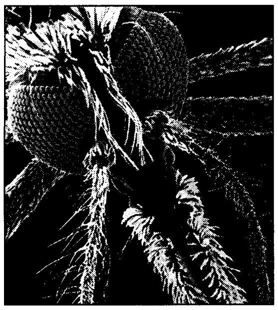
cur. In such debates, both sides present sound arguments. For the case in point, I think both sides are right—but for different time frames. In the short term, Curtis is right: Genomics still does not have much to contribute to malaria control activities; in his own words, it has yet to pass the "so 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*.

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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.

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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.

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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) × 10^{16} grams per year (*2, 4*). Des Marais estimates the global anoxygenic productivity to be about (0.2 to 2.0) × 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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enormous net oxygen production of cyanobacteria should have completely overwhelmed the inorganic hydrothermal "sinks" and Earth should have experienced a rapid oxygen build-up.

A parsimonious way to avoid these dilemmas is to consider an early Earth with an ambient oxygen content sufficient to support aerobic recycling of the net organic carbon productivity in surface waters (*3*) and sufficient to stimulate the early evolution of photosystem II in the cyanobacteria (*5*). It is difficult to assign a value of oxygen that would make this possible, but a level near 1 to 2% of the present atmospheric level is minimal. This is also enough to provide some minimal ozone protection against the ultraviolet flux from the young sun, which would have been higher than today.

The major groups of photosynthetic bacteria, such as cyanobacteria (blue green filaments, ~5-micrometer diameter) and purple sulfur bacteria (spheres), have formed stable ecological associations for billions of years.

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Response

OUR KNOWLEDGE OF THE ARCHEAN ENVIRONMENT and its biota is exceedingly fragmentary, but the few available clues are helpful. Atmospheric oxygen levels must have been very low before oxygenic photosynthesis developed. During the Archean Eon, high-temperature hydrothermal emanations contributed reduced species such as H_2S, H_2, Fe^{2+}, and CH_4 at a rate that probably exceeded the modern rate of (0.12 to 1.2) × 10^{12} moles per year (*1*) and therefore easily overwhelmed the major nonbiological source of free oxygen, namely the photodissociation of H_2O vapor estimated at ~10^{10} moles of O_2 per year (*2*). Even in the absence of an ozone ultraviolet shield, benthic photosynthetic microorganisms could have survived during the day-night cycle by migrating vertically within ultraviolet-shielding minerals (*3*) and by using organic pigments (*4*), to optimize the beneficial versus deleterious effects of solar radiation. As Towe points out, ferrous iron is indeed a sink for aqueous sulfides. However, nonoxygenic photosynthetic bacteria can also use alternative substrates such as hydrogen from hydrothermal sources, and even ferrous iron itself (*5*).

Towe proposes a constraint on ancient oxygen levels from an estimation of Archean global rates of organic carbon cycling, including its sedimentation. However, uncertainties in our knowledge make estimating such constraints extremely difficult. For example, despite their generally much greater productivity, the presence of ancient oxygenic photosynthetic ecosystems cannot be clearly discerned from nonoxygenic photosynthetic ecosystems by using estimates of sedimentary organic carbon contents, as the following illustrates. For a pre-3.0-billion-year-old Earth lacking oxygenic photosynthesis but with a geothermal heat flow that was two to three times the modern value (*6*), global primary productivity might have been in the range of 10^{14} grams of carbon per year (*7*). Assuming an Archean global sedimentation rate equal to the modern value [1.3 × 10^{16} grams per year (*8*)], and assuming that half of the organic carbon from global primary productivity was ultimately buried in aqueous sediments, the mean organic carbon concentrations of sediments would have been about 0.4 weight %. This value would be indistinguishable from carbon concentrations of modern marine sediments, which scatter widely but have a mean value near 0.6 weight % (*9*).

Once oxygenic photosynthesis arose, respiration probably developed simultaneously, perhaps even earlier, given the enormous benefit of respiration both as an effective energy-harvesting strategy and as a sink to mitigate potentially toxic oxidants. About 99.9% of today's primary productivity is soon recycled by the biosphere (*8, 10*). Therefore, over the long term, biological oxygen production has probably always been nearly balanced by biological and abiotic oxygen sinks.

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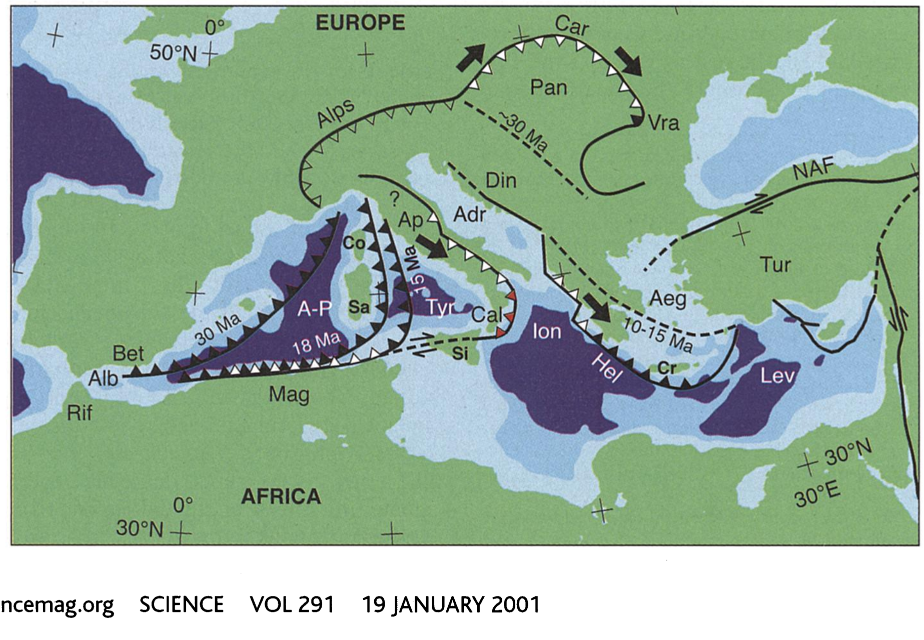
CORRECTIONS AND CLARIFICATIONS

REVIEW: "Subduction and slab detachment in the Mediterranean-Carpathian region" by M. J. R. Wortel and W. Spakman (8 Dec., p. 1910). In Fig. 1, intended to illustrate the evolution of plate boundaries in the Mediterranean-Carpathian region, the text and graphics on the map did not correspond with the appropriate geographic features. The correct figure is shown below.



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