Letters

Kuwaiti Fires and Nuclear Winter

I was surprised to see in Michelle Hoffman's article "Taking stock of Saddam's fiery legacy in Kuwait" (Research News, 30 Aug., p. 971) an attribution to me of what are described as "doomsday scenarios . . . that the fires could touch off a global weather catastrophe," which is also described as "nuclear winter." While there is a great range of nuclear winter scenarios (*1*), they generally involve global temperature declines in the 10° to 25°C range. The concern that Richard Turco and I expressed, before the massive oil well fires in Kuwait were set, was about much more minor effects, both in geographic extent and severity; in particular, we described (*2*) the possibility that, if a significant amount of smoke rose to high altitude, temperatures might fall by 1° to 2°C (not 10° to 25°C) over much of South Asia (not the entire planet). The comparison explicitly made was with the temperature decline after the Mount Tambora explosion of 1815, estimated to be about 1°C (*3*). We also clearly stated that "we cannot be certain that this extent of cooling and darkening of the ground would be the result of massive burning of the Kuwaiti oil fields," and predicted that the contribution to global greenhouse warming would be negligible. Indeed, the smoke does not appear to have risen as high as we feared, in part because of synoptic weather conditions limiting "self-lofting"; and significant climatic effects seem to have been restricted to the war zone itself.

The Kuwaiti fires can be considered a very small-scale intimation of nuclear winter, with regional temperatures falling by about 7°F (4°C) below regional averages (*4*). Because a slight *warming* is expected from smoke in the lower few kilometers of the atmosphere (*1*), where much of the Kuwaiti smoke has been confined, such regional temperature declines result from the small amounts of sooty smoke at higher altitude and represent a remarkable validation of nuclear winter effects.

It would be a mistake to conclude, because the Kuwaiti oil well fires have not produced a "global weather catastrophe," that we have nothing to fear from nuclear winter. Test fires and hydrodynamic simulations repeatedly show (*1, 5*) that the fine sooty smoke from the burning of cities in a nuclear war would be more abundant, would be carried higher into the atmosphere, and would be blacker than the Kuwaiti oil fire smoke; and the temperature declines for a wide range of nuclear war scenarios would be much larger than occurred in Kuwait. Massive reductions in world nuclear arsenals appear to be the most reliable way to guarantee that nuclear winter will not transpire.

CARL SAGAN
Laboratory for Planetary Studies,
Cornell University,
Ithaca, NY 14853–6801

REFERENCES

1. R. P. Turco, O. B. Toon, T. P. Ackerman, J. B. Pollack, C. Sagan, *Science* **222**, 1283 (1983); A. Pittock *et al.*, SCOPE (Scientific Committee on Problems of the Environment of the International Council of Scientific Unions) Report 28, *Environmental Effects of Nuclear War*, vol. 1, *Physical and Atmospheric Effects* (Wiley, Chichester, 1986); R. P. Turco, O. B. Toon, T. P. Ackerman, J. B. Pollack, C. Sagan, *Science* **247**, 166 (1989); *Annu. Rev. Earth Planet. Sci.* **19**, 383 (1991); C. Sagan and R. P. Turco, *A Path Where No Man Thought: Nuclear Winter and the End of the Arms Race* (Random House, New York, 1990).
2. C. Sagan and R. P. Turco, *Los Angeles Times*, 30 January 1991, p. B7.
3. R. B. Stothers and M. B. Rampino, *Science* **221**, 411 (1983); M. R. Rampino, S. Self, R. B. Stothers, *Annu. Rev. Earth Planet. Sci.* **16**, 73 (1988).
4. A. Burdick, *The Sciences* **31** (No. 5), 5 (September/October 1991).
5. W. R. Cotton, *Am. Sci.* **73**, 275 (1985).

Fertility, Health, and Consanguineous Marriages

One conclusion of A. H. Bittles *et al.* (Articles, 10 May, p. 789) is that in the populations they examine "the high gross and net fertility of consanguineous couples . . . would make rapid elimination of deleterious recessives improbable. . . ." Although this argument appears intuitively to be correct (probably because the frequency of recessive *genotypes* is higher in the progeny of consanguineous couples than of unrelated couples), in fact, higher fertility of consanguineous couples results in a faster elimination of deleterious alleles than in the case of equal fertility. Haldane (*1*) showed that the equilibrium frequency of a detrimental recessive allele is lowered by increased inbreeding. This occurs because inbreeding exposes more detrimental alleles to selection by increasing the frequency of recessive homozygotes.

That higher fertility of consanguineous couples lowers the frequency of detrimental alleles can be simply demonstrated if one assumes consanguinity occurs with a frequency I and that these couples have a relative fertility of $1 + X$ (assuming that random mating occurs with a frequency of $1 - I$ and that these unrelated couples have a relative fertility of 1). Therefore, the contribution to the progeny generation from the consanguineous couples is

$$I' = I(1 + X)/(1 + IX)$$

If $X > 0$, then $I' > I$, or the effect of higher fertility of consanguineous couples is really to increase the proportion of progeny from these matings. This actually enhances the ability of inbreeding to decrease the frequency of detrimental alleles just as if the inbreeding coefficient were increased. As an example, if $I = 0.335$, as in the Hindu group of Bittles *et al.*, and $X = 0.2$, then $I' = 0.376$.

Some South Indian communities may have had high rates of consanguineous matings for more than two millenia (*2*). If inbreeding continues over several generations then it is preferable to use the value of the inbreeding coefficient at equilibrium (f_e). For the Hindu group of Bittles *et al.*, (f_e) = 0.0476. Therefore, with a fertility effect of $X = 0.2$,

$$f_e I'/I = (0.0476)\ (0.376)/(0.335) = 0.0534$$

which is a 12.2% increase in inbreeding. This in turn leads to a similar percentage reduction, not an increase, in the equilibrium allele frequency.

PHILIP HEDRICK
Department of Biology,
Pennsylvania State University,
University Park, PA 16802

REFERENCES

1. J. B. S. Haldane, *Ann. Eugen.* **10**, 417 (1940).
2. L. D. Sanghvi, *Eugen. Q.* **13**, 291 (1966).
3. P. W. Hedrick, *Am. J. Hum. Genet.* **38**, 965 (1986).

Response: P. Hedrick is correct that, from a theoretical viewpoint, the greater fertility of consanguineous couples should result in increased numbers of recessive homozygotes, with enhanced opportunity for selection against detrimental alleles. However, one of the lessons of the South Indian study (*1*) has been that correlating theoretical concepts with empirical data drawn from human populations is difficult, and this applies when one considers the elimination of lethal alleles from a gene pool.

The Hindu population of Karnataka is heterogeneous, comprising multiple ethnic, language, and caste groups which, even in the present generation, are strictly endogamous. Since the subpopulations exhibit widely variant effective population sizes and levels of preferential consanguinity, both random and nonrandom inbreeding effects can be expected to operate to concomitantly differing degrees. When one makes assumptions about patterns of reproductive preference and behavior in previous generations, one treads on thin ice, especially when dealing with time scales greater than two millennia (*2*). Although there are a few anecdotal reports indicating that marriages between cousins have some regional precedent

among the former ruling classes (*3*), the in-depth pedigree data necessary to validate presumed historical mating patterns has not been forthcoming and probably does not exist. Under such circumstances, calculations based on the premise of consistent fertility differentials through time in a homogeneous population at equilibrium have limited practical application.

Contrary to the prediction of a reduction in detrimental alleles as a result of inbreeding, the results of both neonatal screening (*4*) and diagnostic studies on infants and children in hospital (*5*) indicate that the current incidence of deleterious recessive alleles in the South Indian gene pool is comparable to those reported in outbred populations. Because we have no knowledge of primary gene incidences in the constituent subpopulations of the region, speculation regarding the intensity of elimination of detrimental alleles from the gene pool does not appear to be useful. More important, the divergence between theoretical expectation and observation in this population serves to emphasize the need for further detailed studies to improve our understanding of the overall consequences of human inbreeding.

ALAN H. BITTLES
Biomedical Sciences Division, King's College, University of London, London WC2R 2LS, United Kingdom

WILLIAM M. MASON
Department of Sociology, University of California, Los Angeles, CA 90024–1551

REFERENCES

1. A. H. Bittles *et al.*, *Science* **252**, 789 (1991).
2. L. D. Sanghvi, *Eugen. Quart.* **13**, 291 (1966).
3. K. M. Kapadia, *Marriage and Family in India* (Oxford Univ. Press, Oxford, ed. 2, 1958), p. 117.
4. N. Appaji Rao *et al.*, *Clin. Genet.* **34**, 60 (1988).
5. A. Radha Rama Devi *et al.*, *J. Med. Genet.* **24**, 362 (1987).

Erratum: In Albert Moyer's review of *Physics and the Rise of Scientific Research in Canada* (11 Oct., p. 314), two lines were inadvertently omitted from the end of the next-to-last paragraph. The sentence affected should have read, "Key players included Ernest Rutherford . . . and John C. McLennan, who helped build the flagship physics program at the University of Toronto."

Erratum: In the report "Oscillations of cytosolic sodium during calcium oscillations in exocrine acinar cells" by Monica M. Y. Wong and J. Kevin Foskett (15 Nov., p. 1014), the affiliation of the authors was inadvertently omitted. They are in the Division of Cell Biology at the Research Institute, Hospital for Sick Children, 555 University Avenue, Toronto, Ontario, Canada M5G 1X8. Correspondence should be sent to Dr. Foskett.

Erratum: In the Erratum of 1 November (p. 631) correcting figure 2 of the report "Regulation of transendothelial neutrophil migration by endogenous interleukin-8" by A. R. Huber *et al.* (4 Oct., p. 99), the *y*-axis was incorrectly labeled. It should have read, "IL-8 (nM)."

Erratum: The radar image of Venus reproduced on page 803 of the 8 November issue was improperly oriented. The page should be rotated one-quarter turn clockwise for the proper orientation.