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<tr>
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<td>SH02A</td>
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<tr>
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<td>PhosphoBind Sampler Kit</td>
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Transparent surface models of the helical nucleoprotein filaments formed by the yeast RAD51 protein (top) and the bacterial RecA protein (bottom). Within RAD51 filaments, DNA (red) is extended and untwisted in a manner similar to its extension and untwisting in RecA filaments. The homology of these filaments suggests that the structures for recombination in eukaryotes are similar to those in prokaryotes. See page 1896. [Computer graphic: Edward H. Egelman]

REPORTS

The Mechanical Response of Gold Substrates Passivated by Self-Assembling Monolayer Films
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C$_{60}$H$_2$: Synthesis of the Simplest C$_{60}$ Hydrocarbon Derivative
C. C. Henderson and P. A. Cahill

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Outgassed Water on Mars: Constraints from Melt Inclusions in SNC Meteorites
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Structural Relationship of Bacterial RecA Proteins to Recombination Proteins from Bacteriophage T4 and Yeast
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The Great Overcoat Scare

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Science. Have you discovered any new hazards that may be big news in the future?

Noitall. Overcoats are a major cause of traffic accidents.

Science. How can that be?

Noitall. The average person walks across a street at 6.7076 kilometers per hour. When weighed down with a 2-kilogram overcoat, a man of 75 kilograms walks at 6.5201 kilometers per hour and a woman of 50 kilograms walks 6.426 kilometers per hour. The added exposure to murderous vehicles can be easily calculated to result in thousands of deaths per year.

Science. How could such a small increase in time result in so many added deaths?

Noitall. Easy. The added risk per person is minor, but there are 4 billion people on the globe exposed to gas-guzzling automobiles, big-wheeled ox carts, wildly careening rickshaws, and out-of-control tricycles. The valley of death is what modern streets have become.

Science. How is the calculation made?

Noitall. We assume that the inverse relationship between weight and walking rate is linear and that pedestrians are spherical. The rest of the calculation is simple.

Science. And who are these scientists?

Noitall. The very best that money can buy. They are part of the Basic Research Institute of the Trial Lawyers Charitable Foundation. They have excellent incentives for superior work because they are offered a percentage of the punitive damage awards.

Science. What punitive damages? Who is at fault in this disaster?

Noitall. The overcoat manufacturers, of course. Their willingness to sell heavy overcoats with no notice to customers of the high-risk traffic is evidence of malice of major proportions, which requires punitive damages and massive law suits.

Science. But aren’t there some redeeming values to overcoats, such as less pneumonia?

Noitall. One great principle of our court system is that we do not allow testimony in regard to the benefits of a product. If there is any risk whatsoever, even an unfounded or imaginary risk, culpability is assumed. Otherwise unscrupulous overcoat manufacturers could get sympathy from gullible juries who would not deliberately try to create automobile accidents.

Science. But can you be sure your calculated walking rate is 6.5201 and not 6.5202?

Noitall. That is one of the great secrets in the propagation of scares. Once a scientist says, “There might be a problem,” some ambitious reporter will say scientists are seriously considering the problem. While scientists are struggling to nail down the fourth decimal place of a nondetectable risk, the regulatory agencies say, “Scientists say there isn’t a risk, but the data are still not certain, so we should be conservative and ban the product pending a final study that determines its exact risk.”

Science. But isn’t it desirable to get the right data?

Noitall. Few governments like to spend billions to prove that a highly dubious risk is not worth worrying about. So the uncertainty fuels the scare and helps the lawsuits.

Science. But if they do a study and show the risk is negligible, you look silly. Don’t you care?

Noitall. I never look silly. If they solve the problem to four decimals, I just ask for five. If they say that’s trivial, I say I’m worried about lives, whereas my opponents care more about money than lives.

Science. Is there a societal solution to the overcoat problem?

Noitall. There is always “a societal solution,” and it is always very expensive. Already, high voltage lines, a nonproven risk, are being put underground or detoured miles around cities at great expense to the consumer to avoid bad publicity or lawsuits. Consumers, of course, never realize this because the expense is buried in the overall bill.

Science. In the case of overcoats, what would you suggest?

Noitall. Society should provide every person with an “overcoat caddie,” whose function is to carry the overcoat across the street for the pedestrian. It will be expensive but less than the lawsuits and regulatory provisions.

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AIDS Virus History

In the News & Comment article of 8 January about the discovery by the U.S. Department of Health and Human Services (HHSS) of Robert Gallo (p. 168), Jon Cohen writes of a time, "when Luc Montagnier of the Pasteur Institute in Paris was first isolating the AIDS virus, which he called LAV." This kind of statement prompts me to tell another side of the story about the circumstances that led to the discovery of the human immunodeficiency virus (HIV) 10 years ago in France.

I have been working on AIDS since the first case of the disease was diagnosed in France. Willy Rozenbaum entrusted David Klatzmann and me with the immunological study of that case in December 1981. In March 1982, I joined in the creation of a multidisciplinary study group on AIDS whose other members were E. Bouvet, J. B. Brunet, J. Chaperon, S. Kernbaum, Klatzmann, L. D. Lachiver, J. Leibovitch, C. Mayaud, O. Picard, J. Revuz, Rozenbaum, J. Villalonga, and C. Weisselberg. None of us was from the Institut Pasteur. This group met regularly in the nephrology department of Pitié-Salpêtrière Hospital, where I was working. We set up the first French epidemiological, immunological, and virological studies of AIDS. The exchange of ideas that took place in this study group was essential for the discovery of the disease’s causal virus.

We adopted the hypothesis that the disease was caused by a retrovirus and defined what we considered to be the most propitious experimental conditions for the isolation of this hypothetical virus. Our idea was that the virus would be isolated more easily from patients with AIDS-associated syndrome (essentially a generalized lymphadenopathy) than from patients with AIDS itself. Because we thought it likely that lymph node hyperplasia was evidence of a localized immune response, which suggested the presence of the virus in the lymph nodes, we decided to search for the virus there, rather than in the peripheral blood of the patients. Rozenbaum and virologist Françoise Brun-Vezinet contacted Luc Montagnier’s group at the Institut Pasteur and brought them a lymph node specimen. That it was not a mere blood sample attests to the study group’s contribution to the isolation of the virus. Montagnier, Jean-Claude Chermann, and Françoise Barré-Sinoussi went on to successfully isolate LAV, now known as HIV, early in 1983.

A second group then formed, the “hard core” members of which were Barré-Sinoussi, Chermann, and Montagnier from the Pasteur Institute; Brun-Vezinet, Klatzmann, C. Rouzioux, Rozenbaum, and me from different medical schools in Paris; and J. B. Brunet from the French Ministry of Health. The aim of our new group was to demonstrate the role played by the virus in AIDS and related diseases and to develop a test capable of detecting viral markers. Results relating to any part of our research were discussed by all of us in weekly meetings, when we took stock of our progress and made plans to determine future experiments. The active participation of each member in the work of the group as a whole was expressed in the collective signing of our publications of that period (1), irrespective of which part of our individual research projects they treated.

It therefore seems inappropriate to speak of “Montagnier . . . isolating the AIDS virus,” as the discovery and characterization of HIV were a collective endeavor. Had this notion been kept in mind during the last 10 years, many problems and controversies harmful to the scientific community and to public health might have been averted.

Jean Claude Gluckman

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References
Japanese Support of U.S. Research

I was dismayed to find attitudes of barely concealed racism in two recent News & Comment articles about Japanese support of research efforts in the United States (27 Nov., pp. 1428 and 1431). Several scientists are quoted to the effect that this kind of support constitutes “stealing” of American know-how by the Japanese, and they voice concern that publicly funded research may become commingled with proprietary research and hence benefit the Japanese.

While I, too, am concerned about loss of competitiveness to other countries and the use of taxpayers’ money for private gain, it is curious that this kind of language is used only when the money comes from foreigners of the nonwhite variety. For example, not far from the Princeton institution funded by the Nippon Electronic Corporation that is criticized in the first article is the Roche Institute for Molecular Biology (not to mention several other research facilities supported by European pharmaceutical companies). In a subsequent issue of Science, an article reports a forthcoming agreement between the Scripps Research Institute and Sandoz, yet another European pharmaceutical giant (News & Comment, 4 Dec., p. 1570). This article is entirely positive, with no hint of any concern that foreigners may be stealing the fruits of American research efforts at the expense of taxpayers, even though Sandoz will have licensing rights and Scripps researchers will be encouraged to get additional funding from the National Institutes of Health.

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Thoroughly Modern Reptiles

It doesn’t necessarily follow that, had the dinosaurs not become meteorically extinct, humans would not have evolved (Daniel H. Janzen, Letters, 13 Nov., p. 1071). They would not have evolved from uneventuated primates, but self-knowing Homo sapiens could have arisen (or descended) from an advanced line of reptiles. This possibility was broached by Dale A. Russell, a paleobiologist at the Canadian Museum of Nature. At the end of a 1981 British Broadcasting Corporation TV program “Death of the dinosaurs,” he presented a model of what a highly encephalized 20th century dinosaur might look like. This creature, as conjectured, was bipedal with no tail, had a...
large head with binocular vision, lacked external ears, and had a deep chest with ribs all the way down the abdomen, opposable fingers, and no external genitalia. Russell did not suggest other characteristics of such beings or the possible nature of their social structure, but from what we know of ourselves, reptiles, and the nature of intelligence, many logical scenarios can be imagined.

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All in the Family

There are some interesting family connections brought to mind by different contributions in the 22 January issue. A report on page 493 by István Lengyel et al. discusses research connected with transient, spatial patterns predicted by the British mathematician Alan Turing, who was mainly responsible for breaking the German “Enigma” military code, believed by many to be decisive in the Allied victory in World War II.

Turing’s maternal great-grandfather was Thomas George Stoney, whose second cousin, George Johnstone Stoney, was a distinguished physicist who, among other things, invented the word “electron” (I). This same man was uncle to George Francis FitzGerald, the proponent of the contraction hypothesis as an explanation of the Michelson-Morley result, described in The Maxwellians, which is reviewed (Book Reviews, p. 536) in that same issue.

Peter Mitchell
Rural Route 3, Box 546,
Elmer, NJ 08318

References

Kuwait Oil Fires: Correction

In our article “Airborne studies of the smoke from the Kuwait oil fires” (15 May, p. 987) (I), we stated that the depletions of sulfur dioxide (SO2) and nitrogen oxides (NOx) in the smoke plume from the Kuwait oil fires were 50 and 60% per hour, respectively. These values were derived from measurements made aboard a Convair C-131 aircraft, and measurements of CO2 were used as a conserved tracer. Subsequent comparisons of these measurements of CO2 (which were obtained from a continuous

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*InPlot costs $395 and is a DOS program. InStat costs $95 and is available in DOS and MAC versions.
Our conclusions with regard to the climatic effects of the Kuwait oil fires are unchanged.

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References

Corrections and Clarifications
In the article "The global carbon dioxide budget" by Eric T. Sundquist (12 Feb., p. 934), the second sentence of the third paragraph on page 933 should have read, "The ice core record from the Byrd station in Antarctica shows that CO₂ concentrations rose from 200 ppm between 17,000 and 18,000 years ago to 280 ppm between 10,000 and 11,000 years ago (2)." The sixth sentence of the second paragraph of column three on the same page should have read, "There is some evidence for a significant deglacial change in terrestrial chemical weathering (10)." The seventh sentence of note 66 on page 941 should have read, "I calculated the latitudinal distribution of the authors' tropical deforestation flux by assuming proportionality to the biospheric destruction terms in (50)."

In the News & Comment article "Clinton's technology policy emerges" by Christopher Anderson (26 Feb., p. 1244), a figure of $2.1 billion was given incorrectly as the amount the Clinton Administration estimates it will save over the next 4 years by restructuring the Space Station. In fact, the Administration has not yet released a projection of those savings, but outside groups estimate that they will be between $2 billion and $5 billion.

In the Random Samples item "Charting biodiversity to guide conservation" (5 Feb., p. 738), Janis Alcom should have been described as the "director of CNA [Conservation Needs Assessment] at the Biodiversity Support Program, a consortium of the World Wildlife Fund, the World Resources Institute, and the Nature Conservancy."
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A crevice-free, stainless steel interior with mirrored finish features 100 percent 1/2” radius covered corners. No harsh finish to harbor contaminants. Construction of the chamber promotes aseptic culturing conditions.

**Precision Control**

A door-mounted control panel features a microcomputer with vacuum fluorescent alpha/numeric display. Programming is easy with the touch key pad. Self-prompting messages allow you to control and monitor the critical performance parameters of the incubator. Proportional and integral (PI) algorithms combine with fuzzy logic characteristics to provide a stable culturing environment. A patented interactive dual temperature probe system provides precise balance of chamber air for ultimate temperature control and uniformity.

**Interface Protection**

Remote monitoring of temperature and CO₂ can be achieved through remote alarm contacts, millivolt output signal and optional RS232 interface. Standard electrical outlet is provided for connection to accessory items, such as temperature recorder, gas guard, etc.

**Product Compliance**

U.L. Listed and C.S.A. Certified, every 3250 incubator is manufactured to conform with the intense guidelines of these testing facilities. Copies of U.L. and C.S.A. documentation are available from Forma for compliance verification.

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<table>
<thead>
<tr>
<th>The Care To Meet, To Know, To Understand</th>
</tr>
</thead>
</table>

Some of the International Meetings Scheduled for 1993 are:

- "Experimental and Clinical Precancerous Lesions: Approaches to Cancer Prevention and Early Diagnosis"
  P. Marks (USA) and R. Weil (CH)
  Montreux (CH), March 29-31

- "Molecular Diagnosis and Monitoring of Leukaemia and Lymphoma"
  F. Grignani (I)
  Perugia (I), April 15-17

- "Molecular Basis of Inflammation"
  J. Navarro (USA)
  Heidelberg (D), April 21-23

- "Metabolism in the Female Life Cycle"
  M.P. Diamond and F. Naftolin (USA)
  Taormina (I), May 17-18

- "Recent Advances on Monoclonal Gammapathies and Related Malignancies"
  B. Barlogie (USA) and F. Dammacco (I)
  Evian (F), June 3-5

- "Inhibin and Inhibin-Related Proteins"
  H.G. Burger (AUS)
  Siena (I), June 17-18

- "Cell and Molecular Biology of the Testis"
  M.L. Dufau (USA) and A. Isidori (I)
  Majorca (E), September 13-14

- "GTPase-Controlled Molecular Machines"
  D. Corda, S. Garattini and A. Luini (I)
  S. Maria Imbaro (I), Sept. 22-25

- "Developmental Endocrinology"
  M.L. Aubert and P.C. Sizonenko (CH)
  Geneva (CH), Sept. 30 - Oct. 2

- "The Challenge of Biotechnology: from Laboratory Diagnosis to Clinical Therapy"
  S.A. Aaronson (USA) and R. Verna (I)
  Rome (I), October 11-12

- "Molecular Basis of Endocrine Diseases"
  C. Pavia (E)
  Barcelona (E), November 18-19

- "Biodegradable Polymers"
  San Miniato (I), May 2-7

- "Biological Structure and Gene Expression"
  Volterra (I), May 2-7

- "Organic Superconductors"
  Il Ciocco (I), May 9-14

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  San Miniato (I), May 2-7

- "Biological Structure and Gene Expression"
  Volterra (I), May 2-7

- "Organic Superconductors"
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<table>
<thead>
<tr>
<th>Current FBS Requirements</th>
<th>FBS Cost (Price @ $320/L*)</th>
<th>% Switch to FETALCLONE®</th>
<th>Total Cost of Using FETALCLONE® &amp; FBS</th>
<th>COST SAVINGS</th>
</tr>
</thead>
<tbody>
<tr>
<td>50 Liters</td>
<td>$16,000</td>
<td>10%</td>
<td>$15,200</td>
<td>$ 800</td>
</tr>
<tr>
<td></td>
<td></td>
<td>30%</td>
<td>$13,600</td>
<td>$ 2,400</td>
</tr>
<tr>
<td></td>
<td></td>
<td>50%</td>
<td>$12,000</td>
<td>$ 4,000</td>
</tr>
<tr>
<td></td>
<td></td>
<td>70%</td>
<td>$10,400</td>
<td>$ 5,600</td>
</tr>
<tr>
<td></td>
<td></td>
<td>90%</td>
<td>$ 8,800</td>
<td>$ 7,200</td>
</tr>
</tbody>
</table>

*Hypothetical pricing based on current FBS market prices only. Not to be interpreted as actual price.
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others were evidently hearing much that was new to them." In fact, what this discussion makes clear is that none of them understood much of anything about bomb physics. In not grasping this Powers, who is not a physicist, shows the limits of his own understanding.

First, 6 August. After a general and rather tumultuous exchange among the Germans following their first news about the bomb, Heisenberg and Hahn find themselves alone. Heisenberg makes a conjecture that the Allies had been able to separate about 30 kilograms of uranium-235 a year from natural uranium. Hahn then asks, "Do you think they would need as much as that?" Heisenberg replies, "I think so certainly, but quite honestly I have never worked it out as I never believed one could get pure '235.'" As a statement of what Heisenberg knew what could be clearer? But then Hahn goes on to ask, "How does a bomb explode?" Heisenberg's answer reveals that at that moment he understood next to nothing about bomb physics. He did know from news reports that the Hiroshima explosion was equivalent to that of about 20 kilotons of TNT. He knew that each kilogram of uranium, if it was entirely fissioned, yielded about that energy. He also knew that there were some $2.58 \times 10^{24}$ nuclei in each kilogram of uranium. He also knew that each fission would produce about two neutrons. Since $2^{80}$ is about $10^{24}$ he reasoned that it would take about 80 fission-producing collisions to do the job. He guessed on the basis of his experience with reactors that the average distance between collisions would be 6 centimeters. If the neutron diffusion is considered a random walk this sphere would have a radius of six times the square root of 80 centimeters—about 54 centimeters. Heisenberg then computes the mass of the sphere as 1 ton. In fact, if you take Heisenberg's 54 centimeters and put in the correct density of uranium, which is 19.04 grams per cubic centimeter, the calculation gives about 13 tons—about 3 tons more than the total payload of a World War II bomber! At Heisenberg's conjectured rate of accumulation of about 30 kilograms a year it would take four centuries to accumulate that much uranium-235. One thing that surprised me in reading the transcripts was Heisenberg's inexperience with arithmetic.

But this calculation is in any event irrelevant to how a bomb works. Heisenberg's picture was of the neutrons diffusing from the center and colliding with the nuclei before leaving the sphere. In fact what is relevant is a comparison of the number of fission neutrons being produced per second in the sphere (whatever its volume) with the number escaping from its surface. When these two numbers are equal the reaction will become self-sustaining. The mass at which this happens is the so-called critical mass. Furthermore, in his calculation Heisenberg did not take into account the fact that the uranium sphere—because of the fission energy—rapidly heats up and expands. Indeed after about $5 \times 10^{-8}$ second the density is reduced enough that the fission process stops. Most of the material will not have fissioned by then. In the Hiroshima bomb, only about 2 percent of the uranium-235 fissioned. So much for Heisenberg's understanding on 6 August. Now to the lecture of 14 August.

By 14 August Heisenberg has come to understand what a critical mass is. He presents his colleagues with a crude diffusion-theory calculation of its. To understand the difference in sophistication between this calculation and what the Allied scientists knew in April 1943 the reader is
advised to consult Robert Serber’s *Los Alamos Primer*—the notes of the lectures Serber gave at Los Alamos, now available from the University of California Press. Serber’s exposition comes from a different world. In his lecture at Farm Hall Heisenberg, making certain special assumptions and guessing at the mean free path, arrived at a critical radius for the uranium sphere somewhere between 6.2 and 13.7 centimeters, giving a critical mass of somewhere between 19 (the transcripts say 16) and 205 kilograms. In his book Serber quotes the actual critical mass of uranium-235 as 56 kilograms. Heisenberg’s discussion then turns to the possibility of putting a material around the sphere that would reflect neutrons and thus reduce the critical mass, something he had brought up with Hahn on 6 August.

This is something that was known as a “tamper” at Los Alamos. Powers makes much of the fact that Heisenberg was aware of this possibility. However, it is evident from the transcripts that Heisenberg did not understand the difficulty with using a tamper. This is clearly discussed in Serber’s book. The neutrons tend to get delayed in the tamper because they scatter in it. Hence they are not reflected back rapidly enough to do very much good in the explosive process. Tamper reduce the critical mass by a significant factor—nearly a quarter in the case of uranium-235—but do not increase the efficiency of the explosion by anything like the same amount. That neither Heisenberg nor any of the other Germans understood this is reflected in their discussion about using carbon as a tamper. Indeed, contrary to a footnote of Powers’s, this is one of the suggestions Heisenberg makes to Hahn on 6 August. No choice of a tamper material could be worse. Carbon scatters neutrons strongly, which is why it is used as a moderator in reactors to slow down the neutrons—the last thing one wants in a bomb. For a uranium-235 bomb natural uranium is the tamper of choice. Because it is very dense it has the additional useful property of retarding the expansion of the fissioning uranium. With a uranium tamper the critical mass of uranium-235 is reduced to 15 kilograms. (In doing his calculation Heisenberg implicitly used a uranium tamper, which is why at the lower limit he got a number that is close to this.) The Hiroshima bomb employed about three tampered critical masses of uranium-235. In proposing carbon as a possible tamper Heisenberg and his colleagues were thinking like reactor physicists, not bomb designers.

But the tangle of misconceptions on the part of the German physicists gets worse. Heisenberg goes on to say, “They [the Allies] claim that the whole mass only weighed 4 kilograms.” Now he has a real quandary—how to reduce his critical mass by a factor of 4. Wolfgang Pauli had a wonderful phrase, “desperation physics,” and it perfectly characterizes the ensuing discussion. Von Weizsäcker, one of Powers’s clairvoyants, volunteers the silly idea of putting carbon—graphite—into the explosive sphere itself. The rest of the participants sound no more astute. The dénouement comes when one realizes that the 4 kilograms that Heisenberg was citing do not represent uranium at all but plutonium. Heisenberg mixed up the Hiroshima and the Nagasaki bombs. To none of the Germans, including von Weizsäcker, who independently invented the idea of using plutonium as an explosive, does it occur that the 4 kilograms might not be uranium at all. So much for their understanding of bomb physics.
tions. Specialists will question minor aspects of Taube's arguments and identifications, but they are basically sound. Readers wanting to learn more about the ways in which deities were and are integrated into ancient and modern Maya myth and ritual will need to read further, but The Major Gods of Ancient Yucatan will provide anyone interested in ancient Maya religion with an excellent introduction.  

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Language and Prehistory


Much of modern linguistics is devoted to identifying what all human languages have in common: which characteristics of languages are necessary and essential and which are accidental or contingent. The field of linguistic typology has tended to approach this problem empirically, by examining the characteristics of large numbers of diverse languages. For example, it has been shown that languages that put demonstratives like this or that after the noun rather than before it have a strong (though not exceptionless) tendency to put adjectives after the noun also.

Other approaches (such as Noam Chomsky's) reject deriving linguistic universals from such linguistic beauty pageants and focus instead on intensive study of particular problems in particular languages, arguing that, if properly investigated, even a single language, like a holographic image, can enable us to fill in much of the picture of language in general.

The approach of the book under review is more akin to the first approach, but with an important difference: Nichols examines the global distribution of linguistic features not in order to identify linguistic universals but rather in order to focus on the differences among human languages and on what the distribution of these differences might indicate about the prehistory of human language. Nichols argues that more traditional methods—such as the comparative method that was used to reconstruct the history of the Indo-European language family—cannot take us earlier than about 8,000 to 10,000 years ago and that to go to greater time depths new methods are necessary. The power of the comparative method, and its very definition, is still under debate in the linguistic community, and it is possible that Nichols's pessimism about it is unfounded. But new tools for investigating linguistic prehistory would clearly be welcome in either case.

Nichols's investigation of the distribution of linguistic traits is not based on the assumption that some of them are more primitive than others, but rather assumes that some features remain relatively stable in a given geographical area or within a single language family and may thus be used as markers to track early speech communities, much as blood types are used as guides to population movements.

For example, Nichols defines a feature she calls "head/dependent marking," which characterizes a language's morphological tendencies. In a sentence like "Noam reads French," the verb reads is marked by the
mathematics and reasoning have in fact changed the way many of us view the world of mathematics.

A hundred years before Russell’s paradox ushered in the 20th century, the great French scientist J. L. Lagrange wrote,

It seems to me that the mine is already almost too deep, and unless we discover new seams we shall sooner or later have to abandon it. Today Physics and Chemistry offer more brilliant and more easily exploited riches; and it seems that the taste of the century has turned entirely in that direction. It is not impossible that the mathematical positions in the Academies will one day become what the University chairs in Arabic are now.

This lament echoes a fin de siècle pessimism that has struck mathematics toward the end of each of the last three centuries. Very likely we will indulge in a similar malaise of millennialism over the next few years. If we are as lucky as were our ancestors, this will be followed, in the manner of an economy coming out of a long recession, by a tremendous burst of productivity in which new and unexpected directions will be taken. Paradigms will shift, perhaps as dramatically as they did at the beginning of both this century (in the shadow of the modern atom) and last century (with the advent of rigor à la Cauchy and the later disquieting discovery of non-Euclidean geometries).

For Lagrange mathematics was prosaically Platonic, intellectual coal to be mined. The lament was not for the passing of mathematics, it was for the passing of mathematicians. Mathematical ore is still there even if no one is digging. It is the cultural loss, or perhaps the loss of a pleasant livelihood, not the lost science, that is found troubling.

Lagrange, comfortable in his Platonic belief in a tangible, physical mathematics and its concomitant discovery and exploitation, might have been much shaken by the crisis induced by Russell and his contemporaries. Others certainly were. Many mathematicians today take a much more formalist, axiomatic, and bloodless approach to their subject. Some take an extreme constructivist position: things that cannot be constructed finitely do not exist. Others take an intuitionist point of view: proofs must eschew the principle of the excluded middle and must be fully (psychologically) analyzable.

The questioning of foundations has led to some of the truly profound insights of the century about the nature of knowledge, uncertainty, randomness and unknowability, the gulf between truth and proof. Out of the brains of logicians like Turing sprang fully formed theoretical computers—with all the power of the physical ones that were still to be built. Thus computers were in fact discovered before they were invented—or perhaps it is the other way around.

This begins to touch the themes of Barrow’s richly woven book, which is really a collection of six long, lucid, loosely linked essays in the philosophy, history, and culture of mathematics.

We would not indulge in “millennialism” if we had six fingers on each hand, nor would we tend to encapsulate by centuries. But we would almost certainly still count, and quite probably in very similar fashion, even if in a different base. In a long chapter on the cultural development of counting and numeration, Barrow asserts, “The Indian system of counting has been the most successful intellectual innovation ever made on our planet”—a grand claim that is persuasively defended. The Indian innovation is primarily the number zero. A Wonder feature, that nothing can be claimed more successful and inevitable than the discovery (invention) of nothing.
Most of this book revolves around making the case for and against the various competing philosophies of mathematics. By and large the case wins each round. It is very hard to embrace any of these philosophies wholeheartedly. We tend to set our own personal demarcations. But for most of us some parts of mathematics exist: natural numbers, triangles, perhaps pi. Some of the more exotic and abstract bits just don’t have the same claim to a life of their own. The average mathematician is a mosaic: perhaps two parts Platonist to one part formalist, with a taste for constructive proofs when possible. (We challenge the reader to find a working mathematician of any philosophical stripe who would refuse authorship of a classically valid but nonconstructive proof of the celebrated Riemann hypothesis no matter what axiomatics that proof demanded.)

What keeps this book so readable is the texture: the historical anecdotes; the careful biographical sketches of Goedel, Cantor, Brouwer, Hilbert, and others; the excursions into the bizarre world of undecidability; the speculations on the future; the thought-provoking ripostes. (In answer to Roger Penrose, Barrow suggests that the capacity to encode undecidable statements is a precondition for consciousness of a structure.) Throughout Barrow demonstrates a remarkable scope, a fine sense of how mathematics works, and considerable insight into how it may be evolving. Occasional minor technical infelicities do nothing to mar the success of his project.

Barrow writes, “Today it is not unexpected to find the ‘computer’ or the ‘program’ as central paradigms in our attempts to interpret the Universe” and observes that “the concept of experimental mathematics has begun to take on a new and more adventurous complexion.” This pervasive use of the computer to attempt to interpret mathematics rather than just the universe is surprisingly new. Mathematicians invented computers and then for several decades proceeded largely to ignore them. It is only recently, with the advent of really successful symbolic manipulation of computer algebra packages, that computers have come of mathematical age—or, more accurately, have entered puberty.

This book is not so much about mathematics as specialist subject as it is about mathematics as universal language. Talking meaningfully about mathematics without talking in mathematics is a difficult and underpracticed art. Barrow’s book is a very welcome addition to this literature.

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