icy of supporting legislation which will be helpful in its overall effect. However, we would rather see no action than compromise action that would open the way to censorship of science. If damaging amendments were to be added to H.R. 5191, NSMR would oppose its passage, because *human* welfare is our first concern.

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Homo habilis

All anthropologists will be grateful to Tobias for his lucid article, "Early man in East Africa" (2 July, p. 22). A great deal more study will be required, however, before it will be possible to arrive at any agreement on the probable status and affinities of *Homo habilis*. Tobias believes that *H. habilis* stands in a position intermediate between the australopithecines and the pithecanthropines. It is a reasonable conclusion. But to judge from the available data, it would be equally reasonable to conclude that *H. habilis* was, in fact, an early pithecanthropine. There is nothing in the published data that would not conform to the requirements of the latter hypothesis. Applying Occam's razor, *H. habilis* could perhaps more appropriately be regarded as an early representative of *Homo erectus*. Such a ligature can allow for the slight morphological differences that exist between *H. habilis* and *H. erectus* and for the recognition of any other differences that may exist between them, without separating them into distinct species. These are matters that can only be resolved by further study.

Tobias writes, "Since they are contemporary with *H. habilis*, the australopithecine populations represented by the actual fossils recovered to date are clearly too late—and possibly slightly too specialized—to have been on the actual human line . . ."

Tobias suggests specialized large teeth. But large teeth represent a persisting ancestral trait, not a late specialization. In *A. boisei*, the teeth were in process of undergoing reduction. The anterior teeth are small, while the molar-premolar series are large.

Tobias' statement that the fossil australopithecines "are clearly too late . . . to have been on the actual human line" is, as it were, putting the chart before the horse. That some australopithecines were contemporaries of some habilines does not necessarily imply that the former could not be ancestral to the latter. Tobias' statement has no more validity than would the statement that a grandparent could not be a contemporary of his grandchildren—or put more generally, that ancestors and descendants cannot be contemporaries. Or put in still another way, that the descendants preserving an ancestral morphology cannot be the contemporaries of descendants of that ancestral type presenting a somewhat different morphology. The coelocanth constitutes an outstanding example to the contrary, and the coexistence of Przwalski's horse and the modern horse constitutes yet another.

It would be difficult at the present stage of our knowledge to designate any of the known australopithecines as ancestral to later hominines, but there is nothing in the morphology of any one of them that would preclude their standing in the direct line, as ancestors, of such later hominines.

One last point: An article so well illustrated that does not include a photograph of the skull of *H. habilis* is akin to a production of *Hamlet* without Hamlet.

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The suggestion that *Homo habilis* be classified under *H. erectus*, proposed as well by D. R. Hughes of Cambridge (*The Times*, London, 10 June 1964), goes further than I believe the available evidence permits. Between the two extremes of this view and the opposite one, that we should call the hominid *Australopithecus habilis*, the interim solution of a lowly species of *Homo* seems a reasonable compromise. Only the discovery of more specimens and refined statistical comparisons can resolve these slightly diverging viewpoints.

Montagu accepts that large teeth represent a persisting ancestral trait. I believe a better case can be made that enlargement of the cheek-teeth was a secondary specialization. The fact that *A. boisei* had enlarged cheek-teeth proves nothing, because we do not know for sure if he was older than the smaller-toothed australopithecines of Taung and Sterkfontein Lower Breccia. It would seem that moderate-toothed *H. habilis*, large-toothed *A. africanus*, and massive-toothed *A. boisei* were roughly contemporary: which was ancestral to which? When we look back to the Mio-Pliocene hominoids, we find support for the idea that the modest dentition of *A. africanus*, with front and back teeth in harmony, was closer to the possible ancestral dentition—if Simons' view on the facio-dental affinities between *Ramapithecus* and *Australopithecus* is correct [*Proc. Nat. Acad. Sci. U.S.* **51**, 528 (1964)]. On these and other grounds, enlargement of the cheek-teeth in some australopithecines is a departure and a specialization.

My point that the fossil australopithecines were too late to be ancestral related specifically to the Lower Pleistocene *populations* of australopithecines, not (as Montagu seems to imply) to the taxon *Australopithecus*. All evidence certainly points to *Australopithecus* as an ancestral taxon. I was concerned specifically with the populations represented by the known fossils. Previously, it could be averred that the Lower Pleistocene populations of *A. africanus* moved forward by phyletic evolution to become the Middle Pleistocene populations of *H. erectus*. Now that we have found a hominine in the Lower Pleistocene, we must infer that earlier populations than those represented by the known fossils moved forward phyletically to become *H. habilis*—unless we hold to a polyphyletic evolution of *Homo* at several time-levels. These earlier populations must have dated from a period earlier than the Bed-I habilines—that is, from the first half of the Lower Pleistocene or even from the Pliocene.

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Teaching by Research Fellows

Having read John Walsh's report on the effects of federally supported research on higher education (News and Comment, 2 July, p. 42), I would like to offer a suggestion. The government, perhaps in collaboration with the universities and colleges, should offer, to qualified individuals, teaching-postdoctoral fellowships of 3 to 5 years' duration that would require the recipient to devote a part of his time to teaching. (Alternatively, the present fellowship and grants programs could be modified to provide an inducement for the recipient to teach.) In order to enhance the prestige of these fellowships, and to compete with the regular postdoctoral fellowships, their monetary rewards must be greater than those of the research fellowships (which *permit* the holders to devote up to 10 percent of their time to teaching). The awards should be flexible enough to permit the recipient either to do research entirely on his own (he may need funds to buy equipment) or to do research with a more established investigator. The teaching requirements would be subject to negotiation between the individual and the institution to which he is going and should not require him to spend more than 10 percent of his time teaching. The kind of teaching I have in mind is giving lectures on agreed-upon subjects, helping to prepare and grade examinations, helping to organize and oversee laboratory exercises, and acting as an adviser to students. With a little foresight and initiative on the part of the universities and colleges, the teaching load of the staff and graduate students (who are sometimes unfairly overburdened) could be reduced. It might be argued that the threat of government control over education makes this suggestion unworkable; however, it is my opinion that the present control government is exerting over education (by supporting research exclusive of teaching) is a much more real threat.

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Sap Pressure in Plants

In their article "Sap pressure in vascular plants" (16 Apr., p. 339), Scholander, Hammel, Bradstreet, and Hemmingsen present some interesting data on the water relations of plants. The experimental procedure that they employed does not, however, demonstrate the existence of negative sap pressures as they stated. This is not to disclaim the existence of negative pressures, but rather to point out that their procedure measures only the difference in free energy per unit volume between water in the plant and the same water outside the plant. Their pressure chamber operates on the same principle as the pressure-membrane apparatus developed by Richards [*Agr. Eng.* **28**, 451 (1947)] for measurement of the potential energy of water in soils. When air pressure is applied to the sample chamber, the free energy of the water is raised. If this pressure increase is carried out isothermally, the free energy of the water would be raised by approximately $V\Delta P$, where V is the volume of water in the sample and ΔP is the pressure increase necessary to establish equilibrium between water in the system and that outside. It is common practice to express this energy difference in terms of energy per unit volume (the water potential), which, of course, is dimensionally the same as pressure. In the experiment of Scholander *et al.*, the plant itself provides the membrane which is permeable to water but not to air.

Richards emphasized that the pressure-membrane apparatus measures an "equivalent" or "apparent" negative pressure within the soil. Nevertheless, many soil scientists have assumed that the pressure necessary to force water out of the soil gave a measure of the actual pressure of the water in the soil. It now seems apparent that the adsorptive force field around soil particles acts throughout a sufficiently long range to account for a significant proportion of the potential-energy lowering in soils. Thus, the pressure membrane measures the free-energy difference but not necessarily the pressure difference. If sufficiently long-range adsorption forces do exist in plants, as seems entirely reasonable, particularly within the cell wall, the same would hold true for the experiments of Scholander *et al.* One could argue that the data presented prove the existence of adsorption forces just as readily as the existence of negative pressures. This is a challenging problem, and it is to be hoped that a means can be found to distinguish between the various components of the potential energy in plants in a conclusive way.

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Gardner and Rawlins argue that our measurements do not demonstrate negative sap pressure in plants, let alone measure it. This criticism appears to stem from a misplaced analogue between their soil and our plant experiments. It is true that the balancing bomb pressure does not differentiate between hydrostatic and osmotic forces