Response

Schechter and Rodgers shed a welcome light on recent advances in the field of sickle cell anemia therapy. However, in my Perspective article, I focused on a historical argument.

Therapies and cures depend on much more than an experimental breakthrough or a promising clinical trial (*1, 2*). The therapeutically "useful agents" Schechter and Rodgers mention remain at a preliminary stage, even according to the review they cite. The review notes "conflicting results" for recombinant erythropoietin, "conflicting data" for butyrate, "limited clinical trials" for 5-azacytidine, and "appropriate concern about the possible induction of tumors" for long-term hydroxyurea treatment (*3*). Apart from hydroxyurea, none of these "useful agents" has gone beyond the stage of "experimental therapy" (*4*) and thus is not yet a social reality for sickle cell anemia patients.

My historical perspective does not deny the progress achieved by medical researchers—including Schechter and Rodgers—in the treatment of sickle cell anemia. My claim was in regard to the origin of such progress, not its very existence. Even the development of hydroxyurea treatment, with its undeniable therapeutic benefits, was not a simple outcome of our understanding of the disease's molecular etiology. Indeed, the initial insights for this therapy were largely of a clinical and epidemiological nature (*5–7*). It would not make sense to address "an implicit criticism of the reductionist approach of the current era of molecular medicine." What deserves our criticism is the overly optimistic promises, made by Pauling and many after him, that knowledge of molecular processes alone would quickly and easily yield effective therapies at the molecular level. The story of sickle cell anemia, along with that of many other diseases, highlights the fact that the development of new therapies has come about not just through basic research reaching the clinic, but rather through "continuous feedback between basic and clinical research," as Schechter and Rodgers have nicely put it—restating the basic point of my Perspective.

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References

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3. H. Bunn, *N. Engl. J. Med.* **337**, 7629 (1997).
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Room-Temperature Hydrogen Storage in Nanotubes

In the reports "Hydrogen storage in single-walled carbon nanotubes at room temperature" by C. Lui *et al.* (5 Nov., p. 1127) and "High H_2 uptake by alkali-doped carbon nanotubes under ambient pressure and moderate temperatures" by P. Chen *et al.* (2 July, p. 91), the authors make the same error in referencing our work on hydrogen storage in carbon single-wall nanotubes (*1*). There is apparently some confusion on the matter, which we wish to clarify here, referring to Liu *et al.*'s report for illustrative purposes.

Liu *et al.* say that we investigated hydrogen adsorption on single-wall nanotubes at 133 kelvin. This is not correct. This was simply the temperature we cooled the sample to in the presence of hydrogen at a pressure of 300 torr. The hydrogen gas was evacuated from the chamber after the sample was cooled, and temperature-programmed desorption spectroscopy was then performed to assess H_2 adsorption onto the nanotubes. As the temperature was raised at 1 kelvin per second, hydrogen was evolved from the sample. Hydrogen was stable on the surface of the nanotubes to temperatures well in excess of 133 kelvin, and the rate of hydrogen evolution peaked between 275 and 300 kelvin (room temperature). To a first approximation (in the absence of mass transport limitations), the shape of the temperature-programmed desorption spectrum is a map of the density of hydrogen on the sample as a function of stability temperature. The fact that the spectrum peaked around room temperature indicated that some hydrogen was stabilized to this temperature by the single-wall nanotubes. This observation was the point of our *Nature* publication and was appreciated and referenced correctly in an earlier paper from this group (*2*).

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References and Notes

1. A. C. Dillon *et al.*, *Nature* **386**, 377 (1997).
2. Y. Y. Fan *et al.*, *Carbon* **37**, 1649 (1999).

Response

Our misunderstanding of Dillon *et al.*'s work (*1*), which we cited in our report (5 Nov., p. 1127), came from the following

interpretation of their *Nature* paper by us. A sentence from their experimental procedure description reads, "Standard H_2 exposures were carried out at 300 torr for 10 minutes at 273 [kelvin] followed by 3 minutes at 133 [kelvin] except where noted." Because H_2 storage capacity is normally higher at lower temperatures, we thought that the 5 to 10 weight percent storage capacity occurred at the temperature of 133 kelvin.

In their letter, Heben and Dillon say, "Hydrogen was stable on the surface of the nanotubes to temperatures well in excess of 133 kelvin, and the rate of hydrogen evolution peaked between 275 and 300 kelvin....The fact that the [evolution rate] peaked around room temperature indicated that some hydrogen was stabilized to this temperature by the single-wall nanotubes. This observation was the point of our *Nature* publication...." It is true, we think, that the H_2 adsorption capacity at lower temperatures such as 133 kelvin is higher than at 300 kelvin, but once H_2 is adsorbed at lower temperatures, it is possible that it is not easy to desorb the H_2 even at higher temperatures. So we argued in our report that the statement that hydrogen was stable on the surface to temperatures well in excess of 133 kelvin and up to room temperature may not directly indicate that the hydrogen uptake just at room temperature, without first cooling down to lower temperature, was as high as 5 to 10 weight percent. We apologize for our misunderstanding of their paper.

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1. A. C. Dillon *et al.*, *Nature* **386**, 399 (1997).

CORRECTIONS AND CLARIFICATIONS

In the report "Crystal structure of *Thermotoga maritima* ribosome recycling factor: A tRNA mimic" by M. Selmer *et al.* (17 Dec., p. 2349), the e-mail address for the corresponding author should have read as follows: anders.liljas@mbfys.lu.se

In the News Focus article "Europe stresses prevention rather than cure" by Michael Hagmann (17 Dec., p. 2258), the source of the graph should have been credited to Massimo Battaglia at the Institute of Experimental Medicine of the National Research Council, Rome, Italy. Full details of the results can be obtained at the following Web address: http://pages.inrete.it/mbiomed/retrax.htm

The study by K. Tajima *et al.* described in the Random Samples item "Backtracking a mummy virus" (10 Dec., p. 2071) appeared in the December issue of *Nature Medicine*, not *Nature Genetics*.

In the report "Transition states between pyramids and domes during Ge/Si island growth" by F. M. Ross *et al.* (3 Dec., p. 1931), the lengths represented by the scale bars in figures 1, 2, and 3 are 250 nm, 500 nm, and 200 nm, respectively.

In the News Focus article "Physicists and astronomers prepare for a data flood" by Mark Sincell (3 Dec., p. 1840), Tufts University particle physicist Krzysztof Sliwa was mistakenly referred to as computer scientist Krystof Sliwa.

In the Perspective "Respiration without O_2" by Lars Hederstedt (*Science*'s Compass, 18 June 1999, p. 1941), the statement in the last sentence in the second paragraph of the middle column on page 1942 is wrong. Heme b_H is in both complex II and complex III close to the negative side of the membrane, that is, there is in this respect no difference between the complexes.