nerves from fragments of embryonic frog spinal cord maintained in clots of lymph. On Burrows' return to the Rockefeller Institute, he and Carrel began growing cells from a variety of tissues.

Carrel did not use organ perfusion in this work, although his organ perfusion studies began at about the time that he took up tissue culture. The perfusion experiments achieved considerable publicity in the 1930s after the collaboration between Carrel and Charles Lindbergh that culminated in the development of the so-called "glass heart" (*2*).

Carrel maintained the "immortal" heart cell cultures for only 6 months of their long life (*1*). The cultures were probably established in January 1912 and became the responsibility of Albert Ebeling in June 1912. Ebeling took them with him when he moved to the Lederle Laboratories of American Cyanamid in 1939, where the cultures were eventually discarded in 1946.

In the light of many subsequent studies, it seems unlikely that Carrel's cells were immortal. Like normal human diploid cells, chicken cells are very stable in culture, and as far as I am aware no authenticated cases of spontaneous transformation of chick cells have been reported. The longevity of Carrel's cultures has been explained by inadvertent or deliberate contamination of the cultures by cells present in the chick embryo extract used to feed the cultures (*1*). However, as B. L. Strehler remarked, the ultimate effects of the aging process made it impossible for Carrel to respond in his own defense to the questions that were being raised already in the 1940s (*3*).

Although Carrel's work on these cells was literally "incredible," Culliton is not the first (and probably not the last) to be impressed. In 1921, a journalist for *The World* wrote that if all the cells had been kept, they would have formed a "rooster big enough today to cross the Atlantic in a stride; it would also be so monstrous that when perched on this mundane sphere, the World, it would look like a weathercock" (*4*).

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Asteroid Paradox

Despite the Hollywood maxim that there is no such thing as bad publicity, the quotes from my lengthy telephone conversation reported in Richard A. Kerr's article "The great asteroid roast" (Research News, 2 Feb., p. 527) tempt me to swear off talking to reporters. There are two reasons for my being disturbed. The first is simply that I don't want people to think I am as intolerably arrogant as these quotations would suggest. The second is that for many years I have been advancing the view that the eagerness of reporters, historians, and many scientists to consider all serious scientific puzzles in terms of personal controversies is detrimental to the progress of science and that the apparent historical importance of controversies stems primarily from their sensational nature. I propose that it would be more illuminating if reporters were to use their skills to persuade scientists to expose what they know to be the most vulnerable aspects of their own work, rather than those that are easiest to defend against criticism by those less knowledgeable in their specialty.

All those concerned would agree that meteorites and their asteroidal sources hold the key to our understanding some of the deepest mysteries of the earliest solar system. It should be embarrassing to all of us that the sources of the most abundant type of meteorite appear to be nearly absent from the asteroid belt. If we can't understand the present solar system, how can we hope to see back 4.5 billion years?

There must be some answer to this "spectroscopic paradox," even though it is easy to give plausible arguments showing that every possible resolution of it is probably wrong. I actually find Jeffrey Bell's suggestion that the ordinary chondrite sources are concentrated among the smaller asteroids quite attractive. If only someone could find out what is wrong with the good reasons that seem to argue against this. We few who are trying to understand the dynamical aspects of the problem are best equipped to identify what may be faulty in our own reasoning and calculations, and spectrophotometric observers have a similar opportunity. I believe discussions with one another must be directed toward attempts to explain and understand, rather than toward trying to win an argument.

It is true that if I had to guess, I would guess there is something wrong with the interpretation of the spectroscopy of either the S or the C asteroids. But guesses aren't worth reporting.

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The Antibiotic Record

Robert P. Crease, in his article "Righting the antibiotic record" (Research News, 17 Nov., p. 883) reports on the symposium "Launching the Antibiotic Era" held at Rockefeller University on 23 October 1989. The symposium marked the 50th anniversary of the discovery of gramicidin by Rene Dubos.

I write to clarify certain points arising in the report as they concern Alexander Fleming, the discoverer of penicillin. We are told that Dubos was the first to put an antibiotic to clinical use, having discovered gramicidin around 1939. This misrepresents the published facts concerning penicillin. Fleming's penicillin had been used on patients in 1930 by C. G. Paine, a former student of Fleming. Paine used crude penicillin successfully on patients by local application in the treatment of particularly severe eye infections in Sheffield, U.K. In the course of treating five patients, he demonstrated a clinical cure of infections caused by the pneumococcus, the staphylococcus, and the gonococcus (*1*).

Some of Fleming's pupils are also adamant that Fleming had not lost interest in the potential clinical use of penicillin even as late as the second half of the 1930s; but Fleming was no chemist, and the purification of crude penicillin had always been beyond his capabilities. The need for purification of penicillin was recognized by the Oxford University workers, who began to take up the demanding challenge around 1938.

Fleming did not "misunderstand" the