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.

A Risky Forest Policy in the Amazon?

IN THEIR POLICY FORUM "NATIONAL forests in the Amazon" (30 Aug., p. 1478), A. Veríssimo *et al.* seem assured that a new system of national forests will solve the problems of uncontrolled forest exploitation in the Brazilian Amazon. Unfortunately, we are far less optimistic. Attached to the laudable effort to develop an expanded network of national forests is an ill-advised plan to harvest timber on half of that land through a system of forest concessions. This plan apparently has been formulated without regard to the widespread problems of forest concessions in developing countries (*1–5*) and will provide the large-scale forest industry with subsidized access to substantial remaining old-growth tropical forests of Amazônia.

The decision to adopt concessions is based on the mistaken premises that harvesting on public lands is more profitable than harvesting on private lands and that the government of Brazil will be better able to monitor forest industry activities, thereby reducing illegal harvesting and increasing the adoption of sustainable forest management practices. In fact, concessions may have considerable unintended and negative side-effects, many of which have so far escaped serious discussion.

Some complications that may arise include the following: subsidized timber production from concessions may crowd out legal logging on private lands; monitoring concessions will add costly administrative and professional responsibilities for which the government is unprepared; concessions will not deter illegal logging; and they will give preferred access to large-scale producers while missing opportunities to direct industry benefits to private land holders.

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The recent effort by the Ministry of the Environment through its National Forestry Program is commendable. They are consulting the public and entertaining a variety of opinions in a transparent manner. The products of this effort, however, are as yet inadequate for a policy decision that encompasses the largest tropical forest of the world. National forests for the Brazilian Amazon are a good idea—they will provide initial protection for vast areas of the forest—but the Brazilian government must think carefully before allowing industrial harvest of these forests.

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Response

MERRY *ET AL.* EXPRESS DOUBTS THAT Brazilians have the capacity to overcome the predatory and illegal logging that could occur within its new system of National Forests (Flonas). Contrary to their opinions, Brazil has learned from troubled concession systems in other tropical countries. The strategy for locating new Flonas is based on social, economic, and biological criteria. The actual concession system will be defined this year, in part, through an open public debate within the National Congress, with participation of nongovernmental organizations, forest scientists, and logging industry representatives. This transparent and democratic process is very different from what has occurred in other countries.

Brazil's forest policy is not based on the idea that harvesting from public forests is more profitable than harvesting on private lands, as Merry *et al.* assert. Private forest management is an important component of the overall strategy to increase the supply of certified timber, but it cannot be expected to meet the entire demand. At present, only 24% of the Brazilian Amazon is privately owned, with much already deforested or degraded by predatory logging and fire (*1*). Given the current demand of 28 million m^3 $year^{-1}$, most timber will have to come from disputed or unclaimed public lands (45% of Amazonia), where uncertain tenure makes sustainable management unlikely without Flona status (*2*).

There is strong political support for expansion and consolidation of Flonas and other protected areas in the Amazon (*3*). Risks inherent in implementing Flonas are being addressed through new concession and reformed monitoring systems that the government will enforce. The Flona system is designed to break the status quo of predatory logging wherein loggers extract timber from unclaimed public lands without paying fees. Stumpage fees will be used to strengthen management, monitoring, and administration of Flonas. A portion may also be returned to local communities. Flonas will therefore benefit local economies and forest conservation much more than current practices that catalyze deforestation and illegal logging without providing sustainable socio-economic benefits.

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The EU and the Dinosaur

THE RECENT ARTICLE ON THE DEBUT OF THE Framework 6 program, the European Union's scientific research program ("Framework 6 debut prompts calls for a better approach," G. Weiss and G. Vogel, News Focus, 8 Nov., p 1163), raises some interesting points, but without more details, I fear that your non-European readers may not realize the full potential of science by eurocracy. The EU in its infinite wisdom took a good look at the most successful research enterprise on the planet and decided that the U.S. NIH/NSF model of individual competitors on a level playing field was not appropriate. Instead, we have been drafted into collaborative networks, which have grown in size from three to five groups at the outset to the current norm of six to ten "collaborating" labs in the currently running projects. In the brave new Framework 6, the EU is now poised to take a great leap forward into an era of "networks of excellence" (whatever that means) comprising tens to hundreds of researchers. If one compares this to the strategy taken by an organization that actually has some experience in funding international networks of excellence based solely on scientific criteria, it is interesting to note that the Human Frontier Science Program Organization has an upper limit of four partners in the networks that it funds.

There is, however, one ray of light in the eurotunnel. The new meganetworks will have their own built-in dedicated administration, which will act as a buffer between the scientist pawns and the central eurocracy in Brussels. As a matter of fact, this strategy is already being implemented for the current "mininetworks," with the recent appointment of outside project technical assistants to help the central Framework program office perform its tasks. The immediate evolutionary analog that springs to mind is the dinosaur that required two brains—one in the head to coordinate strategy and one based near the tail to control "production." And we all know what happened to the dinosaurs.

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Assessing Environmental Changes in Grasslands

AN INTRIGUING QUESTION IS RAISED IN THE Report by M. R. Shaw *et al.*, "Grassland responses to global environmental changes suppressed by elevated CO_2" (6 Dec., p. 1987), and the accompanying Perspective by J. A. Morgan ("Looking beneath the surface," 6 Dec., p. 1903). What mechanisms caused elevated CO_2 to decrease root allocation and offset the positive effects of warming, increased precipitation, and nitrogen addition on net primary production (NPP) in a California grassland? Both papers emphasize the potential role of increased nutrient limitation, although, as Shaw *et al.* acknowledge, increased nutrient limitation more commonly increases root allocation (*1*). We would like to suggest an alternative mechanism—elevated CO_2 may have increased pathogen load on the C_3 grasses dominating the grassland, thereby decreasing root allocation and NPP.

In a Minnesota grassland, free air CO_2 enrichment (FACE) increased by one-third the percentage of C_3 grass leaf area infected by foliar fungal pathogens (leaf spot and rust diseases), averaged across treatments varying in plant diversity and nitrogen addition (*2*). In both the Minnesota and California FACE experiments, species of the C_3 grass genus *Bromus* were dominant species (*3*). How might increased pathogen load impact NPP and root allocation? In a late-successional Minnesota grassland in which C_3 graminoids made up 29% of above-ground biomass, foliar fungal pathogens decreased root production by 25% and total plant biomass (an indicator of NPP) by 24% relative to plots in which foliar fungal disease was experimentally reduced over 3 years (*4*). Together, the Minnesota results suggest the hypothesis that elevated CO_2 can decrease below-ground allocation in C_3 grasslands by increasing pathogen load. Moreover, increased moisture, warming, and nitrogen addition can each increase foliar fungal pathogen load (*2*, *5*, *6*), so positive interactive effects of these variables and elevated CO_2 on pathogen load may explain their negative interactive effects on NPP and root allocation. Two other recent studies have also highlighted the potential importance of plant disease as a mechanism for the effects of global change (*6*, *7*). We believe that increased pathogen load presents a plausible mechanism for the inhibitory effects of elevated CO_2 reported by Shaw *et al.*

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THE RESEARCH OF M. R. SHAW *ET AL.* ("GRASSland responses to global environmental changes suppressed by elevated CO_2," Reports, 6 Dec., p. 1987) is described in This Week in *Science* (6 Dec., p. 1843) as applying "a suite of realistic changes" to appraise future growth in a California grassland. Although the authors' experimental treatments expose surprising interactions, they are not realistic. Because Shaw *et al.* do not report the soil moisture or aeration caused by their irrigation, neither its realism nor its surprising failure to increase growth substantially can be judged. None of the 40 International Panel on Climate Change (IPCC) emission scenarios (*1*) project the experimental CO_2 treatment of 680 parts per million (ppm) applied by Shaw *et al.* by the year 2050. About 15 of the scenarios (the "high" A1 family) could reach 680 ppm by about 2075. A business-as-usual CO_2 scenario (*2*), incorporating the decarbonization trend of the past 200 years, and many of the IPCC B family scenarios never reach 680 ppm. An Iowa farmer might fertilize corn with the experimental N deposition of 70 kg ha^{-1} $year^{-1}$ that Shaw *et al.* apply to the California grassland. But around the world, today's bulk deposition from the air ranges from about 5 to 15 kg ha^{-1} and has changed little for a century. Even adding the leveling global fuel and transport emission near 25 Tg NO_x-N to the leveling 80 Tg of N fertilizer that might or might not reach the air, annual global deposition would still increase an average of only 2 kg ha^{-1}, leaving N deposition at 10 to 25% of the authors' 70 kg ha^{-1} (*3*). Using remote or extreme assumptions rather than more realistic ones limits the authors' relevance to a few fields in an improbable or distant future. We agree with Richard Moss, executive director of the U.S. Global Change Research Program, and Stephen Schneider that global change researchers should show the probability of their predictions (*4*).

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Response

THE LETTERS FROM MITCHELL AND REICH and Ausubel and Waggoner highlight different aspects of the need for multifactor global change experiments with complete ecosystems, like that discussed in our Report. The suggested role for pathogens illustrates the importance of studying a real ecosystem with the complete suite of the potentially important agents. Experiments with artificial or simplified ecosystems run the risk that responses are distorted by the absence of a key regulator. The concern about the choice of experimental treatments emphasizes the value of observations across a range of appropriate treatments. Experiments with multiple global changes address the fundamental nature of global change and make it feasible to assess the generality of the results and the sensitivity of the treatments.

The suggestion from Mitchell and Reich that pathogens might be involved in the CO_2 responses we discussed is an interesting possibility. Although we did not see visible signs of leaf or root pathogens, we do not have evidence to establish that pathogens were not involved. We intend to quantify pathogens in future observations of the Jasper Ridge Global Change Experiment.

Ausubel and Waggoner feel that "a suite of realistic changes," a phrase from This Week in *Science*, is an inaccurate characterization of the experiment we described. The Jasper Ridge Global Change Experiment was designed to explore the effects of four potentially important aspects of a possible future at approximately doubled atmospheric CO_2. This concentration is close to the middle of the range for IPCC reference scenarios for 2100 (*1*) and enough of a standard in global-change research to facilitate comparisons among experiments. The four global-change factors are elevated CO_2, warming, increased precipitation, and increased N deposition, with elevated and ambient levels of each. Our experimental warming is at the low end of the IPCC range for doubled CO_2 (*1*). The nitrogen deposition is at a level not uncommon in Northern Europe today (*2*), and the precipitation change is somewhat larger than that observed by Snyder *et al.* (*3*) in a recent climate-model study of California responses to doubled CO_2. The elevated level of each of the four global-change factors is broadly consistent with a doubled-CO_2 world, recognizing that global changes in temperature, precipitation, and N deposition are less certain and likely to be more spatially heterogeneous than changes in atmospheric CO_2 (*1*). Because we studied treatments with all possible combinations of ambient and elevated levels of the four factors, we actually explored a wide range of possible futures. The treatment with all four factors at elevated levels was one of the 16 treatments, as was the treatment with warming and elevated CO_2, but ambient precipitation and N deposition, and so forth. This diversity of treatments is critically important for assessing the generality of results, isolating controlling factors, and untangling interactions.

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TECHNICAL COMMENT ABSTRACTS

COMMENT ON "The Influence of the Proinflammatory Cytokine, Osteopontin, on Autoimmune Demyelinating Disease"

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The identification by Chabas *et al.* (Reports, 23 November 2001, p. 1731) of osteopontin (OPN) as an important gene for inflammatory diseases, based on experiments with OPN-deficient mice with mixed 129 and B6 genes, is questioned. We show that a genetically controlled OPN-deficient mouse does not show any evidence of effects on models for multiple sclerosis (MS) and arthritis.

Full text at www.sciencemag.org/cgi/content/full/299/5614/1845a

RESPONSE TO COMMENT ON "The Influence of the Proinflammatory Cytokine, Osteopontin, on Autoimmune Demyelinating Disease"

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OPN is involved in the progression of experimental autoimmune encephalomyelitis (EAE). Transcripts for OPN are elevated in MS lesions, and OPN can be detected in the white matter in MS. DNA vaccination to osteopontin attenuates the chronic phase of EAE. The findings of Blom *et al.* may stem from differences between the EAE model they employed and those used in other studies.

Full text at www.sciencemag.org/cgi/content/full/299/5614/1845b

CORRECTIONS AND CLARIFICATIONS

STATEMENT ON SCIENTIFIC PUBLICATION AND SECURITY (21 Feb., p. 1149). In the full list of authors of the statement, which appeared online, Steven Salzberg's name was mispelled and his affiliation was listed incorrectly. He is at The Institute for Genomic Research. His name and affiliation appear correctly at www.sciencemag.org/cgi/content/full/299/5610/1149/DC1.