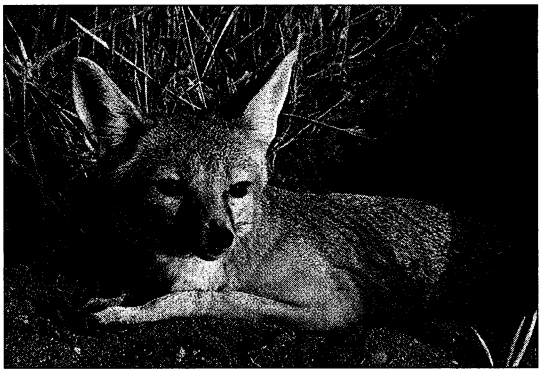
Canine Assistants for Conservationists

ALTHOUGH SOME FORENSIC USES OF SCENT-following dogs may be questionable, as Brisbin, Austad, and Jacobson point out in their letter "Canine detectives: the nose knows—or does it?" (10 Nov., p. 1093), the scent discrimination abilities of canines are still impressive. Controlled behavioral tests indicate that dogs can distinguish the odors from different species of animals, male and female dogs, and even different individuals within a species (*1*). These abilities are proving invaluable in conservation programs for endangered and difficult-to-distinguish species (*2*), particularly when combined with molecular techniques for the analysis of DNA extracted from the sloughed intestinal cells contained in feces (*3*).



Conservationists might have a new way to identify the calling cards of endangered species such as this San Joaquin kit fox.

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We have obtained molecular genetic confirmation of the ability of trained scenting dogs to distinguish among the scats (feces) of sympatric canid species. We trained dogs (*4*) to detect scats of the endangered San Joaquin kit fox (*Vulpes macrotis mutica*) and ignore those of coyotes (*Canis latrans*). One German shepherd recovered 435 presumed kit fox scats along 140 kilometers of transects in the Carrizo Plain Natural Area, California, in 16 days. We were able to isolate DNA from 329 of the scats. Mitochondrial DNA tests developed in the National Zoological Park's Molecular Genetics Laboratory (*5*) revealed that all 329 scats were indeed from kit foxes. Thus, the dog was 100% accurate in identifying kit fox scats in the presence of coyote, skunk (*Mephitis mephitis*), and badger (*Taxidea taxus*) scats along the transects. Species identification based on mitochondrial DNA (*5*) costs about $40 per sample. Hence, use of scent-detection dogs to distinguish scats from species of interest could provide a cost-effective alternative to laboratory methods in some conservation applications.

Fecal DNA analysis is potentially a powerful method for identifying species, population size, sex ratio, home range, paternity, and kinship (*6*). However, finding enough samples for this approach is difficult and time-consuming in many habitats, such as dense vegetation. Along transects through vegetation, our trained dog found about four times as many kit fox scats as an experienced person searching for scats visually. Thus, trained scent dogs can greatly increase the utility of DNA techniques in conservation and might be the only way to obtain information on the presence or absence of some endangered species around the world.

DEBORAH A. SMITH, University of Washington, Seattle, WA 98195–2100, USA; **KATHERINE RALLS**,* National Zoological Park, Smithsonian Institution, Washington, DC 20008, USA; **BARBARA DAVENPORT**, Department of Corrections Canine Program, Steilacoom, WA 98388, USA; **BRICE ADAMS, JESUS E. MALDONADO**, National Zoological Park, Smithsonian Institution
*To whom correspondence should be addressed. E-mail: rallsk@thegrid.net

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2. Dogs have been used successfully to find feces of grizzly bears (*Ursus arctos horribilis*) (S. Wasser, personal communication), black bears (*Ursus americanus*) (S. Wasser, personal communication; P. Paquet, personal communication), wolves (*Canis lupus*), and coyotes (P. Paquet, personal communication).
3. M. H. Kohn and R. K. Wayne, *Trends Ecol. Evol.* **12**, 223 (1997).
4. Our training methods were similar to those used when training dogs to detect narcotics.
5. E. Paxinos *et al.*, *Mol. Ecol.* **6**, 483 (1997).
6. M. H. Kohn *et al.*, *Proc. R. Soc. Lond. Ser. B.* **266**, 1429 (1999); H. B. Ernest *et al.*, *Mol. Ecol.* **9**, 433 (2000).

Present and Future Control of Malaria

CONFLICT WITHIN A SCIENTIFIC DISCIPLINE HAS the elements of a Dr. Jekyll–Mr. Hyde persona. On the positive side, it results from different interpretations of data gathered at the forefront of a fast-breaking field. Conflict in this light lends an air of excitement and stimulates new experiments that will resolve the contentious issues. On the negative side, conflict arises from clashes of rigid ideals, political agendas, control over research prioritization, and the competition for limited resources. Conflict from this perspective has a stifling effect on research progress as one group or another exerts its control.

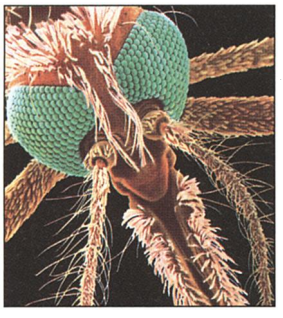
After reading the Policy Forums in the 24 November issue by C. F. Curtis ("The case for deemphasizing genomics in malaria control," p. 1508) and S. L. Hoffman ["Research (genomics) is crucial to attacking malaria," p. 1509] about the merits of genomics research in the fight to control malaria, I was left wondering what kind of debate I was witnessing and if the people who suffer from this disease don't deserve better from all of us working in the area. The urgency of committing resources to solutions that work today cannot be denied. The life of a child is worth more than a research grant. At the same time, one of the lessons of drug-resistant pathogens and insecticide-resistant vectors is that we must set the groundwork for solutions that are 5, 10, and even 20 years down the line. Not thinking of future approaches is as irresponsible as ignoring immediate needs. Let us provide today insecticide-impregnated bed nets and the best available therapeutic and prophylactic drugs, but also invest in genomics research that can lead to vaccines and better vector control.

ANTHONY A. JAMES
Department of Molecular Biology and Biochemistry, University of California, Irvine, CA 92967–3900, USA. E-mail: aajames@uci.edu

WHETHER GENOMICS IS IMPORTANT FOR malaria control is the topic of Curtis's and Hoffman's Policy Forums, but this type of debate is not new. Similar discussions often take place when new discoveries, revolutionary technologies, or paradigm shifts oc-

cur. In such debates, both sides present sound arguments. For the case in point, I think both sides are right—but for different time frames. In the short term, Curtis is right: Genomics still does not have much to contribute to malaria control activities; in his own words, it has yet to pass the "so what?" test. In the long term, no doubt Hoffman is right: Genomics will be the basis of new technologies and approaches for the development of improved and affordable tools that we need to control this killer disease—vaccines, better drugs to control the spread of resistance, and simpler and cheaper diagnostics.

The new strategies of the World Health Organization (WHO) (*1*) and of the United Nations Development Programme/World Bank/WHO Special Programme for Research and Training in Tropical Diseases (TDR) (*2*) avoid this false dilemma. These strategies are best described by WHO's director general: "WHO has to relate to two time frames. One is immediate. Member states have immense problems today, and cannot be asked to wait decades for their solution....'Roll back malaria' and the 'stop TB initiative' both address this issue. There are no ideal tools to fight malaria or tuberculosis, but this is no excuse to delay action. The other time frame is long term. We must keep alive the dream of developing new and better tools for the future....In short: we have to act now with what we have at our disposal; and we have the responsibility for fostering the development of new and improved tools against the problems of today and tomorrow" (*3*).



Eye to eye with a malaria vector, *Anopheles gambiae*.

CARLOS M. MOREL*

World Health Organization, 20 Avenue Appia, 1211 Geneva 27, Switzerland. E-mail: morelc@who.int

*Director of TDR

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Responses

I WHOLEHEARTEDLY AGREE WITH JAMES'S and Morel's conclusions. I stated a similar position in the closing paragraph of my Policy Forum (24 Nov., p. 1509): "We must invest heavily in international efforts like Roll Back Malaria to increase our application of current tools to the control of malaria. However, we would be foolish and irresponsible not to invest in research, if we are serious about improving and sustaining current control interventions and eventually eradicating malaria. Genomics is a critical component of 21st century biomedical research."

Some might read Curtis's Policy Forum and assume that funds for malaria control are being diverted to malaria genomics research. I know of no evidence that supports that contention. In fact, the funds devoted to malaria research worldwide are minimal as compared with the funds spent on malaria control. Nonetheless, given the enormous impact of malaria worldwide, both malaria control and malaria research are underfunded.

STEPHEN L. HOFFMAN

Naval Medical Research Center, Silver Spring, MD 20910–7500, USA. E-mail: hoffmans@nmrc.navy.mil

MOREL SAYS THERE IS NO DILEMMA BETWEEN short-term operational work on malaria control and genomic research, which he says is bound to pay off in the long run, and, furthermore, that both are now being adequately covered. However, as I discussed in my Policy Forum (24 Nov., p. 1508), I doubt if this is so. My plea to donors is to keep in mind that every milllion dollars given to a few molecular biologists in developed countries could have been used to employ many low-paid, but skilled, technicians in tropical countries and provided transport to take them to the villages. Alternatively, the money could have been spent on insecticide and antimalarial drugs that tropical villagers cannot (and should not be expected to) buy for themselves in sufficient quantity to achieve effective malaria control.

It is not enough for donors to decide that a genomic proposal is scientifically interesting and feasible with modern technology. They should also ask if it passes the "so what?" test, and if there is a genuine prospect that it could contribute to malaria control in the long run.

CHRIS CURTIS

London School of Hygiene & Tropical Medicine, London WC1E 7HT, UK. E-mail: chris.curtis@lshtm.ac.uk

On the Origins of Photosynthesis

THE MAJOR TYPES OF PIGMENTS USED BY bacteria that perform anoxygenic photosynthesis were shown through phylogenetic analyses by Xiong and colleagues in their report (*1*) to have already been present when oxygenic photosynthesis developed. In the Perspective accompanying the report ("When did photosynthesis emerge on Earth?" 8 Sept., p. 1703), D. J. Des Marais concludes, "As the great antiquity of photosynthesis becomes more and more apparent, it also becomes easier to envision an ancient, global biosphere sustained principally by anoxygenic photosynthesis," under conditions where "ambient oxygen levels were insignificantly low." If true, then the following premises must be accepted: (i) in the absence of an ozone screen, these light-requiring organisms lived under intense ultraviolet radiation from the early sun; (ii) these organisms obtained their primary electron donor (hydrogen sulfide, H_2S) in an ocean supposedly saturated in ferrous iron (*2*), a geochemical condition consistent with banded iron formations in the geologic record, but one that would make H_2S a trace species; and (iii) the heterotrophic organisms in which aerobic respiration oxidizes and recycles today's primary productivity back to CO_2 would not have been active (*3*).

There are additional difficulties with Des Marais' conclusion. A global rate of sedimentation and sediment recycling has been estimated at about (1 to 2) $\times 10^{16}$ grams per year (*2, 4*). Des Marais estimates the global anoxygenic productivity to be about (0.2 to 2.0) $\times 10^{12}$ moles of carbon per year. If so, virtually all of the organic carbon produced must have been buried for the average rock to contain even half of the 0.5 to 0.6% carbon that most authorities accept (*2*). Des Marais further estimates that, after oxygenic photosynthesis arose, primary productivity would have increased "by at least two to three orders of magnitude." Because $CO_2 + H_2O$ yields "CH_2O" + O_2, this net productivity would release the same number of net moles of oxygen annually. With no aerobic respiratory organic carbon "sink," a yearly 10^{14} moles of photosynthetic O_2 would require 4×10^{14} moles of Fe^{2+} (*2*) and would place the iron "sink" at 2.2×10^{16} grams of Fe (as Fe_2O_3) per year, which exceeds the entire global sedimentation rate! Thus, without global aerobic organic carbon recycling, soon after photosystem II arose the

Letters to the Editor

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