LETTERS

Letters to the Editor

Letters (~300 words) discuss material published in *Science* in the previous 6 months or issues of general interest. They can be submitted by e-mail (science_letters@aaas.org), the Web (www.letter2science.org), or regular mail (1200 New York Ave., NW, Washington, DC 20005, USA). Letters are not acknowledged upon receipt, nor are authors generally consulted before publication. Whether published in full or in part, letters are subject to editing for clarity and space.

Forensic Science and Academic Science

IN HIS EDITORIAL "FORENSIC SCIENCE: Oxymoron?" (5 Dec., p. 1625), Donald Kennedy questions the scientific basis of forensic evidence examination and concludes by questioning, and prodding at, the efforts of forensic scientists to improve the reliability of forensic evidence. To some extent, the field of forensic science must acknowledge these criticisms. Overshadowing this scolding, however, is the more troubling divide between academic and forensic science that is prevalent throughout and, unfortunately, encouraged by Kennedy's Editorial. A glaring illustration of this division was the unsuccessful National Academies' project on Science, Technology and Law "to examine science and its uses in forensic examinations." A review of the members of this program reveals that not a single forensic scientist was included. Would such a project examine "science and its uses in chemistry" without a chemist? This attitude ignores the fact that, although forensic science has developed through the integration of principles from every scientific field, it has evolved into its own scientific discipline. The fact is that there is a great deal of science that cannot be packaged into standardized and verifiable techniques developed to be run by technicians. A recent example was the use of chemical microscopy to tie microscopic paint spheres found on victims' bodies to a specific manufacturer and end-use: the truck painting plant where Gary Ridgway, the Green River serial murderer, worked. Rather than have a drawn-out trial over four counts of murder based on "verified" DNA evidence, this paint evidence was significant enough to induce Ridgway to forgo a trial and admit guilt to 48 counts of murder to avoid a likely death sentence. The mystery in forensic science is not why practitioners do not want a more scientific technology for analyzing crimes, as Kennedy asks, but rather, why traditional sciences will not work with forensic science, rather than above it. Forensic science may be a redundant phrase, but it is not an oxymoron.

CHRISTOPHER S. PALENIK[1,2]* AND SAMUEL J. PALENIK[2]

[1]Department of Geological Sciences, University of Michigan, 425 East University Avenue, Ann Arbor, MI 48109–1063, USA. [2]Microtrace, 1750 Grandstand Place, Elgin, IL 60123, USA.

*To whom correspondence should be addressed. E-mail: cpalenik@microtracescientific.com

Fingerprinting techniques are just some of the many areas of forensic science.

More on Molecular Electronics

TWO RECENT NEWS STORIES BY ROBERT Service ("Next-generation technology hits an early midlife crisis," 24 Oct., News Focus, p. 556; "Nanodevices make fresh strides toward reality," 21 Nov., News of the Week, p. 1310) provide the impression that the field of molecular electronics stalled and then suddenly revived. The specific contributions described in the 21 Nov. news story are just two of a large number of breakthroughs that have been reported by many research groups over the past several years. Progress in the field has been continuous and is accelerating dramatically.

The entire premise of the 24 Oct. article was based on a straw man created through inflated expectations and knocked down with rumor. Rather than being based on published material (as was the 21 Nov. story) or presentations made in public meetings, this article relied on unattributed sources and fragmentary second-hand information. There were several errors and omissions in the article that yielded a distorted view of the field.

For example, the article erroneously stated that, in 1999, the Hewlett-Packard (HP)–University of California, Los Angeles (UCLA) team tried to create "transistors that used the movements of molecules." We were not attempting to create transistors but were demonstrating the concept of two-terminal electronic-tunneling switches that could be toggled by electrical pulses. Stating that a "transistor" was a goal set false expectations for the research and the applications for which the switches were intended.

A schematic of rotaxane molecules between wires shown in the 24 Oct. news report is described by Paul Weiss as "somewhere between naïve and misleading." We note that this figure was commissioned by editors of *Science* for its 21 Dec. 2001 issue, in which molecular electronics was hailed as the breakthrough of the year. Neither Fraser Stoddart nor Jim Heath was consulted to confirm if this figure accurately depicted their research—yet this figure was by inference attributed to them. Taken out of the context originally intended by the editors of *Science*, it is easy to ridicule such a diagram.

Service also made much of a nonpublic presentation to the Defense Advanced Research Projects Agency (DARPA) contractors by Stan Williams, which included a brief description of reversible switching in one particular metal-molecule-metal system that was caused by the growth and dissolution of metallic nanoparticles. This was reported in the article as an unanticipated failure that caused Jim Heath to end his partnership with HP. This is a fabrication on the part of Service's unnamed sources, and neither Heath nor Williams was asked if this was indeed true. The UCLA, Caltech, and HP groups formed a team with a common architectural vision in 1997. As a risk minimization strategy, each partner has pursued somewhat different paths to ensure that the team is successful. We have recently reported two different 64-bit memories (*1*, *2*), which was a major aim of the DARPA program. Having two alternative and complementary approaches from which to select or blend is a major strength of the team, and just one of the reasons we continue to work together.

JAMES R. HEATH,[1] J. FRASER STODDART,[2] R. STANLEY WILLIAMS[3]

[1]Department of Chemistry, California Institute of Technology, 1200 East California Boulevard, Pasadena, CA 91125, USA. [2]Department of

CREDIT: VARIE/ALT/CORBIS

Chemistry and Biochemistry, University of California, Los Angeles, 405 Hilgard Avenue, Los Angeles, CA 90095, USA. [3]Hewlett-Packard Labs, 1501 Page Mill Road, Palo Alto, CA 94304, USA.

References
1. Y. Luo *et al.*, *Chem. Phys. Chem.* **3**, 519 (2002).
2. Y. Chen *et al.*, *Nanotechnology* **14**, 462 (2003).

ROBERT SERVICE'S NEWS STORY ON MOLECULAR electronics, addressing memory and logic devices based on self-assembled organic monolayers ("Next-generation technology hits an early midlife crisis," News Focus, 24 Oct., p. 556), provides an important update on a controversial topic. The proponents of this "technology" have buried us in hype for several years. They promised workstations that will operate for decades powered by only a small battery (*1*), 100 high-end workstations on a grain of sand (*2*), and, more recently, 1000 Pentiums on the same base (*3*). Although it has been admitted that logic will require devices with gain (*4*), no such molecular device has yet been identified. Such unreasonable advertising should have aroused the skepticism even of novices.

Serious questions about this field have been raised for several years after talks by proponents at American Chemical Society and Materials Research Society meetings. Results that are claimed to represent technology must be subjected to examination of manufacturability and device reliability, issues that have been nearly completely ignored. Any device that cannot be made reliably in the lab is unlikely to become the basis of a technology in 5 years, despite what Jim Heath said about molecular-based memory at the UCLA debate in September (*5*).

Much of the present situation is a result of publication, primarily in the press, and of reports that do not include important experimental details. The lack of full papers is a prime characteristic of the field of molecular electronics, and this makes it impossible to fully evaluate the experiments. It is time to demand much more information.

EDWIN A. CHANDROSS
MaterialsChemistry LLC, 14 Hunterdon Boulevard, Murray Hill, NJ 07974, USA.

References
1. J. Heath, S. Williams, *Chem. Eng. News* **77**, 6 (1999).
2. See www.chem.ucla.edu/dept/Faculty/heath/.
3. J. R. Heath, in *Reducing the Time from Basic Research to Innovation in the Chemical Sciences: A Workshop Report to the Chemical Sciences Roundtable* (National Academies Press, Washington, DC, 2003), pp. 56–63.
4. J. F. Stoddart, J. R. Heath, *Chem. Phys. Chem.* **3**, 519 (2002).
5. Much of the UCLA debate is now available on the California Nanosciences Institute Web site (www.cnsi.ucla.edu/mainpage.html). A number of issues in molecular electronics are addressed in detail in my presentation.

ROBERT F. SERVICE'S NEWS STORY "NEXT-generation technology hits an early midlife crisis" (News Focus, 24 Oct., p. 556) misquotes me and does not sufficiently delineate two of the effects observed in molecular electronics.

I am quoted as referring to results reported in the literature on negative differential resistance (NDR) as "artifacts." As I pointed out to Service, the earlier data were measured in different experimental testbeds than ours. I did not imply that the earlier published data were artifacts nor that the published data were in question, but that they gave different apparent results, and that we could use all of the data in toto to understand the underlying phenomena. The discussion of this work also does not correctly attribute our recent results to our collaboration with James Tour at Rice University and David Allara at Penn State, nor does it correctly name the instruments used in recording our data—scanning tunneling microscopes. NDR is a well-established phenomenon and has been observed when atoms or molecules have been reacted on semiconductor surfaces (*1*, *2*) or when single metal atoms have been deposited on metals (*3*).

There appears to be further confusion in the molecular electronics community over separating two different effects—NDR and conductance switching. Our work on switching, also done in collaboration with Tour and Allara, and the earlier work indicate that the switching is robust, controllable, and due to the molecules in the junction (*4*–*6*). Once again, the results of measurements in all testbeds must be considered in understanding, testing, and exploiting this phenomenon.

Finally, I made a point to the article's author that all cartoons (such as the one shown in the article) have the potential to be misleading. It is incumbent upon those of us in nanoscience who draw them to test them against reality. In our own work, by discovering where our cartoons were not correct, we have repeatedly been led into fruitful, unanticipated areas for further exploration.

PAUL S. WEISS
Department of Chemistry and Physics, The Pennsylvania State University, 152 Davey Laboratory, University Park, PA 16802–6300, USA. E-mail: stm@psu.edu

References
1. P. Bedrossian *et al.*, *Phys. Rev. Lett.* **63**, 1257 (1989).
2. I. W. Lyo, E. Kaxiras, Ph. Avouris, *Phys. Rev. Lett.* **63**, 1261 (1989).
3. M. F. Crommie, C. P. Lutz, D. M. Eigler, *Phys. Rev. B* **48**, 2851 (1993).
4. M. A. Reed *et al.*, *Appl. Phys. Lett.* **78**, 3735 (2001).
5. Z. J. Donhauser *et al.*, *Science* **292**, 2303 (2001).
6. Z. J. Donhauer *et al.*, *Jpn. J. Appl. Phys.* **41**, 4871 (2001).

Response

THE USE OF THE WORD "ARTIFACT" WAS NOT a quotation but was based on Weiss's comments, which indicated that his findings challenged earlier results. The article omitted details of Weiss's collaborations for space reasons and misidentified the instrument that made the experimental measurements as a scanning electron microscope. Finally, the cartoon shown in the article was intended as an example of the potential fallibility of all models.

ROBERT SERVICE

International Agricultural Research

A NEW EVALUATION BY THE WORLD BANK OF the Consultative Group for International Research (CGIAR) and its 16 research centers has prompted me to give my views on the importance and contributions of international agricultural research and the confusion in which CGIAR now finds itself. The World Bank reports that plant breeding research at CGIAR centers has declined 6.5% annually for the last decade. Moreover, growing restrictions have been placed on the funding the centers receive.

International agricultural research began in Mexico in 1943 and has grown into an international system of collaborative research, seed exchange, and training organizations that helped build many national agricultural research systems in developing countries.

In only 10 years, wheat and rice harvests in Asia doubled, hunger declined, and incomes improved. The international wheat, rice, and maize programs that had developed high-yielding technologies became the models for a collaborative international research network.

In 1971, the Rockefeller and Ford Foundations, the World Bank, FAO, UNDP, and USAID created CGIAR, a donors' club dedicated to funding an expanded international research system. Over the next 30 years, the number of research centers grew from 4 to 16, covering the major food crops and farming systems in food-deficit, low-income countries. The total budget increased 10-fold—to nearly US$400 million per year.

But somehow in this evolution, the CGIAR lost touch with its original purpose—to feed the hungry. It has become an unwieldy and uncoordinated beast, with too many masters and proliferating goals.

Yet, a well-focused international agricultural research system that backstops and complements national agricultural research organizations and smallholder farmers is a vital component in a global research system. CGIAR must return to its original purpose and to its greatest comparative advantage—developing improved food crop varieties, using a combination of conventional plant breeding techniques and new techniques of biotechnology, with complementary crop

management practices, to address major production issues in both the favored and the more difficult marginal lands.

Another concern stems from the spilling over of the controversy about genetically modified (GM) varieties from industrialized into developing countries, which has paralyzed legislative action on GM crops. We should not underestimate the degree of resistance to GM crops in many countries, although it is heartening to see Argentina, Brazil, China, and India moving ahead with well-considered applications of biotechnology.

I am optimistic that multinational biotechnology companies are willing to devote more resources to solving the problems of poor farmers and consumers. Creative partnerships have been established between private and public research institutions—especially universities, but also CGIAR centers—with financial support provided by private companies, governments, and private foundations. In addition, CGIAR, with seed collections representing much of the genetic diversity in the major food crops, is in a unique position to negotiate with the private sector to generate GM technology that benefits the poor, in return for access to its gene banks.

The World Bank is in a unique position, with its US$50 million of CGIAR funding (until recently completely unrestricted; it now assigns half its contribution to multicenter research initiatives called Challenge Programs), to work with other donors to expand unrestricted funding in the CGIAR, which will help greatly to rationalize priority setting. The Bank can also help refocus the CGIAR mission on raising smallholder agricultural productivity in the near term, rather than trying to be all things to all people.

NORMAN E. BORLAUG

Texas A&M University, College Station, TX 77843, USA.

CORRECTIONS AND CLARIFICATIONS

Reports: "Demographic threats to the sustainability of Brazil nut exploitation" by C. A. Peres *et al*. (19 Dec., p. 2112). The affiliations for Claudia Baider and Robert P. Freckleton were incorrect. For Claudia Baider, the affiliation is Departamento de Ecologia, Universidade de São Paulo, São Paulo - SP, 05508-900, Brazil, and The Mauritius Herbarium, MSIRI, Reduit, Mauritius. For Robert P. Freckleton, the affiliation is Department of Zoology, University of Oxford, Oxford OX1 3PS, UK.

News Focus: "Wanderlust in the western margin" by R. A. Kerr (12 Dec., p. 1889). The observation made by Brian Mahoney of the University of Wisconsin, Eau Claire, and his colleagues was misstated. They did in fact find evidence that the Belt Supergroup supplied sediment to British Columbia's Nanaimo basin. Their conclusion—that the Nanaimo is not far traveled—was correctly stated.

News of the Week: "The ultimate gene gizmo: Humanity on a chip" by E. Pennisi (10 Oct., p. 211). In the second column, second paragraph, *Arabidopsis tumefaciens* should be *Arabidopsis thaliana*.

Reports: "DCP-1, a *Drosophila* cell death protease essential for development" by Z. Song *et al*. (24 Jan. 1997, p. 536) and "Requirement for DCP-1 caspase during *Drosophila* oogenesis" by K. McCall and H. Steller (9 Jan. 1998, p. 230). Annotation of the *Drosophila* genome [M. D. Adams *et al.*, *Science* **287**, 2185 (2000)] has shown that the *dcp-1* gene is located within an intron of another gene, CG3941. P-element alleles of *dcp-1* described in these two papers also disrupt expression of CG3941. Several phenotypes that were previously attributed to loss of *dcp1*, including melanotic tumors, small imaginal discs, and dumpless egg chambers, have now been shown to be due to disruption of CG3941 [B. Laundrie *et al.*, *Genetics* **165**, 1884 (2003)]. However, single mutations in *dcp-1* show defective germline cell death, and loss of *dcp-1* contributes to the ovary phenotype previously reported for the P-element alleles. Thus, the original ovary phenotype reported for the P-element alleles was due to the combined loss of *dcp-1* and CG3941. The role of *dcp-1* in somatic cell death is currently under investigation.