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Schematic of the unimolecular dissociation of ketene (CH2CO), in which stepwise increases of the dissociation rate are seen as the total energy increases through transition-state vibrational thresholds (white lines). Highly vibrationally excited ketene moves to the transition state of the reaction by localizing energy in the C=C bond (blue). Once at the transition state at the peak of the potential energy surface (yellow), CH2 and CO repel each other, converting potential energy into translational and rotational energy of the fragments. See page 1541 and the Perspective on page 1523. [Illustration: Tim Robinson, University of California, Berkeley]

PERSPECTIVES

Skiing the Reaction Rate Slopes 1523
R. A. Marcus

Epidemic Cholera in the Americas 1524
R. I. Glass, M. Libel, A. D. Brandling-Bennett

ARTICLES

Experimental Constraints on the Theory of High-T, Superconductivity 1526
P. W. Anderson

Shock Waves in Stellar Atmospheres and Breaking Waves on an Ocean Beach 1531
G. Wallerstein and S. Elgar

REPORTS

Slow Crack Growth in Single-Crystal Silicon 1537
J. A. Connolly and S. B. Brown

How to Make Water Run Uphill 1539
M. K. Chaudhury and G. M. Whitesides

Observation of Transition-State Vibrational Thresholds in the Rate of Dissociation of Ketene 1541
E. R. Lovejoy, S. K. Kim, C. B. Moore

Mechanism of the Rhodium Porphyrin-Catalyzed Cyclopropanation of Alkenes 1544
J. L. Maxwell, K. C. Brown, D. W. Bartley, T. Kodakek

Glassy Microspherules (Microtektites) 1547
from an Upper Devonian Limestone
K. Wang

In Vivo Gene Transfer with Retroviral Vector-Producer Cells for Treatment of Experimental Brain Tumors 1550
K. W. Culver, Z. Ram, S. Wallbridge, H. Ishii, E. H. Oldfield, R. M. Blase

Oxytocin Gene Expression in Rat Uterus 1553
D. L. Lefebvre, A. Giaid, H. Bennett, R. Larivière, H. H. Zingg

Early Stages of Motor Neuron Differentiation Revealed by Expression of Homeobox Gene Islet-1 1555
J. Ericson, S. Thor, T. Edlund, T. M. Jessell, T. Yamada

Galactose Oxidation in the Design of Immunogenic Vaccines 1560

Calcium Entry Through Kainate Receptors and Resulting Potassium-Channel Blockade in Bergmann Glial Cells 1563
T. Müller, T. Möller, T. Berger, J. Schnitzer, H. Kettenmann

Calcium-Permeable AMPA-Kainate Receptors in Fusiform Cerebellar Glial Cells 1566
N. Burnashev, A. Khodorova, P. Jonas, P. J. Helm, W. Wisden, H. Monyer, P. H. Seeberg, B. Sakmann

Hebbian Depression of Isolated Neuromuscular Synapses in Vitro 1570
Y. Dan and M.-m. Poo

1547
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EDITORIAL

Conversational Pork Versus Peer Review

The National Science Foundation (NSF) and the National Institutes of Health (NIH) recently got caught in cross fire between President Bush and Senator Robert Byrd (D-WV), chairman of the powerful Senate appropriations committee. The true extent of casualties might not be known for some time, but there is plenty of reason for the scientific community to be worried.

It started with Bush criticizing the practice of congressional pork barrel and requesting a rescission of funds for a number of such projects. Byrd, who is known for appropriating funds for numerous projects in West Virginia, decided to retaliate by comparing NSF and NIH grants with congressional pork. In a letter to AAAS, Senator Barbara Mikulski (D-MD), who chairs the subcommittee that handles NSF's appropriation, states: "At the request of Senator Byrd... grants given by the NSF and the NIH were reviewed for potential rescissions." She goes on to say, "Thirty-one NSF grants and three NIH grants were selected and approved by Senator Byrd...." The Senate subsequently proposed killing these grants outright, but the House balked, and in the final bill $2 million was taken back from NSF without legally requiring cancellation of the 31 grants. However, the bill was accompanied by a "report" providing "guidance" to NSF. While not having the force of law, such guidance is seldom ignored, because agencies fear the powerful appropriation committees.

Referring to NSF, the report states, "The conferees agree to this rescission of $2 million because it represents the approximate total amount originally awarded by the Foundation for 31 research projects contained in the original Senate amendment. The conferees do not believe that these 31 awards represent a prudent use of taxpayer funds...[and] strongly urge the NSF to review the option of eliminating funds which remain unobligated for the 31 research projects...."

It is regrettable that NSF and NIH grantees have become pawns in a battle between Congress and the Administration. These grants simply are not in the same category as congressional pork barrel, and it is shameful for Congress to try to make it seem that they are. The grants resulted from a competitive process in which scientific excellence on a national level was emphasized. In contrast, pork barrel grants deliberately bypass competitive evaluation. Bush was correct to attack congressional pork. All Americans resent pork barrel politics in which their tax dollars go to a few districts represented by powerful legislators.

The congressional report does not stop with an attack on specific grants. It actually seeks to change the criteria by which NSF selects all grants. The report states, "The conferees believe the Foundation should emphasize research that is focused on the fundamental laws and systems of science, that supports the nation's technological base, that supports the nation's economic competitiveness, and that improves the nation's mathematics and science education endeavors. As a result, the conferees direct the NSF to review its grant selection process and report to the Congress on how it intends to ensure that projects which do not meet these criteria go unsupported with taxpayer dollars in the future."

This is a poor way to make national science policy. Excellence, which most scientists would say should be the principal criterion, is not even mentioned. Economic competitiveness, while important, is not a good basis for selecting individual projects in basic research because, as any scientist knows, basic research is too unpredictable. For example, who—scientist or politician—might have predicted at the time that research leading to discovery of restriction enzymes was going to become a major contributor to economic competitiveness?

Most of the 31 grants listed in the report are in the social sciences. Moreover, the criteria proposed for selecting grants could be construed as intentionally biased against the social sciences. An attack on the social sciences is an attack on all science and should be rejected by the scientific community.

NSF's response to this situation to date is surprising because it seems to imply that they agree with the report. NSF's official spokesman stated, "They're sending us an important message, and I think we'll take a long hard look at it."

Perhaps NSF believes all of this will blow over and Congress, having made its point with the Administration, will never again try to play NSF program officer. On the other hand, a precedent has been set, and if no one objects, it will be all that much easier the next time around.

Richard S. Nicholson
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AIDS Origin

Steve Sternberg's article "HIV [human immunodeficiency virus] comes in five family groups" (Research News, 15 May, p. 966) contains a misattribution that I wish to correct. I did not "claim" during the Stanford University Center for AIDS Research seminar cited by Sternberg, nor do I believe now, that Gabon in particular is the "source of AIDS in the world." The data are far too complex and our findings too preliminary to justify so simple an inference. I explained to Sternberg after he published an earlier version of the article (1) why I regarded his decision to single out Gabon as scientifically irresponsible, reminding him of specific evidence presented in my talk that argued against this interpretation. That information could have been included in his Science article.

The "acrimonious" debate to which Sternberg refers in Science is one that has been unnecessarily rekindled. This is unfortunate at a time when free exchange of information is essential to our efforts to detect, treat, and ultimately prevent a devastating disease. Inquiry into the origin and evolutionary path of HIV is important insofar as it serves these purposes. I am saddened by the fact that my talk on global surveillance of viral forms did contribute to Sternberg's focus on a particular country and its people. Where AIDS arose, as distinct from when and how, is irrelevant to medical science and should not bring any criticism to any country.

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REFERENCES

Museum Collections: Why Are They There?

I must take exception to the attitudes implicit in the letter of Gary R. Graves and Michael J. Braun (13 Mar., p. 1335). As a museum-oriented systematist, I appreciate why we have systematic collections and what they do. However, I cannot stand by and watch systematics as a science, and natural history museums in particular, literally die on the vine.

All too often museum scientists view museum collections as "irreplaceable" and thus somehow sacred, almost untouchable. The implication is that these collections must be saved from the likes of nonprofit museums which do not have "any real appreciation" of what the collections represent. What are those specimens there for if not to further our understanding of the natural world? Why do we "protect" this material? So after 500 years our descendants can say, "See, we still have . . ."? I worry that we may not have any descendants at all.

Let's face it, our museums have become the backwaters of science. Once upon a time, taxonomy dazzled everyone as cutting-edge investigation. However, since Darwin "the action" has moved elsewhere. If we as museum professionals cannot get our precious collections used more than they are, then we too will pass into extinction.

So I say let our specimens provide protein and DNA samples to molecular biologists; encourage researchers and students to dissect museum materials to further education and understanding; and let our specimens go on public exhibit everywhere and anywhere they will do some good to educate people about nature. We couldn't ask for more!

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Mammalian Diversity in South America

Although it is unfortunate that many non-forest (dryland) habitats in South America have been destroyed or are threatened by human activity, Michael A. Mares (Report, 21 Feb., p. 976) presents a biased justification for their preservation. He concludes that mammalian diversity in the South American drylands exceeds that of other South American macrohabitats and that the drylands contain more families (39 to 36), genera (183 to 151), endemic genera (44 to 10), species (509 to 434), and endemic species (211 to 138) than the lowland Amazonian rain forest. We believe that Mares' conclusions are a consequence of his unorthodox division of macrohabitats.
Mares divides forested habitats into five macrohabitats (lowland Amazon rain forest, western montane forests, Atlantic rain forest, upland deciduous forest, and southern mesophytic forest), but lumps all nonforest habitats into the single macrohabitat "drylands." Mares' drylands encompass 57.3% of South America and include the llanos of Venezuela and Colombia, the cerrado and caatinga areas of Brazil, the chaco woodlands of Bolivia and Paraguay, the paramo and puna of the high Andes, the lowland Pacific deserts, the Pantanal, the xeric regions along the Caribbean coast, the steppes of Patagonia, and the isolated Gran Sabana. These drylands span South America longitudinally and latitudinally, including elevations from sea level to snow line, and thus represent a degree of heterogeneity shared by none of Mares' forest categories.

Usually the nonforested area of South America is divided into several distinct macrohabitats. We have not found another study that lumps together all South American nonforest habitats. For example, Hueck and Seibert (1) recognized 88 types of South American habitat, of which 39 correspond to Mares' five forest categories and 49 to the single drylands category. Haffer (2) recognized 12 major vegetation formations in South America; three were forest types and nine were components of Mares' drylands. Among mammalogists, Eisenberg (3) divided South America into four forested macrohabitats, eight drylands macrohabitats, and one mixed macrohabitat. M. A. Mares and Ojeda (4) recognized four forest macrohabitats and seven drylands habitats.

We would have been surprised, given the wide range of ecologically and historically distinct nonforest habitats included by Mares as "drylands," if the number of mammalian species in this category had not exceeded that of the Amazonian rain forest. The fact that the number of families, genera, and species in all nonforest macrohabitats only slightly exceeded those of Amazonia attests to the unusually high mammalian diversity of the lowland Amazonian rain forest.

Mares' most striking result is that the number of endemic genera in his "drylands" macrohabitat (44) greatly exceeded that in lowland Amazonian forest (10). We have re-analyzed patterns of generic endemism among the South American nonforest mammalian fauna [as listed in (3, 5–7)], dividing the nonforest areas of South America into eight macrohabitats: the llanos and Caribbean dry zone, chaco, steppe, caatinga, campos cerrados, paramo and puna, the pampas of the Buenos Aires region, and the Pacific arid zone.

Our results reveal 42 genera that are more or less confined to nonforest regions of South America. Of these, only 13 are endemic to a particular nonforest macrohabitat: three to the steppes, one to the chaco, two to the Pacific arid zone, and seven to the paramo and puna. All but one are monotypic genera, and they represent five orders: Marsupialia (two endemic genera), Chiroptera (two), Carnivora (one), Artiodactyla (two), and Rodentia (six). By comparison, according to Mares, 23 South American genera are endemic to forest macrohabitats: ten to lowland Amazonian rain forest, seven to western montane forest, four to southern mesophytic forest, and two to Atlantic rain forest. Thus, not only do individual forest macrohabitats contain more total endemic genera than nonforest macrohabitats but one forest macrohabitat, the lowland Amazonian rain forest, contains nearly as many endemic genera as all of the nonforest areas combined.

Other comparisons reveal the great diversity of the forest. For example, the puna and paramo of the high Andes, which are closest to Amazonia in terms of generic endemism, contain approximately 65 mammalian genera, 110 species, and 50 endemic species (5, 8, 9). The lowland Amazonian rain forest, in contrast, contains 151 genera, 434 species, and 138 endemic species, according to Mares.

We believe that lowland Amazonia con-
tains the highest mammalian diversity in South America; patterns for many other taxa in South America, including birds, higher plants, and freshwater fish (2,11,12), also reveal the overall richness of the Amazonian biota (10). Although we agree with Mares that overly negative scenarios of tropical extinction may prove counterproductive, we see no evidence of what he refers to as "the myth of Amazonian biodiversity." We also believe that conclusions based on studies of single groups, such as mammals, should not be viewed as representative of an entire ecosystem. Nevertheless, we concur with Mares that South American drylands require efforts on a scale appropriate for conservation and that more funding for drylands research is needed.

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REFERENCES


Response: In essence, Chesser and Hackett suggest that if the "drylands" are divided into enough units of sufficiently small size, no single unit will contain a greater amount of diversity than the lowland Amazon rain forest. In this I concur, although I also feel that their view of drylands diversity illustrates the power of the prevailing myth of Amazonian diversity. I included in my lowland rain forest category the Colombian Chocó, the Pacific lowland rain forest, the Amazonian lowlands, and all other wet and dry forests below 1500 meters precisely in an attempt not to bias the data against the lowland rain forest. However, defining macrohabitat categories for an entire continent in order to discern patterns of higher order taxonomic diversity is risky. Any lumping of habitats can lead to bias in the data. The Amazon forest itself can be subdivided into 8 to 14 major phytogeographic regions (1). Should diverse rain forest habitats be combined to form a superhabitat but non-forest habitats be limited to their smallest definable unit? I think not.

Chesser and Hackett determined that the puna and paramo macrohabitats of the high Andes are extremely rich in numbers of endemic taxa, although they point out that Amazonia is richer. However, Amazonia extends over an area of 5.34 million square kilometers, whereas the puna and paramo total only 1.9 million square kilometers. These drylands, 36% the size of the Amazon, contain 43% as many genera, 25% as many species, and 36% as many endemic species (numbers from Chesser and Hackett). On a per-area basis, they are similar to the Amazon. When one combines these areas with the other drylands on the continent to examine total diversity, the result is that Amazonia is the richest of the Amazonian macrohabitats.
the Amazon fares poorly in comparison.

I have not seen the data to which Chesser and Hackett refer to support the idea that other organisms also show that the Amazon is richer than the drylands, but factors other than sheer numbers of species must be considered in calculations of diversity. For example, the largely tropical countries of South America contain many species of vascular plants, but they have few endemic species (2), with the most found in temperate Chile and Argentina. The real question is not how many habitats comprise South America, but how the biodiversity (however it is defined) of rain forests compares with nonforest areas. The Amazon contains many animal species, but diversity at all taxonomic levels is higher outside the lowland forest in areas largely ignored by conservationists. I agree with Chesser and Hackett that there is a bias in conservation research, but not the one they suggest. Approximately 6.3% of the area included in the lowland rain forest category is protected, whereas only 2.4% of the drylands is protected (3). Funding dollars are allocated on the perceived richness of tropical forests as defined by numbers of species (4, 5), but there has been little attention given to definitions of biodiversity that include higher order diversity or taxonomic uniqueness (for exceptions see (2)) and almost no attention given to biodiversity research in drylands. Conservation efforts should not be biased for or against any area; they should be based on an unemotional determination of what it is we wish to preserve.

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REFERENCES
5. N. I. Platnick [J. Nat. Hist. 25, 1063 (1991)] found that anapid spiders are much more diverse in species numbers and taxonomic distinctness in South temperate zones than in the tropics and concluded (p. 1064), "at least for these spiders and I suspect for most non-vertebrate groups as well, the picture of a species-rich tropics and species-poor north and south temperate zones is inaccurate."
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and the initiation of direct phasing applications to small proteins. The review by Fortier summarizing developments in the direct phasing efforts in macromolecular crystallography is thorough, balanced, and up-to-date. The reader may find Bricogne’s presentation of the maximum entropy method a bit too mathematically technical and involved, but one need only refer to the author’s background articles in Acta Crystallographica to realize that great pains have been taken to simplify and focus the material in an attempt to make it more palatable to the uninitiated.

One of the main features of interest in the book is the discussion of capabilities of various direct-methods phasing programs and recent modifications that improve their performance. The programs described include MULTAN, SHELX, MITHRIL, XTAL, SAPI, MICE, DIRDF, SIR, and SIMPEL. These are compared as a group in a chapter by Gilmore and in varying detail by their developers throughout the volume. It is somewhat surprising that the majority of the expositions on these programs are not accompanied by meaningful structural test examples on which calculations have been performed and evaluated, the presentations of Sheldrick, Schenck, and Beurskens being notable exceptions.

In format, the quality of the papers varies according to what their authors submitted, and it is apparent that a number of them were not carefully proofread. It is fortunate that the mechanical defects do not seriously affect the quality of the science presented.

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