift galaxies. Sometimes the quasars and galaxies appear to be interacting, and there is sketchy evidence of a linkage of hydrogen gas in more than one case between a quasar and galaxy—all of which seems to contradict the fundamental redshift theory. These oddities haven't proved Arp's point, but they have forced other scientists to respond and defend their assumptions.

That is, they did force a response—until Arp's time on the big 200-inch Mt. Wilson telescope was cut off. His peers at the Carnegie Observatories (now belonging to Caltech) ruled in 1981 that "Arp's researches have failed to shift opinion in favor of his views" and that "objective evidence of this failure can no longer be ignored." For this reason the allocation committee decided that "it is no longer reasonable to assign time to Arp to pursue researches aimed at establishing the association of quasars with nearby galaxies" and warned him to "fundamentally redirect" his efforts or lose access to the telescope. He refused to cooperate and

**Quasars, Redshifts, and Controversies* by Halton C. Arp (Interstellar Media, Berkeley, CA, 1987).

was excommunicated—his time on the telescope cut off.

Allan Sandage, a former colleague at Caltech, finds it "unconscionable" that Arp's peers asked him to change his topic of research, a gesture which he says smacks of "the Middle Ages." Yet he thinks the standard interpretation of quasar redshift is correct. "For me," Sandage said, accepting Arp's ideas "would be like saying Newton's laws are wrong."

So it's hardly surprising that, in recent years, Arp has received little attention from his peers. He now works in exile at the Max Planck Institute for Astrophysics in Garching, West Germany. Since losing access to big telescopes, he also has trouble finding new ammunition. Now, at age 63, Arp is seeking to use the latest technology—spaceborne x-ray sensors on the ROSAT satellite and the deep-probing Hubble Space Telescope that went into orbit this year—but with no great success. In proposals submitted with colleagues, he has won a chance to make one observation on each.

In frustration, Arp made a special plea for unorthodox science this year. At his Goddard talk, he said that he no longer expects his peers to share resources with him, for "human organizations rarely reform themselves from within. External pressure is needed to effect change." But he wants federal agencies to adopt a policy that would permit "respectable" workers to retain 90% of the use of public facilities but guarantee a set-aside of 10% for "innovative observations or testing of apparent contradictions of fundamental assumptions."

The idea is not, to say the least, wildly popular. Astronomy centers are short on funds and oversubscribed. In a period of shrinking budgets it is hard to justify projects that look chancy or speculative. According to Laura P. Bautz, director of the astronomy division at the National Science Foundation, the ratio of demand to time available at the big centers is running about four to one. Meanwhile, membership in the American Astronomical Society (AAS) has grown 50% since 1985, raising the number of observers more than 300 a year.

The most powerful devices are oversubscribed by nine or ten to one. Given ratios like these, why should a director turn away projects supported by the entire community to reward mavericks?

Most astronomers think this would be a bad principle to follow in general and specifically a waste of time in Arp's case. AAS executive director Peter Boyce says, "We've been much better to Arp than the geological community was to Wegener." He thinks Arp was given a chance to persuade the community in the 1960s and 1970s, and failed. "He got a tremendous amount of 200-inch [telescope] time," says Boyce, but his ideas "kept not quite panning out."

"We've been rebutting those same ideas now for 20 years, and in the last decade we haven't seen any new observations . . . that would make us feel that the [standard interpretation] of quasars has been challenged," says Daniel Weedman, an astronomer at Pennsylvania State University. A theory has been developed that explains the "one case that would have been the most dramatic evidence" in favor of Arp, says Weedman.

The evidence, discovered about 5 years ago by John Huchra of Harvard, was a low-redshift galaxy with a high-redshift quasar sitting right in its center. However, according to Weedman, the theory of gravitational lensing—which says galaxies can bend light from a distant object to make it appear in a different place and brighter—provided a "clean" explanation that preserves the standard view. According to this view, the quasar is located behind the galaxy, but has been made to appear in its center by gravitational effects. Today, "even though we may continue to stumble upon bizarre configurations in the sky, we now have a very clear theory in place that explains" the combination of high- and low-redshift objects.

To Arp, on the other hand, this is a sign of how hard the majority must labor to prop up a crumbling edifice. Meanwhile, he claims to have found new evidence in the archives of a 1979 x-ray observing satellite that supports his contention that there is a physical link in a quasar-galaxy pair he has made famous (Markarian 205-NGC 4319). It is scandalous, Arp says, that no one ever published this information. He is trying to do so himself.

Arp can claim one highly regarded ally: physicist-astronomer Geoffrey Burbidge of the University of California at San Diego, former director of the Kitt Peak Observatory. Burbidge points out that scientists may have more than an intellectual stake in such arguments. In Arp's case, he says, the community has built up "a huge structure based on assumptions which I think are questionable." If Arp is right, "much of cosmology based on quasars is irrelevant." But the fact that mainstream astronomers have a vested interest in the status quo doesn't make them less credible.

On the other hand, it can be hard to join the mainstream, even playing by conventional rules, once you've been labeled a maverick. Arp's ideas, for example, have been attacked by many of his peers on the grounds that they do not form a coherent theory or put forward a testable prediction. The ability to support a prediction is often cited as the key index of merit in scientific thought. But when a researcher is judged to be beyond the pale, even successful predictions may not win him entry to the mainstream. This at least is what Stephen Brush, a science historian at the University of Maryland, shows in a recent paper in *Eos*† on Nobel laureate Hannes Alfvén, the plasma physicist at the Swedish Royal Institute of Technology.*

Brush looked at five predictions arising from Alfvén's theories of space plasma, including those on magnetohydrodynamic waves, field-aligned currents in Earth's atmosphere, the critical ionization velocity of a gas cloud encountering a plasma, double layers of electrostatic energy near Earth, and magnetic braking of angular momentum in the solar system. Brush reports that the first three are "well confirmed," the fourth is "still controversial," and the last (magnetic braking of angular momentum) is considered "probably wrong." Yet despite Alfvén's success by conventional standards—three winners out of five—most scientists who use his ideas still regard him as unorthodox and rarely cite his work.

Brush attributes this hostility to the fact that Alfvén's peers consider him abrasive and contentious. Brush also suspects that authors omit Alfvén from their notes because they fear being associated with "his band of renegade physicists." Brush concludes that if a theory is not acceptable to

Bildarchiv Preussischer Kulturbesitz

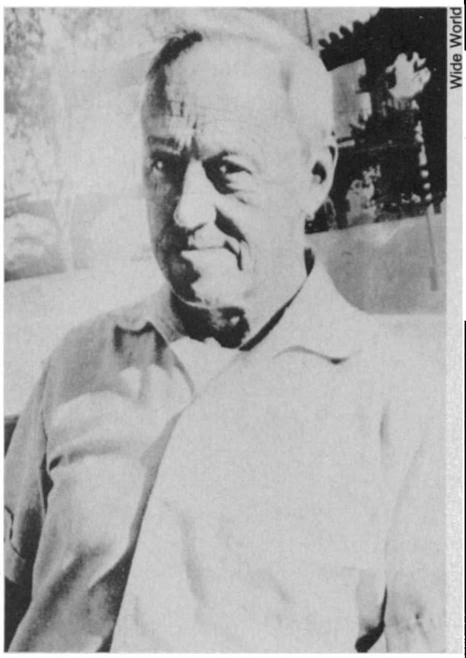
Thinking continentally. *Alfred Wegener, a German meteorologist, was ridiculed for his "impossible" thesis of continental drift in the 1920s.*

†"Prediction and theory evaluation: Alfvén on space plasma phenomena," *Eos*, 9 January 1990, p. 19.

scientists—for whatever reason—"it may not gain any credit from successful predictions."

Anthony Peratt of the Los Alamos National Laboratory—a member of Alfvén's "renegade band"—confirms that these ideas are still being given the cold shoulder. Peratt also says that cosmological theories derived from Alfvén's work have been ignored by federal agencies. This basic concept is that the universe is expanding, but not as rapidly as redshifts indicate, and that it is filled with ionized particles and densely compacted plasma filaments. He claims that 99.9% of the support for work on cosmology goes toward the accepted Big Bang theory. The plasma model of the universe's structure, with no bang, "just doesn't get funded." He and other physicists who work in this area do it on their personal time.

Peratt recently wrote about the difficulty of conducting research beyond the pale, using Darwinian terms: "Where resources are limited, the dominant theory will have an advantage in procuring research funding for advancing its ideas, and, ultimately placing its progeny in influential and flourishing positions."



Wide World

Neglected. *Hannes Alfvén won science's highest honor, but many of his ideas are regarded as unorthodox.*

Burbidge likewise believes that self-interest and turf protection often add fire to theoretical debates. And that's why he thinks peer review sometimes "rewards the lowest common denominator." In his 6 years sitting on the time-allocation committees at Kitt Peak, he observed that very few people propose an original idea. The common view, Burbidge says, is: "If I go out and do something that's unpopular, I'm not going to get tenure. . . . That's true. So there aren't very many unorthodox proposals, and those that come in are usually treated very harshly."

Burbidge says this is the way the system works: "When we come across things we don't like . . . we cut them off, we referee them to death, we don't give them observing time"or grants.

Robert Park, executive director of the American Physical Society, speaks for many when he argues that maverick theories already get a lot of attention. In the end, he says, "most screwy ideas just turn out to be screwy ideas." The prime example is the one physicists have dealt with over the last year: cold fusion. In this case Park thinks the world's scientific institutions have squandered $50 to $100 million disproving an idea that was "preposterous to begin with."

Park thinks public officials go out of their way to accommodate novel theories and risky experiments. One example of risk-taking, he says, is the National Science Foundation's decision to give top priority in 1991 to building a pair of huge laser interferometers to detect gravity waves traveling through space. "It's very controversial," Park says, because some argue it won't get credible results unless another supernova happens to explode nearby. The concept of the experiment is not new, for it originated in Einstein's work on relativity in 1916. But it is chancy in practice because it will cost a lot of money ($192 million) and will have difficulty isolating signals from background noise.

The Laser Interferometry Gravity-Wave Observatory or LIGO is "about as high risk and far out as you can imagine," says Marcel Bardon, director of NSF's physics division. But it fits in with mainstream thinking and has won approval from the division chiefs at NSF and from the National Science Board. Now it's awaiting congressional approval.

Richard Garwin, an IBM physicist who criticized earlier gravity-wave experiments as lacking credibility, finds that LIGO meets even his demanding standards. Yet he questions the wisdom of spending so much on a single experiment, and one whose cost could finance hundreds of smaller investigations. The problem he sees in public agencies is not a tendency to ignore new ideas, but to opt too often for those that promise dazzling and spectacular results. Meanwhile, he says, the things that appear mundane to the world at large—such as research on ceramics and solid-state physics—are neglected. Like Park, he found the cold fusion idea "ridiculous" from the start and thinks it didn't deserve the attention it got.

So, what, if anything, should be done to ensure that unorthodox ideas get a fair chance in the competition for public resources? The NSF's physics director, Marcel Bardon, says the agency relies on the good judgment of its staff. They are told they should ignore peer comments, when it seems right to do so. "We get people who are really knowledgeable about what's going on," says Bardon. "Many of them are here on a rotating basis from the universities; they pay attention to unorthodox ideas."

Rolf Sinclair, chief of NSF's cross-disciplinary physics programs, agrees. He says program officers are delighted to be challenged by new ideas. They are always on the alert for conflicts in peer reviews, and when they think a proposal is getting unfairly criticized, they are encouraged to use their own discretion and override the advice they get. In addition, the work of each program at NSF is itself reviewed by an independent panel every 3 years. That tends to break up any potential clubby exclusiveness.

More important, says Bardon, the NSF began a new agency-wide program a year ago called "small grants for exploratory research." It authorizes each program office to spend as much as 5% of its budget on non-peer-reviewed awards of up to $50,000. Several division chiefs say it's working well, although it's designed to get fresh new researchers started and not to help mature ones avoid their enemies. James F. Hays, director of NSF's earth sciences division, concedes however that mavericks aren't likely to find bureaucrats any easier to deal with than their peers: "The person who can't convince anybody [on a peer panel] is probably going to have a bad time, because he's not going to convince a program officer either."

Astronomer Daniel Weedman suggests that the best way to soothe controversies in his field is to build more telescopes. Then everybody "with a hare-brained idea" could go out and investigate, leaving serious people to do their mainstream work. It might be possible to silence controversy this way, at least temporarily, if the money were available. But fights always seem to occur at the frontier, where people are vying for access to the newest and least accessible equipment. A bought peace might last a few years at best.

But the money is not available to make this solution work, and in the end there may be no good formula for guaranteeing that unorthodox ideas get a fair shake. The best approach may be simply to ensure that people who control public facilities are tolerant and fair-minded about new ideas.

■ Eliot Marshall