In this era of ever-increasing printed and electronic publications, we often take for granted the accuracy of the information provided. As the meaning of the presentation is debated, we assume that the data on which we base our judgment have been honestly and accurately developed. Occasionally, it is wise to remember that editorial lapses may lead to false conclusions, as the following story will illustrate.

The American Association for the Advancement of Science, along with its weekly publication, *Science*, had served for more than a century as a dependable public source of scientific information. It was no surprise, therefore, that when an article appeared in the September 12, 1952, issue of the magazine claiming success in the prevention and treatment of cataracts by the administration of fish lens protein, the proposal received widespread attention. Soon, an estimated 1000 volunteers had been treated. Within a few months, however, severe complications were being reported by competent ophthalmologists and there were calls for an investigation. The entire matter was quickly settled by the National Research Council, whose Committee on Ophthalmology concluded that the proposed treatment was not only useless but also dangerous.

The article that had aroused such consternation was authored by Ralph F. Shropshire, Jacob R. Ginsberg, and Mendel Jacobi, but included none of the authors’ qualifications. The idea of treating cataracts with lens protein was not new; previous researchers had tried it without success, using beef lens and human lens injections. Even though the organ specificity of lens protein had already been established, Shropshire and associates claimed that the protein in the lenses of fish differed from that found in the lenses of mammal and could be administered safely. They made no mention of the possibility that they might be inciting a hypersensitivity in their patients.

In their meandering and vaguely worded article, Shropshire et al described an indeterminate number of rats, which under dietary stress developed cataracts. The stress later proved to be caused by galactose. After a subcutaneous injection with a secret vaccine prepared with fish lens protein, the cataracts disappeared, especially when the galactose was discontinued. They also described the findings in 14 human patients between the ages of 35 and 70 years, all having received several fish lens protein injections. They claimed spectacular results, although they gave no details describing their method of evaluating visual improvement. There were no control subjects, and no mention was made as to whether vision was tested with or without correction. In the end, they believed it unwise to withhold this wonderful news from the anxious public. Ginsberg’s office address was prominently displayed at the bottom of page 1. Treatment was available for a price.

The 2 junior authors, Ginsberg and Jacobi, were indeed physicians, but neither of them had any experience or train-
shing in ophthalmology. They were apparently naive, and certainly gullible, but provided a degree of legitimacy to the project. They admitted later that they would have been more useful had they taken the time to learn how to use a slit lamp.

Shropshire, the leader of the team, had no scientific qualifications whatever and, apparently, no scruples. He had been appointed to the US Naval Academy in 1920, but resigned after 2 years. He went with Richard Byrd to the Antarctic in 1928, with the impressive title of scientific coordinator, but was dismissed before the expedition ended. He later tried to convince people that the commemorative medal struck for the Byrd expedition was, actually, a Congressional Medal of Honor. In the early 1930s, he showed up in Buffalo, NY, where he convinced the elderly F. Parke Lewis, an ophthalmologist, that he had a good idea about a treatment for cataracts and went to work as an assistant. He was soon able to persuade his employer to try the fish lens antigen he had developed for the treatment of cataracts and was appointed the number of patients before surgery. No accurate estimates of the number of patients who had been treated with the vaccine were done. Towne reported later to the National Research Council that the treatment had no merit. They even attempted to sell the idea to COL Harry King at Walter Reed Hospital, Washington, DC. King did not feel comfortable about using the vaccine, but the vaccine could be picked up in the refrigerator at a certain gas station. On one occasion, a call was made to the hospital, stating that a shipment of the vaccine was to be sent to an unnamed ophthalmologist, and that he had been sent to prison. Posner estimated that more than 1000 patients had received the injections. At the request of the Council of the American Academy of Ophthalmology and Otolaryngology, the matter was referred to the Committee on Ophthalmology.

The committee, composed of several prominent and highly respected ophthalmologists, had heard testimony from several people concerning the vaccine and the individuals involved in the enterprise. In addition, they had heard testimony from 2 editors of Science, both of whom had defended their decision to publish the article by Shropshire et al.1 “The Editorial Board was too small and to too many manuscripts,” they said. This one had been sent to an unnamed ophthalmologist in New York, who thought it should be published with a few corrections that were never made. The committee promptly published its decision.11 The committee said that the treatment was useless and dangerous and that it did not recommend further investigation. Shropshire returned to jail in Florida.

Thus ended the saga of the fish lens protein. Was it simply a sordid tale about an unscrupulous unqualified amateur whose activities caused an unknown degree of distress, and whose persuasive manner made it possible for him to recruit several others to promote his self-centered project, or is there more to the story? Historians tell us we should learn from past experience. Fifty years have elapsed. Experimental, ethical, and journalistic norms have undergone enormous changes. What have we learned?

Shropshire and his colleagues must have fancied that they had reported a clinical trial, but even in 1952, their simple and flawed attempt could hardly qualify as such. Clinical trials have been a favorite method for the study of diseases and their treatment for centuries. Controls were deliberately included in a nutritional study reported in the book of Daniel (Daniel 1:11-16), and later, by James Lind in 1747 in a study of patients with scurvy.12 Clinical trials today bear little resemblance to those published in the past.
Standards for successful trials are outlined in a recent textbook by Meinert. Precise criteria have been established and widely accepted. Present clinical trials often include data collected at numerous academic institutions, monitored by impartial judges, and subjected to close scrutiny by each member of the team. Before publication, these studies are further scrutinized, often by several impartial referees.

In 1952, the scientific community had few rules by which to judge clinical trials. Even so, ophthalmologists recognized the nature of the fish lens scam, and moved quickly to investigate its source and to take measures to bring it to an end. The National Research Council acted with great alacrity, and the damage was, thus, limited to a few patients.

There remains the serious moral issue regarding human experimentation. Did the patients who received fish lens protein injections know how they were being used? Those from the Committee on Ophthalmology who interviewed some of the patients were impressed by the naive acceptance of risk by those who were anxious for a good result.

In 1952, the doctrine of informed consent had been talked about, but was hardly a topic of daily discourse. In 1907, Osler had contended that "as long as participants had full knowledge of the circumstances and willingly submitted to the experiment, studies on volunteers were not only permissible but praiseworthy." Although much discussion had taken place before 1952, Osler's sentiments could be considered the norm. Lederer, in her book Subjected to Science, reviewed the subject of human experimentation as it occurred before World War II, observing that the modern history of human experimentation began with the Nuremberg Code, written in 1946. She properly distinguishes between therapeutic and nontherapeutic experimentation. She also asks the question: "Is it fair to apply the ethical standards of the 1980s and 1990s to researchers working in the 1930s and 1940s when, presumably different ethical norms were in place?"

Publication in reputable journals is the ultimate goal of all investigators. Medical publications number in the hundreds, and elaborate editorial boards sit in judgment on the quality of the manuscripts submitted to them. It seems hardly likely that articles such as those by Shropshire would be accepted today, either by Science or the Archives of Ophthalmology.

Francis Adler, who was then editor-in-chief of the Archives of Ophthalmology, was a champion of a new movement to improve the quality of medical journals by insisting on proved valid scientific information. In his address before the 1953 meeting of the American Medical Association's Section on Ophthalmology, he chastised the editors of Science for accepting the manuscript by Shropshire, but made no mention of the equally flawed articles by Shropshire that had appeared in the Archives of Ophthalmology in 1937, during the term of a previous editor.

Yet, we owe much to editors like Adler, Derrick Vail, David Cogan, and Frank Newell, who brought ophthalmic journalism from the relatively uncritical mid-20th century to the excellence we see in the hands of a new generation of superb editors.

It might be assumed that the type of story described herein would be unlikely to be repeated. On October 30, 2002, however, a similar tale was recounted in an Associated Press release. Once again, Science was victimized, this time because of false data provided by a physicist in a series of studies on superconductors. In this case, a single physicist had submitted many manuscripts based on fanciful information and had convinced several coauthors to lend their names to his projects. Once they discovered that the information serving as the basis for these publications was fabricated, these coauthors demanded retraction of 8 of the articles already published. Some reputations were already severely damaged. Robert C. Haddon, one of the coauthors, stated the obvious: "When researchers combine their studies to produce a single paper, each scientist depends upon the honesty of work provided by the other co-authors. That is the way science is supposed to work."