with an exposed position. The relative absence of decay in old trees in locations such as the White Mountains rests primarily on these climatic features of the local environment, together with low night and winter temperatures, long distances from spore sources for decay fungi, a high resin content of exposed wood, and the presence in the wood of fungistatic phenolic substances such as pinosylvin (*5*).

A tree may be reproduced vegetatively through numerous generations, and therefore one may deduce that it is capable of indefinite life, yet all field evidence points to the conclusion that any individual tree, as such, ages as does any other complex organism, with death as the final outcome. Foresters recognize this in the formulation of tree classifications (*6*). For most conifers, the end comes through insect attack or other visitation after physiological decline has become marked. For *Sequoia gigantea,* which is unusually free of insect enemies, it hardly seems necessary to suggest, as Schulman has done, the possibility that all then living specimens were wiped out by some catastrophe 3000 to 4000 yr ago. The end for these forest giants comes when reduction in root systems through deterioration reaches a point at which the tremendous bulk of trunk and top can no longer be mechanically supported, and they fall. This accounts for the lack of standing sequoia snags on which Schulman has remarked.

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Received April 19, 1954.

I am grateful that my report of some precise dating work on newly discovered Methuselahs among stunted conifers has called forth a professionally competent review of possible factors favoring their existence. The rich category of old *little* trees, which, in contrast to the category of old *big* trees, appears to have been quite neglected until recent years, may well owe its existence, as Wagener implies, to a set of factors largely different from those that favor lush growth.

Although the reasons for the occurrence of old drouth conifers summarized by Wagener are surely appropriate, my own field experience strongly suggests that special factors play a major role in the cases of those relatively few examples of absolute maximum longevity, to which my report in *Science* was necessarily limited. It was repeatedly observed that, in a general and usually rugged area of high aridity in which occurred very old trees growing on adverse sites, only one or two small localities bore the individuals of maximum ages, and these ages were often quite markedly greater than those elsewhere in the area. That natural selection has operated at such sites is, perhaps, obvious. Local concentrations of soil antibiotics may well exist. And that the wood may contain special substances, as Wagener notes, is indeed likely; a more satisfactory statement in this regard may perhaps be made when chemical analysis has been completed of the stem of the 1650-yr Sun Valley limber pine (tree No. 3966, Table 1, of my article), which was felled in part for this purpose.

Wagener's preference for the second of the two possibilities suggested by the lack of standing giant sequoia snags seems well based.

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Received May 7, 1954.

## The Piltdown Nasal Turbinate and Bone Implement: Some Questions

Now that the Piltdown mandible has been established as that of an anthropoid ape (*1*) and the larger cranial bones shown to have originated from three different sources (*2, 3*), some questions and doubts seem to be in order concerning the remaining Piltdown skull bones and artifacts. When I examined the original Piltdown bones in 1951 (*4*), I was astonished to find that, among the human bones recovered at Piltdown, substantial portions of a turbinate were represented. Dawson (*5,* p. 85) described the discovery of the turbinate as follows:

> While our labourer was digging the disturbed gravel within 2 or 3 feet from the spot where the mandible was found, I saw two human nasal bones lying together with the remains of a turbinated bone beneath them *in situ.* The turbinal, however, was in such bad condition that it fell apart on being touched, and had to be recovered in fragments by the sieve; but it has been pieced together satisfactorily by Mrs. Smith Woodward.

Woodward in the same paper (*5,* p. 87) observed:

> The remains of a turbinal found beneath the nasal bones are too much crushed and too fragmentary for description; but it may be noted that the spongy bone is unusually thick, and has split longitudinally into a series of long and narrow strips.

I have not studied this turbinate, so that I can say nothing useful concerning its anatomical characteristics, but what strikes me as most remarkable about this bone is its very existence. I do not recall any instance in the annals of paleoanthropology of this extremely fragile bone ever having been recovered in a fossil hominid. Indeed, the delicacy of the turbinates is such that these bones are among the first to crumble even in comparatively recent burials. In view of the doubt that at present surrounds the whole Piltdown find, it seems necessary to explain the presence of the turbinate bone.

Is it possible that the turbinate does not in fact

naturally belong with the other skull bones? If it does not, how does it come to be together with the other Piltdown bones? If it does naturally belong with the Piltdown bones, then it may be regarded as casting considerable doubt upon the antiquity of the Piltdown skull, or else as representing a unique example of the preservation of this delicate bone in a fossil hominid. The third alternative is that the turbinate belonged to the chimpanzee owner of the mandible.

Marston has several times drawn attention to the probable significance of the turbinate. In his Swanscombe report, Marston (*6*) pointed out that if the turbinate belonged to the Piltdown skull, then the horizon of Piltdown is to be judged from the turbinate bone. Later, he wrote (*7*, p. 275)

> . . . since the frail turbinate bone and nasal bones belonging to the Piltdown skull had been preserved and recovered, their preservation is incompatible with the period represented by the fauna which includes the broken teeth of Mastodon, Stegodon, Rhinoceros, Hippopotamus, nor with the so-called associated flint implements which are [sic] battered and abraded during the distribution of the Piltdown gravels. The Piltdown remains therefore belong to a later period from the deposits in which they were found.

Referring to Dawson's account of the finding of the turbinate, Marston (*8*, pp. 293–294) writes:

> Now the turbinal bones, because of their frailty, are the first bones of the skull to disintegrate even in protective burials. Mr. Dawson has here stated clearly that it and the nasal bones were found when digging the disturbed gravel. This supports the view that the turbinal and nasal bones fell away from the skull when the skull was broken up by the workmen at least six years before. The pit was a shallow excavation by the roadside not more than five feet in depth. These bones, the turbinal and nasal, fell to the floor of the pit. The great spread at Piltdown has generally been considered to have been deposited by running water, *i.e.*, to have been fluviatile, but solifluxion phenomena cannot be excluded from any of the four strata of which they consist. Solifluxion meaning the flow of semi-solid material in thawing out from frozen conditions. Now it is obvious that if the nasal and turbinal bones had fallen from the skull at the period of the distribution of the gravel spread and had thus become naturally geologically separated from the skull, under fluviatile or river conditions they would have been carried down-stream, and under solifluxion conditions where the semi-frozen sludge is churned up and festooned, these frail bones would have been pulverized out of existence. Hence it would appear probable that the skull to which the nasal and turbinal bones belonged found its way into the Piltdown gravels after and not during the period of distribution of the gravel-spread by fluviatile action.

Marston's argument seems to me unanswerable.

There are several other points. Dawson stated that he "saw two human nasal bones lying together with the remains of a turbinated bone beneath them *in situ*." This is very curious. The nasal bones, unless they are united at the internasal suture, are likely to become separated from each other as soon as they are detached from the skull. If these bones were not so united how does it happen that they were found together? Even more curious is the finding of the turbinate beneath the nasal bones "*in situ*." Neither the ethmoidal nor the inferior turbinates are in any way attached to the nasal bones. They were found in *disturbed gravel*; how then is it possible that these bones came to lie together in the manner described? It is highly improbable that they came to lie together naturally, for reasons such as those already given by Marston. A more likely explanation is that the person who placed the mandible in the pit also put the nasal and turbinate bones together. This reveals some book knowledge of human anatomy but clearly that of a person who has never really studied the skull in detail. The nasal bones might have passed, but the turbinate in itself and in relation to the nasal bones was a mistake.

The "*in situ*" is more than strange, for Dawson distinctly says that the laborer was digging *disturbed* gravel when he, Dawson, saw the nasals and turbinate. How anything can be "*in situ*" in disturbed gravel is a puzzle. Perhaps Dawson meant the "*in situ*" in an anatomical sense to refer to the relation of the turbinates to the nasal bones—in which case he would again have **been in error.**

Finally, there is the matter of the "bone implement" found at Piltdown. We know that there were no flint implements associated with the Piltdown skull fragments. The "doubtful artifact" that was recovered from the same seam of gravel as the Piltdown bones has been shown by Oakley and Weiner (*9*) to have been a fake. In 1915, Dawson and Woodward reported the discovery of a "bone implement." This was made from part of the femur of a fossil elephant and, according to Dawson and Woodward (*10*, pp. 144, 147), the bone, although found

> about a foot below the surface in dark vegetable soil, beneath the hedge which bounds the gravel-pit, and within 3 or 4 feet of the spoil-heap whence we obtained the right parietal bone of the human skull, . . . originally occurred in the lowest layer of the Piltdown section.

Oakley (*11*) has pointed out that, while there can be no doubt that the terminal facets on this bone are human work,

> On the other hand, they do not bear close comparison with the scratchy cuts made by a flint knife or primitive chopper. It is possible that the bone was picked up in a fossilized condition and hacked with an even-edged chopper or heavy metal knife during late prehistoric or more recent times.

These points had already been made at the Geological Society meeting at which Dawson and Woodward presented their paper. Reginald Smith suggested (*10*, p. 148) that

> The possibility of the bone having been found and whittled in recent times must be considered; and, if it were not shaped in its fossil state, it had evidently never been used for any purpose such as grubbing for roots, as the cuts were unscratched, and must have been made with an even-edged chopper.

A. S. Kennard pointed out (*10*, p. 149) that "From the differences between the cut portion of the bone and the natural surface, he considered it possible that the bone was not in a fresh state when cut."

I think it highly probable that when this alleged "bone implement" is carefully studied it will be found that the terminal facets were produced by a sharp metal blade probably of the Sheffield steel variety; in short, that this "bone implement" is quite as much a fake as the mandible.

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Received April 19, 1954.

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## Security System (*cont. from page 7A*)

Then came the expression that seems so frankly astonishing as a criterion of security—the degree of enthusiasm:

> The board finds that if Dr. Oppenheimer had enthusiastically supported the thermonuclear program either before or after the determination of national policy, the H-bomb project would have been pursued with considerably more vigor, thus increasing the possibility of earlier success in this field.

The criterion of enthusiasm should be examined both by itself and in connection with the question of whether a standard that is appropriate for a lesser man should be applied to one of Dr. Oppenheimer's great talents and contributions. The board argued that except in time of critical national need the same standard must apply to all. But if identity of standard is to apply to the denial of clearance, it would seem logical to apply a similar doctrine to the granting of clearance. It is doubtful whether that was done. It was apparently not alone Dr. Oppenheimer's lack of enthusiasm for the thermonuclear program, or his opposition to that program at one stage of its discussion: it was his prominence, the fact that his opposition was widely known, his failure to publicize the fact that he was supporting a decision which had gone counter to his recommendation; it was these things that the majority members of the board concluded had delayed progress on the H-bomb. Would an unknown and uninfluential technician have been judged by the same standard? The wording of the report suggests not.

It is well that the criterion of enthusiasm was so clearly put. Its implications are too grave and frightening to have the basic issue clouded. It has been pointed out by a variety of writers that adherence to such a doctrine will dampen free discussion—not only in public but in secret councils. Who wants to risk such drastic punishment, years after a decision was made, for having honestly opposed the decision before it was made? Does the same fate now face the other members of the AEC advisory committee who agreed with Dr. Oppenheimer? In perhaps the most trenchant "editorial" on this point, Herbert Block, the brilliant cartoonist of the *Washington Post and Times-Herald* pictured an office labeled "U.S. Govt. Atomic Science." On the wall hung an admonitory plaque reading, ENTHUSE. In the wastebasket rested its discarded predecessor, THINK.

In two quite distinct senses the outcome of the review board's work has been contradictory and confusing. Dr. Oppenheimer has been found to be loyal and discreet, but two of the three board members voted against restoring his clearance. But Dr. Oppenheimer was not alone on trial. The case also constituted a trial of the security system itself. Like Dr. Oppenheimer, in one sense it too came out with unblemished reputation. A thoughtful board devoted weeks to the case; many witnesses came to the defense of a man whose character had been questioned; Dr. Oppenheimer was permitted to cross-examine adverse witnesses; a valuable analysis of some of the underlying and terribly perplexing problems of the relations between national security and individual freedom of action has become available; nor is that all, for there will be further review before a final decision is reached. In this democratic, judicial, fair procedure, the country can take great pride.

But the process does not go on in a vacuum. The process brings out some of the difficulties of the security regulations, some of the troublesome aspects of the attempt to judge who is a security risk, some of the tremendous cost to the nation that must lose the services of a uniquely qualified advisor in order to comply with regulations of unknown validity and perhaps temporary applicability. The majority report leaves the status of Dr. Oppenheimer in doubt. It also leaves doubts about the security regulations under which he was judged.

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*June 5, 1954.*