As a long-time watcher of the Templeton Foundation, I have found myself continuously bewildered by the proliferation of programs, initiatives, awards, symposia, and so on proffered by this group and by the frequent about-faces they conduct: just as I conclude that they are one thing, I encounter a press release that suggests that they are in fact something else altogether. Holden's article is therefore a tour de force precisely because she captures this pleomorphism and offers the scientific community the opportunity to experience bewilderment at such a force within our midst. Whether that bewilderment is followed by outrage or something more akin to bemusement or approval, it is important that we all take in the fact that a billionaire can attempt to wield this much influence. With many more billionaires out there, it is a cautionary tale.

Ursula W. Goodenough
Department of Biology, Washington University, St. Louis MO 63130, USA. E-mail: ursula@biodec.wustl.edu

The Systematics of *Homo*

Bernard Wood and Mark Collard (Review Article, 2 Apr., p. 65) propose taking the habiline species (*1*) out of the genus *Homo* and placing them in a broadly defined australopithecine group. There are good reasons to do this. I made the same taxonomic assessment in the second edition of *Paleoanthropology* (*2*), where the chapter dealing with the habilines is entitled "Homo-like Australopithecines." Moving the habilines to *Australopithecus* adds to the uncertainty and confusion of australopithecine phylogenetics, a group marked by an undue amount of parallel evolution (*3*). At the same time, it might be expected to clarify and simplify the systematics of *Homo*. But instead, the authors present a bewildering array of *Homo* species. This is not the majority interpretation of Pleistocene human evolution. Many paleoanthropologists continue to accept the traditional view of a geographically dispersed polytypic species, *Homo erectus*, evolving into a geographically dispersed polytypic species, *Homo sapiens*. Others take a phylogenetic approach, defining the unbroken human lineage as a single evolutionary species. Taxonomically, this means that there is but one species of *Homo*, *Homo sapiens* (*4*), which is the only interpretation that accounts for both species-wide evolutionary trends and the persistence of different regional features in what would otherwise have to be arbitrarily defined successive species. A network of genic exchanges, perhaps promoted by exogamy rules, provides a framework in which to promote geographic differentiation by isolation-by-distance, differentiation that also reflects adaptive variation and is tempered by historic differences, while advantageous features may spread throughout the species range (*5*). This multiregional interpretation is increasingly well supported by analyses of some nuclear DNA sequences, which reveal coalescence time estimates with ranges of uncertainty that extend to the beginning of the Pleistocene, if not earlier (*6*, *7*), and provide evidence of population subdivision that greatly precedes any skeletal or archaeological evidence of modernity (*7*).

Milford H. Wolpoff
Department of Anthropology, University of Michigan, Ann Arbor, MI 48109–1382, USA. E-mail: wolpoff@umich.edu

References and Notes

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Response

Wolpoff's substantive criticism of our paper is that we recognized far more species of *Homo* than are compatible with the "majority interpretation of Pleistocene human evolution." In response, we should like to point out that we never actually claimed that our interpretation is the majority one. Rather, we made it clear (in the second column of page 65) that there are two schools of thought regarding the number of species of *Homo*, and that we were deliberately opting for the more speciose of the taxonomies favored by these schools. We suggested that there were theoretical and practical reasons for recognizing multiple *Homo* species, and cited a paper by Tattersall in which those reasons are explained. In short, Wolpoff may disagree with our taxonomy and reject our reasons for choosing it, but he cannot say that we presented a misleading account of current views on specific diversity in *Homo*.

Bernard Wood
Department of Anthropology, George Washington University, 2110 G Street, NW, Washington, DC 20052, USA. E-mail: bwood@gwis2. circ.gwu.edu
Mark Collard
Department of Anthropology, University College London, Gower Street, London WC1E 6BT, UK. E-mail: m.collard@ucl.ac.uk

Wound Healing

The excellent work of Vishwanath R. Iyer and his colleagues, described in the report "The transcriptional program and the response of human fibroblasts to serum" (1 Jan., p. 83), will surely lead to important advances in wound healing. However, some of the comparisons to wound healing that are made by the authors and in the accompanying News of the Week article (E. Pennisi, 1 Jan., p. 17) do not appear to be fully warranted.

Reexposing starved, pure cultures of fibroblasts to dilute serum is only superficially similar to wound healing, where fibroblasts (i) are not alone, (ii) are not serum starved, and (iii) are exposed to an environment which, although based in serum, is highly modified.

The implication should not be given that fibroblasts are commonly thought to be passive responders in wound healing. We know that fibroblasts participate actively in wound healing. We know that they condition the environment with a variety of substances ranging from lactate to growth factors. However, fibroblasts are not prime movers, either. In wound healing, the temporal relationship is injury, followed by inflammation, followed by fibroplasia and angiogenesis. Without inflammation, fibroplasia is severely limited. In terms of spatial relationships, macrophages lead fibroblasts and endothelial cells into the blood or fibrin clot or the residual connective tissue matrix. It is well understood that fibroblasts replicate much of what macrophages and lymphocytes do, but to a lesser degree.

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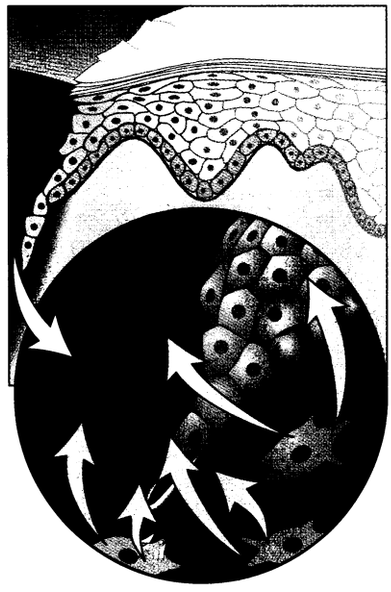

"Reexposing starved, pure cultures of fibroblasts to dilute serum is only superficially similar to wound healing."

Thomas K. Hunt
Department of Surgery, School of Medicine, University of California, San Francisco, San Francisco, CA 94143–0522, USA
John Burke
Emeritus, Department of Surgery, Harvard University Medical School, Boston, MA 02115, USA
Adrian Barbul
Department of Surgery, Johns Hopkins Medical School, and Department of Surgery, Sinai Hospital of Baltimore, Baltimore, MD 21218, USA
Michael L. Gimbel
Department of Surgery, School of Medicine, University of Pittsburgh, Pittsburgh, PA 15213, USA, and Department of Surgery, School of Medicine, University of California, San Francisco, San Francisco, CA 94143–0522, USA

Critical Volume Fraction: Second Model

With respect to the report I co-authored with S. F. Ackley and V. I. Lytle, "The percolation phase transition in sea ice" (18 Dec. 1998, p. 2238), I would like to thank Jay Janzen for making me aware of his work on the critical volume fraction ϕ_c for conduction in a compressed powder of large polymer particles and much smaller metal particles. Had I been aware of two of his papers (*1, 2*) (which were