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searchers still believe that self-regulation, rather than outside investigation, is the best way to keep the halls of science clean, some say Baltimore and Imanishi-Kari themselves helped bring on the federal fraud-squad by their failure to resolve the issue and/or publish a correction when O'Toole first voiced her objections.

James B. Wyngaarden, then director of NIH, expressed that view in a letter sent to the CELL authors just before public release of the NIH panel report in February 1989. "Even though the allegations have been known to you and the other coauthors of the CELL paper at least since the spring of 1986," he wrote, "the coauthors never met to reexamine the data to determine whether there might be some basis for the allegation; such an analysis on the part of the paper's coauthors, followed by appropriate action to correct such errors of oversights, may well have made a full investigation unnecessary."

But Baltimore responded with a different version of the events. In a letter published in the winter 1989-90 ISSUES IN SCIENCE AND TECHNOLOGY, he wrote: "Wyngaarden was wrong; Thereza Imanishi-Kari, David Weaver and I met with O'Toole under the aegis of MIT professor Herman Eisen within two weeks of learning of her challenge."

O'Toole told the House subcommittee that Imanishi-Kari and Baltimore acknowledged to her in May 1986 that the necessary experiments had not been done, and stated they did not intend to publish a correction.

Indeed, in his Sept. 9, 1986 letter to Eisen, Baltimore wrote: "The literature is full of bits and pieces now known to be wrong but it is not the tradition to point out each one publicly. A retraction generally goes to the heart of a paper and implies that the data is generally unreliable. If the work came solely from Thereza's laboratory I would wonder about what else might be wrong but I am quite certain that what David [Weaver at the Whitehead laboratory] did is solid."

Initially at least, Baltimore opposed the NIH panel's recommendation that he and his coauthors publish a correction to the flawed Table 2. In their November 1988 response to the NIH panel's draft report, the CELL authors wrote: "It was our belief that [the originally published] Table 2 was the best way to summarize a large amount of data in easily accessible form."

The multifaceted controversy reveals deep flaws in the scientific community's current system for handling internal disputes, Gilbert says. That system, in the form of two university-level reviews, dismissed O'Toole's scientific concerns early on. That's partially because the scientific establishment is something of an insider's club, Gilbert told SCIENCE NEWS, adding: "It's also partially that scientists are not suspicious of human behavior."

Meanwhile, the controversy has exacted a heavy toll from the major players involved. Imanishi-Kari continues to work at her Tufts laboratory with NIH funding. However, says her attorney, Bruce A. Singal, "there has been great cost [to Imanishi-Kari] in terms of adverse publicity, harm to her reputation and distraction from her important scientific pursuits."

For his part, Baltimore's selection as president of Rockefeller University prompted strong opposition last fall when a number of faculty members objected to the appointment because of the ongoing congressional inquiry.

O'Toole has been job-hunting since the summer of 1986 and so far remains unemployed in her field of immunology. "The immunology research community is quite small and it functions basically by word of mouth in terms of recommending who is good and who is not," she told Dingell's subcommittee last May. "I was left without a recommendation. I was left without a job. I was left without any support from anybody in the community." □

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Master vs. machine

Michael Lee Jacobs' comments about Gary Kasparov (Letters, SN: 1/6/90, p.3) are presumptuous, to say the least. Kasparov may have used overly romantic language in claiming that chess transcends logic and calculation ("Computer Chess: A Masterful Lesson," SN: 10/28/89, p.276), but he is far from naive about the mathematical basis of computer chess.

In comparing chess to tic-tac-toe and implying that computers can arrive at an optimal chess strategy simply by using their speed of calculation to enumerate and compare different board configurations, Jacobs misses a fundamental point: No chess computer, in choosing a move, can take into account all possible future sequences of moves by itself and its opponent. If it did, it would take longer than the age of the universe to make a single move. (There are more possible games of chess than there are atoms in the universe.) This is a practical consideration that cannot be ignored.

Wayne Schmittberger
East Windsor, N.J.

Jacobs accuses Kasparov of "innumeracy" and states that chess is a "solvable" game that a computer of sufficient capacity should always win against a human opponent. What Jacobs fails to perceive is that good chess is not just a game; it is also an art, an expression of soul, creativity and elegance. Kasparov's two games against Deep Thought are a case in point. In the first game, he played carefully and soundly and won. In the second game, having taken the measure of the machine, he played more daringly and thrashed the thing in a beautiful game.

A computer lacking a sense of artistry may win, but given a choice between a safe, prosaic win and a somewhat riskier but far more elegant win, can there be any doubt which it would choose? When Gary Kasparov said good chess is more than logic and calculation, he was right, and Jacobs missed the point.

Milton B. Garber
Fulton, Mo.

When humans and computers play chess, they both look at the possible configurations some moves ahead and then compare those *without* considering further configurations. No one has been able to find absolute logical rules to do this comparison. Hence, chess as played by man and machine has proved "wider than calculation and logic," as Kasparov says.

In the absence of absolute logical rules to compare different configurations, a human chess player resorts to intuition and a few guiding principles, which are far from absolute. Intuition has enabled good chess players to beat the best computers, even though the computers look at a far greater number of possible configurations.

Haukur Arason
Gainesville, Fla.

As a former chess expert as well as designer and programmer of Seymour Chess (for the Altair 8800), I beg to differ from Jacobs' criticism of Kasparov as "ignorant of elementary mathematics." Kasparov is right in characterizing chess as wider than calculation and logic, requiring imagination.

The trouble with Jacobs' claim that a computer "theoretically can outwit any human player" is revealed by his qualifying "if armed with enough capacity." Give me a computer whose bits lie as close to each other as atoms in molecules and whose calculations occur at the speed of atomic interactions, but with a memory the size of the galaxy and a time limit equal to the age of the galaxy, and I'll write the program to beat Kasparov or any other human chess expert.

Because no computer has world enough and time, the best chess programs must use a truncated form of the algorithm, plus certain shortcuts, plus some approximation of human chess skills (very hard to define in the precise terms a computer needs). Progress in computer chess comes in several ways: by refining assessments at stopping points, by refining the procedures for ensuring that these stopping points are "restful" and by extending the "look-ahead" to twigs farther down each branch. Eventually a computer will beat a human world champion because its deeper "look-ahead" will more than compensate for its deficiencies in simulating human skills.

Danny Kleinman
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