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.

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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.

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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."

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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-