On the 5 to 6 meters of vertical relief created by movement on the Meers fault, Slemmons finds three distinct surfaces. One has a slope of about 25 degrees. Farther from the fault, there is a slope of 12 to 15 degrees, presumably created by greater erosion of a scarp formed by an older movement. An even gentler and thus even older slope can also be recognized, Slemmons says.

In the deserts of Nevada, where Robert Wallace of the USGS in Menlo Park first developed the technique, an initial 35-degree fault scarp slope would erode to 25 degrees in only a few thousand years. In southwest Oklahoma, however, there are 75 centimeters of precipitation per year versus Nevada's 16 centimeters per year. There are also soil differences, so that the calibration of slope steepness in Nevada cannot be used rigorously in Oklahoma. But Slemmons believes that the last movement on the Meers fault must have been "a matter of a few thousand years ago" to have left such a steep slope.

Earthquakes on the Meers fault seem to have occurred in the geologically recent past, but determining their magnitudes is more difficult than estimating their ages. Magnitude depends on the amount of fault movement during each earthquake. Five meters of vertical movement is evident at the fault scarp and both Slemmons and Gilbert detect signs of at least three earthquakes contributing to that movement. How much horizontal movement has occurred of the sort that the San Andreas experiences is more controversial. Gilbert sees about an equal amount of vertical and horizontal slippage whereas Slemmons favors 4 meters of horizontal movement for every meter of vertical movement.

If only three events contributed to the present height of the scarp and the motion was predominantly vertical, each event could have had a magnitude as high as 7, Slemmons says. Those would have been damaging though moderate earthquakes. If there was in addition considerable horizontal motion, the magnitudes might have been as high as 7.5, which is larger than all but one of the California earthquakes since the great San Francisco earthquake.

Unlike the situation in the American West, the cause of the activity on the Meers fault is unclear. It lies within the "stable" central region of North America thought to be old enough and far enough from a plate boundary such as the San Andreas to remain relatively quiet. But the region is still subject to certain strains. Measurements show that the central United States is being compressed in a roughly east-west direction, perhaps by the drag of the North American plate across the mantle. Whatever its origin, this compression could reactivate a fault oriented so that the present strain could drive movement on it. Such reactivation of an ancient rift seems to have produced the three great earthquakes near New Madrid, Missouri, in 1811–1812.

The Meers fault is an awkward new phenomenon for those searching for the sites of future damaging earthquakes. "The astonishing thing about it is that there seems to be essentially no seismicity" associated with the fault, says Stephan Brocoum of the Nuclear Regulatory Commission in Silver Spring, Maryland. Neither regional nor recently installed local seismograph networks have detected even microearthquakes directly on the fault, and historical records contain no events that can be confidently placed on it. Should regulators be more cautious than to depend solely on seismicity? The Meers fault could help provide some answers.—**RICHARD A. KERR**

The Taung Baby Reaches Sixty

Sixty years ago the first australopithecine fossil was found in South Africa; prejudiced against it, the establishment did not want to know

Innocent, unbiased observation is a myth.—SIR PETER MEDAWAR

When Sherwood Washburn said recently that "Molecular biology has settled the problems of human relations," meaning the shape and chronology of our family tree, he touched on a raw spot for most comparative anatomists.

It is the comparative anatomists, after-all, who are supposed to be able to infer phylogenetic relations—that is, genetic distance—from morphological difference. It is they who traditionally have enjoyed a virtual monopoly on reconstructing family trees. Molecular biologists—neophytes in this long established business—wield new and unproven techniques, it is held, which yield only an uncertain key to the past. For their part the molecular biologists view comparative anatomy as having proved to be wrong too often and the molecules as offering for the first time an unbiased measure of relationship. This is a bit of an exaggeration, of course, but not too far from the truth.

Washburn was speaking at a gathering of paleoanthropologists in various guises, called together to celebrate 60 years since the first discovery of an australopithecine, prehuman, fossil.* That fossil—the Taung child, which goes by the name *Australopithecus africanus*—received a distinctly negative reception when in February 1925 its discovery was announced, a mixture of indifference and scorn. Its discoverer, Raymond Dart, was considered to be, at best, misguided. Like the data of molecular biology today, said Washburn, the Taung child fell outside the mainstream of palaeoanthropology, which was bad enough, but also implied answers that fell outside established patterns of belief, which was worse. Such is the nature of science and of this science in particular.

The Taung Diamond Jubilee, which was organized by Phillip Tobias, successor to Dart in the Department of Anatomy at the University of The Witwatersrand, Johannesburg, was in part an opportunity to view the modern science of paleoanthropology through the lens of that early discovery and controversy.

At the time of the Taung fossil's announcement, theories of human origins flourished lustily, relatively unconstrained by actual fossil evidence. The Taung fossil was so firmly rejected by the establishment, simply because it did not fit with current theories. With the subsequent discovery of many hundreds of similar fossil hominids, the harnessing of scores of sophisticated techniques, and the increase in enthusiasm for the discipline as a science, particularly in the United States, the human element in

*Taung Diamond Jubilee International Symposium, 27 January to 4 February, University of The Witwatersrand, Johannesburg, and University of Bophuthatswana, Mmabatho.

paleoanthropology, the element of uncritical subjectivity, has been greatly decreased. Decreased, but not eliminated. Perhaps it never can be.

Dart, an Australian, studied neurology with Grafton Elliot Smith in London and it was only with great reluctance that he accepted the chair of anatomy in Johannesburg in 1922. Not long after his arrival he bestowed a letter of introduction on a young man, Solly Zuckerman, who sought to leave that continent and establish himself in the "real" world of medicine and science, that is, with Elliot Smith in London. Ironically, Zuckerman, who was later ennobled first with a knighthood then a peerage, became during the 1950's and onward one of the most severe and outspoken critics of Dart's (and others') claims for the australopithecines.

When, late in 1924, the Taung fossil came into his hands, Dart, the neurologist, recognized it as something other than an ape because, although its brain was not particularly enlarged, the shape, partly preserved as a natural endocast, displayed some human features. The teeth and facial elements were not those of an ape, he thought, and the position of the foramen magnum, the aperture through which the spinal cord leaves the braincase, implied that the creature had walked on two legs, not four.

Dart published these observations, which were embellished with some colorful inferences about the creature's probable behavior and concluded that *Australopithecus africanus* was a humanized type of ape. He did not go so far as to say it belonged in the same family as us, the Hominidae. He suggested a new subfamily, something in between apes and humans, the Homo-simiadae. Dart's announcement, coincidentally, was made the same year in which John Scopes was convicted in Tennessee of the crime of teaching that humans had evolved from lower creatures.

Dart's cocktail of caution and flamboyance in his announcement provoked among the European, particularly the British, establishment the near universal conclusion that Dart's child was nothing but an ape, probably something like a chimpanzee. As William Howells of Harvard pointed out at the recent meeting, apart from Ales Hrdlicka, of the Smithsonian Institution, who echoed the British chorus, the U.S. establishment largely ignored Dart because paleoanthropology was not then a well-focused discipline in the country.

There were, of course, good reasons to be cautious about Dart's claims. The evidence was just a single fossil, and a

Tobias displays the Taung fossil.

juvenile at that. Infant anthropoids tend to be physically rather similar to each other and it is not easy to predict what the mature individual will look like. None would choose to erect a new species on the basis of an immature specimen, because of these hazards. It has happened subsequently, of course, when Louis Leakey, Phillip Tobias, and John Napier named *Homo habilis* in 1964, for which pains they paid the price of an almighty controversy, the reverberations of which continue to this day.

But the Taung child's immaturity was not the real reason its discoverer was heaped with contumely, however: the fossil was, by the lights of current theory, in the wrong place, at the wrong time and was of the wrong form.

In 1925 Asia, not Africa, was known to be the cradle of mankind. Although Charles Darwin, in his *Descent of Man*, had opined that the African continent was likely to have been man's first home, his ideas had by the turn of the century fallen into some disfavor. In any case, Eugène Dubois had in the 1890's

Australopithecus africanus *reconstruction in the Transvaal Museum.*

found potential, if somewhat controversial, human ancestors in Java. The American Museum's Henry Fairfield Osborn was a great champion of an Asian birthplace, and he promoted grand expeditions to the high plateaus of that continent, which he considered appropriate for so noble an origin.

Even though Osborn's expedition leader returned only with dinosaur eggs, the idea of an Asian origin would not be shaken. Hence the flat dismissal of Dart's suggestion that, instead, the Dark Continent was home to our first ancestors. And hence the ready acceptance into the human fold of the Peking man fossils, which, when first discovered shortly after the Taung announcement, were pretty scrappy: but they fit the established pattern of belief.

As for Taung's geological youth, being Pleistocene rather than Oligocene, this fact seemed to rule out of contention this little ape-like creature. As Washburn pointed out, the idea of an ancient origin of humans derived in part at least from inadequacies in the geological time scale. At the turn of the century, the entire age of the earth was considered to be just 65 million years, with the mammals squeezed into the last 3 million years. With some evidence of human ancestors of apparent antiquity to hand, and set against this compressed time scale, humans were inevitably considered to have diverged from the primate stock early in mammalian history.

Nevertheless, even when a better understanding of radioactivity showed the earth to be billions, not millions of years old, many still stuck to the notion of a distant origin. Given this picture, Taung was simply too late in geological time to be a human ancestor.

Even if the Taung child had emerged from suitable Oligocene deposits, it still would not have qualified as an early hominid, because its form was wrong. Everyone knew that the human ancestor had an enlarged brain and an ape-like face and body, an idea, incidentally, that came principally from Dart's mentor, Elliot Smith. Taung had a puny brain, a human-like face and an upright body, which was not in accord with that famous jewel of a fossil for the British establishment, *Eoanthropus dawsoni*. This latter is better known as Piltdown man, which was later revealed to be the fraudulent chimera of a modern human cranium and an orangutan jaw.

Further discoveries of australopithecine fossils in both South and East Africa over the next three decades or so eventually brought most of the paleoanthropological community to Dart's view. And

the unmasking of the Piltdown fraud in 1953 served to remove what increasingly had been an awkward piece of evidence and underlined the embarrassing fact that, as Washburn put it, "the most highly regarded scientists had been unable to tell a modern jaw from a fossil for over 30 years."

Zuckerman made much of this latter fact, and from the 1950's onward he and his students strove to show that, as always, paleoanthropologists see in the fossils only what they want to see. Proper examination with metrical analysis fails to classify the australopithecines as especially human, concluded Zuckerman, a view he still holds and is championed too by his most prominent student, Charles Oxnard of the University of Southern California.

Zuckerman once said, "It is something of a record for an active team of research workers whose strength has seldom been below four, never to have produced an acceptable finding in some 15 years of assiduous study." Which observation must mean either that the entire profession of paleoanthropology is wearing blinders, focusing only on what they wish to believe; or that Zuckerman and his co-workers are wrong.

Not that Washburn is any great champion of conventional comparative anatomy. "It is regarded by some as a major science," he says, and yet continuing controversies through the years—excluding Zuckerman—"shows that well trained anatomists could reach diametrically opposed conclusions."

These controversies have included the supposed hominid status of the Miocene fossil *Ramapithecus*; the question of whether *Homo habilis* is indeed a true species, and if so, which specimens should be assigned to it; whether the fossils from the Hadar, Ethiopia, which Don Johanson and Tim White have named as *Australopithecus afarensis*, are ancestral to all other hominids; and whether *Australopithecus afarensis* is in fact merely a geographical variant of *Australopithecus africanus*. Serious practioners hold very different opinions.

Washburn has, of course, long been a proponent of the efficacy of molecular biology in addressing issues of paleoanthropology, and was instrumental in encouraging Vincent Sarich to pursue this line of investigation at Berkeley, in company with Allan Wilson. The problem they addressed was the branching order and times of the great apes and humans, and in 1967 they came up with an answer that was as acceptable to paleoanthropologists then as the Taung skull had been to the adherents of Piltdown. In contrast with paleoanthropological opinion, which put human origins some 15 to 25 million years ago, the molecular biologists said humans split away from apes a mere 5 million years ago. This meant that *Ramapithecus*, every major anthropologist's candidate as the first hominid, could not be one, as it was too old.

More than a decade was to pass before the anthropologists were to admit that Sarich and Wilson were more right than wrong (some still don't), which spurred Washburn to suggest that "To some, a well guarded intellectual territory seemed more important than acceptance of an answer to the problems." It was not a popular remark.

Washburn's intention was not to suggest that nothing has changed since the time of the Taung baby. It has, dramatically so. To be sure, elements of emotionalism still color intellectual views, a fact of all scientific life, but perhaps more vigorously so, and certainly more visibly, in paleoanthropology. But, by concentrating on the warts, it is easy to imagine that the whole face is grotesque, which usually is not the case.

Tobias revels in the continuing controversies. "A sign of an intellectually vigorous profession," he says. "And onlookers forget what tremendous agreement there now is in our field, agreement on the most fundamental of questions." Dart, now 92, was on hand to celebrate the strength of the profession. His aside to a colleague earlier in the meeting: "I wish that man Zuckerman could be here to see this."—**ROGER LEWIN**

The Immune System "Belongs in the Body"

Pervasive anatomical and biochemical links between the immune and nervous systems help explain how mood might influence disease susceptibility

Over the years, a great many reports, some anecdotal but others more rigorous, have suggested that psychological factors such as stress might influence a person's immune response and therefore his susceptibility to infectious diseases and cancer. Largely missing from all this, however, was an explanation of how the nervous and immune systems might communicate.

That situation is now changing. A great deal of evidence shows that the two systems are inextricably interconnected. "The evidence for neuroimmunomodulation is enormous at every level," says N. Herbert Spector of the National Institute of Communicative Diseases and Stroke, who helped to organize a recent workshop* devoted to the topic.

Investigators have shown that stresses, both severe or more mundane, can alter immune responses and that classical Pavlovian conditioning, a form of learning, also influences them. Moreover, there are both anatomical and chemical connections between the immune and nervous systems that may serve to integrate their activities. Not only can the nervous system influence immune responses but, the new work shows, immune responses can alter nerve cell activities. In fact, the cells of the immune system may function in a sensory capacity, relaying signals to the brain about stimuli, such as invading foreign pathogens, which would not be detected by the more classical sensory system.

All this contrasts with previous thinking that the immune system is largely autonomous, a conclusion based on numerous studies showing that immune cells can be activated and make their responses in test-tube systems, away from other bodily influences. These in vitro studies have been highly successful in elucidating many facets of the immune response. However, the picture they present is incomplete, according to a theme echoed by many of the researchers investigating the interactions of the immune and nervous systems. As Karen Bulloch of the State University of New York at Stony Brook says, "It's time to put the immune system back in the body where it belongs."

Some of the recent work that has been attracting interest in the possibility that the brain may be regulating the immune

*The "First International Workshop on Neuroimmunomodulation" was held 27 to 30 November at the Bethesda campus of the National Institutes of Health.