LETTERS

Prescribed Fire and Natural Disturbance

THE RECENT COVERAGE OF OUR WORK ON THE relationship between fire history and an emerging forest epidemic called sudden oak death highlights landscape-level aspects of disease spread, which are often overlooked ("Fighting sudden oak death with fire?", J. Withgott, News Focus, 20 Aug., p. 1101). Although we are interested in the possible role of prescribed fire in managing this disease, subsequent reports in the popular press have claimed that we advocate such an approach as treatment. A cautionary note is therefore required at this point. We have not found a direct connection between fire suppression and this disease, and there is reason to suspect that the effects of past wildfires could be very different than those of the typical controlled burn. The decision to use prescribed fire in an ecosystem should be guided by location- and case-specific considerations (*1*).

Skeletons of federally listed (threatened) Morro Manzanita shrubs (*Arctostaphylos morroensis*) immediately after a prescribed burn, which led to its local extirpation (*2*).

As Lindenmayer *et al.* note in their Policy Forum "Salvage harvesting policies after natural disturbance" (27 Feb., p. 1303), natural disturbances such as fire are integral to the healthy functioning of most ecosystems and are often poorly understood in policy and management arenas. The emphasis here is on "natural" disturbances and the important role they play. Most prescribed burns, however, are attempted during conditions when fire is not likely to escape control (e.g., outside the normal fire season). Burning under these conditions will not necessarily produce the natural range of fire severities and subsequent fire effects that could result from past wildfires.

Restoring fire regimes is of great importance, but prescribed fires must ultimately mimic natural events to fulfill their role in disturbance-mediated ecosystems. Prescribed fires that do not attain this goal can have harmful ecological effects, even if successful for goals of fuel reduction and fire reintroduction. Populations of fire-dependent native species can be decimated (*2*) if timing or heating requirements for regeneration are not met. Invasive species may also be promoted, which can lead to near-permanent alteration of fire regimes and ecosystem functioning (*3*). Whether for ecosystem health in general, or management of forest pathogens in particular, prescribed fire will need to be tailored to the societal goals and ecological requirements of the situation at hand.

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The Origins of Afroasiatic

IN THEIR REVIEW "FARMERS AND THEIR languages: the first expansions" (25 Apr. 2003, p. 597), J. Diamond and P. Bellwood suggest that food production and the Afroasiatic language family were brought simultaneously from the Near East to Africa by demic diffusion, in other words, by a migration of food-producing peoples. In resurrecting this generally abandoned view, the authors misrepresent the views of the late I. M. Diakonoff (*1*), rely on linguistic reconstructions inapplicable to their claims (*2*), and fail to engage the five decades of Afroasiatic scholarship that rebutted this idea in the first place. This extensive, well-grounded linguistic research places the Afroasiatic homeland in the southeastern Sahara or adjacent Horn of Africa (*3–8*) and, when all of Afroasiatic's branches are included, strongly indicates a pre–food-producing proto-Afroasiatic economy (*1, 7, 8*).

A careful reading of Diakonoff (*1*) shows his continuing adherence to his long-held position of an exclusively African origin (*4, 5*) for the family. He explicitly describes proto-Afroasiatic vocabulary as consistent with non–food-producing vocabulary and links it to pre-Neolithic cultures in the Levant and in Africa south of Egypt, noting the latter to be older. Diakonoff does revise his location for the Common Semitic homeland, moving it from entirely within northeast Africa to areas straddling the Nile Delta and Sinai, but continues to place the origins of the five other branches of the Afroasiatic language family wholly in Africa (*1*). One interpretation of the archaeological data supports a pre–food-producing population movement from Africa into the Levant (*9*), consistent with the linguistic arguments for a pre-Neolithic migration of pre–proto-Semitic speakers out of Africa via Sinai (*8*).

The proto-language of each Afroasiatic branch developed its own distinct vocabulary of food production, further supporting the view that herding and cultivation emerged separately in each branch after the proto-Afroasiatic period (*7*, *8*). Diamond and Bellwood adopt Militarev's (*2*) solitary counterclaim of proto-Afroasiatic cultivation. However, not one of Militarev's proposed 32 agricultural roots can be considered diagnostic of cultivation. Fifteen are reconstructed as names of plants or loose categories of plants. Such evidence may reveal plants known to early Afroasiatic speakers, but it does not indicate whether they were cultivated or wild. Militarev's remaining roots are each semantically mixed, i.e., they have food-production–related meanings in some languages, but in other languages have meanings applicable to foraging or equally applicable to foraging or cultivating.

Furthermore, the archaeology of northern Africa does not support demic diffusion of farming populations from the Near East. The evidence presented by Wetterstrom (*10*) indicates that early African farmers in the Fayum initially incorporated Near Eastern domesticates into an indigenous foraging strategy, and only over time developed a dependence on horticulture. This is inconsistent with in-migrating farming settlers, who would have brought a more abrupt change in subsistence strategy. The same archaeological pattern occurs west of Egypt, where domestic animals and, later, grains were gradually adopted after 8000 yr B.P. into the established pre-agricultural Capsian culture, present across the northern Sahara since 10,000 yr B.P. (*11*). From this continuity, it has been argued that the pre–food-production Capsian peoples spoke languages ancestral to the Berber and/or Chadic branches of Afroasiatic, placing the proto-Afroasiatic period distinctly before 10,000 yr B.P. (*8*). Furthermore, there is evidence that cattle domestication occurred independently in the early Holocene eastern Sahara, earlier than in the Near East (*12*), casting doubt on the idea of a single origin of food production in the Levant.

A critical reading of genetic data analyses, specifically those of Y chromosome phylogeography and TaqI 49a,f haplotypes, supports the hypothesis of populations moving from the Horn or southeastern Sahara northward to the Nile Valley, northwest Africa, the Levant, and Aegean (*13–15*). The geography of the M35/215 (or 215/M35) lineage, which is of Horn/East African origin, is largely concordant with the range of Afroasiatic languages. Underhill *et al.* state that this lineage was carried from Africa during the "Mesolithic" (*13*). The distributions of the Afroasiatic branches and this lineage can best be explained by invoking movements that originated in Africa and occurred before the emergence of food production, as well as after.

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Response

EHRET *ET AL.* SUGGEST THAT EARLY Afroasiatic languages were spread by Mesolithic foragers from Africa into the Levant. In our Review, we did not positively favor either the African or the Levant origin hypothesis (p. 601). But in the map (Fig. 2), I chose the Levant hypothesis, because I believe, on balance, that it provides the best explanation for the evidence that has survived through 12,000 years of prehistory.

In linguistic terms, Ehret (*1*) has presented a phylogenetic history for Afroasiatic languages, based on shared phonological innovations, that contains a primary division between the Omotic languages of Ethiopia and an Erythraean subgroup that includes all other Afroasiatic languages (including Semitic and Ancient Egyptian). This ordering, if correct, suggests an African origin for the family. But is it correct? Diakonoff (*2*, *3*) has offered a completely different grammatical subgrouping structure for Afroasiatic, in the process, casting doubt on Omotic as a member of the family and suggesting [(*2*), p. 218] that the predomestication [but probably early cultivating (*4*)] Natufian archaeological complex of Palestine matches well with proto-Afrasian (Afroasiatic) cultural and environmental vocabulary reconstructions. Militarev's reconstructed proto-Afroasiatic vocabulary (*5*), whether "agricultural" or not, is also peopled with animals and plants of Levant, not African, origin and matches a Natufian cultural landscape. Ehret *et al.* point out that Militarev's semantic reflexes are mixed, but perhaps this is to be expected given that plants of Levant (winter rainfall) origin did not spread prehistorically into the desert or summer rainfall belts of northern Africa beyond the Mediterranean coast, Egypt, and highland Ethiopia.

In archaeological terms, I agree that early Saharans managed cattle, and Ehret himself convincingly relates the earliest appearance of this tradition to Nilo-Saharan–speaking populations (*6*). The Egyptian Neolithic economy, however, was manifestly of Levant and not African origin. Domesticated sheep and goats were probably introduced via Arabia into the Horn of Africa at a similar time, circa sixth millennium B.C.

My assumption is that the spread of Afroasiatic occurred as a result of actual human movement, not language diffusion alone. There is no significant archaeological evidence for a population movement from Africa into the Levant, whether Mesolithic or Neolithic, at the time in question. The genetics papers quoted by Ehret *et al.* do not settle this matter. The Y chromosome evidence appears to signal complex two-way population movements, with very uncertain chronologies. My working assumption, therefore, is that early Afroasiatic languages spread from the Levant into Africa between 7000 and 12,000 years ago, probably in more than one movement. Subsequent history has seen an enormous spread of Semitic languages, including Ethiopian Semitic and, of course, Arabic, on such a scale that the original phylogenetic geography of the Afroasiatic language family must have been considerably erased. Because of this, the geographical source of this family will not reveal itself easily. I have just published a detailed discussion of Afroasiatic prehistory from archaeological and linguistic perspectives (*4*), and the above points are made in more detail there.

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Earth's Entropy

RALPH LORENZ'S PERSPECTIVE "FULL STEAM ahead—probably" (7 Feb. 2003, p. 837) on the recent groundbreaking work of Roderick Dewar (*1*) mentions the puzzle that "All else being equal, MEP [maximum entropy production] would predict a planet's meridional temperature contrast to be independent of its rotation rate. This disagrees with some rudimentary GCM [general circulation model] experiments, and with meteorologists' intuition."

It is well known that tidal and atmospheric motions exert torque on the solid Earth, which detectably affects its rotation rate (*2*, *3*). Hadley-cell–driven trade winds, for example, exert torque on Earth's surface in a direction that promotes continued rotation. This could conceivably amount to ordered work that acts as an additional mode of entropy production. Perhaps climate modelers should investigate whether one consequence of maximum entropy production on Earth may be partial regulation of planetary rotation rate.

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Response

PHILLIPS SUGGESTS THAT THERMODYNAMICS may guide planetary rotations. For Earth, at least, this is unlikely to be so. The usefulness of maximum entropy production (MEP) is only as a selection guideline among dynamically permitted steady states, and the rotation state of the planet may control which states are dynamically possible. The system must first comply with the rigid laws of physics, notably the conservation of mass, energy, and angular momentum: These factors are imposed as constraints on the system before MEP applies.

Even if Earth's whole atmosphere were to spin up to the speed of sound (an extreme case!), angular momentum balance means the rotation period of the solid Earth (where much of the solar heat is absorbed and reradiated) changes by only about one part in one million—a level unlikely to affect heat transfer. Thus, even if the dynamics allowed such a spin-up, it seems the system would gain little from the effort.

However, Phillips' basic suggestion, that optimality in heat transport may guide rotation rates, may have merit for the atmospheres of extrasolar giant planets (*1*) where atmospheric motions at the relatively high altitudes where starlight is absorbed and thermal radiation emitted are largely decoupled from the motion of the planet's interior. If the motions are guided by an MEP heat transport criterion, close-in extrasolar planets, even if tidally locked to their parent star, may nonetheless have only modest day:night temperature contrasts.

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The Brain, Neurons, and Behavior

I OPENED THE SPECIAL ISSUE ON COGNITION and Behavior (15 Oct., pp. 431–452) with a "there we go again" feeling. So it was a relief to read Donald Kennedy's Editorial "Neuroscience and neuroethics" (p. 373). It has become fashionable to equate the brain with the mind, which in turn controls behavior. Presumably it's hard science, because neurons are involved. But it isn't. It's just a confusion of the necessary with the sufficient, a point made in the Editorial. Altogether too often, sight is lost of the fact that any particular brain can evolve into any particular mind, depending on the experiences encountered.

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CORRECTIONS AND CLARIFICATIONS

News Focus: "RNAi shows cracks in its armor" by J. Couzin (12 Nov., p. 1124). On page 1125, in the second column, second paragraph, the sentence, "At a meeting last week in Titisee, Germany, Sharp presented preliminary data from his lab showing a 10-fold change in protein levels with only a twofold microRNA difference, the level commonly seen from an off-target effect," the term "microRNA" should have read "mRNA."

Random Samples: "Good as new" (5 Nov., p. 971). This item incorrectly reported that a new laser technique for cleaning ancient coins was developed by Italian archaeologists. It was devised by physicists at IFAC-CNR in Florence, Italy. The accompanying photo credit should have read S. Siano.

Reports: "Requirement for caspase-2 in stress-induced apoptosis before mitochondrial permeabilization" by P. Lassus *et al.* (23 Aug. 2002, p. 1352). This paper reported that silencing expression of caspase-2 with an siRNA prevented apoptosis. Since the time of publication, the authors have identified an siRNA that silences expression of the caspase-2 protein but fails to prevent apoptosis. The authors are investigating three possibilities to explain their results: (i) These siRNAs differentially silence caspase-2 isoforms, which alters the outcome of drug-induced apoptosis; (ii) one of the two siRNAs silences an unidentified gene(s), whose product is involved in apoptosis; and (iii) one of the two siRNAs has some effect unrelated to RNAi.

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