The contributions of feminism to science and the lack of women who have contributed to organic total synthesis are pointed out. A reader says that he "cannot remember reading a paper on total synthesis from which [he] did not learn something useful." **Phylogenetic studies are said to lead us to expect chimp multiculturalism. It is emphasized that the discovery of DNA was reported in 1865 and that of DNA structure in 1953. And a reader points out that** "90% of NASA-funded protein crystallization research is conducted on the ground."

How Women Contribute

I am grateful to Florence P. Haseltine for reviewing my book "Has feminism changed science?" (Harvard University Press, 1999) (*Science*'s Compass, 23 July, p. 538). Her review, however, presents the central question of the book as, "What contributions have women made in science that a man could not have made?" This is precisely what the book argues against. The whole point of my book was to argue that it is wrong to imagine that women do science differently simply because they are women. My claim is that "feminism" as a broad-based social movement and an academic theoretical perspective and that "feminists"—both men and women—have changed science.

CREDIT: FROM D. FORT AND H. VARNEY, *SCIENCE AND CHILDREN*, (MAY 1989), P. 12/COURTESY NATIONAL SCIENCE TEACHERS ASSOCIATION

The question of who or what might create change in science beneficial to women has been confused by a mistrust of feminism. Feminism is, for many, still a dirty word, even though few people would claim to be against equal opportunity for women. People seem to prefer to discuss women rather than feminism. This refusal to acknowledge politics has led many to overemphasize women as agents in the process of opening up science to feminism. Primatologist Linda Fedigan once remarked how dismayed she was when, after having put many hours into learning to identify individual female monkeys within a large group, some of her colleagues attributed her success to her sex; females are "empathetic," she was told, and this approach therefore is easy for them. In fact, Fedigan's success depended on, among other things, her taking seriously the project of a feminist enlargement of science.

Some of the desire to map the successes of feminism directly onto women derives from the fact that, historically, women as a group were excluded from science for no reason other than their sex. Some of the confusion derives from the fact that feminists have been far more likely to have been women than men. Confusion also derives from the fact that nonfeminist women have benefited from battles won by feminists.

Because modern science is a product of hundreds of years of actively shunning women, one can identify systematic gender bias in the institutions, cultures, and content of the sciences. Putting the test of gender bias to science is simply one further aspect of the critical method for improving human understanding of nature. As I argue in my book, the goal is not to create a "feminist science," if that means a special or separate science for women or feminists. Science is a human endeavor; it must serve us all, including women and feminists—male and female.

A prominent (male) biologist once told me that he could name many more examples of feminist perspectives in biology than the ones I document in my book, but that as soon as they become part of mainstream biology, they are no longer identified as "feminist." It is interesting that when feminist insights become mainstreamed in a science, they are thought of simply as "good science." And perhaps they simply are. Nonetheless, we should not forget the historical circumstances of their origins.

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"Macho Total Synthesis"

Robert F. Service (News Focus, 9 July, p. 184) has written an insightful critique of what insiders call "macho total synthesis," in which huge teams (20 to 30 postdocs and graduate students representing a critical mass) race to be the first to accomplish the synthesis of extremely complex natural products. In his list of superstars, Service does not mention one, who not only is a striking exception—hardly ever publishing a paper with more than a couple of collaborators—but also one who has trained at least three of the "superstars" listed by Service: Gilbert Stork of Columbia University, whose accomplishments over the course of five decades have won him every major award except the Nobel Prize.

But there is an interesting cultural point that to my knowledge has also never been raised. Women now represent 30 to 40% of the graduate student pool in most chemistry departments of major American research universities, and just about every branch of chemistry but one can now boast of some distinguished female tenured professors. The striking exception is "organic total synthesis." Even worse, I know of no serious total synthesis team that has more than a couple of female members. Any practitioner of this testosterone-drenched sub-branch of chemistry should consider what practices deter talented women from even entering such academic laboratories, let alone leading them. I could provide a list of such practices, but I believe that it would be more persuasive if women cited them.

Service concludes that making natural products and their kin in large quantities so biologists can study their effects should be one of the justifications for pursuing such macho syntheses. Worthy as such an aim might be, it seems to me unrealistic; nor do I believe that it applies to any of the 10 examples listed in Service's table of "total synthesis highlights." That should not be surprising, because practitioners in the field cannot afford the effort—in terms of financial resources, manpower, and time—to repeat such multi-step syntheses on the required scale. In fact, what possible incentives could be offered to new graduate students or postdocs to repeat over and over again synthetic sequences that their predecessors had already published?

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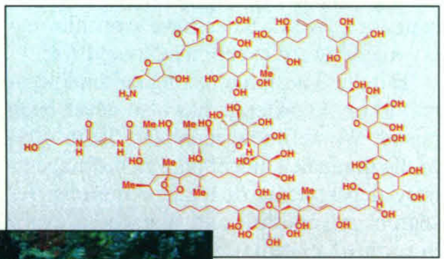
Service's article is a poor catalyst for the debate it attempts to stimulate. For example, I cannot remember reading a paper on total synthesis from which I did not learn something useful. Papers dealing with contemporary "molecular summits" are among the most educational. Also, the most fundamental benefits of apprenticeship in and practice of total synthesis are not mentioned by Service. No other field of organic chemistry provides a richer context for personal scientific growth, both intellectually and technically. And it is largely because of total synthesis that other fields and interfaces of organic chemistry, and the study of basic chemical principles, have recently flourished. I would not trade my training in the competitive environment of total synthesis for any other. I



The structure of polytoxin (right), a compound from *Palythoa vestitis*, a soft coral (left)

owe my professional spirit and any future successes to this background.

Synthesis, with all of its facets, must persevere as a mainstay of the chemical frontier. Chemistry as a whole will always enjoy a steady advance sprinkled with dramatic breakthroughs. For decades, the steady advance has been fueled in good measure by the example and excitement of total synthesis. Dramatic breakthroughs in chemistry will often be made by those schooled in total synthesis. Total "synthetikers" enjoy the advantage of being able to make any molecules we want by simply "taking known reactions and putting them in a new order" (humor intended). We can think deeply about chemistry from broad experience, and so extend our imaginations and productivity to any chemical problem we choose. What other concern can claim this continuing impact?

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Facts about Artificial Intelligence

Ray Kurzweil (Letters, *Science*'s Compass, 16 July, p. 339) responds to my review (*Science*'s Compass, 30 Apr., p. 745) of his *The Age of Spiritual Machines* (Viking, New York, 1999) as follows.

1) My review "mires the reader in obscure and misleading factual objections." Kurzweil attempts a history of computing; in history, facts matter. He challenges only one of my historical objections, concerning the UNIVAC computer. His book, in an entry labeled "1950," says, "Eckert and Mauchley develop UNIVAC, the first commercially marketed computer. It is used to compile the results of the U.S. census" (p. 269). In fact UNIVAC was under more or less continuous development from 1947; it was not the first commercially marketed computer, nor was it operational until 1951.

2) I "drag out old anti–artificial-intelligence (AI) arguments." I do not. Rather, I hold that make-believe about basic conceptual issues, such as we find in Kurzweil's book, are hindering AI.

3) I complain "about anthropomorphizing, but there is no harm...." In AI, anthropomorphizing leads to an emphasis on human qualities that are irrelevant to, and a distraction from, the real aims of AI.

4) My review "ignores [the book's] salient arguments...." I do not detect any, only fantasy, Kurzweil's own "laws" of physics, unjustified assertions, and factual errors.

His letter is no different. For example, Kurzweil insists that Wittgenstein's *Tractatus* is about the brain, supporting this with a fallacious argument. In fact, the *Tractatus* is a technical work of symbolic and philosophical logic and abstract metaphysics and has nothing to say about the brain. Moreover, when Wittgenstein later did discuss the brain, he denied precisely Kurzweil's argument, that to talk about "thinking" or "knowing" is to talk about brain activity. Kurzweil also says that "there is nothing to prevent these efforts [modest connectionist experiments] from scaling up to the entire human brain." How could he, or anyone else, possibly know this, given the vast discrepancy in scale that is involved (there are perhaps as many as 10^{14} neurons in the human brain)?

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Chimp Cultural Diversity

The special News Focus of 25 June (p. 2070) by Gretchen Vogel highlights papers in *Nature* and the *Journal of Human Evolution* reporting that chimpanzees show regional learned behavioral differences (multiculturalism), but it does not mention that phylogeographic studies would lead us to expect such differences (*1*, *2*). What is often erroneously referred to as "the chimpanzee" comprises at least two well-differentiated allopatric populations that have diverged genetically for more than 1.5 million years. The same heterogeneity is now recognized in "the gorilla" and "the orangutan." There is several times more mitochondrial DNA variation in a single chimpanzee social group than in the entire human species (*2*) and more sequence variation at chimpanzee nuclear coding (MHC) and noncoding (HOXB6) regions than in humans (*3*). It is perhaps more surprising that there is any cultural variation in our own relatively homogeneous species than that there is any in our far more variable hominoid relatives. Although a few scholars still deny any role for genetics in the regulation of behaviors, and others posit the existence of nongenetic mental replicators (memes) to account for cultural transmission, we can no longer ignore the genetic diversity of the chimpanzees.

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Vogel quotes Carel van Schaik as speculating that tool-using in early hominids became more common as a result of higher "social tolerance." John Fleagle is quoted as agreeing with this speculation, noting that the reduction in canine tooth size seen in the early hominid fossil record was probably indicative of increased tolerance.

In 1993, we proposed a mechanism that would have led to increased social tolerance in basal hominids (*1*). The social change that led both to greater tolerance and to the origin of habitual bipedal posture evolved as an extension of the behavioral complex of bipedal threat displays and appeasement behaviors observed in great apes. These behaviors evolved in ape societies as means to mitigate aggression and avoid physically injurious confrontation. We speculated that these behaviors became more important in prehominid populations of the late Miocene in Africa, in part because of environmental changes. We also indicated that this behavioral change would have led to a reduction in canine size because conflict resolution would have increasingly relied on bluff and display rather than physical attacks involving biting. We have since demonstrated, using a demographic model (*2*), that a behavioral innovation leading to greater social tolerance that was effective at reducing morbidity and mortality in long-lived ape species would have been strongly favored by natural selection. This mechanism is best seen as an exaptation which, by promoting habitual bipedalism, made possible the anatomical and neurological changes associated with increased manual dexterity and tool use. One need search no farther than this to understand the origins of increased social tolerance in human ancestors.