Letters

Laser Patents

There has been so much misunderstanding arising from reports about a recent California court decision involving lasers that it seems desirable to set the record straight.

The San Francisco court did *not* decide that the Gould patent had precedence over the earlier Schawlow-Townes patent for the invention of the laser. Quite the contrary, in 1966, in the only direct confrontation between Schawlow-Townes and Gould, in the Court of Customs and Patent Appeals, Schawlow and Townes were victorious. This issue is settled beyond further legal challenge.

A. A. PENZIAS
Bell Laboratories,
Murray Hill, New Jersey 07974

As noted in an article on this subject (News and Comment, 23 Apr., p. 392), Gould narrowed his claims after losing the priority dispute with the Schawlow-Townes patent in 1966. However, he did win a narrower patent in 1977 for a device described as an "optically pumped laser amplifier." The San Francisco decision affirmed the validity of that patent. But on 9 July, AT&T, Bell Laboratories, and Western Electric sued in New York to have the patent declared invalid. Gould and his partners have countersued, charging AT&T *et al.* with anticompetitive practices.

—ELIOT MARSHALL

Nuclear Proliferation

The Reagan Administration has approved the export to West Germany and France of special lasers that will be used to develop an advanced isotopic separation technique called laser isotope separation (LIS) (*1*). Once this technique is developed, each country will be able to produce highly enriched uranium and weapons-grade plutonium at potentially less cost than current techniques. Furthermore, France could divert reprocessed plutonium from its commercial nuclear reactor program to its weapons program, something it could not do without this technology.

The export of this technology further erodes the separation between the military and civilian nuclear programs. The Office of Technology Assessment of the U.S. Congress has stated that "the sale of LIS and other advanced enrichment technologies presents a greater proliferation danger than indigenous development of the technologies"(*2*).

France and West Germany both have an extensive commercial nuclear reactor program and a reprocessing capability. By 1990, when each country could possibly have a LIS pilot plant in operation, France would have produced about 37 metric tons of plutonium-239 in its light water reactors' spent fuel and Germany would have produced 21 metric tons (*3*). The plutonium-239 could be extracted from the other plutonium isotopes at this pilot plant. In France, this weapons-grade plutonium could then be diverted to its weapons program.

By the turn of the century, both countries would have access to about 185 metric tons of weapons-grade plutonium. To date the United States has only produced about 100 metric tons for its weapons program (*4*). This 185 metric tons of plutonium could be used to produce about 30,000 Nagasaki-sized fission bombs.

Currently, Congress is on the verge of enacting legislation that would prohibit the transfer of plutonium generated by commercial nuclear power plants to the Department of Energy for the manufacture of nuclear weapons, except in a case of national emergency. The export of essential parts of the LIS technology to France and Germany does not conform with the intent of Congress to discourage proliferation.

DAVID ALBRIGHT
Environmental Policy Institute,
317 Pennsylvania Avenue, SE,
Washington, D.C. 20003

References and Notes

1. M. R. Benjamin, Washington *Post*, 9 July 1982, p. A-1.
2. Office of Technology Assessment, *Nuclear Proliferation and Safeguards* (Praeger, New York, 1977), p. 184.
3. *Fuel Trac* (Nuclear Assurance Corporation, Atlanta, Ga., October 1981). The values were derived from data on fissile and total plutonium inventories.
4. For a discussion of this estimate, see T. B. Cochran and W. M. Arken, "Nuclear weapons data book" (in preparation) or H. Morland, "Technical memorandum on the DOE's alleged shortage of bomb-grade plutonium" (Coalition for a New Foreign and Military Policy, Washington, D.C., January 1982).

Genetic Screening

Constance Holden's article about the Office of Technology Assessment's (OTA) study of genetic screening in the workplace (News and Comment, 23 July, p. 336) illustrates a problem with that study. According to Holden, OTA, ". . . said that at least 59 major corporations may be planning to inaugurate some kind of genetic testing of employees in the foreseeable future." This conclusion may be more a reflection of biases in OTA's questionnaire than of corporate intent.

As one of those in industry who was consulted by OTA on the questionnaire design, I objected strongly to the question, "Does your company anticipate conducting biochemical genetic [cytogenetic] testing during the next five years?" The only choices for answers were "yes," "no," and "possibly." With no specific place to say "don't know," anyone who responded with other than an unequivocal "no" "may be planning to inaugurate . . . testing."

Holden correctly points out the controversial, emotional aspects of biochemical and cytogenetic screening. It certainly is not necessary to add to the controversy by using a questionable research technique and then reporting the results as facts.

HOWARD L. KUSNETZ
Safety and Industrial Hygiene,
Shell Oil Company, One Shell Plaza,
Houston, Texas 77210

Study of Anomalous Phenomena

Constance Holden's account (News and Comment, 25 June, p. 1390) of the first meeting of the Society for Scientific Exploration contains errors that I would like to correct.

By the time of the June meeting, our total membership was indeed 130 (and is now larger), but the requirement of a professorial appointment at a major university applied only to the 100 founding members. One-third of our membership is from physics, chemistry, and mathematics; one-quarter each is from space science and astronomy and from the life sciences; and the remainder is from the social sciences.

Holden's perception that "the assumption of the group is that it is not getting a fair shake" is not shared by the council of the society. On the contrary, we are most gratified that the society has been received with courtesy and open-minded interest by our colleagues in a wide range of disciplines and by the public.

Concerning the potential of scientific study into anomalous phenomena, Holden asserts that certain topics are "completely unsusceptible to research." In fact, both examples cited by Holden are being investigated by competent scientists. Extraterrestrial intelligence is the focus of the SETI (Search for Extraterrestrial Intelligence) program, which is the search for radio signals from other stars. Ian Stephenson, of the University of Virginia, has published several compilations of evidence suggestive of reincarnation.

The study of anomalous phenomena requires caution, thoroughness, and objectivity. So does the reporting of this study.

PETER A. STURROCK
Institute for Plasma Research,
Stanford University,
Stanford, California 94305

Origin of the Theory of Solutions

I first heard this story from W. J. V. Osterhout (1871–1964), by then a member-emeritus of what was still the Rockefeller Institute. Thinking that it was too interesting to be lost, I begged him to write it out, but he never did. I'll tell it first as he told it to me; then I'll authenticate it.

One day in Amsterdam Jacobus Henricus van't Hoff (1852–1911), the "father of physical chemistry," was walking down the street when he encountered his fellow professor, the botanist Hugo de Vries, one of the trio that in 1900 rediscovered Mendel's Laws, out walking with his wife. Having met, the three went on together; but what had they to say to each other? Finally de Vries ventured, "The other day I had a letter from Pfeffer." (Wilhelm Pfeffer, 1845–1920, another botanist, pioneer in using semipermeable membranes to measure osmotic pressures.) To the desultory Dutch equivalent of "Oh yeah? What's he up to?" de Vries replied, "He says he's measuring the effect of temperature on osmotic pressure." "What does he get?" asked van't Hoff. "Well," said de Vries, "he writes that for each degree rise in temperature the osmotic pressure goes up by about 1/270."

By that night van't Hoff was well launched on the theory of ideal solutions, with its fundamental equation, the exact equivalent of the ideal gas law $pv = RT$, becoming in dilute solutions $p/c = RT$, in which p is now the osmotic pressure, c the concentration, R the Rydberg constant, and T the absolute temperature.

I was happy to find mention of this story in Ernst Cohen's monumental biography of van't Hoff (*1*). That, in turn, led me to van't Hoff's own telling of the story, in a lecture on "How the theory of solutions arose," given to the German Chemical Society on 8 January 1894, with Emil Fischer, president of the society, in the chair (*2*).

van't Hoff explains that he had come across a measurement by Eilhard Mitscherlich (1794–1863) that troubled him (*3*). Mitscherlich had tried to measure the strength of binding of water of hydration in salts by noting the lowering of the vapor pressure of water in their crystals. He put crystals of Glauber's salt ($Na_2SO_4 \cdot 10\ H_2O$) into the vacuum space of a barometer and saw that the mercury sank 5.45 millimeters (mm), whereas water would have made it sink 8.72 mm. He concluded that the difference, 3.27 mm, measures the affinity of Na_2SO_4 for its water of hydration. This would amount to a binding force of about 1/32 kilogram per 2.615 square centimeters (about 12 grams per square centimeter). Having quoted Mitscherlich to this effect, van't Hoff goes on to say, "This value, 1/200 atm[osphere], seems to me unprecedentedly low, since I have the impression that even the weakest chemical forces are very large, as seems to me also to be concluded from Helmholtz's Faraday lecture." Wouldn't aqueous solutions offer a much simpler way to make such measurements than dealing with the state of water in crystals?

"Leaving the laboratory with this question on my lips, I encountered my colleague de Vries and his wife, who was just then busy with osmosis experiments and acquainted me with Pfeffer's measurements." van't Hoff then goes into a discussion reconciling Mitscherlich's measurements with Pfeffer's, ending with the words:

"So it occurred to me that with the semipermeable barrier all the reversible transformations that so materially ease the application of themodynamics to gases, become equally available for solutions. . . .

"That was a ray of light; and led at once to the inescapable conclusion that the osmotic pressure of dilute solutions must vary with temperature entirely as does gas pressure, that is, in accord with Gay-Lussac's Law"—that is, $p \propto T$.

"There followed at once, however, a second relationship, which Pfeffer had already drawn close to: the osmotic pressure of dilute solutions is proportional also to concentration, i.e., alongside Gay-Lussac's Law, that of Boyle applies. Without doubt the famous mathematical expression $pv = RT$ holds for both. With that in hand I could demonstrate my [further thermodynamic formulation], and had achieved my goal."

So the essential story is authentic. How about Osterhout's circumstantial details? Here I have recourse to Lawrence Blinks' biographical memoir on Osterhout, his former teacher (*4*). Osterhout taught at the University of California in Berkeley from 1896 to 1909 in the company of "sophisticated colleagues who remembered Pfeffer's laboratory." Benjamin Ide Wheeler, president of the university and eager to build up its intellectual status, took to bringing over European scholars, each for a year. Among them were de Vries, Arrhenius, and Ostwald. "There exists a photograph taken in 1905 showing de Vries beside an *Oenothera* plant in the botanic garden, flanked with the portly Arrhenius [Svante Arrhenius, 1859–1927, who propounded the theories of ionization and energy of activation for chemical reaction, and was the prototype of Sondelius in Sinclair Lewis's *Arrowsmith*] . . . with [Jacques] Loeb smiling beside them, and Osterhout [in bowler hat] in the back row." So Osterhout got to know de Vries, and I think had the story, trimmings and all, directly from him.

Osterhout died at 92, de Vries at 87, Pfeffer at 75; but van't Hoff at 59. Is botany that much more preservative than physical chemistry? To be sure, van't Hoff drove himself hard, and to the very end; but it was not physical chemistry that killed him. "Something seems to have altered in my constitution," he wrote on 1 August 1906 (*1*, p. 548); and on 1 March 1911 he died of tuberculosis. Even an enforced stay in a sanatorium led to a paper, "Sanatoriums-Betrachtung," on heat and work relationships in active life and in the bedridden (*5*).

GEORGE WALD
Marine Biological Laboratory,
Woods Hole, Massachusetts 02543,
and *Biological Laboratories,*
Harvard University,
Cambridge, Massachusetts 02138

References

1. E. Cohen, *Jacobus Henricus van't Hoff, sein Leben und Wirken* (Akad. Verlagsgesellschaft, Leipzig, 1912), pp. 205–206.
2. J. H. van't Hoff, *Ber. Dtsch. Chem. Ges. 27*, (1894), vol. 1, p. 6.
3. E. Mitscherlich, *Lehrbuch der Chemie* (1829; ed. 4, 1847), p. 565.
4. L. R. Blinks, *Biogr. Mem. Natl. Acad. Sci.* **44**, 213 (1974).
5. J. H. van't Hoff, *Biochemische Zeitschrift, Festband für H. J. Hamburger* (Springer, Berlin, 1908), p. 260.