

Letters

Space Commerce

M. Mitchell Waldrop (News and Comment, 24 Aug., p. 812) does not describe the segment of the commercial space industry that has received the largest total private investment to date: upper-stage rockets. More than \$100 million in private funds has been committed to the development of upper-stage systems, which are needed to transport satellites from the low earth orbit (180 miles) of NASA's space shuttle to geosynchronous orbit (22,300 miles) and other high-energy orbits. Such stages have been developed without charge to the government, providing exactly the kind of "leveraging" of the government's budget sought by NASA.

Although upper stages generally have the capability to fly on U.S. expendable launch vehicles, such as the Delta or Titan, as well as on the space shuttle, most upper-stage manufacturers believe that no substantial increase should be made in the launch price of the space shuttle. The reason is that the launch services industry has been targeted as an area of strategic technological and economic importance by several countries and is, appropriately enough, global rather than national in scope. A shuttle price increase is far more likely to send customers to government-supported launch facilities abroad than to increase the demand for U.S. expendable launch vehicles.

BRUCE W. FERGUSON
*Orbital Sciences Corporation,
1951 Kidwell Drive,
Vienna, Virginia 22180*

Stereochemistry

Thomas H. Maugh II's article "Stereochemistry in a new light" (Research News, 31 Aug., p. 915) discusses a recent paper of ours (1). The main thrust of our paper was to dispel some confusion in modern stereochemical theory. The natural emphasis of stereochemistry on structure makes it important that the concepts are visually exemplified. Unfortunately, several errors occurred in the structural formulas printed

in Maugh's article. (i) The formula for vespirene on page 915 (column 3) is missing two connecting bonds between the rings; the positions para to the $(CH_2)_n$ bridges should be pairwise-linked to form two fluorene units. (ii) One of the two H's attached to the central carbon atom in the glyceraldehyde formula on page 916 (column 2) should be deleted. (iii) The two H's that are directly attached to the carboxylate carbons in the *meso*-tartaric acid formula on page 916 (column 3) should be deleted. In addition, the reference in the last line of page 915 to the "center of symmetry" in the vespirenes and the doubly bridged biphenyls should be to their centers of mass. The interested reader is encouraged to consult the primary paper.

KURT MISLOW

JAY SIEGEL

*Department of Chemistry,
Princeton University,
Princeton, New Jersey 08544*

References

1. K. Mislow and J. Siegel, *J. Am. Chem. Soc.* **106**, 3319 (1984).

DeBranges' Proof

Louis de Branges' resolution of the Bieberbach conjecture (Research News, 7 Sept., p. 1006) is an event of unusual mathematical and human interest. However, the account by Gina Kolata does not discuss several important aspects of this and sensationalizes others.

I am not sure what Kolata's description of de Branges as being on the "fringe of the active research community" means. In fact, de Branges has been consistently publishing articles throughout his career and is also the author of two books. He has won Sloan and Guggenheim fellowships. During the past few summers he has been an invited participant in mathematical activities in Paris and Israel, in addition to the Soviet Union. He has also made errors in several theorems, but I think it inappropriate that only this aspect of his past was deemed worthy of mention.

Some attention should have been paid to the interdisciplinary aspects of this event. By early 1984, de Branges had

reduced his approach to the Bieberbach conjecture to an explicit question concerning special functions, specifically the nonnegativity of certain sums related to Jacobi polynomials. That there could be any connection between univalent functions and these sums even now astonishes the mathematical community. (However, the interplay between these classical spaces of functions with entire function theory has been a major theme of de Branges' work.) An important role in showing that this approach had promise was played by his colleague Walter Gautschi, who, using his own software and the Purdue computer, was able to verify de Branges' conjecture concerning these sums, at least up to n about 30. Soon after, Gautschi contacted Richard Askey at the University of Wisconsin, and Askey reported that he and George Gasper had already settled these questions for all n in 1976. The Askey-Gasper results were of considerable depth, and the fact that de Branges was led to them for completely independent reasons appears to have made a dramatic impression on Gautschi and Askey.

Of course, Milin and his colleagues in Leningrad performed an extraordinary service to de Branges and mathematics, and Kolata's account captures this very well. In the light of this history, it is odd that Kolata included the gratuitous remark that "publication in a Soviet journal would not have [had] the credibility of an American publication."

DAVID DRASIN

*Department of Mathematics,
Purdue University,
West Lafayette, Indiana 47907*

Lamarckian Evolution

In Jean L. Marx's interesting article about a recent plant biology symposium ("Instability in plants and the ghost of Lamarck") (Research News, 29 June, p. 1415), several experimental observations are described as being "reminiscent of the Lamarckian concept of evolution—a change acquired in response to an altered environment became hereditary." The experiments in question showed that plants whose phenotype had been altered as a result of growth in different concentrations of inorganic nutrients (K, N, P, and so forth) gave rise to progeny in which the changes had become heritable. These changes affected a variety of characters, such as size and form.

The essence of "the Lamarckian concept of evolution," often imprecisely summarized as the inheritance of ac-

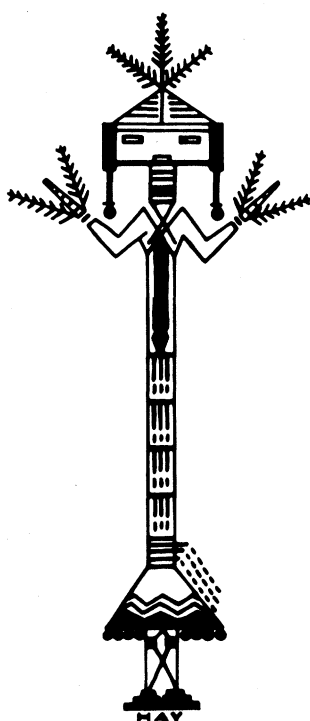
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quired characteristics, is more precisely the stable inheritance of *adaptive* changes induced in an individual organism by an altered environment. In other words, the acquired characteristics that are inherited must be adaptive with respect to the environmental change that induced them if the inheritance is to be considered Lamarckian. So far as I know, no authenticated evidence for this type of heredity exists, although many false claims have been made.

In the cases described by Marx, the phenotypic changes seem to have had no *adaptive* relation to the altered environments that provoked them and therefore have nothing whatever to do with Lamarckian evolution. They are, to me, not "reminiscent" of Lamarckism, but of a number of misguided attempts to use experimental results showing nonadaptive hereditary change induced by an environmental alteration as evidence for the Lamarckian model of evolution; these experiments, like those cited by Marx, are interesting and important, but do not constitute evidence for, or even bear on, "the Lamarckian concept of evolution."

J. GORDIN KAPLAN

Department of Biochemistry,
University of Alberta,
Edmonton, Alberta, Canada T6G 2J9

On Making Comparisons: Reminded Again

As scientists we frequently make comparisons: between arbitrarily defined groups, between species, between countries, between ideas, theories, or whatever else takes our fancy. Yet, in making these comparisons, we are frequently guilty of a gross illiteracy. I refer to the common, almost universal, practice of comparing one thing to another. The verb *to compare* may be used with either the preposition *with* or *to*, but the two uses carry almost opposite meanings. If one thing is *compared with* another, the two are set side by side and the degree to which they differ is determined. This is the meaning usually implied in scientific writings. If one thing is *compared to* another, however, the two things are being likened and their similarities emphasized, as, for example, in "Shall I compare thee to a summer's day?"

It could be argued perhaps that the common usage of *to compare to* is sanctioned, by the frequency of its use, as an acceptable alternative to the less common (but correct) usage of *to compare*

with. At the risk of being pedantic, I do not think this argument can be accepted even for common American parlance; the latest edition of *Webster's Unabridged Dictionary* clearly reiterates the above distinctions.

Scientists pride themselves on their clarity and precision of thought. We should be no less concerned about the clarity and precision of what we write. This applies not only to authors of scientific works but more important to their editors, who have a particular responsibility to ensure the accuracy of the written word; the latter could surely afford the extra two ems to make comparisons *with* instead of *to*.

IAN A. GREAVES

Occupational Health Program,
Harvard School of Public Health,
Boston, Massachusetts 02115

Recombinant RNA Research

I would like to offer supplementary information about the article "The birth of recombinant RNA technology" by Roger Lewin (23 Dec. 1983, p. 1313). First, *in vitro* recombinant RNA has been practiced in many laboratories for several years, thanks to the pioneering work of Uhlenbeck, Gumpert, and their co-workers at the University of Illinois. Lewin's article attributes this technology to E. Miele, D. Mills, and F. R. Kramer (*1*). It is clear in Lewin's article that the major impact of the work by Miele *et al.* is the *in vitro* amplification of RNA with biological function by the introduction of foreign RNA into a vector derived from Q β RNA. However, in a similar article published in May 1982, Shen Tongjian and Jiang Meiyuan reported the introduction of poly(A) into Q β RNA, which was subsequently used to infect and thereby amplify the RNA insert. Although characterization of both the *in vitro* recombinant RNA and the product of replication *in vivo* was not fully described, it appears that the Chinese scientists have achieved the *in vivo* amplification of recombinant RNA.

R. J. CEDERGREN

Department of Biochemistry,
Université de Montréal,
Montréal, Québec, Canada H3C 3J7

References and Notes

1. E. Miele, D. Mills, F. R. Kraker, *J. Mol. Biol.* 171, 281 (1983).

Erratum: In the last paragraph of Arthur L. Robinson's Research News article "High spatial resolution ion microprobe" (14 Sept., p. 1137), J. Ronald Hass' name was misspelled.