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L. E. Scriven is a member of the Department of Chemical Engineering and Materials Science at the University of Minnesota-Minneapolis where he is also a Professor of Fluid Mechanics and Fellow of the Supercomputer Institute. He is affiliated with 3M, the National Research Council, Exxon Research and Engineering, the Massachusetts Institute of Technology and the United Nations Industrial Development Organization. He was a Guggenheim Fellow, received the Colburn Award and the Walker Award and is a member of the National Academy of Engineering.

Letters to the Editors

Plant pioneers

To the Editors:

John G. Torrey's excellent article "The Development of Plant Biotechnology" (*Am. Sci.* 73:354-63, July-Aug. 1985) is not only interesting in itself but also valuable as a historical document by a pioneer in the field. Like a number of similar reviews, it will become an important source of information for those who are interested in the history of the plant sciences. For this reason, it is important to point out that Georges M. Morel did not originate meristem culture.

The American botanist Ernest A. Ball developed meristem culture in 1946, and, according to the French botanist Roger J. Gautheret, for a time an associate of Morel, Ball's work laid the foundation for micropropagation. Morel's work came much later. Even before that, the Chinese botanist Loo Shih Wei was successful in culturing stem tips of *Asparagus* and dodder. Later the French plant pathologists P. Limaset and P. Cornuet found that, like those in root tips, meristematic cells in shoot apices were virus free. At their suggestion, Morel and C. Martin successfully produced healthy shoots from virus-infected *Dahlia* and potato plants. However, these shoots failed to form roots at first. This was achieved by others in 1961, 1968, and later.

According to one report, Morel became interested in shoot-tip culture of orchids as early as 1955 or 1956. However, his first publication on the subject in 1960 did not make much of the potential for clonal propagation. This and a number of his subsequent publications on orchid meristem culture were sparing in detail and gave contradictory information on the culture medium he employed, listing it once as Knudson III (the correct name is Knudson C) and a second time as Knopp's solution. Morel called attention to the micropropagation potential of shoot-tip

cultures only in 1964. However, his paper appeared after the American botanist D. E. Wimber had published a detailed account of *Cymbidium* mericlone by means of shoot-tip culture in an entirely different medium.

The reasons for the delayed publication by Morel and the paucity of details in his early papers are not clear at present. Some have suggested that his close contacts with a French orchid nursery may have had something to do with it.

Regardless of how one chooses to interpret the historical developments which led to Morel's work, the first successful attempt to propagate orchids—and probably any plant—in vitro by a method that would now be called tissue culture micropropagation or mericlone was made by the American botanist and orchid specialist Gavino Rotor at Cornell University and published in 1949. Rotor placed sections of *Phalaenopsis* flower stalks on Knudson C medium and obtained plantlets from their buds. His method was used sporadically by orchid growers for 15 to 20 years until improved procedures replaced it.

Joseph Arditti
Department of Developmental
and Cell Biology
University of California, Irvine
Irvine, CA

Professor Torrey replies:

I believe that most scientists involved in research on plant organs and tissue culture, including Ernest Ball, would agree with my statement that Georges Morel was one of the pioneers in the study of shoot meristem culture as well as an early advocate for its practical use in multiplication of virus-free plants. More important in reply to Professor Arditti's comments is the need to dispel the notion that Morel's horticultural contacts may have interfered with the publication of his findings on cultured meristems and tissues. No one could have been more interested in the free exchange of scientific information and discoveries than Georges Morel. His publications of details for "establishment of cultures from different species and their growth on different media" go back at least to 1950. Collaborative work with R. Wetmore was published in 1951 and thereafter. The practical ap-

plications for horticulture and agriculture followed in the late 1950s and early 1960s.

In 1970 I received a personal letter from Dr. Morel expressing concern over the threatened restriction of the practical use of meristem culture techniques by an application to the US Patent Office for a patent on a "method for rapidly reproducing orchids," drafted by an American "inventor," in which Morel's published works were actually cited as sources relied upon for information. In that letter Morel wrote: "I didn't take any patent myself because I feel that a scientist doesn't have to do this. . . . I thought that in all countries it was like France. Once a discovery on new techniques has been published, it is impossible to patent it."

Plant scientists around the world, funded by government, foundation, and private resources, contributed to the many aspects of the development of plant biotechnology summarized in my article. Implicit in this account is the recognition that society must continue to support a broad spectrum of basic research undirected toward practical ends if the flow of new ideas and new facts is to be available for the biotechnology of the future.

Athletic anemia and oxygen extraction

To the Editors:

In his interesting article "Physiological Adaptations to Aerobic Training" (*Am. Sci.* 73:334-43, July-Aug. 1985), Ethan R. Nadel raises some important issues regarding how anemia is defined and measured, particularly in the context of "sports anemia."

Clinically, anemia is considered to be present if either the number of red blood cells per liter of blood, the hemoglobin concentration, or the hematocrit is below established norms. The situation is generally pathological. The low relative values usually reflect absolute decreases in the mass of red blood cells, with an imbalance in the ratio of oxygen supply to demand, manifested by breathlessness and lethargy.

This clearly is not the case in those subjects with sports anemia. While there may be a mild clinical anemia due to hemolysis in the initial phases of exercise training, which is displayed as an absolute decrease in the

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mass of red blood cells or total body hemoglobin, sports anemia in the trained athlete is characterized by an absolute *increase* in the mass of red blood cells. It is the larger relative increase in the plasma volume which results in the apparent decrease in the hemoglobin concentration. The anomaly of a low hemoglobin concentration in the presence of either an absolute increase or decrease in the mass of red blood cells clearly demonstrates the confusion which may arise by the classification of anemia based only on nonabsolute measures. In other words, the hemoglobin concentration is a poor measure of the total mass of red blood cells, and perhaps the term anemia should be restricted to those instances where an absolute decrease in mass has been demonstrated.

In athletes the so-called anemia is probably a nonpathological, physiologically normal part of the process of adaptation to a new condition. Hematological data on athletes suggest that the optimum hematocrit—one that provides for adequate oxygen transport without a counterproductive increase in blood viscosity—may

be somewhat lower than that in more sedentary subjects. Many theoretical and mathematical models of the regulation of production of red blood cells have been constructed using either the hematocrit or hemoglobin concentration as the prime regulator. Few if any models use the total blood volume or mass of red blood cells for this purpose. Indeed, we know very little of how total blood volume affects erythropoiesis.

Christopher D. R. Dunn
Wotton-under-Edge
Gloucestershire
United Kingdom

To the Editors:

Though Professor Nadel's article is an interesting account, there appears to be a serious omission in the section on "oxygen delivery and extraction." A significant aspect of the delivery of oxygen to muscles is the ease of dissociation of oxygen from hemoglobin, a matter that involves any variable affecting the affinity of hemoglobin for oxygen. Some of the variables change with the demands



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of exertion. For example, red-cell organic phosphates (particularly 2,3-diphosphoglycerate) promote dissociation of oxygen from hemoglobin, and levels of these substrates are altered in the erythrocytes of those who exercise regularly.

J. M. Ramsey
Department of Biology
University of Dayton
Dayton, OH

Professor Nadel replies:

I agree that under certain conditions changes in 2,3-diphosphoglycerate can be stimulated and act to shift the oxygen dissociation curve to the right, thereby promoting greater dissociation of oxygen from hemoglobin at a given partial pressure of oxygen. In writing my article, I was not aware of the importance of this as an adaptive response to training, having assumed that the very low partial pressure of oxygen of venous blood draining skeletal muscle ensured near-maximal oxygen extraction during heavy exercise in both unfit and fit people.