(*Science*'s Compass, 20 Nov., p. 1420) about the use of Cre-loxP mouse technology for biomedical research reveals many analogies with plant biotechnology. Proprietary research tools such as promoters and transformation systems have found popular use among the global plant biotechnology community in the last decade or more, and many research projects that used these tools are now in a position for commercial exploitation. Academic institutions such as Michigan State University are now exploring the options open to us and approaching the patent holders of some of these technologies to determine how we can proceed to commercialization. To our surprise and dismay, the initial response has been very different from the Cre-loxP agreement described by Block and Curran and has resulted in a scenario where transgenic plants developed with obvious commercial value are effectively vetoed by the patent holders of these "upstream" technologies.

This is an undesirable situation for agricultural biotechnology (in particular, of transgenic plants), in that the holders of these proprietary "upstream" technologies have effective veto power over whom universities can and cannot approach with their technologies for commercial development.

By inhibiting fair competition and innovation, the development of this sector may well be stifled by a select number of companies holding key basic research tool patents. So while Block and Curran present a favorable picture for the mouse in the laboratory, the situation for maize in the field looks very different to us at present.

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Replacing Ancient Forests

Anne Simon Moffat's article "Temperate forests gain ground" (News Focus, 13 Nov., p. 1253) might more accurately have been titled "Industrial forests gain, ancient forests and biodiversity continue to lose." Conservationists welcome reforestation in North America, but the working forests of industry or the mongrel successional forests of the suburbs and abandoned farms are not everywhere a fair trade for our native old-growth forests. The continued ecological losses that attend the destruction of bottom-land hardwood forests of the Southeast (*1*), the native oak woodlands of California (*2*), or the ancient temperate rain forests of the Pacific Northwest (*3*) are hardly rectified by the proliferation of genetically altered loblolly pine, exotic eucalyptus, or plantation Douglas-fir. The silvacultural trends described by Wernick *et al.* (*4*) are welcome not only if they can provide timber and fiber or sequester carbon but also if they can help stop the bleeding in our final few ancient forests.

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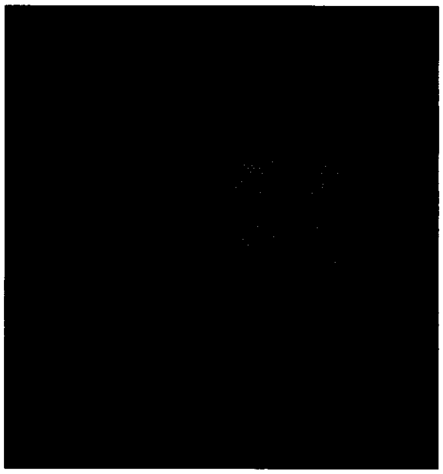
Hominid Brain Volume

Having calculated the brain volumes of several australopithecine and early Homo fossil hominid brain endocasts (*1–3*), I read with considerable interest the report by Glen C. Conroy *et al.* (12 June, p. 1730) and the commentary by Dean Falk (Perspectives, *Science*'s Compass, 12 June, p. 1714). Reexamination of these older specimens by other scientists is a welcome enterprise and, needless to say, I hope that my early attempts will be validated. However, it is important to note that my earlier volume estimations were, in fact, significantly smaller than those previously published. The Sts 71 specimen, for which I obtained a value of 428 cubic centimeters (cm^3), had been estimated as somewhere between 480 and 520 cm^3. My estimate of the Taung child was 404 cm^3 (*4*), a drop from Raymond Dart's earlier value of 525 to 562 cm^3.

The following facts should be noted by readers. First, Conroy *et al.*'s citation of my 1983 article (*5*) is rather late. The original volumes were published in 1970 (*1*), again in 1972 (*2*), with specific discussion of Sts 71, and again in 1973 (*3*). Second, as I pointed out in the 1972 article in particular (*2*), the Sts 71 cranium was distorted in the occipital region, and the volume I determined was based on correcting the original endocast. I also graded my attempts according to methods used and found Sts 71 to have the lowest rating (C2-3). Neither Conroy *et al.* nor Falk mentions the plastic deformation that causes the planum occipitale to be at right angles to the endoclast, where the mastoid process is practically at the same plane as the occipital planum, a condition I have seen only on this cranium. Third, pouring one-half of 370 cm^3 of water into a cast of Sts 71 without correcting for the distortions and shrinkage is, mildly put, without scientific rigor. In 1970 (*4*), I wrote, "The standard deviation and coefficient of variation I calculated for the gracile forms are possibly too low, and can be attributed to the small sample size and bias created by using certain gracile values and dimensions to reconstruct less complete specimens." Fourth, those who have access to the casts of Sts 5 and Sts 71 will find that the facial measurements (undistorted) of the two crania are nearly identical, while Sts 5 has a cranial volume of 480 cm^3; I know of no evidence disputing that figure. It seems highly unlikely that its cranial volume will be some 110 cm^3 more than that for Sts 71.



Cranium of Stw 505, showing "virtual endocast"

I look forward to the use of better technology to pursue these difficult reconstructions, but hope that the attempts to do so will be truly scientific.

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Response

Holloway has made many important contributions to paleoneurology, and we are therefore pleased that his comment finds our work to be of "considerable interest." He correctly reminds readers that he was one of the first to realize that many of the early endocranial estimates were overestimates, a situation he corrected in a series of important studies, many of which he cites in his comment. Because Holloway reserves his more specific comments for Sts 71, a specimen not particularly germane to the main focus of our report,

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