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THE METAMORPHOSIS OF UNKNOWN ANIMALS INTO FABULOUS BEASTS AND OF FABULOUS BEASTS INTO KNOWN ANIMALS

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ABSTRACT: The unknown animals with which cryptozoology is concerned are by definition incompletely known animals. As a result, we unconsciously tend to fill the gaps in our knowledge of them by borrowing missing traits from the mythical archetype that they best seem to fit. The less we know of them, the more mythicized they are thus bound to become. When mythopoeic thought has to supply the lion's share, it is no wonder that such animals may be considered mere figments of the imagination.

At the same time, when a conspicuous animal is discovered by zoology and is scientifically described, it is often found to be at the root of the rumors which previously surrounded a particular "monster," which then becomes a well-authenticated new species. This has happened repeatedly throughout the history of zoology.

So it is that two opposite transmogrifications—from the unknown to the fabulous and from the fabulous to the known—normally succeed each other along our progressive inventory of the animal world. This precedence of a mythical apprehension upon a rational treatment may well be linked with the succession of steps which the human brain has to perform for a proper integration of all original messages from the outer world. To achieve this, such messages have to pass through the limbic system, where they seem to be emotionally tinted, and prepared to be categorized according to inborn mental structures—and thus more or less distorted and disguised—before being subjected to the action of reason in the cerebral cortex.

The main objective which is generally raised to the existence of apparently unknown animals is that they are adorned with too many fantastic traits to be taken seriously; that is, to be considered flesh-and-blood species worth listing in zoological catalogues. Sober-minded scientists—too sober-minded perhaps—and narrow-minded folklorists often jump to the conclusion that such obviously "legendary monsters" are but figments of human imagination. This debunking attitude actually betrays not only an ignorance of the history of zoology, but also a total misunderstanding of the mythicizing process which happens to pervade our *whole* apprehension of the world and its subsequent reconstruction in our minds.

Humans, Emotional Animals Smitten with Reason

From a zoological point of view, humans are just like other animals. Even their general anatomy is not particularly original. They are bipedal beings like half of all terrestrial vertebrates—one has merely to think of the birds, which form the order containing the largest number of species: more than 9,000! Plato had already pointed out judiciously in his *Politicus* that humans are nothing but featherless bipeds.

From a psychological view, however, humans are much more peculiar animals. Like all the others, they are, of course, motivated mainly by their emotions, but they are certainly the only ones which try to pass themselves for essentially rational beings.

Strikingly enough, the mental images that are generated in their minds by all and sundry sensations, are categorized at once according *not* to some logical system, but actually to the particular feeling or emotion which is associated with them: gratification or unpleasantness, pleasure or pain, warmth or coldness, fear or anger, joy or sadness, attraction or repulsion, appetite or disgust, lust or aversion, admiration or contempt, envy or scorn, love or hate, etc. As a result, numerous clusters of sensorial messages of all kinds—forms and colors, sounds, odors, tastes, and feelings—usually linked with a specific emotion, are planted deeper and deeper in the mind of each of us. And, since all humans are built of the same biological units identically organized, these significant clusters, which could be called "semantic pools," are grossly similar all over the world. As the environment varies in every country, each human culture uses its own imagery, and this accounts for all superficial differences.

Now, it is quite natural that even the structure of our thoughts should be dependent on the finer anatomy of our nervous system, and on its functioning. As a matter of fact, it is an interplay of constant associations—by no means fortuitous but based nevertheless upon mere resemblances, analogies and parallelisms—which governs our mental processes. It all happens as if every concept had its own array of natural harmonics. This helps to explain how easily symbols are born from an almost imperceptible shift, how real life can be disguised in our dreams, how poetic images spring up, how a language—and later writing—develops step by step, how a simple list of names can convey an ethical message (how, for instance, what was originally meant to be simply a catalogue of all known animals, the *Physiologus,* became almost spontaneously the moralized bestiary of the Middle Ages), and, finally, how the imaginary works in general. If imagination deserves to be called *la folle du logis* (the madwoman of the household), as Malebranche (1674) proposed, its madness is surely of the paranoiac type, because it is always strictly systematized. It is this rigorously structured way of thinking which seems to be responsible for what we usually refer to as "mythical thought," or just "myth."

The Dawn of Biomythology

The French philosopher of science Léon Brunschvicg once stated: "Primitive man wants to explain everything, civilized man acknowledges gaps of ignorance" (Brunschvicg 1934). This distinction between "primitive" thought and "civilized" thought now strikes us as artificial. Whether we like it or not, the Unknown frightens us all: to say the least, we find it extremely inconvenient. If, to push it away, the "primitive" mind invents explanatory myths, the modern scientist fills the gaps in his knowledge by constructing rational hypotheses. It amounts to the same thing.

Mythopoeic thought appears to me, more and more, to be an evolutionary adaptation enabling the members of our species to cope with the traumas linked with new experiences, which cause all the more anguish as they have never been lived through before.

Owing to the clustering process already described, it would seem that all the information received through the senses from the outside world, when passing through the limbic system—that part of the central nervous system sometimes dubbed the "emotional brain"—are stored in the same mental categories, distorted and completed according to the same deeply embedded stereotypes, and finally forced into the same molds of the mind as those that shape our mythologies, our heroic epics, and our "old wives' tales." What Carl Gustav Jung has called "the collective unconscious" (Jung 1933) is most probably nothing more than a specific, genetically programmed adaptation serving to protect the mind against psychological disorders, just as how the entire organism has gradually developed natural defenses to fight against the various threats to it.

This hypothesis is but the logical extension and end of the progress of biological and human sciences during the last decennia. Since Thomas H. Morgan's chromosomal theory of heredity (Morgan 1919), we have known that, in every living organism, all physical traits are transmitted by the genes inherited from its parents, and this is true also of all the processes which rule the development of the various organs, and even of the mechanisms that set the functioning of the whole organism. It has been suspected for a long time that the same holds true with respect to the current behavior of living beings, and, therefore, of the psychological processes linked to it. This has now been largely established through the results of the ethological research conducted by Konrad Lorenz (1965) *et al.,* and crowned by the sociobiological theory of Edward O. Wilson (1975). Even our feelings seem to be genetically programmed, which, of course, greatly reduces the influence of culture on our personal development. Noam Chomsky (1957), the father of modern linguistics, went even further by showing that language itself—at least some fundamental characteristics of it, such as grammar—is inscribed in our genetic heritage. The capital discovery by Claude Lévi-Strauss of "structural invariants," which shape the thought of all the peoples of the

world, proves that the very organization of our understanding is ruled by certain rigid mechanisms obviously associated with some of our genes (Lévi-Strauss 1958, 1962).

What I have personally been trying to discover in this perspective, adopted by a sociologist like Gilbert Durand (1984), a mathematician like René Thom (1980), and biologists and geneticists such as C. H. Waddington and, more recently, Rupert Sheldrake (1981), are the biological reasons which necessitated this structuring of the mind expressed in all the different facets of myth. It is, in my opinion, the fear of the Unknown or, more precisely, of the sundry mental disturbances which the vicissitudes of our humble individual lives could bring about when they are experienced for the first time: the painful experience of birth, expulsion from the womb, cold hostility of the world outside, momentary deprivation of the first and deepest alimentary bliss, thwarted passion for the mother, rivalry with the father, that kill-joy; and soon, competing brothers and sisters, feeling lost, first solitude; then, unsatisfied sexual desire, strife and defeat, disappointment and humiliation, tricks and treason, separation, death itself. What enables us to experience these daily tribulations as familiar is that they happen to be written in our genes before we are confronted with them. This is also why the scenario of our lives seems to follow the same basic pattern on which humans embroider all over the world to describe the awe-inspiring manifestations of the deified forces of nature, to tell the high adventure of the great heroes' career, or the simple fairy tale, even the popular joke. Imagination is strictly limited in the choice of both plots and motives, and so planned are the broad outline of its products.

I am convinced that, someday in the not too distant future, this situation will be checked experimentally by neurophysiologists, and that a new discipline of science will then emerge, one which I would name "biomythology."

We All Like a Good Monster

What happens in our minds when we are confronted with the problem of apparently new animals, which represent the Unknown in zoological systematics. Well, to neutralize the frightening nature of this particular Unknown, or its simple inconvenience—and thus to comfort us—we will be irresistibly driven to identify an animal of this sort with one of the more familiar forms of our mythical world, which Jung called "archetypes," choosing of course the one it best seems to fit. These archetypes are what Freud used to refer to as "archaic residua."

Fabulous beings are almost innumerable. To be convinced of this, one has only to browse through the pages of the incomparable *Dizionario Illustrado dei Mostri* (Illustrated Dictionary of Monsters) by Massimo Izzi (1989)—a must for both cryptozoologists and mythologists. This volume numbers

several thousand entries. However, when one carefully analyzes the symbolic significance of the various fantastic beings listed, one finally realizes that it is possible to reduce them all to a score or so (Izzi 1982). This results from the fact that each of them seems to be linked with one or another of our fundamental psychological problems, which are of course relatively limited. "Monsters," as we call them, are actually a reflection of them on the surface of our unconscious, not only of our individual unconscious, but also of the "collective unconscious" of our species, revealed with such finesse by Carl Jung.

Now, let us rapidly review what I would thus call our "fundamental monsters," and try to discover what each of them really means to us.

First of all, the *Chimera,* this absurd assemblage of anatomical parts borrowed from totally unrelated animals, is the prototype of all apparently impossible beings, *whether real or faked*: the platypus, an egg-laying mammal; the inconsistent horned hare, and its offspring (the innumerable Wolpertinger, which are a tradition in German-speaking countries, and the North American jackalopes); the chalicothere, an ungulate, which is a hoofed animal yet armed with terrible claws; the basilisk, a cock (rooster) which lays eggs (and snake eggs at that); the jumar, an alleged cross between a horse and cow; in short, the endless file of paradoxical creatures: dwarfish giants and gigantic dwarfs, spotless leopards and zebras without stripes, flying reptiles and tree-dwelling fishes, birds both wingless and furry, plant-eating carnivores, and—this is not cryptozoology any more, but cryptophytology—man-eating plants and animal-bearing trees.

All these chimera-like beings actually deliver a message of hope, as they prove that everything is really possible, including our wildest delusions. The *Western Dragon* is a personification of evil, of everything we must fight in order to survive with dignity. The *Eastern Dragon* is, on the contrary, a benevolent creature symbolizing authority, strength, experience, wisdom and goodness, everything we should honor and respect. The *Unicorn,* phallic symbol if there ever was one, is the image of aggressive virility, male potency, but liable to be vanquished by a defenseless woman at her most pure, most naive, but at the same time most sly—her surest weapons! The *Mermaid,* on the other hand, represents the enveloping, devouring mother, the Belle Dame sans Merci, the vamp, to whom the male is but an eternal victim. Being cannibalistic as well, she also awakes in men the nostalgia of former times, when he floated securely in the amniotic fluid in his mother's womb. The *Amazon* fills a similar niche on the social level: she does not seduce the male in order to eat him, but she rapes him and keeps him as a castrated slave once his reproductive duty is done. The *Wild Man,* the satyr, offers a double aspect: on the one hand, he enhances the human civilized state by representing its undeveloped, bestial, repulsive counterpart (Bernheimer 1952); on the other hand, he evokes the idyllic, nostalgic image of a lost

paradise; animality seen as freedom from the constraint of work and the harrowing consciousness of sin. The oversexed *Hermaphrodite* fulfills the dreams of those who want to experience the feelings and sensations of both men and women. The *Ogre* and *Ogress,* man-eating giants, represent the adult and parental world as seen through the eyes of the terrified child.

The *Little People,* gnomes, brownies and leprechauns, show the other side of the same myth, a transposition of the world lived in by children, set apart as they are by their small size: imperfectly understood, seen as belonging to an inferior level, and yet compelled to do the adults' biddings in order to merit their protection and win their favors. The greedy *Bogey-Man,* the beast that eats people, is as ambivalent as many other monsters: he embodies the fear of being devoured, suppressed, stamped out, but also the yearning—nostalgic in a way—to return to the warm security of the maternal womb. The *Werewolf* apparently emphasizes our fear of a sudden reversal to the animal status, but his ill fate actually induces us not to yield to our lower instincts, imperfectly repressed. The *Lake Monster,* hidden under the surface of murky waters, is the image of all the perverse, shameful, unspeakable thoughts we hide in the very depths of our hearts. The *Great Sea Serpent* is an eloquent symbol of the Devil, the Prince of Darkness, in this case of what was formerly called the "outer darkness," the vastness of the oceans beyond the horizon, imagined as the chosen domain of the powers of Evil. And just as the *Tentacular Monster,* surging up from the deep, represents the dangers that threaten us from below (from inside, our inner conflicts), the *Kidnapping Bird,* be it Roc-Bird, Thunderbird, or even the more reptile-like Griffin, represents those that may strike us from on high (the Demiurge, the moral authority that judges us, and, as the case may be, smites us).

The *Phoenix,* messenger from a marvelous realm far away, and always reborn from its ashes, reminds us of the lost Eden, the country where death was not, and encourages us to believe in a possible return to that Golden Age. The *Vampire* also evokes dreams of immortality, but reminds us of its darker side, the malediction that follows it like a shadow. And, at last, the long horde of *Different Men*—headless or bicephalous, one-eyed or with a hundred eyes, noseless, mouthless, or wrapped up in enormous ears, one-legged or many-legged, four-footed or with feet turned backwards, dog-headed or bird-headed, with goatlike or horselike bodies—stresses what we escaped from by being as we are, and thus reassures us, in a way, of our station, however miserable it may seem (Heuvelmans 1983, 1987).

To sum up, there are myths for every age, every sex, even for every singularity. That is why they attract, charm, trouble and captivate us, why they enlighten us, be it "as through a glass darkly," and on a whole, comfort us. No wonder we are so fond of monsters and imaginary beings, in films and in books!

From Obscurity to Fame

Paradoxical as it may seem, fabulous monsters are surely the animals nearest to us, the most closely bound up with our daily lives. The dog, the cat, the horse, and some others live with us; the mythical beasts live *inside us.* In the waves of our unconscious tumble mermaids, Krakens, and sea serpents; the forest bordering this inner sea shelters unicorns and dragons, satyrs and Amazons, ogres and pixies, werewolves and vampires. And across the sky of our dreams flies the giant Roc, clutching elephants in its talons, and the Phoenix, eternally returning to the paradise of its birthplace. Such myths concern us all, as the guardians of our soul's order.

That is the deeper reason why we are so eager, so impatient even, to shift the hide of our fabulous beasts onto the shoulders of ordinary, sometimes quite unglamorous, flesh-and-blood animals; an operation which is that much easier if the latter are not well-known, hardly known, or even not yet quite known. . . .

The apparently unknown animals with which cryptozoology is concerned—i.e., those which are heard of either because they are named and described by natives, or reported to have been sighted by travelers, or merely because there are traditions or depictions extant of them—are by definition never totally known. They are, in fact, *incompletely known* animals. As a result, we unconsciously tend to fill the blank spots and gaps in our knowledge of them by borrowing some missing traits, sometimes of a fantastic and even supernatural nature, from their mythical archetype. The less we know of them (either because they are aquatic animals of which we can only catch a fleeting glimpse, or because they are nocturnal, or burrowing, or hidden in dark equatorial forests or inhospitable deserts), the more mythicized they are thus bound to become. So it is no wonder that little-known animals will sooner or later be considered what we call "monsters," or "fabulous beasts."

Now, when one of these animals is eventually discovered by zoology and is scientifically described, it is sometimes found that is was previously taken for an imaginary creature. Nevertheless, the beast in question, fated to be stripped someday of all its fancy attributes, has become, almost overnight, a well-authenticated new species, rising from a sometimes ludicrous folkloristic reputation, and even a disputed cryptozoological fame, to a respectable and recognized zoological status.

The mythifying process can sometimes be carried to the point of altering its object beyond recognition. Since the manatee has pectoral mammae—like its cousin the elephant and also humans—and a body that tapers to a fishlike tail, it has always been identified, on both sides of the Atlantic, with the fascinating mermaid—despite its (to our eyes) ugly face—and by the same token, considered cannibalistic, and suspected of the most horrible

crimes. Along Central African rivers, manatees are usually described as aquatic vampires, draining imprudent bathers of their blood, even of their brains, by sucking them through the nostrils. Now, can anyone imagine a more peaceable, inoffensive creature than this sea-cow, which passes its days lazily browsing water hyacinths and other succulent plants? (Heuvelmans 1978).

The original unicorn was actually a man with a horn on his forehead. However, when a similarly endowed quadruped from the Orient was described by a Greek physician named Ktesias, this animal took the place, in our mythic pantheon, of the one-horned man, whose ithyphallic state was perhaps too crudely suggested. It was actually nothing but the Indian one-horned rhinoceros. This spectacular beast was subsequently mythicized to such an extent—becoming a graceful white horse with cloven hooves and a narwhal's ivory tusk—that the Renaissance encyclopedists of zoology failed to recognize it, and thus classified the armor-plated rhinoceros side by side with the slender unicorn (Heuvelmans 1963).

From Fabulousness to Zoological Status

These are but two examples of transmogrification dating from ancient times, but nothing has really changed since.

In 1828, a specimen of the largest fish presently known, the whale shark (*Rhineodon typus*), was harpooned in Table Bay, near the Cape of Good Hope. The representatives of this species are said to grow up to almost 20 meters (65 feet!), but are quite harmless, as they feed only on plankton. One of them, locally known as *chacon,* had nevertheless been spreading terror for several years in the Bay of Manila, in the Philippines, and had been described in the press as a most voracious sea serpent, marked with leopard's spots, another sign of ferocity (Heuvelmans 1965, 1968).

The largest of all rays, the great devilfish, another plankton-feeder, sometimes spanning 6 meters (18 feet) and weighing then 1.5 tons, was described at about the same time, in 1829, as *Manta birostris.* It had previously been spoken of, along the coast between Panama and Ecuador, as a sort of living counterpane, always ready to jump clear of the water and pounce on the fisherman's small boats, swallowing the crew at once. And years later, in 1866, owing to the two long appendages that flank the corners of its wide mouth (whence *birostris*), it was still taken for a true "monster" by a prominent South American philologist, Don Enrique Onffroy de Thoron: a gigantic prehistoric frog with the white enfolding arms of a classical mermaid (Heuvelmans 1958).

It was only in 1856 that the most unlikely of all sea monsters, the *kraken* of Scandinavian folklore, was unmasked at last, and scientifically named *Architeuthis.* It had been rumored for centuries as a tentacular island-beast

that was in the habit of picking up sailors from their boats with its innumerable slimy arms. At the same time, some stranded carcasses of a smaller size had been described variously as "sea-monks," "sea-bishops" and "sea-knights." Both the "living island" and the various "mermen" turned out to be a truly gigantic squid, which can be more than 15 meters (50 feet) in length, probably a *lot* more, and weigh several tons (Heuvelmans 1958).

All this happened, admittedly, in the immensity of the oceans, but also in the terrestrial realm there has often been some sudden turn in events just as spectacular.

In 1816, the zoological world realized with a shock that the fabulous *Mê* of Chinese legends—a black and white bear-like animal, with the trunk of an elèphant, the eyes of a rhinoceros, the tail of cow, and the feet of a tiger, which was credited moreover with feeding on iron and copper—was, in fact, an Indian tapir, *the* Indian tapir (now known as *Tapirus indicus*). An alien animal indeed, since tapirs were supposed to be strictly confined to South America (Heuvelmans 1984).

On that very continent, in 1829, a kind of "hairy ghost" of Colombian folklore, the *pinchaque,* said to be as big as a horse, was discovered to be, for its part, a high mountain tapir (*Tapirus pinchaque*), which had thus far preserved is incognito status (Heuvelmans 1984).

During the past centuries, there were rumors all over equatorial Africa about a hairy, bloodthirsty, and lascivious giant, who bludgeoned elephants with a club and dragged women into the forest to rape them. In the end, this half-satyr, half-ogre turned out to be the gorilla (*Gorilla gorilla*), a large man-ape, which we now know to be a quite unaggressive, good-natured animal, practically vegetarian, and much less sex-obsessed than its naked cousin, *Homo.* But now that we know him much better, after having properly demythicized him, and now that the job of the archetypic Wild Man thus looks vacant again, the gorilla's bad reputation has had to be passed on to another anthropoid ape, still unrecognized, from the Himalayas. Nowadays, it is he who is reported to fell yaks with his bare fists before disemboweling them and drinking their blood, and to abduct girls, preferably young: the Abominable Snowman, to give him his name, a complete misnomer if there ever was one, as there is nothing abominable about him, he is not a man, and he does not live in the snow (Heuvelmans 1962a, 1962b).

When, just before World War I, a small Indonesian island yielded the discovery of the largest lizard yet known, a monitor lizard over 3 meters (10 feet) long (*Varanus komodoensis*), the common name it received was, quite naturally, the Komodo dragon. It was, however, of rather modest proportions compared to the largest crocodiles, which are at least twice as long, and weigh 8 times more, theoretically at least (sometimes over a ton). Those truly monstrous creatures are actually not honored with the impressive

name of "dragon" today, because they were too well known—and for too long—to be identified with some mythical beast.

The Marriage of Myth and Science

Such examples could be multiplied according to order. The unfolding of events is invariably the same. After having been largely mythicized because they were too little known, or only known by hearsay, ordinary animals become fabulous, and fabulous animals are bound to be stripped of their fantastic attributes as soon as they are closely scrutinized by science. Two apparently opposite metamorphoses—from the unknown to the fabulous and from the fabulous to the known—actually succeed and complete each other along our progressive and rarely sudden discovery of resolutely new forms of animals.

This is exactly what happened also during the human step-by-step conquest of new lands. One should remember how, in the past, far-away islands, unexplored countries, or even whole continents, which had allegedly been sighted or visited by undaunted sailors or adventurers, were first rumored to be either dreamlands or nightmarish places, or a subtle mixture of both. They were always very vaguely located, sometimes described as being covered with lush vegetation, bearing delicious fruits, sometimes said to be totally barren, but always littered with invaluable treasures—guarded, however, by terrifying beasts—and populated with enchanting, sensuous females, but unfortunately also with monstrous cannibalistic men. It is only gradually that these "Lands Beyond,"—as they were once called in an authoritative book by L. Sprague de Camp and Willy Ley (1952)—were discovered and duly explored, if and when they were found, which did not necessarily always occur. To wit, Atlantis, Lemuria, Mu, and the *Terra australis incognita*: Vinland, and the lands of Prester John, of El Dorado and of the Amazons; Brendan's Island, and the islands of Thule, Ophir, Antilia, and Taprobane, have been faced with very different fortunes. It all depended on the extent to which they had been mythicized and sometimes confused with each other. But all of them passed for a certain period through a state of fabulousness.

One may even consider this mental process, in which myth and fact seem to play hide and seek, to be quite normal, if not inescapable, whenever we stumble and grope about hesitantly into the darkness of *any* area of knowledge.

If a mythical apprehension of an unknown phenomenon always precedes its scientific treatment, it is quite naturally because emotion begins at once, reason intervening only afterwards to control it if possible. Incidentally, science—which is supposed to be the result of soundly reasoning upon facts—should never be taken as equivalent to truth, and even less to "the truth, the whole truth, and nothing but the truth." It is nothing but that part of

our knowledge which is liable to quantification; that is, which can be measured and thus expressed in mathematical formulas, and so transmitted to other rational beings. To put it differently, science is simply rationalized knowledge. Unfortunately (for scientists), much of what we know happens to be irrational—to *know* a woman, for instance, or to define charisma, or to tell the difference between what is clockwise and what is counterclockwise. These phenomena cannot be expressed in figures, and this stresses the limits of science very clearly.

To summarize, mythification and rationalization succeed, overlap, and complete each other. Fact and folklore are so closely interwoven that it is often difficult to separate them sharply. As a result, to be able to understand mythology as well as science—or, under the circumstances, cultural folklore as well as cryptozoology—both approaches are imperative.

Supported thus jointly by folklorists and cryptozoologists, monsters are not about to die. Not that they are eternal: surely they were born with human thought and will vanish with it. Yet, I wish a long survival to all the animals of flesh and blood that were successively embodied in them, that somehow fed them and kept them alive, all for the comfort of our souls. We owe them that at least.

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THE THYLACINE: A CASE FOR CURRENT EXISTENCE ON MAINLAND AUSTRALIA

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ABSTRACT: The thylacine, *Thylacinus cynocephalus,* also known as the Tasmanian Tiger or Wolf, has been believed extinct on the island of Tasmania since 1936, and on the mainland of Australia for several thousand years. However, sightings of an animal apparently identical to the thylacine have been reported consistently from many parts of the Australian mainland for decades. Investigation of reports from people who have reported sightings of the thylacine in Western Australia are reviewed, and doubt is cast on the carbon^{14} dating of a thylacine carcass found in a cave at Mundrabilla Station, Western Australia.

INTRODUCTION

Well into the Holocene, the thylacine or Tasmanian Tiger or Wolf, *Thylacinus cynocephalus,* ranged over the whole of Australia. However, by the time of European arrival, it was considered extinct on the mainland, surviving only in Tasmania, where it was relatively common. Because it attacked stock, it was considered vermin, and thousands were killed. In 1936, the year it was officially declared protected in Tasmania, the last known specimen died in Tasmania's Hobart Zoo.

On the mainland, many reports have been made of sightings of a strange animal, apparently identical to the thylacine. These reports have been disregarded by most zoologists and by those who unquestionably accept scientific literature, which states that the thylacine is extinct on the mainland, and most likely also in Tasmania. The sightings mostly represent chance encounters, and have come mainly from reputable individuals with nothing to gain by fabricating stories.

Ludwig Glauert followed up many such reports when he was the director of the Western Australian Museum. As a member of the scientific staff of the Museum, I was sent by him on several occasions to investigate unknown predator reports, but I uncovered nothing that could determine their identity. (I retired, as Senior Experimental Officer, in 1974 following a tenure of 40 years.)

On one occasion, in 1951, a man from near Dwellingup went to the Museum with photographs and casts of footprints. He described the animal he had seen as identical to the thylacine. Unfortunately, I did not accept the evidence at the time, but I have subsequently seen, in the same locality, carcasses of kangaroos and sheep mutilated in the characteristic manner of a thylacine kill, so unlike that of a dog or dingo. Death is caused by the prey's neck being broken after a sometimes prolonged struggle.

FIG. 1.—One of a series of photos taken in Western Australia which shows what appears to be a thylacine digging at the base of a tree. The author believes that this is the only photo of the animal alive. (Kevin Cameron.)

On another occasion, I was sent to investigate reports of a strange sheep-killing animal near Wagin. I trapped and shot an Afghan hound with long, matted hair that had been living in the bush. This feral dog reinforced my disbelief in the existence of the thylacine until 1985. That is when I was shown photographs of an animal burrowing at the base of a tree.

THE CAMERON PHOTOGRAPHS

In October, 1981, the Agricultural Protection Board of Western Australia hired a tracker of Aboriginal descent, Kevin Cameron, and supplied him with a .357 magnum handgun. He was instructed to seek out a strange animal which had been reported by numerous individuals, including government employees, throughout the southwestern part of Western Australia. Cameron claimed several thylacine sightings during his employment, and later, on his

FIG. 2.—Another photograph in the same series of what appears to be a thylacine. The author believes that this photograph was taken several hours later, and that the animal was already dead. (Kevin Cameron.)

own time, produced five photographs of what appears to be a thylacine burrowing at the base of a tree (Figs. 1 and 2). Skeptics rejected his account and photos outright. Cameron, for his part, was uncooperative, and would not show the photographs publicly or allow them to be reproduced. He believed they had commercial value.

When I met Cameron, he showed me the photographs, and I accepted the veracity of his account. His sightings, the animal's reported behavior, its appearance, and the photographs convinced me that his tracking accounts were genuine and that he had indeed seen a living thylacine. This prompted me to write an article on the subject, which was published in *New Scientist* (Douglas 1986).

Cameron continued to be extremely secretive, and would not allow the photographs or negatives out of his possession. He withdrew permission for me to use the color photographs in my article, but gave permission for the use of a black-and-white enlargement of one of the color negatives. This was unacceptable to the editors of *New Scientist,* who had been promised color prints. When my article was completed and ready for submission, Cameron finally agreed to the use of color negatives, but accompanied me to the photographic laboratory to have the enlargements made. This was the first time I saw the negatives with full-frame, good-quality enlargements. When I saw the negatives, I realized Cameron's account with regard to the photographs was inaccurate. The film had been cut, frames were missing, and the photos were taken from different angles—making it impossible for the series to have been taken in 20 or 30 seconds, as Cameron had stated.

There were no photographs of the animal bounding away. Furthermore, in one negative, there was the shadow of another person pointing what could be an over-under .12 shotgun. Cameron had told me he had been alone. It would have been practically impossible for an animal as alert as a thylacine to remain stationary for so long while human activity was going on in its vicinity. In addition, it is significant that the animal's head does not appear in any of the photographs.

When the photographs were returned by *New Scientist,* I took them to a laboratory for color separation tests, and determined that, besides the time lapse deduced from shadow movement, there was a difference of several hours in the exposure times of the first and subsequent prints.

In my opinion, the thylacine depicted in the photographs published in *New Scientist,* with the exception of the enlargement in the lower right corner of page 45 (Fig. 1), is a dead animal rigid in *rigor mortis.* I believe this enlargement (Fig. 1)—what must be the first photograph—depicts a live animal. The full frame of this negative is the one which shows the shadow of the man with a rigid gun-like object pointing in the direction of the thylacine at the base of the tree. This shadow was deliberately excluded in the photo published in *New Scientist.* If I am correct in this supposition, the thylacine was alive when the first photo was taken, but had been dead for several hours by the time the second photograph was taken.

At no time would Cameron inform me of the location where the photographs were taken. Even though there were discrepancies associated with the photographs, I decided to proceed with publication of the *New Scientist* article; I hoped that the carcass would later be found, presumably shot by "persons unknown." In subsequent issues of *New Scientist,* letters were published which criticized the Cameron photos, including the shadow discrepancies. These criticisms were, of course, valid. When the media became aware of this, Cameron broke off all relations with me. Unfortunately, I have not seen or spoken with him since.

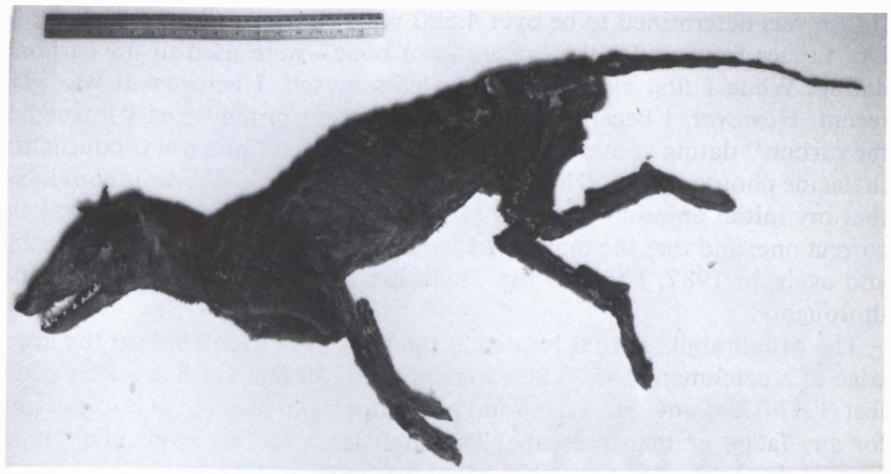

FIG. 3.—The thylacine specimen recovered from a cave on Mundrabilla Station, Western Australia, in 1966. Carbon[14] dating indicated an age of over 4,500 years. The author proposes that the specimen was only months old when found, as it was not a dehydrated carcass. (Western Australian Museum.)

THE MUNDRABILLA STATION SPECIMEN

I will now address another form of evidence in support of the thylacine's continued survival on mainland Australia. A carcass of a thylacine was recovered by a Western Australian Museum party in 1966. The specimen was found in a cave, Thylacine Hole, on Mundrabilla Station, near the Western Australian border (Lowry and Lowry 1967). This specimen is now housed in the collection of the Western Australian Museum (Fig. 3).

The carcass was fully covered with hair, had a musty odor, and looked like a recent dried-out carcass after the maggots had left but before the hide- and fur-eating invertebrates had begun their attack. It was *not* a dehydrated carcass, with dried intestines and flesh.

There is a sequence in which invertebrates and bacteria attack a carcass after death. Putrefaction occurs and the odor attracts blowflies of different species. Their larvae enter through wounds or through the natural orifices of the body. Bacterial action liquefies organs and flesh, and the resultant fluid seeps into the ground away from the carcass. By the time the fly larvae have left, the carcass is more or less dry, and other invertebrates—such as members of the families Trogidae and Dermestidae and the larvae of moths related to the clothes moth—attack the dried skin and fur, leaving only the skeleton; this is then often disturbed or eaten by vertebrates seeking phosphates or other minerals.

The Mundrabilla carcass was carbon[14] dated at the University of Sydney.

Its age was determined to be over 4,500 years (Lowry and Merrilees 1969). Dry tissues from under the carcass—not bone—were used in the carbon^{14} dating. When I first examined the carcass myself, I believed it was very recent. However, I became dubious of its recent origin when I learned of the carbon^{14} dating at over 4,500 years. Later, when Cameron produced the thylacine photographs and his supporting field evidence, I became convinced that my initial opinion—that the Mundrabilla carcass was recent—was the correct one, and that the matter needed further investigation. Thus, in 1986 and again in 1987, I visited the Thylacine Hole, and I explored the cave thoroughly.

The Mundrabilla cave is located at the bottom of a sink hole at the lower edge of a catchment area. The entrance shaft, 38 feet (11.5 m) deep and 6 feet (1.8 m) in diameter, is steep and precipitous, out of which it is impossible for any fallen animal to escape. The shaft leads to two levels. The upper level is dry, and shows no signs of recent flooding. The lower level begins below the shelf of the upper level, and it is to this level that flood waters pour. These carry mud and debris, and follow a deep channel around the periphery of the lower level. To reach the end of the lower level, where the thylacine carcass was found, it is necessary to crawl most of the 450 feet (137 m) over a dry, dusty floor typical of the upper level.

The ceiling of the upper level is mostly covered with stalactites, some of which are pure salt (NaCl), while others are of carbonate. Many have small pedicels, especially those composed of salt. Where the ceiling is impregnated with salt flakes, it is very friable, and the stalactites fall to the floor under their own weight.

The cave is repeatedly flooded. As recently as November, 1988, the immediate area recorded 3 inches (7.6 cm) of rain, and in April, 1989, at least 1.5 inches (3.8 cm) of rain fell. The humidity recorded in October, 1966, was 67 percent (Lowry and Lowry, 1967). During my two visits to the cave, in April, 1986, and August, 1987, the lower level of the cave was unpleasantly humid even though it was dry. The upper floor of the cave was dry and showed no signs of flooding. The lower level had a floor partly composed of deep, dry mud. There was also considerable debris, consisting of rabbit dung, sheep dung, emu bones, and vegetation obviously washed in from the surface.

Lowry and Lowry (1967) reported that the thylacine was found in the deepest part of the cave, on a rock pile—with its head raised (Fig. 4). As dead animals do not raise their heads, it must have been moved to that location by water or human hands, possibly individuals prior to the Lowrys' visit. The find was described by them as follows: "The animal lay on its right side with its head raised off the ground. The skin and hair were largely intact on the exposed surfaces, and the characteristic dark bars were clearly visible. The soft tissue had decomposed to a tarry substance which coated

FIG. 4.—Close-up view of the head of the Mundrabilla thylacine specimen. The carcass was still covered with hair, and had not yet been attacked by fur-eating invertebrates. (Western Australian Museum.)

the exposed bones. . . . However, the tongue and left eyeball were still recognizable, and a musty odor of decomposition was noticeable. The tail was some 12 inches away from the rest of the body, probably moved there by murids [rats] which appeared also to have chewed the abdomen. Blow-fly pupal cases and faeces (probably murid) were scattered around the carcass" (Lowry and Lowry 1967).

In my opinion, the tarry substance referred to by Lowry and Lowry is adipocere. This is a creamy white, waxy substance often formed on carcasses which are exposed to wet or damp conditions. It is not inhibited by a saline or lime environment. It is persistent, and I have found that carcasses in damp mines and caves have had this creamy white substance (grave wax) eventually dry to a black, tarry-like substance, as described by Lowry and Lowry.

The published description is not that of a 4,500-year-old carcass, and I believe its age could be given in months or, at an extreme, a year or so. An inaccurate dating could have resulted from contamination from the groundwater which saturated the carcass.

During my 1986 visit to the cave, I found a dingo carcass; it was hairless, dry and odorless, and its skin was like parchment (Fig. 5). The thylacine

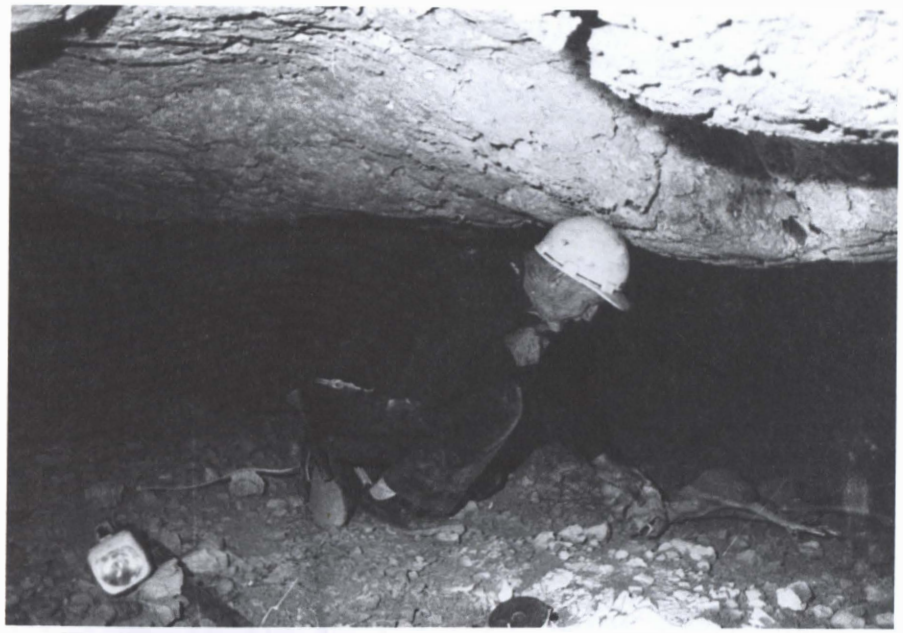

FIG. 5.—The author in Thylacine Hole in 1986 with newly-found dingo carcass. It could not have been in the cave for more than 20 years, due to the 1966 retrieval of all faunal specimens. Unlike the thylacine specimen, which is supposed to be much older, the dingo carcass was hairless, dry, and odorless, and had skin like parchment. (Bruce M. Douglas.)

carcass had been—and is—in a far superior state of preservation than this dingo carcass, yet the dingo carcass could not have been in the cave for more than 20 years, as the Western Australian Museum party had removed all specimens and bones in 1966. A map in the paper by Lowry and Lowry (1967) shows all locations of skeletons taken from the cave. Twenty years later, I found—besides the dingo—carcasses of snakes, lizards, small bats, and owls; some of these were devoid of all hair or feathers (Fig. 6). Many of these carcasses were still under attack from invertebrate larvae. Live barn owls, rats, bats, and a snake were also seen in the cave.

Because the 4,500-year-old dating of the thylacine carcass had been so readily accepted in scientific circles, I was not happy to be so completely at variance with the finding—until my lingering doubts were completely removed by information received from John Bannister, Director of the Western Australian Museum. On October 16, 1987, I wrote to Bannister requesting a reassessment of the age of the thylacine carcass. In his response, dated November 18, 1987, he stated that a Western Australian Museum geologist, George Kendrick, had reported: "The rocks on which the thylacine was found were creamy white abrakurrie limestone."

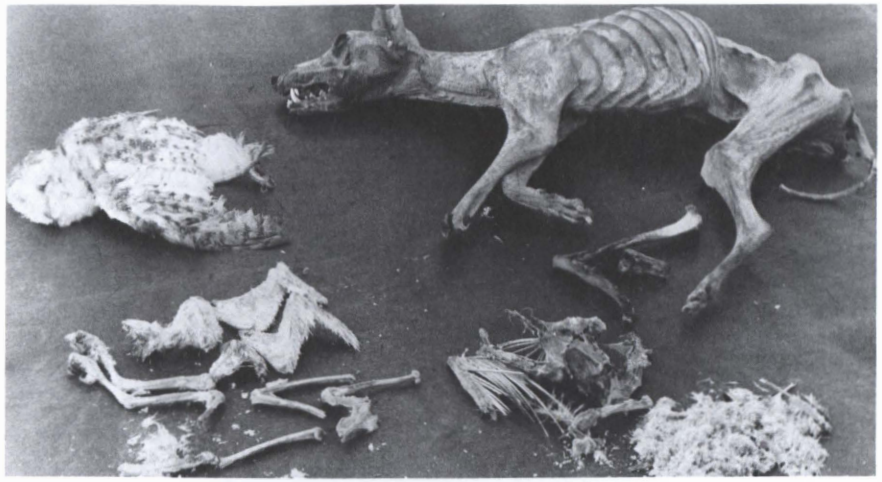

FIG. 6.—The dingo carcass and other faunal remains recovered by the author in Thylacine Hole in 1986. Many of the remains were still under attack by invertebrate larvae. (Athol Douglas.)

This creamy white limestone is of a very recent fall, some of which is below the high water mark and is not stained. It is a feature of this cave that numerous salt stalactites exist, and these have a very small pedicel and soon fall to the cave floor under their own weight. Above this limestone, the ceiling had no salt or carbonate stalactites, and on the limestone itself are no dust, stalagmites, fretted roof material, debris, or animal remains. It is in a pristine state and quite different from the rest of the cave floor. My companions on both of my excursions into the cave—my son Bruce M. Douglas and Robert Cooper—concur with me on the pristine state of this limestone.

Bannister also stated: "It is more likely that high salt levels were more important in inhibiting bacterial decay of the flesh. Specimens of other carcasses are present in the collection that show high levels of salt impregnation." However, there is no way that salt, sufficient to preserve an animal for 4,500 years, could enter the flesh, particularly as the thylacine carcass is erroneously claimed to be a dried, mummified carcass, which it is not. The only way that salt could reach high concentration is by immersion of a dry carcass in a strong saline solution. Such conditions exist in the Mundrabilla cave, as numerous salt stalactites form, drop to the floor, and are dissolved in the occasional floodings.

However, if the carcass was found on pure white abrakurrie limestone, there is no possibility of the carcass being salt-impregnated in such a location, as assumed by Bannister, and also by McNamara and Murray (1985).

Salt impregnation could only occur from positioning a dry carcass in concentrated saline solutions. This could not have occurred above the water level on pure-white, newly-fractured abrakurrie limestone. In my opinion, the carcass was washed to the lowest level as a dry carcass, and deposited there in an area which soon drained, leaving pools heavily saturated in salt from dissolved salt stalactites. The carcass may have been only months old at the time it was found. This carcass represents strong evidence of the existence of the thylacine in very recent times. It is also significant that recent sighting reports have come from Mundrabilla Station and the surrounding area.

In a subsequent communication dated April 13, 1988, in answer to a query, Bannister informed me that the carcass has not been X-rayed. An X-ray would probably show internal damage caused by the 38-foot (11.5 m) fall which would have prevented the thylacine from crawling or walking the 450 feet (137 m)—in darkness—to the deepest part of the cave. In my opinion, the thylacine did not die where it was found. It was probably placed on the rock pile by previous unknown visitors to the cave.

Thylacine Behavior and Recent Eyewitness Reports from Western Australia

Hundreds of people throughout Western Australia have clearly described an animal similar to the thylacine. Most of these people are from different localities and are separated by a considerable distance, yet their observations tally in minute detail. I have interviewed witnesses from the areas of Augusta, Boyanup, Dwellingup, Boyup Brook, Wagin, Bannister, Denmark, Nannup, and Busselton, all of whom, without any prompting, have described known facts and characteristics of the thylacine. None of these witnesses had read publications dealing with the thylacine before their sightings.

One sighting, in 1987, was near the Augusta golf course. Some months later, I was informed that a fire had passed through the area where the sighting had taken place. I visited the locality, and just inside the burn section I found the remains of a kangaroo. I am certain it was a thylacine kill. Typically, the skull and long bones were crushed and mostly broken, and showed teeth marks. I spent the whole day inspecting the burn section, and found five additional dismembered partial skeletons. All except one were within a short distance of the quarter-circle depressions where kangaroos shelter in the shade of a tree or shrub, and move as the sun shifts. I have found carcasses—or have been shown where I believe thylacines had been feeding on their prey—with these typical depressions nearby. Two years later, in 1989, Rory Neal found another probable thylacine kangaroo kill in the same area (Fig. 7). The neck was broken, and the tongue, face, and throat were eaten.

Thylacine sightings have also been supported by the presence of carcasses

FIG. 7.—A kangaroo which the author believes was killed by a thylacine. The neck was broken, and the tongue, face, and throat were eaten. (Rory Neal.)

of sheep killed in the manner peculiar to solitary predators, such as the larger cats, but definitely not dogs. Often, the head is eaten, especially when the kill is a young animal. The neck is broken, and the throat is torn open after the animal is pulled down and choked to death. The predator appears to have an immediate blood meal, thus little if any blood is at the scene of the kill. The tongue, throat, and nasal area are eaten at the same time. Later, the predator may return and eat the lungs, liver, and spleen, remove the paunch, and attack the hindquarters. Little fur is discarded, and the animal is opened up almost as cleanly as with a knife or scalpel.

It is of historical record that thylacines do not return to their kill after the carcass has been visited by humans, and so they are difficult to poison or trap (Smith 1982). This was the case with a large wether, which had been killed near Boyup Brook in 1986—a probable thylacine kill (Fig. 8). The neck was broken, and the face, throat, and tongue were eaten. The carcass was poisoned and traps were set, but the attacker did not return. It is possible that thylacines are able to sense metal, a substance foreign in any natural situation. Their prey could later be eaten or mutilated by dogs, dingoes, foxes, or feral pigs, but there are no records of these animals leaving a carcass with its throat torn out, with its tongue eaten, little if any blood, a broken neck, and the absence of internal organs such as heart, liver and lungs.

One interesting detail arising from my interviews is that sightings have never been reported by fox hunters. They deride its existence, maintaining

FIG. 8.—Carcass of a large wether killed in 1986 near Boyup Brook. The face, throat, and tongue were eaten, and the neck was broken. The carcass was poisoned, and traps were set, but the predator, thought to be a thylacine, did not return. (Athol Douglas.)

that their nocturnal spotlighting activities while hunting foxes would have verified its existence. However, all the reported Western Australia thylacine sightings that I have investigated were made in the morning or evening, and all were chance encounters on roads for seconds or a brief time. I suspect that the thylacine is not a nocturnal hunter, but hunts by smell in late afternoon or early morning. Having located its prey by scent, it probably makes a prodigious leap—of which it is capable—and grasps its prey with its jaws wide open, breaking the neck and proceeding to consume a blood meal as previously described. It has been conjectured that the thylacine wears down its prey (Smith 1982), but I propose that it stalks a feeding or resting animal and, by either a leap or a sudden rush, claims its prey.

Because of the dense bush and scrub over much of southwestern Australia, the thylacine may exist, yet very seldom be seen. This is characteristic of most mammalian predators, which are solitary hunters. Perhaps the most common predator in Australia is the introduced feral cat, an animal that is only seen fleetingly on chance encounters. The feral cat must not be confused with the farm or domestic cat, often seen while hunting within a mile or so of its home. Other common predators are native cats, *Dasyurus,* which are rarely seen but can be heard—or trapped—during their destructive visits to the fowl run.

CONCLUSION

In concluding, I should emphasize that my investigations, and thus my findings, concern only the thylacine in Western Australia. My findings do not necessarily apply to other parts of mainland Australia, or to Tasmania, where the species has been thought extinct since only the 1930's.

It is not possible to convey in a paper the credibility and enthusiasm of individuals who have reported sighting the thylacine. For the past 6 years, I have had to seek out eyewitnesses. Those who seek publicity have been the exception. It is not easy to accept laughter, disbelief, and derision, which is what most observers unfortunately experience when reporting their sightings.

It is regrettable that official bodies do not seem very interested in the possibility of surviving thylacines. This may be because of the possible high cost of recompense for stock damage should it be established that the species survives—which would make it a protected species. It would certainly not be possible to restrict it to a preserve.

Nevertheless, thylacine reports in Western Australia should be investigated seriously and thoroughly. It is very probable that relict, cryptic populations of this important Australian mammal still survive on mainland Australia, and have done so since European colonization.

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Cryptozoology, 9, 1990, 26–40

THE KELLAS CAT: REVIEWING AN ENIGMA

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ABSTRACT. A literature review is presented, covering the discovery, morphology, and identity of the Kellas cat—a black, gracile felid recorded from Scotland during the 1980's, and verified by a series of preserved specimens. Three other black felids, discovered in Scotland during the same period, but of less distinctive appearance, are also included; their identifies have bearing upon that of the Kellas cat.

INTRODUCTION

Over the past few decades, numerous reports of large, unidentified felids have been filed from all parts of Great Britain (Shuker 1989). Usually, such reports have not been substantiated by the procurement of specimens, the most notable exception being a female puma, *Felis concolor,* captured alive in Cannich, northern Scotland, on October 29, 1980 (Dow and Airs 1980, Shuker 1989).

During the later 1980's, however, several specimens of an unusual felid were obtained from various localities within the Grampian and Highlands regions of Scotland. As some of the earlier specimens were captured near to the hamlet of Kellas, and are the ones to have attracted most media attention, the form as a whole is nowadays popularly referred to as the Kellas cat (Shuker 1989).

The reason for public interest in the Kellas cat is its distinctive morphology. Its notably gracile body and limbs, jet-black coat, and wildcat stature collectively set it apart from the British wildcat subspecies (now thought to be restricted to Scotland), *Felis silvestris grampia,* and also from the feral domestic cat, *F. s. catus*—this subspecific ranking follows Corbet (1978), and Honacki, Kinman, and Koeppl (1982).

Information concerning the Kellas cat has not previously been collected and presented in the form of a scientific paper. To rectify this, a review of the morphology and likely identity of the Kellas cat is presented herein.

DISCOVERY

The area within which the Kellas cat specimens have been obtained (Fig. 1) is contained within the West Moray district of Scotland, which straddles the Grampian-Highlands regional border. The principal specimen provenances are the hamlets of Kellas and Advie, each set within a valley and surrounded by forests and mountains.

In June, 1984, an unusual felid was trapped in a fox snare on the grounds of Revack Lodge and discovered by the lodge's gamekeeper Ronald Douglas (Anonymous 1984a). An adult male, >1 m long, it was taken to the lodge

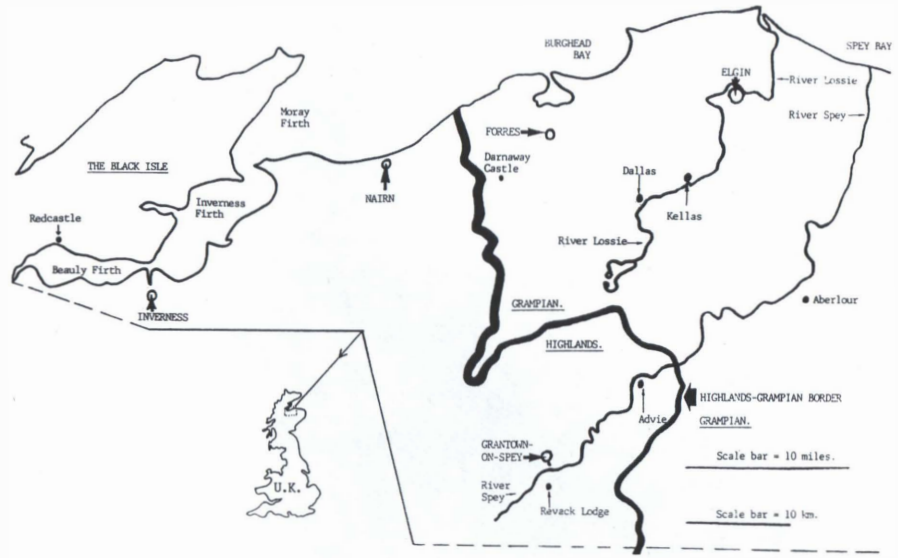

FIG. 1.—Scottish locality in West Moray from which all specimens of Kellas cat currently known have been obtained.

dead, and was later lost after having been sent to a taxidermist for preservation (Morgan 1984, Anonymous 1985a, Shuker 1989).

In October, 1984, an almost identical felid became known. Owned by Tomas Christie, it was a taxiderm specimen; the cat in question had been shot in 1983 while crossing the River Lossie near Kellas (Anonymous 1984b, Shuker 1989). Another adult male, it will be referred to hereafter as Specimen K (Figs. 2 and 3).

In April, 1985, a third specimen was obtained, a male aged approximately 12 months, shot by a gamekeeper near Advie (Anonymous 1985b, Shuker 1989), and hereafter referred to as Specimen A. In October, 1985, a fourth specimen, a young male 880 mm in length (Fig. 4), was procured near Kellas (Morgan 1985, Shuker 1989). All of these cat discoveries attracted considerable coverage by British newspapers. Since then, further Kellas cat carcasses have been obtained from Aberlour and Darnaway (Johnston 1987), and the author has received eyewitness accounts of comparable creatures from as far afield as Aberdeenshire and the Black Isle, thereby increasing appreciably the Kellas cat's known range. A more detailed account of the history of the Kellas cat is presented elsewhere (Shuker 1989).

MATERIALS

Excluding age-related size differences, the Kellas cat specimens so far obtained appear similar to one another in terms of superficial external mor-

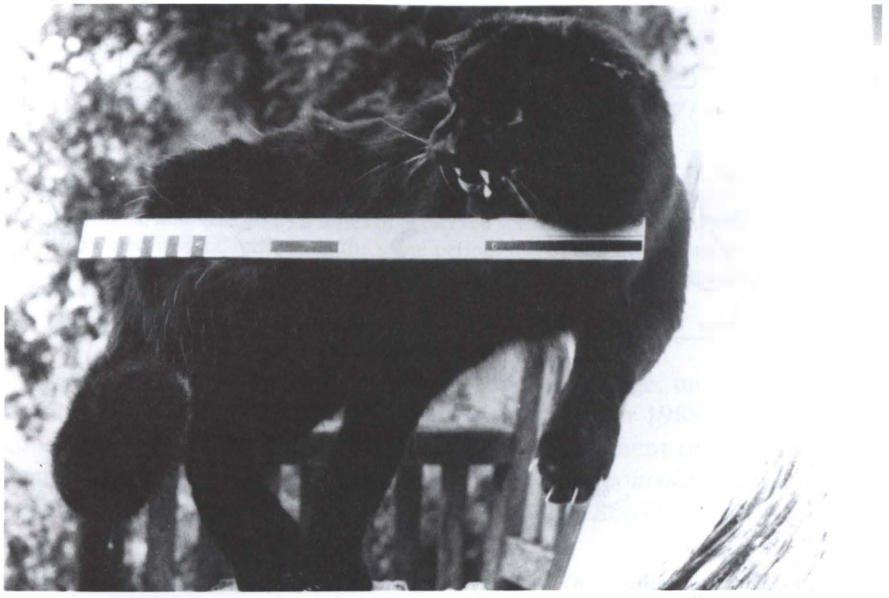

FIG. 2.—Specimen K in mounted form. Ruler in photograph = 40 cm long. (Charles Thomas.)

phology (Shuker 1989). To date, however, information obtained from detailed scientific examination of Kellas cats is available only in relation to Specimens K (Hills 1985) and A (Hills 1986), both of which were examined by mammalogist Daphne M. Hills of the British Museum (Natural History). Also, the author has examined Specimen K. Thus, the following description of the Kellas cat is based upon these two specimens, and drawn from the above-mentioned sources. Specimen A (BMNH 85.815) was donated to the British Museum by Di Francis; the remaining specimens are currently in private ownership.

MORPHOLOGY

Table 1 provides a list of measurements recorded from Specimens K and A. Most of the former are reproduced from Hills (1985) and the latter from Hills (1986), by kind permission of Daphne Hills; additional measurements for Specimen K are drawn from the author's examination of this felid.

The skeletal measurements recorded from Specimen K were obtained indirectly, by x-ray analysis of the taxiderm exhibit (Hills 1985); those for Specimen A were obtained directly from the prepared skeleton (Hills 1986). Thus, the measurements given for Specimen A are more accurate than those for Specimen K.

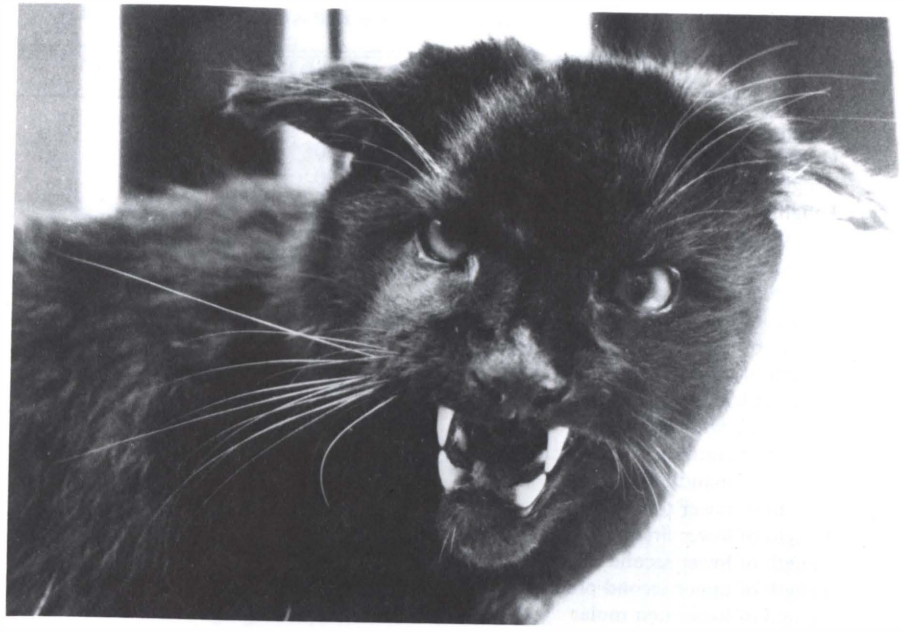

FIG. 3.—Head of Specimen K in mounted form. (Charles Thomas.)

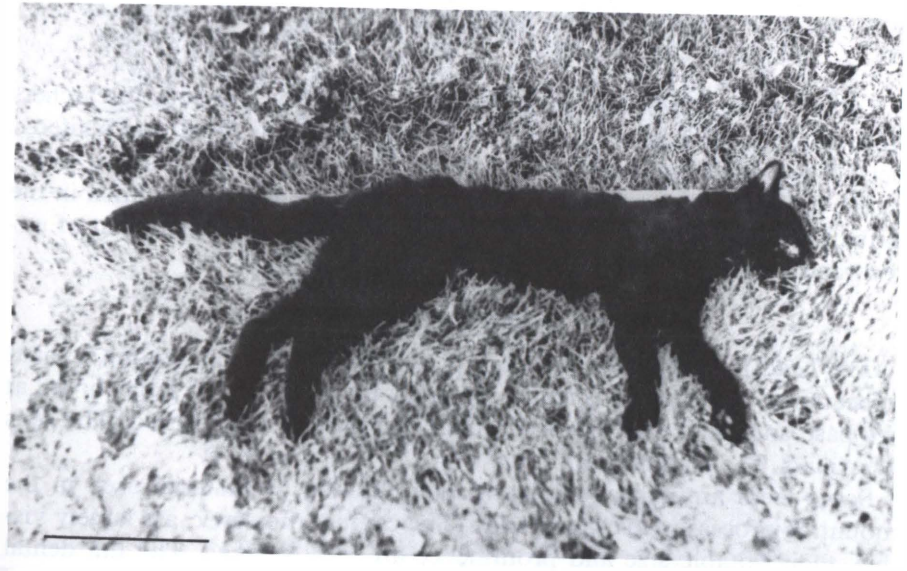

FIG. 4.—Young male Kellas cat obtained in October 1985. Scale bar = 200 mm. (Sandy Brander.)

TABLE 1.—Table of measurements recorded from two Kellas cat specimens, K and A.

Measurements (mm)	Specimen K (Kellas, mounted)	Specimen A (Advie)
Length of head and body	650	590
Height of shoulder	*360	–
Length of tail	300	290
Length of hind foot	130	138
Length of ear	50	68
Length of femur	–	132
Length of tibia	144	137
Length of humerus	–	117.5
Length of radius	120	112
Length of ulna	*140	132
Greatest length of skull	105	100
Zygomatic width	75	69.5
Length of cranium	57	–
Length of mandible	69.5	–
Length of lower tooth-row	21.5	**23.8
Length of lower first premolar	5.2	5.6
Length of lower second premolar	7.3	6.7
Length of upper second premolar	6.7	–
Length of lower first molar	8.0	7.8
Weight (kg)	–	4.525
Cranial capacity (cm^3)	–	38
Cranial index	–	2.63

* = approximate.
** = includes a supernumerary anterior molar.

Table 2 summarizes comparative data recorded by previous researchers from the Scottish wildcat, and from U.K. domestic cats (feral, and neutered pets). The wildcat documented by Pocock (1934), obtained in January, 1934, by Sir R.W. Brooke at Ardgay, in Ross-shire, is the largest specimen in the British Museum's collection. There are some longer specimens, but of unrecorded weight (Tomkies 1977).

DESCRIPTION

The head is fairly long, the back of the skull is broad, yielding a squat appearance. The muzzle is relatively short; the nose is quite large, broad at the base and pale. The ears are rounded, untufted, and pricked. The eyes are round. The chin is prominent.

Some teeth are partially or completely hidden by material used in the mounting process with Specimen K. The canines, however, are largely visible, their notable size and prominence are very striking. The full dental complement of specimen A is readily visible, and corresponds with that of *F. s. grampia,* with the addition of three supernumerary teeth—an extra pair

TABLE 2.—Size comparisons of Scottish wildcats, Kellas cats, and domestic cats (ferals and neutered pets).

Felid	Data source	Date of specimen procurement	Specimen sex	Number of specimens	HBL (mm)	Mean HBL (mm)	TL (mm)	Mean TL (mm)	HFL (mm)	Mean HFL (mm)	Weight (kg)	Mean weight (kg)
Scottish wildcat	a	1919–39	M	102	365–653	589	210–342	315	127–147	138	3.0–6.9	5.1
Scottish wildcat	b	1958–73	M	26	515–650	564	235–356	307	115–147	134	3.5–7.1	4.7
		1958–73	F	16	507–595	543	240–360	293	105–140	126	2.5–5.6	3.9
Scottish wildcat	c	–	M	–	370–590	–	–	310	–	140	–	–
		–	F	–	350–540	–	–	290	–	130	–	–
Scottish wildcat	d	1934	M	1	625	625	360	360	145	145	7.1	7.1
Kellas cat	e	1983	M	1 (K)	650	650	300	300	130	130	–	–
	f	1985	M	1 (A)	590	590	290	290	138	138	4.5	4.5
Feral domestic	g	–	M	–	–	567.5	–	277.5	–	–	2.7–5.0	4.1
cat		–	F	–	–	500	–	242.5	–	–	1.6–3.4	2.6
Neutered domestic	g	–	M	–	–	555	–	287.5	–	–	2.1–4.6	3.5
cat			F	–	–	527.5	–	272.5	–	–	1.8–3.6	2.8

Abbreviations: HBL = Head and Body Length; TL = Tail Length; HFL = Hind Foot Length. a = Kirk and Wagstaffe (1943); b = Kolb (1977); c = Freethy (1983); d = Pocock (1934); e = Hills (1985); f = Hills (1986); g = Tabor (1983). M = Male; F = Female; K = Specimen K (Kellas cat); A = Specimen A (Kellas cat).

of anterior lower premolars, and a well-developed extra upper left incisor (Hills 1986).

The pelage is less dense than that of *F. s. grampia*; it is composed of three hair types. Type 1 is awn guard hair, distributed over all of the body, and glossy black, 50 mm in Specimen A (Hills 1986). Type 2 is primary guard hair, extremely long (>100 mm), stiff, white, and much less numerous than awn hair; it is widely scattered, but most common on the ventral body surface. Type 3 is under-fur; it is short, soft, and dark chocolate-brown. The upper coat is coarse, and uniformly black except for Type 2 hairs and two small white spots (one inguinal, one median thoracic). Specimen A exhibits a limited degree of cryptic striping; there is a series of dark bands upon the upper limbs and encircling the tail (Hills 1986), visible when viewed at certain angles and comparable to the cryptic rosettes of the melanistic jaguar, *Panthera onca,* and leopard ("black panther") *P. pardus.*

The vibrissae are extremely long and coarse; white in Specimen K, brown in Specimen A. The eyebrows are similarly long, harsh, and white. The vibrissae and eyebrows resemble the pelage's Type 2 hair.

The tail is broad and thickly-furred; it does not taper towards the tip. The paws are long and narrow, not round; the claws are white, and retractile; the pads are pale; the underside of the feet is thickly furred.

All specimens are visibly gracile, long-limbed, with a relatively long body (slender, but well-muscled), short tail, and proportionately small head. The hind limbs are especially lengthy and powerful in appearance.

Features of Specimen A's skull of particular taxonomic significance include the following: the junction of the nasals with the frontals continues the plane of the skull, the angular mandibular processes are pronounced, the presphenoid bone is broad, and the upper and lower postorbital processes almost meet (Hills 1986). The above bones are either obscured or missing in Specimen K.

Equally important in relation to felid taxonomy is gut length; a length of 135 cm was recorded from Specimen A (Hills 1986).

"Intermediate" Specimens

Since 1985, three black felids that have bearing upon the Kellas cat have been recorded from northern Scotland. A 1986, BBC-TV documentary, *On the Trail of the Big Cat,* produced by Martin Hughes-Games, reported the capture, by a BBC-TV research team, of a living felid in the Highlands that initially seemed to be a Kellas cat. An adult female, it was of similar size to the younger preserved Kellas cats previously obtained, and was predominantly black with widely scattered, white primary guard hairs. Closer observation of its morphology, however, exposed differences between this specimen and the series of preserved Kellas cats. Compared with these, the living cat's head was larger in relation to its stocky body, its tail was somewhat

longer, its limbs were shorter, and its teeth smaller, whereas its white primary guard hairs were less prominent. Its overall appearance was that of a wildcat-sized *F. s. catus,* with a fierce temperament characteristic of wildcats and feral domestics alike. Thus, it was not a Kellas cat as defined here. This view was also put forward by the Nature Conservancy Council, and by Edward Orbell, Director of the Highland Wildlife Park, where this specimen is presently housed (Morgan 1986). Unpublished chromosomal analyses subsequently carried out with this felid at the University of Aberdeen by David Fox, based upon morphological comparisons of specific chromosome pairs between *F. s. grampia* and *F. s. catus,* revealed that it was a wildcat × domestic cat hybrid (Shuker 1989).

Another wildcat-sized black cat was caught alive on February 28, 1988, at Redcastle, on the Black Isle, a fertile peninsula of the eastern Highlands, with patches of young forest. This felid, an adult male, measured approximately 900 mm in total length, and weighed 6 kg. Long white primary guard hairs were present amidst its otherwise jet-black pelage, and a small white spot was present upon its chest and between its eyes. Its limbs, although relatively lengthy, appeared thicker than those of the preserved Kellas cats (Jacks 1988, Shuker 1989). It is now housed at the Highland Wildlife Park. In contrast to the BBC black cat, Orbell considers that this second living specimen closely resembles the Kellas cat form as defined here (Shuker 1989). Thus, the Redcastle cat would seem to be intermediate between this and the BBC cat.

In their recent wildcat hybridization paper, French, Corbet, and Easterbee (1988) documented a black cat from Aberdeen in northern Scotland's Grampian region that was assumed from its basically black pelage to be a pure-bred *F. s. catus.* Morphometrical analysis, conversely, revealed it to be a hybrid of *F. s. catus* and *F. s. grampia.* In terms of superficial external morphology, this specimen therefore comprises an intermediate between the BBC cat and pure-bred *F. s. catus.*

Identity of the Kellas Cat

Following examination of Specimen K at the British Museum (Natural History), it was concluded there that, although the possibility of this felid being either an *F. s. catus* × *F. s. grampia* hybrid or a feral *F. s. catus* could not be totally dismissed, available evidence suggested that it was most probably a melanistic *F. s. grampia*—and, as such, the first example recorded from Great Britain (Hills 1985). This mounted specimen was also examined by Frank Turk, a retired zoologist at the University of Exeter, who shared the British Museum's opinion that it was most likely a melanistic wildcat (Goss 1985). Nevertheless, Specimen K was not an ideal specimen for taxonomic purposes; being a taxiderm exhibit, its internal tissues were not available for study, and those parts of its skeleton retained within the mount

could not be examined directly (certain taxonomically important cranial bones, including the nasals, were missing). Specimen A, conversely, had been made available as a perfectly preserved, complete carcass. As will be discussed below, this revealed that it exhibited certain similarities to *F. s. catus.* As a result of those, it was therefore concluded at the British Museum that this felid was not a pure-bred melanistic *F. s. grampia,* but, instead, an individual with some *F. s. catus* ancestry too; i.e., a hybrid, although one more closely allied to *F. s. grampia* than to *F. s. catus* (Hills 1986).

Yet, despite the Museum's pronouncement, debate has continued concerning the precise scientific identity of the Kellas cat. Four contenders have been put forward at various times; with particular reference to Specimens K and A, these can now be successively assessed.

1) Separate Taxonomic Status?

Although appealing and a favorite of media reporters, the possibility that the Kellas cat warrants separate taxonomic status from both *F. s. catus* and *F. s. grampia* has little or no merit. The evolution and continuing existence of an entirely new species of felid unknown to science in as well-explored a country as Great Britain is remote, to say the least (Shuker 1989). Equally, as its known distribution is wholly overlapped by that of *F. s. grampia,* with which it also shares the same habitat, it cannot by definition be a separate subspecies of *F. silvestris.* In any event, although, as will be seen, certain of its morphological features differ in relative dimensions from those of typical *F. s. grampia,* the Kellas cat does not possess any anatomical characteristics unique to itself, i.e., exhibited neither by *F. s. grampia* nor by *F. s. catus.*

2) Feral Domestic Cat?

Some of Specimen A's features suggest an *F. s. catus* link; e.g., the developed condition of its postorbital processes (Hills 1986) and the presence of a supernumerary upper incisor—a condition more commonly associated with *F. s. catus* than with *F. s. grampia* (Colyer 1936). Conversely, there are many more Kellas cat features that correspond more closely with those of *F. s. grampia.*

For example, the skull, limb, and dental dimensions of Specimen K not only exceed the upper limit of those on record for *F. s. catus,* but are at the upper limit of those recorded from *F. s. grampia* (Pocock 1934, Hills 1985). All other external measurements recorded for Specimen K, and also Specimen A, exceed those typical for *F. s. catus* and lie well within the range for *F. s. grampia* (Kirk and Wagstaffe 1943, Kolb 1977, Schauenberg 1977a).

One of the genes responsible for black coat coloration in *F. s. catus* is pleiotropic, with one of its effects being to increase overall body size (Jude 1955). Thus, it may be expected that frequently (although not invariably—Bernard Heuvelmans, personal communication), black specimens of *F. s. catus* may be larger than those of other coat colors. Nonetheless, this could not explain the presence of certain more specific features exhibited by Specimen A that once again ally it with *F. s. grampia.*

For example, the junction of its nasal and frontal bones continues the plane of the skull as in *F. s. grampia*—in *F. s. catus,* conversely, the junction occurs in a pit below the skull's plane, thereby yielding a notched appearance (Kolb 1977). Specimen A's broad presphenoid bone and prominent angular processes also compare most closely with those of *F. s. grampia.* Similarly, its cranial index of 2.63 and cranial capacity of 38 cm^3 once again link it to *F. s. grampia,* which, according to Schauenberg (1969), has a cranial index of <2.75 and a cranial capacity of >35 cm^3.

In addition, the possession by Specimen A of supernumerary lower premolars is a condition more commonly exhibited by *F. s. grampia* than by *F. s. catus* (Hochstrasser 1970). Pocock (1916) noted that of 11 *F. s. grampia* skulls in his possession, three had a pair of supernumerary lower premolars, and a fourth had one unpaired example. The well-developed teeth of Specimen K lie within the upper portion of the range of measurements recorded from *F. s. grampia* (Hills 1985).

Finally, at 135 cm, the gut length for Specimen A is notably less than the lower limit of the range recorded for *F. s. catus,* which is >200 cm (Haltenorth 1957). In contrast, a length of <110 cm is normal for *F. s. grampia* (Schauenberg 1977b), and <150 cm for *F. s. catus* × *F. s. grampia* hybrids (Hills 1986).

Consequently, there are far too many deviations from the *F. s. catus* condition that concomitantly are similarities to that of *F. s. grampia* for a pure-bred *F. s. catus* identity to be tenable in relation to the Kellas cat.

3) Melanistic Wildcat?

The possibility of the Kellas cat comprising a pure-bred melanistic morph of *F. s. grampia* was greatly popularized in early press reports, most probably due to Specimen K. Yet this attitude failed to acknowledge that the British Museum's identification of this felid as a black wildcat was only a provisional one, as the internal features that could have provided a more conclusive identification were either absent or obscured. When present—in Specimen A—however, these features included some that were most comparable to those of *F. s. catus,* thereby arguing against a pure-bred *F. s. grampia* identity for the Kellas cat.

In any event, melanistic individuals normally differ from typically pigmented individuals of their respective species in color and sometimes in overall size, but not in actual outline (i.e., relative body dimensions). Kellas cats, conversely, appear notably more gracile than typical tabby-colored specimens of *F. s. grampia,* judging not only from photographic evidence, but also from the testimony of veterinary surgeons and other persons with

zoological knowledge who have examined Kellas cat specimens (Morgan 1984, Anonymous 1984b, Morgan 1985, Shuker 1989).

Indeed, although the morphological dimensions recorded from Specimens K and A all fall within the respective ranges recorded for *F. s. grampia,* it is important to note that they do not all fall within the same section of these ranges. Judging from these two specimens, and in comparison with documented ranges for *F. s. grampia* (e.g., Pocock 1934, Kirk and Wagstaffe 1943, Schauenberg 1969, Kolb 1977, Tomkies 1977, Freethy 1983, Hills 1985, 1986), the Kellas cat's body is more slender and below average in weight, its limb lengths are much above average, as are those of its head, body, and teeth, whereas its tail is rather short relative to that of wildcats of comparable limb and body stature. Hills (1986) provides further comparative data for Specimen A.

Of particular note in relation to this issue is an unpublished multivariate analysis carried out by Don French (cited in Hills 1986), utilizing measurements taken from Specimen A, and those from specimens of *F. s. catus, F. s. grampia,* and *F. s. catus* × *F. s. grampia* hybrids as recorded by French, Corbet, and Easterbee (1988). Although the result was contained within the *F. s. grampia* range, it closely approached the borderline of *F. s. grampia* with *F. s. catus* × *F. s. grampia* hybrids (Hills 1986), thereby underlining the atypical overall morphology of this Kellas cat specimen in comparison with pure-bred *F. s. grampia.*

4) Hybrid of Domestic Cat and Wildcat?

From the above considerations, it is clear that the most satisfactory identity for the Kellas cat is that of an *F. s. catus* × *F. s. grampia* hybrid, which would naturally explain its otherwise anomalous amalgam of *F. s. catus* and *F. s. grampia* features.

Examples of such hybrids documented in the past have most resembled *F. s. grampia* in pelage coloration (e.g., Pitt 1934, Gray 1971, Robinson 1972), but French, Corbet, and Easterbee (1988) have revealed from crossbreeding experiments that hybrids with pelage colors ranging from mottled tabby to uniformly black also can occur. Hence, it is likely that both the black domestic-lookalike hybrid reported by French, Corbet, and Easterbee (1988) and the BBC's black hybrid are simple crossbreeds, quite probably F_1 hybrids, as they exhibit a straightforward combination of *F. s. catus* and *F. s. grampia* features (Shuker 1989).

However, in view of the noticeable gracility of the Kellas cats—and, to a lesser extent, of the Redcastle cat—these are most probably of more complex hybrid origin; i.e., the product of extensive introgression (Shuker 1989), which can produce organisms that are markedly different in morphology from either of the two pure-bred taxa contained within their ancestry (Ricklefs 1979).

It is easy to comprehend why, when only the Kellas cats were on record, the concept of a hybrid identity for them seemed somewhat implausible, as their gracility and black pelage seemed to set them apart from the stockier, tabby-colored wildcat-like hybrids previously known. However, the subsequent appearance of French's black domestic-lookalike hybrid and the BBC specimen, plus the corresponding findings documented by French, Corbet, and Easterbee (1988), confirmed that black hybrids can occur, and that the extent of hybridization that has been occurring in modern times between *F. s. catus* and *F. s. grampia* is more than sufficient to produce striking specimens of complicated, introgressive ancestry. Indeed, French, Corbet and Easterbee (1988) have noted that the phenomenon of *F. s. catus* × *F. s. grampia* hybridization in Scotland has occurred to such an extent that the "pure" form of *F. s. grampia* may be effectively extinct there. They record that most of this hybridization probably occurred in the early part of this century, when *F. s. grampia* numbers were low. It could be predicted from this that subsequent back-crossing between such hybrids and pure-bred specimens of *F. s. catus* and *F. s. grampia* over succeeding generations would ultimately yield felids of highly complex, hybrid lineage—and, sure enough, during the 1980's the Kellas cat form became known.

Other Cats

Nonetheless, there is some intriguing evidence to suggest that such forms may have arisen at least spasmodically in the past too. One of the many unusual beasts of Highland folklore is the fairy cat or *cait sith.* According to Briggs (1976), it is supposedly a sizeable animal, predominantly black in color, but with a white spot on its breast, with an arched back, and bearing erect bristles in its fur. Some authors also allege that is possesses "sparks" over its fur (Shuker 1989). It is clear that the above description of the *cait sith* compares closely with that of the Kellas cat (even incorporating its median thoracic white spot), especially if the "sparks" are equated with the Kellas cat's white primary guard hairs, gleaming conspicuously among its jet-black pelage of bristle-like awn hairs (Shuker 1989).

Moreover, if a Kellas cat happened to be startled by a chance encounter with an unwary human, it would automatically arch its back and erect its bristly fur; these are typical features of feline defensive behavior, serving to increase the cat's apparent size (Leyhausen 1979). Also worth noting is that, despite its name—and unlike many other mythical creatures of Highland folklore—the *cait sith* was not held to be a true fairy form by the local people. Instead, it was merely considered to be a transformed witch (Briggs 1976), and hence of corporeal rather than ethereal status. Certainly, witch-cats feature prominently within Highland folklore (Neil 1924).

One final matter worthy of brief mention in this review is the documentation by Satunin (1904) of a black felid form from Transcaucasia. Satunin

formally named it *Felis daemon,* declaring that it comprised a distinct species, but subsequent researchers classified it as a black morph of the Caucasian wildcat, *F. s. caucasica* (Smirnov 1917), and ultimately as a feral *F. s. catus* (Ognev 1935, Pocock 1951). Although its black pelage with long white guard hairs is comparable to that of the Kellas cat, its dimensions as given by Satunin (1904), and more recently by Aliev (1972), reveal it to be smaller in overall size, with shorter limbs and a longer tail, but within the recorded size range for *F. s. catus.*

Conclusion

The Kellas cat is certainly a hybrid of feral domestic cat, *F. s. catus,* and Scottish wildcat, *F. s. grampia,* albeit one that has arisen from a complex, introgressive ancestry, responsible for its superficially distinctive morphology. Therefore, although it is of no taxonomic significance, its discovery is of particular cryptozoological interest, as it vindicates many reports in recent years of black, long-limbed, fox-sized felids from northern Scotland, which were hitherto dismissed as sightings of pure-bred specimens of feral domestic cats, or even as misidentifications of stray dogs.

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SCIENTIFIC DISCOVERY AND THE PLACE OF THE YAHOO IN AUSTRALIAN ZOOLOGICAL HISTORY

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Abstract: Discovery is an extended process in which observation needs to be accompanied by the necessary conceptualization. The Yahoo (or Australian "gorilla") may be seen as an unresolved anomaly set against a background involving such anomalies as platypus eggs, marsupial birth, the African gorilla, and the Queensland lungfish.

Introduction

In an influential account of how change in scientific thinking actually occurs, Thomas Kuhn (1970: 1–3) has called for an historiographic revolution in the study of science. According to Kuhn, history could produce a decisive transformation in the image of science by which we are now possessed, an image drawn from a study of finished scientific achievements.

Scientific method has often been regarded as the process by which items have been added to the stockpile of knowledge, while the history of science is the discipline that chronicles this accumulation. The task of the historian then appears to be to determine when and by whom each scientific fact was discovered. The historical record of the research activity will, however, reveal a quite different picture, one in which the questions "when" and "by whom" discoveries were made are seen as symptomatic of an erroneous view of what constitutes science. An analysis of discovery will also emphasize the role of accident in the emergence of totally new fact, and the need to transcend the conceptual boundaries of what Kuhn calls normal science.

According to Kuhn (1970: 52–64), discoveries are complex events which demand an alteration in the way we see nature, but do not necessarily lead to a fundamental reconstruction of a field of study. Anomalies are defined by Kuhn as violations of expectation, or as phenomena for which the scientist is not prepared, rather than in terms of what is considered acceptable to science. In this way, the anomaly of one generation may be accepted by the next. For instance, the discovery of X-rays occurred by accident, and was greeted with not only surprise but also shock because the existence of X-rays violated deeply held expectations.

Prior to discovery, only what is anticipated and usual will be experienced, even where anomaly is later to be observed. Discovery therefore commences with the awareness of anomaly; that is to say that nature has somehow violated the expectations of normal science, develops through exploration of the anomaly, and concludes only with the resolution or assimilation of that anomaly. This frequently protracted and convoluted process can be seen

as a weakening of the grip of tradition upon the mind in the face of the unexpected, so that finally the anomalous becomes the expected. Observation and conceptualization are inseparably linked in discovery. Consequently, failure to recognize the existence of anomaly should be sufficient to prohibit development of discovery. On the other hand, if the adjustment of conceptual categories is prepared in advance, then the phenomenon will not be of a new sort and there will be no surprise.

The aim of the present exercise is to examine scientific attitudes towards one particular anomaly—the Yahoo or Australian "ape"—against the background of an historical re-evaluation of the discovery of other more familiar zoological phenomena.

Harper and Montgomery

Kuhn (1970: ix) discusses instances of discovery from the physical sciences, but points out that evidence for his reorientation of the nature of science also comes from the history of biological science. Kathleen Dugan has surveyed the links between observation and theoretical assumption in the development of Australian zoology, and has demonstrated weaknesses in the conventional historical view. Dugan (1980: 1) has pointed out that scientific theories had developed largely without reference to the new animals, so that theorists were confronted with a number of unexpected facts which did not fit their theoretical frame of reference. The intellectual conservatism of 19th century Australia, and the dominant position held by theorists in Europe, also meant that Australian scientists could easily overlook or discount important evidence. The history of Australian zoology therefore provides an excellent opportunity to observe the process of scientific discovery at work. A convenient point to enter a discussion about the discovery of Australian animals and their characteristics is provided by a defence of science made in a now largely-forgotten controversy which took place in 1912 around the so-called Australian "gorilla." It is therefore to this apology for 19th century zoological science, and the event which precipitated it, that we now turn.

Argument about an Australian "ape" or Yahoo had arisen sporadically throughout most of the 19th century. Despite the primate label, the matter probably should be (and indeed was) regarded as a controversy about the existence of a large and yet undiscovered marsupial. In 1912, a rare public discussion about the creature arose, by far the most significant feature of which was the publication in a Sydney newspaper of an account by the surveyor Charles Harper (Joyner 1977: 19). Harper, who claimed that he himself had encountered the animal, asserted that reports of an Australian "gorilla" had for many years past emerged from the eastern or coastal slopes of the ranges of southern New South Wales. While scientists denied its

existence, reliable men unhesitatingly asserted that they had seen this animal in the forest at short distances. They were so terrified at the sight, and the hideous noise it made, that they departed at once from the locality, leaving their tools and work done (as timber getters) behind them. Furthermore, the Aborigines of the region themselves asserted that the creature existed.

Harper's graphic description of the animal which approached his camp fire one night includes some interesting details. For instance, the phalanges of the feet were extremely long, indicating great grasping power. The mouth was equipped with two large canine teeth which protruded over the lower lip when the jaw was closed. The stomach seemed like a sack hanging halfway down the thighs. When finally the animal made off, it did so at first erect and then on all fours.

Notwithstanding the startling nature of this announcement, it produced no response of a scientific nature other than a commentary by Alexander E. Montgomery which appeared in the Sydney *Sun* for November 24, 1912, p. 13. Montgomery (1912) was a traveller turned writer whose first love, as he put it, was science. His remarks on Harper's story are worth noting, not only because they stand alone, but also because they are undoubtedly representative of 19th century scientific opinion. If we hope to learn something about scientific attitudes towards the concept of an Australian "ape" and the possibility of its existence, then Montgomery's remarks will provide almost the only clue.

Montgomery's theme concerns the correctness of zoological science in its attitude towards the apparently incredible; by this, he means more or less what Kuhn has called "anomaly." In contrast, however, Montgomery deplores the prejudice and ignorance of the non-scientist in such matters, in particular the Australian bushman. To him, science spells "enlightenment," and is therefore never dogmatic but always strictly judicial. It is simply a question of evidence. Science gives the only explanation of the phenomenon compatible with the evidence available. Produce contrary evidence, and science will modify its explanation accordingly. Montgomery illustrates his argument with several examples. It was zoologists, reasoning from analogy, who first suggested that the monotremes were oviparous, or egg-laying. It was the Australian bushman who rejected the idea, until it was conclusively demonstrated by Caldwell in 1884. Again, it was the bushman who believed that the marsupial fetus was formed upon the teat and whose preconceptions allowed him to remain unconvinced even by dissection showing the fetus in the uterus. Furthermore, it was the general public who ridiculed du Chaillu's account of the gorilla when it appeared. Science, however, recalled the close similarity with Hanno's account from the 5th century B.C. As a result, further investigation was made, and the gorilla was ultimately added to the list of major anthropoids. Finally, it is the Australian bushman who is responsible for the suggestions of what many had referred to as a "marsupial

anthropoid ape," a belief which, for reasons unstated, Montgomery cannot share.

Science and Early Australian Zoological Discovery

It can be seen at once by anyone familiar with the cases concerned that these short accounts are only loosely based on what is known of the actual discoveries. The reason for this is clear. Like most scientists, Montgomery believed that science was impartial, and that scientific discovery was methodical, being understandable in terms of evidence alone. The cases he brought forward are described in such a way as to make this appear to be true. Kuhn, however, maintains that discoveries cannot be made using the normal, cumulative procedures of science, and that, if the record of the process is examined, this will be apparent. As he puts it (Kuhn 1970: 28), the trouble with the view of science as the accumulation of evidence without prior theoretical commitment is that history offers no support for so excessively Baconian a method. Cumulative acquisition of unanticipated novelty is an almost non-existent exception to the rule of scientific development (Kuhn 1970: 96). That is to say, where discovery is concerned, it is not evidence which tends to compel, but the preconceptions common to the discipline, against which novelty emerges only with difficulty. Put even more succinctly, discovery may be irrational and need not follow any recognized method (Feyerabend 1975: 165).

The influence of prevailing theories on the interpretation of empirical data is perhaps most clearly seen in the case of 19th century attempts to explain the egg-laying mammals (Dugan 1980: 7). When the first of these (the echidna) was discovered in 1792, naturalists assumed its reproductive anatomy followed the standard placental pattern. Full dissection of the platypus in 1801 showed that its reproductive structure more closely ressembled a lizard than a bird or a mammal. This, coupled with the apparent absence of mammary glands, led to astonished speculation that the animal was oviparous. The problem could be solved only by field observation, and most reports from Australia confirmed that the monotremes laid eggs.

However, the controversy over whether or not the platypus laid eggs was essentially a taxonomic debate, one in which in which European naturalists could select or reject evidence on the basis of its agreement with their own theoretical preconceptions. Of such a kind was the debate between Etienne Geoffroy Saint-Hilaire, who argued that the animals laid eggs and did not give milk, and Richard Owen, who argued that they were normal mammals giving milk and therefore producing live young. In 1834, Geoffroy conceded that the platypus gave milk and for half a century no one seriously challenged the position held by Owen that the monotremes gave birth to live young (Dugan 1980: 191–226). This erroneous opinion became so firmly established in scientific circles that reports of platypus eggs were either ignored or explained away (Dugan 1980: 8).

In 1884, monotreme eggs were found simultaneously by Wilhelm Haacke, Director of the South Australian Museum, and the British scientist William H. Caldwell. Characteristically, little notice was taken of Haacke's discovery, even in Australia, while the reaction to Caldwell's announcement was sensational. No one doubted his claim. Like other European scientists, Caldwell believed that the monotremes gave birth to their young live, and had earlier thought he had confirmation that this was so. He was presumably surprised to find he was wrong, but declined to discuss what he now considered a fact (Dugan 1987: 93–94).

Early accounts of marsupial reproduction were conflicting and confused. Many observers concluded that the young originated upon the teats. However, in 1698 Tyson gave the first detailed description of opossum anatomy. His examination revealed an internal uterus and showed that previous authorities were wrong in stating that the marsupial young originated and developed within the pouch. The persistence of this idea among Australian bushmen in the 19th century is rightly called by Montgomery the dogmatism of unenlightenment. Yet the observation does nothing to illustrate the capacity of zoological science to respond to the seemingly impossible. In fact, the record of awareness of the nature of marsupial reproduction demonstrates continuing disparity between theoretical preconceptions arising from laboratory examinations and observations in the field. For instance, Tyson erred in assuming that the opossum young developed internally until birth, at which time they had reached a stage comparable to that of new-born young in other mammals. He therefore denied that the pouch had any special function in the reproductive process.

Observers in the field could clearly see that the state of development of the new-born pouch young corresponded to that of uterine fetuses. Thus, the identification of the pouch as an external uterus fitted their observations much better than the claim that the pouch served only to protect the young after birth. European scientists, unfamiliar with living marsupials, assumed their reproductive physiology was substantially similar to that of placentals, and ascribed to ignorance all reports to the contrary (Dugan 1980: 25–44).

In 1763, the Compte de Buffon offered a two-stage theory of marsupial development which accounted for both anatomical observation in the laboratory and the appearance of the new-born young. But Buffon wrongly believed that the primitive stage of development of the marsupial young indicated that they were born prematurely. Buffon therefore resolved one anomaly, but left unresolved another, that of how a prematurely born fetus could travel from the uterus into the pouch (Dugan 1980: 45–48). Owen (1834) noticed that the living fetus was far from being the inert embryo it was supposed to be, but, influenced by the belief that it was born prematurely,

concluded that it did not have the power to move unaided from the vulva to the pouch. He rejected accounts of unaided transference and his view that removal was effected by the lips of the mother prevailed, despite evidence to the contrary. The matter was finally resolved by the Hartmans in 1920 (Troughton 1965: 15–18).

Also long deferred was definite knowledge of the route taken by the young kangaroo in passing from the uterus to the exterior. Home had suggested in 1795 that the young were born through a median passage rather than through the lateral vaginal canals. Owen (1834) denied this possibility, stating that the median canal was always closed in his specimens, and that the young must pass through the lateral canals. The question was unaccountably not settled until as late as 1889 when a cook in a camp of rabbiters made a crude dissection of a specimen, which he forwarded to Stirling, showing the embryo in transit through a median canal (Frith and Calaby 1968: 102–103). Thus, while the non-scientist might be led into error over certain matters, they might also perceive facts hidden from—or unacceptable to—the scientific community (Dugan 1980: 57).

Discovery of the Gorilla

Definite knowledge of the African gorilla is relatively recent. The eventual identification of crania and other skeletal parts belonging to the gorilla was due to the enterprise of the American missionaries Savage and Wilson in collaboration with the anatomists Wyman and Owen. The publication by Savage and Wyman in 1847, the first scientific account of the gorilla, deeply stirred scientific interest. Du Chaillu, to whom Montgomery wrongly attributes the discovery, was mainly responsible for later bringing the gorilla to the attention of the world. Yet why was the animal discovered so late? Of early references, Hanno's description from the 5th century B.C. was perhaps inapplicable, although an unidentified ape mentioned by Battell in 1625 was surely a gorilla. Buffon was acquainted with the writings of Battell, but unfortunately confused the issue by the misapplication of names, and so for decades delayed the scientific discovery of the gorilla (Yerkes and Yerkes 1929: 382).

An account contained in Monboddo's work of 1774 ought to have stirred naturalists to a determined search for the gorilla, but it did not do so. Even an unmistakeable reference by Bowdich in 1819 failed to lead to the discovery of the ape. Savage and Wyman noted the failure of naturalists to recognize a second species of large ape in Africa (after the chimpanzee), but attributed this to the absence of any evidence on which its existence might be predicated other than vague statements of travellers. These, resting upon information derived from natives and not on personal observation of the narrator, were said by the authors to be so mingled with absurd and marvellous accounts that they were deservedly regarded as unworthy of credence.

At the same time, Savage and Wyman asserted that their anatomical description of the gorilla would most satisfactorily confirm the statements of Battell and Bowdich (Yerkes and Yerkes 1929: 32).

There appears, however, to be a certain contradiction, first in supposing that the early accounts were altogether incredible, and then in supposing that the final discovery neatly confirmed them. In any case, Montgomery's suggestion that the gorilla's discovery arose from a search following upon a comparison of reports was incorrect.

Formal scientific recognition for the gorilla depended upon the availability of anatomical evidence, but failure to clarify the issues involved might have impeded the process. In 1852, Isidore Geoffroy Saint-Hilaire published a note on the gorilla in which he tells us that his father had since 1828 suspected a second type of African ape, but that for nineteen years nothing had confirmed his prediction at a time when most other naturalists still doubted its existence. The greatest authority of the time, Cuvier, went so far in his book *Le Rène Animal Distribué d'Après son Organisation* of 1817 as to deny categorically the gorilla's existence, a condemnation repeated in the edition of 1829. He thereby retarded considerably the date of its discovery.

After Cuvier's death in 1832, the climate of incredulity which he had encouraged continued to prevail. To such an extent, it appears, that when in 1836 material evidence in the form of a mutilated skin was brought back from Gabon by a sea captain named Thouret, no one dared to see in it proof of the existence of Battell's large ape (Heuvelmans 1980: 281–283). This skin presumably belonged to the young animal referred to by Savage and Wyman, who say only that it was not known whether the skeleton was preserved (Yerkes and Yerkes 1929: 32–33).

Enter the Yahoo

We come at last to a consideration of the Australian "ape," and of Harper's account of it. According to Kuhn, and in opposition to the view of Montgomery, novelty in science emerges only with difficulty, and the unexpected will at first be ignored or forced into conformity with accepted views of the world. An inclination by scientists to pass over Harper's description, or at best to see in that lurid scene the translation to an Australian setting of a passage from du Chaillu, is therefore understandable. A rational approach, however, might accept that there are several good reasons for transcending such indifference.

First, references to an Australian "ape" precede formal discovery of the gorilla, and there was evidently debate among Australian naturalists on the question long before the public became aware of that African ape (Joyner 1977: 6). Secondly, Harper's account had been preceded by an exceptionally large number of references to the subject (far more, for example, than in the case of the gorilla prior to 1847), all of which were either ignored or over-

looked by Australian scientists. Among these were detailed descriptions from the same area by Osborne (Joyner 1977: 1) in 1871 and McCooey (Joyner 1986: 1) in 1882, the latter ridiculed by Ramsay, Curator of the Australian Museum (Joyner 1986: 6). Reports from elsewhere in the southern ranges included a description of an animal killed in 1893 near Braidwood (Joyner 1977: 10, 11) and there were also reports from further to the north like that of Telfer in 1900 (Millis 1980: 55, 56). Together the reports show a considerable degree of consistency among physical details (Joyner 1989).

A third reason is provided by Montgomery's presentation, or rather, in some respects, his silence. Oddly enough, given his intentions, Montgomery refrained from comment on Harper's story. The best indication that it was entirely Harper's own and intended to be believed lies in the fact that Montgomery makes no suggestion to the contrary. In addition, Montgomery left unstated his objections to the idea of such an animal, allowing a suspicion that he had none of any substance. A fourth consideration is that Harper (1840–1930) was in a position to know what he was talking about, having worked as a surveyor in or around the region concerned, first at Bombala and then at Moruya, for some fifty years. Only a few months before (see the *Moruya Examiner* of 16 March 1912, p. 2) he had been forced to camp out in the ranges in much the same circumstances as when he made his sighting.

Finally, certain minor features of the tale compel our attention. Harper's contention that the existence of such an animal was known to the Aborigines is supported by a note about the *dooligal*—an Aboriginal name for the Yahoo—made by the linguist R. H. Mathews at the turn of the century (Joyner 1977: 23). Recent linguistic surveys (Hercus 1969, Kelloway Eades 1976) throw further light upon the matter, even though they do not consider relevant historical data. Next, Harper's remark about timber getters is worth noting because it remains amenable to confirmation. For instance, I have a letter dated February 20, 1978, from Mrs. Eileen Pittman of Bega, a part Aboriginal woman who had lived all her life on the south coast between Nowra and Bega. After providing some stories of a familiar kind about Aborigines and the *dulagarl,* she added: "My Uncle Bill Taylor was a sleeper cutter up in the mountains near Braidwood. He said they often saw them at the camp eating all the scraps. Bill Taylor is a white man."

Another matter of interest is the physical description of the animal, particularly the assertion that it was equipped with two long canine teeth which protruded over the lower lip when the mouth was closed. While there appears to be no precedent for this depiction, the remark is at least open to interpretation. For example, the description might well apply to two separated incisors, like those possessed by the late Pleistocene diprotodontid *Hulitherium tomasettii.* This was a large marsupial—ecologically equivalent to the mountain gorilla in Africa, the giant panda in China, or the spectacled bear in South America—which formerly inhabited montane rain forests in Papua New Guinea. *H. tomasettii* may have been capable of an upright, bear-like or panda-like posture (Flannery and Plane 1986).

Where there existed among scientists no awareness of anomaly, no recognition that something was amiss, then this failure could itself prohibit development of discovery until such time as observational data might be matched by the requisite conceptualization. The case of the Queensland lungfish (*Neoceradotus forsteri*) also exemplifies this. In 1870, Gerard Krefft, then Curator of the Australian Museum, published a description of what he called one of the most interesting animals ever to be discovered in Australia, and he conjectured that the discovery would take the scientific world by surprise. Krefft (1870) found that the Queensland lungfish possessed a dentition unlike other lungfishes, but ressembling that of certain fossil sharks. The skeleton was part bone, part cartilage, the backbone being pure cartilage. Krefft noted how strange it was that this curious creature, well known to the settlers at Wide Bay and other Queensland districts, should so long have escaped the eyes of those interested in natural history.

For at least ten years prior to his announcement, however, Krefft had been tantalized by stories of the fish about once a week when dining with William Forster, then Minister for Lands in the New South Wales government (Whitley 1929). Krefft (1870) believed Forster must have been mistaken in describing a fish with a cartilaginous backbone. At length, under the influence of repeated affirmations, Krefft relented sufficiently to demand proof. Forster thereupon asked his nephew W. F. McCord, an enthusiastic naturalist, to dispatch specimens from the Burnett River, which duly arrived and had the intended effect (Perry 1928: 177, Whitley 1929). Krefft later apologized for questioning Forster's observations (see the *Sydney Morning Herald,* January 18, 1870, p. 5). Yet, for at least a decade, the Queensland lungfish remained unknown to science because Krefft could not believe that such a fish might exist.

Conclusions

Zoology in 19th century Australia provides an opportunity for an historiographic re-orientation of the nature of science. This suggests how unsatisfactory was the simple cumulative view of scientific innovation which prevailed, and how frequently a phenomenon went unremarked upon by scientists because it lay beyond their expectations.

Of the Yahoo itself, it can be said that its discovery was never resolved, and that the restrictive attitude of contemporary science contributed to the lack of a solution. A similar restriction of vision is found in recent demands for a mundane or commonplace solution (e.g., Smith, 1989: 30). But there are important distinctions evident in the history of 19th century Australian zoology which are not so apparent in the physical sciences, concerned as they are to a greater extent with causal analysis and common observation.

The distinctions are those between the fixed and the transient, between the views of European scientists (and frequently their Australian counterparts) on the one hand, and those of settlers, colonial naturalists, and the Aborigines on the other, and between the requirements of comparative anatomy and the field observations which might complement them. It was perhaps a unique combination of considerations like these as much as the nature of scientific discovery or the novelty of Australian animals which have shaped the distortions characteristic of Australian zoological history.

Postscript

It has been correctly pointed out by Groves (1989) that the imagery and language used to describe the Yahoo are invariably drawn from the hominids or the larger apes, particularly the gorilla. Groves has further suggested that the title of my 1977 book *The Hairy Man of South Eastern Australia* is perhaps evidence that I then thought in terms of a hominid/hominoid solution. But this is not so.

The limitations of thought and language require that anything new be described in terms of what is already familiar. Any large creature that stood on two feet (such as a bear) might be man-like, but the resemblance need not be—and obviously often is not—more than superficial. Designations like "Hairy Man," "Australian Gorilla," or "Tasmanian Tiger" are merely names, not descriptions.

It has never seemed to me possible that the words "Hairy Man" referred to an unknown hominoid (that is, a placental primate) because (1) there were obvious geographical difficulties, and (2) some of the recurring features described (such as the shape of the foot and the existence of claws or long nails) could hardly belong to a hominoid. In the foreword to my book, I explained that the work was an inquiry into what the expression "Hairy Man" might mean, but that I was then unable to supply an answer.

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A MORE APPROPRIATE PROCEDURE FOR NAMING SASQUATCH

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ABSTRACT: Krantz (1986) assigned footprint casts of Sasquatch (Bigfoot) to the type specimen of the Chinese fossil species *Gigantopithecus blacki*. This procedure will result in future Sasquatch evidence having to be tested against the tooth which serves as the type specimen. A more appropriate procedure would be to erect the Sasquatch footprint casts as the type specimen for a new North American species, *Gigantopithecus canadensis*. The cladistic analyses of Jeffrey H. Schwartz, and their implications for the Sasquatch-*Gigantopithecus* equation, are briefly reviewed.

The purpose of this paper is to address Grover Krantz's scientific naming of the Sasquatch (Bigfoot) (Krantz 1986), and the amplification of his thinking on the topic (*ISC Newsletter* 1987a, 1987b).

The formal description by Krantz (1986) was based on a paper given at the symposium "Cryptozoology: The Search for Unknown or Supposedly Extinct Animals," held as part of the III International Congress of Systematic and Evolutionary Biology, held at the University of Sussex, Brighton, England, in July of 1985. In that original paper, he proposed the reassignment of the name *Gigantopithecus blacki*—a large fossil primate from China—to the type specimen represented by the casts—taken at Elk Wallow (Walla Walla) in 1982—of the footprints of a supposed Sasquatch. This procedure was ruled inadmissible under the rules of taxonomic nomenclature established by the International Commission on Zoological Nomenclature (1985). Effectively, Krantz had to assign the footprints to the existing type specimen of *G. blacki,* or erect a new species based on his proposed type specimen.

It appears from the subsequent article (Krantz 1986) that he chose the former course. He still wishes to equate the Sasquatch with *G. blacki,* and is prepared to "downgrade" his footprint casts accordingly, stating that, if future evidence proves him wrong in the species identification, then the Sasquatch should be renamed *Gigantopithecus canadensis.* (The title of the article reflects his original position: to be accurate, it should not have been titled "A Species *Named* from Footprints," but, rather, "A Species *Identified* from Footprints.")

This is a sadly mistaken course. First, as Krantz himself states, it reverses the normal nomenclatural procedure. The rules of nomenclature have been developed not just to make life complicated, but to solve real and practical problems of describing the natural world, so that, literally, we can know what we are talking about. The practical consequences of Krantz's decision

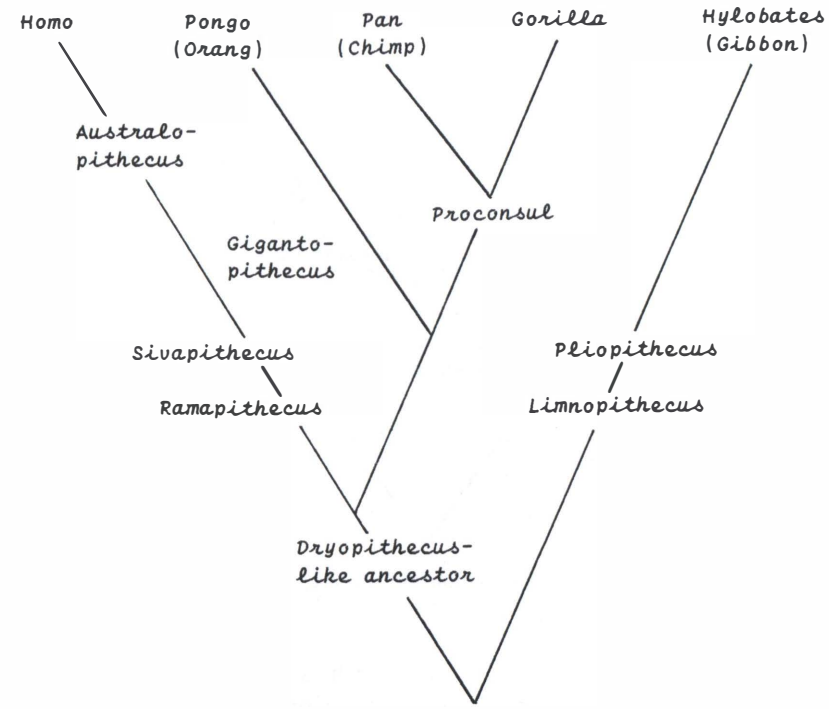

FIG. 1.—A formerly accepted scheme of the relationships among fossil and living hominoids. (Based on Schwartz [1987].)

include the difficulty that any new Sasquatch evidence now has to be tested against the fossil tooth which is the type specimen of *G. blacki.* It would be much more sensible to adopt the nomenclaturally correct course, and erect the footprints as the type specimen for a new species, *G. canadensis.* This immediately makes it much easier to test other physical evidence for the Sasquatch against the very good evidence Krantz now holds. It is, in any case, very difficult to make a good case for the precise identification of footprints made in modern America with a tooth from half a world and a thousand millennia away.

I see no practical use in Krantz's present procedure, other than as a statement of faith in the outcome of future research. He gives no reason, for instance, for preferring to identify the Sasquatch with *G. blacki* rather than with *G. bilaspurensis.* His reasons for the precise identification of Sasquatch with *G. blacki* are that "the possibility of two gigantic, bipedal higher primates [sic] species can be considered very unlikely" (Krantz 1986: 97), and that "the natural biological condition of species is to remain unchanged indefinitely, unless outside forces intervene" (*ISC Newsletter* 1987b: 2).

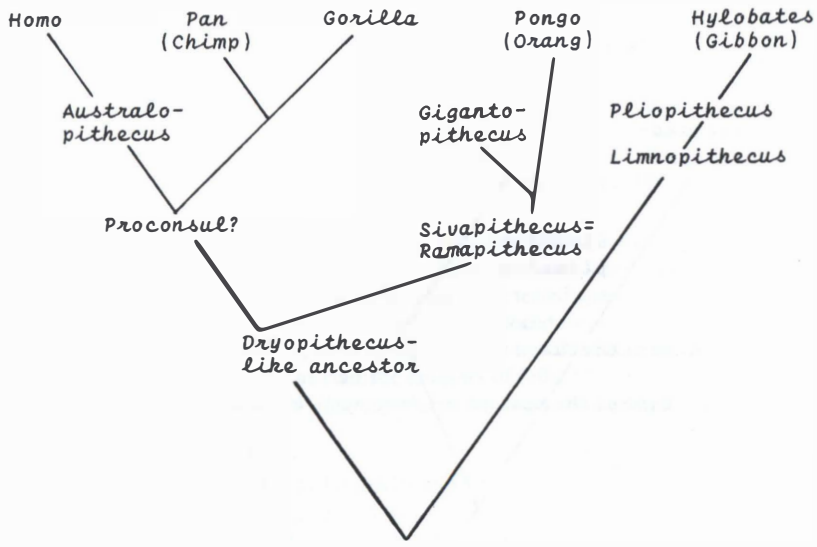

FIG. 2.—A more recent scheme of the relationships among fossil and living hominoids. (Based on Schwartz [1987].)

To take the second point first, if Sasquatch is *G. blacki,* then it has undergone a migration from Asia to America, a probable change of climate from tropical to temperate, and may even have survived several glaciations. As for the first point, how many people could distinguish between a tiger and a lion on the basis of a few teeth or mandible parts of one and three footprints of the other? Krantz simply does not have the physical evidence to make the equation he wishes; he should, instead, make the best use of the evidence he has, and name the Sasquatch *G. canadensis* on the basis of his evidence.

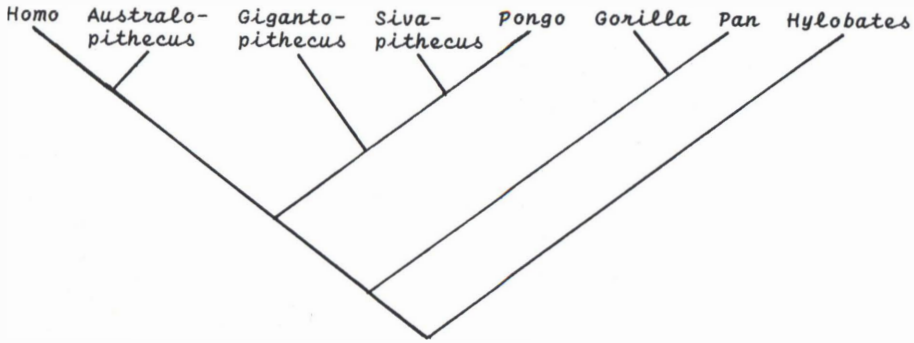

FIG. 3.—Schwartz's proposed scheme of the relationships among fossil and living hominoids. (Based on Schwartz [1984b].)

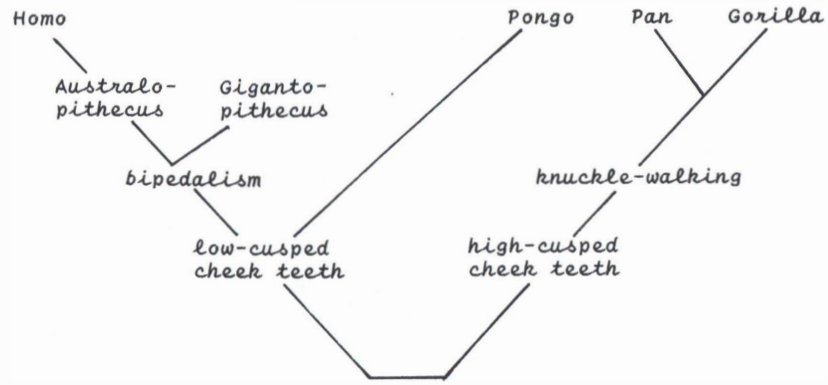

FIG. 4.—The author's proposed scheme of the relationships among some fossil and living hominoids. It provides a single ancestry for both the characteristic human/orang dentition and for bipedalism.

In a published interview, Krantz (*ISC Newsletter* 1987b: 2) states that he is "of the minority school which considers *Gigantopithecus* to have been a hominid." What he does not state is which model of hominid ancestry he favors; and, in particular, whether he has considered the views recently put forward by Jeffrey H. Schwartz, which have direct bearing on the status of *Gigantopithecus* in the hominid line. Schwartz (1984a, 1984b, 1987) has proposed that the orang-utan is the ape most closely related to humans. The starting point of his analysis is the dentition of the hominoids, and his approach is cladistic. His characterization of theories of hominid evolution appear as Figs. 1 and 2. Schwartz's proposed scheme, showing only phylogenetic relationships, appears as Fig. 3.

The realization that *Gigantopithecus* dentition has affinities with that of the orang-utan is what has removed this fossil form from the generally accepted view of hominid ancestry. What I have not seen mentioned in the literature is that, by altering Schwartz's phylogeny in just one particular, we can provide a single ancestry for both the characteristic human/orang dentition and for bipedalism, as shown in Fig. 4.

These schemata grossly oversimplify Schwartz's position and his evidence, of course; and I am neither paleontologist nor systematist. I do hope, however, that someone better qualified than I—perhaps Krantz himself—will reassess the position of *Gigantopithecus* in light of Schwartz's theories.

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Field Reports

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Cryptozoology, 9, 1990, 57–65

AN INVESTIGATION OF THE *ORANG-PENDEK,* THE "SHORT MAN" OF SUMATRA

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INTRODUCTION

In July, 1989, I arrived in southwestern Sumatra, a large Indonesian Island, with the intention of producing travel features on the area—I am a freelance writer. I had no previous knowledge of the "Short Man," and was intrigued to learn of the possible existence of the animal known locally as *orang-pendek.*

It was not until my return to Britain that I learned that reports of an as-yet unrecognized bipedal primate have persisted throughout this century in the mountainous central spine of Malaysia and the high rain forests of northeastern Borneo. However, nowhere do these reports seem more firmly anchored in reality than in the montane rain forests of the remote Kerinci region of southwestern Sumatra (Fig. 1).

The Kerinci region lines in the central southern part of the Barisan mountains of western Sumatra. The area takes its name from a 12,500 foot (3,800 m)-high active volcano. The rain forests there are among the richest surviving in Southeast Asia; they contain large mammals, including the Sumatran tiger, the Sumatran rhinoceros, the Malayan tapir, the sun bear, and the siamang ape. A total of 3.7 million acres (1.5 million ha) of virgin rain forest have been declared as the Kerinci Seblat National Park—one of the largest yet least known or penetrated such reserves in Asia.

FIG. 1.—Sumatra, a large Indonesian Island, is the location of many reports of a human-like bipedal primate named *orang-pendek*. The author collected information on this supposed animal from villages in the Mt. Kerinci region (see arrow).

Many more millions of acres of rain forest remain as yet untouched by commercial interests—although a French gold mining consortium is pressing for exploration licenses in the area. Away from the densely-populated Sungeipenuh valley, it is possible to trek for up to a week in dense, high altitude rain forest without encountering either a village or a road.

NARRATIVE DESCRIPTION

I arrived in Kerinci in July, 1989, spending three weeks in the area. I returned in September for a further two weeks. My introduction to the Short Man came one July day at dusk on the slopes of Mt. Kerinci, although I had already come across the name *orang-pendek* in a guide book; it described the animal as a "legendary hairy hominid which terrifies the local population."

From our camp site at 11,150 feet (3,400 m), we could see the Indian Ocean—perhaps 100 miles (160 km) to the west—and my guide, Jamruddin ("Din"), pointed out areas where rhinoceros and tiger are commonly found. He then indicated, quite casually, that, in the densely-forested mountains east of the crater lake of Mt. Tujuh (Fig. 2), one could sometimes see *orang-pendeks*. I commented that one never seems to meet anybody who had actually seen such mythical beasts first-hand. He seemed a little surprised, but then admitted, apologetically, that he had "only" seen an *orang-pendek* twice. Din then went on to give a detailed description of the animal. It was, he said, still common, although becoming rare in the Mt. Kerinci area—possibly due to incursions by local farmers.

Over the next weeks, I became increasingly intrigued as I visited settlement after settlement seeking further information. In the increasingly densely-populated region around the foothills of Mt. Kerinci, villagers reported no local sightings of *orang-pendeks* in more than three years, although hunters reported seeing them on rattan-gathering forays into the forest. In the lower-lying Sungeipenuh valley, the animal seems quite unknown. Only in the more remote villages, high in the hills, are recent sightings of the animal reported.

After two weeks, it seemed that the most likely area in which to find proof of the animal was an untracked wilderness east of Mt. Kerinci and south towards the market town of Bangko (Fig. 2), which lies on the Trans-Sumatran highway. Despite offering well over the normal guide price, I did not succeed in finding a guide willing to take me into this region.

The people of villages such as Palompet and Kersiktua insist this area is the habitat of a big cat they call *cigau*. Described as being slightly smaller—but apparently more heavily-built—than the Sumatran tiger, the *cigau* appears to prompt real fear among the hunters of Kerinci—the only animal to produce such a reaction. They claimed the *cigau* attacks without provocation. "*Cigau* hates man," I was repeatedly told.

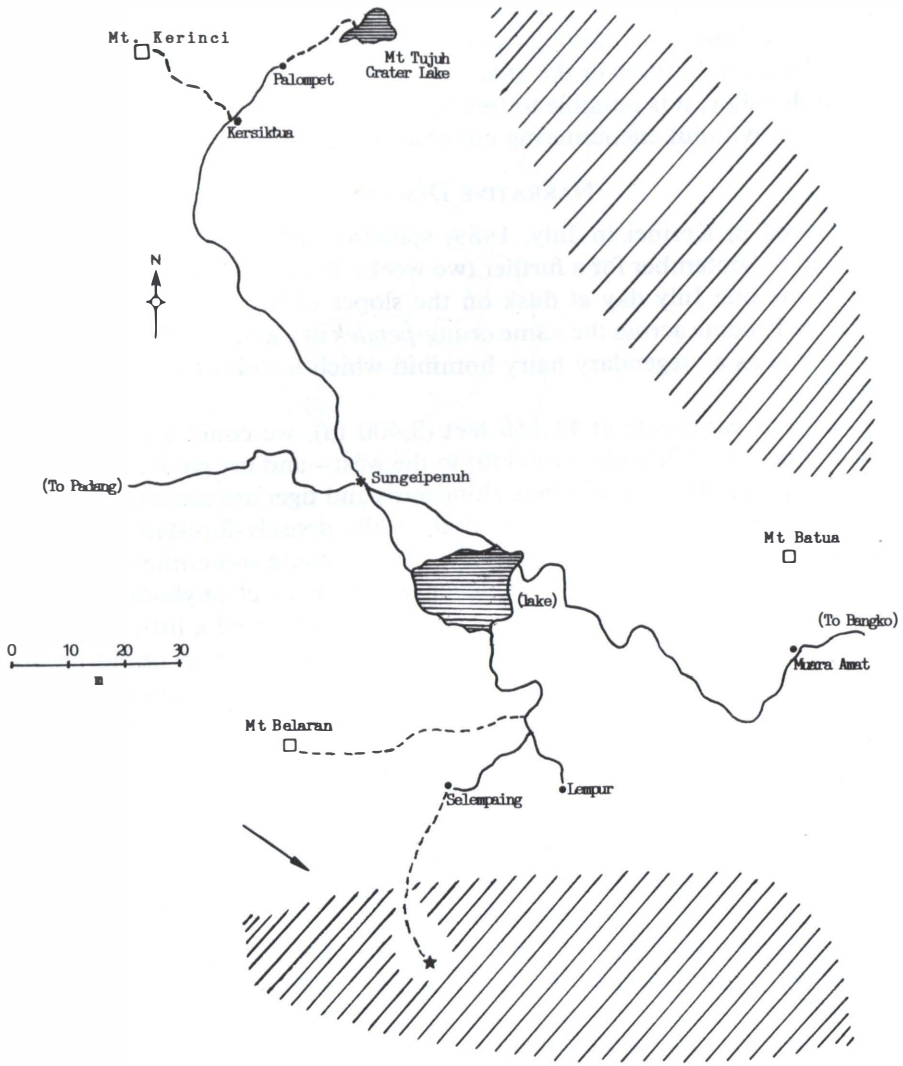

FIG. 2.—The Mt. Kerinci region, showing villages visited by the author. Diagonal shading indicates areas where native people believe *orang-pendeks* live. These areas are in high and remote heavily-forested mountains. The broken line south of Selempaing indicates trek taken by author to where *orang-pendek* tracks were found—see arrow. (Scale only approximate; map not necessarily authoritative.)

Despite this setback, I was able to gather a detailed description of the *orang-pendek* by talking to headmen in villages up to 60 miles (100 km) apart. The older men were more helpful. The younger village men appear reluctant to enter the forest, and on the two occasions that I hired young

guides, they succeeded in getting lost—a not very pleasant experience. The *orang-pendek* is described by these older witnesses as ranging from 3 to 5 feet (1–1.5 m) in height. The forehead is high, and the ears are more prominent than those of the siamang. All reports included the information that the animal has a large and prominent belly—something not mentioned in previous literature on the subject. The animal was described as heavily-built, not the slender little creature I was later to see illustrated in a book by Bernard Heuvelmans (1958, *On the Track of Unknown Animals,* Ruper Hart-Davis, London).

A number of native people remarked that, although the Short Man normally boasts a long, black mane of hair extending almost to the base of the spine, in some cases this mane may be dark yellow or tan. The body is covered in a light coating of black or dark-grey hair, thicker on the limbs than the body. Villagers initially insisted that the animal has arms the same length as in humans. However, when questioned further, it became plain that the index finger extends to, or just below, the knee.

One witness, an elderly *dukun* or traditional doctor living in a settlement near Muara Amat, provided the additional comment that, when running, the Short Man may hold out his arms (for balance?). All witnesses insisted that *orang-pendeks* are bipedal, although some sources claimed that they walk with their weight placed on the outer edge of the foot. Not one person suggested that *orang-pendeks* have reversed feet. I placed least credence in reports in which hunters claimed to have seen *orang-pendeks* in the forest, since it is possible they may have seen sun bears or siamangs. The most reliable sightings—five—were from villagers who had seen an *orang-pendek* in their *ladang* fields at the edge of the rain forest. One of these sightings came from the Mt. Kerinci area, near the village of Palompet. The remainder came from settlements in the area around the small town of Lempur, 33 miles (54 km)—two hours by bus—southeast of Sungeipenuh. All these sightings appear to have been made before 7 a.m. and after 3 p.m. In all but one reported sighting, the animal had been feeding on sugar cane; another report involved feeding on bananas. Villagers said they thought that the *orang-pendek* lives on the forest floor, although it clearly does utilize the trees.

One 32-year-old man, from near Palompet, described his sighting thus: "I was in my grandfather's house [a bamboo hut] in his fields, and looked out and saw two *orang-pendeks.* One was bigger than the other. They were eating sugar cane, I went out to look at them more closely. The big one saw me. Then they both ran away. They ran away like a man. Quite fast." Upon further questioning, the witness stated that the *orang-pendek* looked like a small, hairy human. He also stated that the animal is not a human, but also insisted that it is not a monkey. He became offended, as were other villagers, when I suggested that it might be either a sun bear or a siamang. Witnesses

also dismissed the suggestion that their sightings were of members of the primitive aboriginal Kubu tribe.

The presence of a second *orang-pendek* appears in a number of reports. However, no villagers informed me of having observed an infant or juvenile.

As time passed, I changed my method of questioning—quite shamelessly—in a bid to prompt certain answers. "He is the same as *orang-utan,* isn't he?" I would suggest. Such suggestions were greeted with a degree of outrage. There are no recorded sightings of orang-utans in southwestern Sumatra. That ape species, so far as is known, is restricted to just one area of northeastern Sumatra. Villagers also firmly denied that they could have mistakenly identified a sun bear or a siamang. Both animals are common in Kerinci, and my witnesses insisted they knew the difference between an *orang-pendek* and a siamang or a sun bear.

One elderly resident of Palompet insisted on demonstrating the difference between a siamang—caught rifling his sugar canes—and an *orang-pendek:* "*orang-pendek,* walks like a man; *orang-pendek* is not a monkey *ibu* [Lady]," I was told by a deeply affronted village elder.

There appear to be no mystical properties attached to the *orang-pendek* of Kerinci. Also, had even one villager told me that the *orang-pendek* kidnapped women or curdled milk, I would probably have dropped my investigation.

At the end of July, I left Kerinci and travelled firstly south, to Bengkulu Province, where I spent some time in the region of Curup and the Bukit Kaba volcano. Virtually no rain forest survives in that area, and villagers appeared to know nothing of *orang-pendeks.* It was only later that I learned that Bukit Kaba was the site of one of the most famous European accounts of an *orang-pendek* (Heuvelmans, 1958, above). From there, I travelled north to Padang, and then to Bukitinggi, in the Minangkabau, some 155 miles (250 kms) north of Sungeipenuh. Once again, *orang-pendeks* appeared quite unknown—although, upon my return to Britain, I was to learn, again, that the animal had been reported there in the early years of this century.

At the beginning of September, I traveled south to Kerinci once more, where I met the headman of Selempaing village (see Fig. 2). He claimed that a female *orang-pendek* had been seen twice in his village fields within recent weeks. With the rains fast approaching, and my Indonesian visa expiring at equal if not greater speed, I made my way to Selempaing, in the mountains, some 30 miles (50 km) southwest of Sungeipenuh—via possibly the worst road in Sumatra. The village is located at around 3,600 feet (1,100 m) above sea level, on the edge of virgin rain forest. There are few trails of any description, and reaching the village involves not only climbing steep ridges, but fording fast-flowing rivers and suffering the constant attentions of leeches. A *parang* (machete) is essential.

There are a number of Sumatran rhinos in the area. I was also told, on

FIG. 3.—Heavily-forested river valley in which the author found *orang-pendek* tracks, one day's trek south of the village of Selempaing.

my first evening in the village, that a local man had been taken by a tiger two years earlier. Since our protection consisted of the village headman's small air-rifle, this did little for my self-confidence.

My guides, the Selempaing headman and a former rhino poacher named Musih, had warned me that it could be weeks before I might see an *orang-pendek.* I reassured them that all I wanted was some evidence of the animal's existence. They were disbelieving when I explained that *orang-sarjana*—scientist man—did not know about *orang-pendek.* Musih said he believed it would be possible to catch an *orang-pendek* using a snare or a net, but said no local people would cooperate if any harm came to the animal. He also insisted that, if an *orang-pendek* were caught, it should be released, and not removed from the forest. I concluded that there is some sort of taboo against killing or injuring this animal.

A full day of trekking south, deep into the mountains, took us to a heavily-forested river valley at an altitude which I estimated to be at around 4,600 feet (1,400 m) (Fig. 3). There we found tracks of what appeared to be two different *orang-pendeks* (see Fig. 2). This assumption was based not on foot size—they were virtually identical—but on the depth of the different tracks on the soft forest loam. There was no possibility that either of my guides had made the tracks.

At one point, I followed the tracks up a steep, muddy bank above a fast-flowing stream for almost 40 yards (36 m) before losing them in a drier area. I concentrated on one particular set of tracks. Each print was clearly delineated, the big toe and four smaller toes easily visible. The big toe was placed as it would be in a human foot. The foot had a clearly-defined high, curved instep. It measured just under 6 inches (15.2 cm) in length, and fractionally under 4 inches (10.1 cm) at the widest point of the ball of the foot. The heel was narrow and well-rounded. If we had been reasonably close to a village, I might have momentarily thought the prints to be those of a healthy seven year-old child.

The ball of the foot was, however, too broad even for a people who habitually wear no shoes, and I also dismissed the possibility of the tracks having been made by Kubu tribespeople.

I am 5 feet, 3 inches (1.6 m) tall, and my left foot is almost exactly 9 inches (23 cm) long. Following this relationship, I calculated that the source of these tracks had therefore been rather smaller—a little over 4 feet (1.2 m) tall. That would make it smaller than the *orang-pendek* described both by the villagers I questioned, and, as I later discovered, by Heuvelmans. However, *orang-pendek* means "Short Man" in Malay, and there are few Kerinci people who are more than 5 feet, 7 inches (1.7 m) in height. It seems therefore reasonable to assume that the native people would not describe an animal as "short" unless it was noticeably smaller than they. The animal's reported broad shoulders and large stomach would increase its appearance of bulk.

I took a number of photographs of the tracks, but they turned out to be of no value due to the poor light and heavy rain conditions. I also made two plaster casts of the tracks, only one of which survived the difficult trek back to Sungeipenuh, and the headquarters of the Kerinci Seblat National Park. I had left Sungeipenuh with the good wishes and frank disbelief of the Park's Director, who did not believe in *orang-pendek* because the local people were "simple." Three days later, I limped back into his office bearing a large and muddy plaster cast. Within an hour, the Director and his deputies expressed agreement that the cast I had taken appeared to come from the track of an animal they did not know. They accepted the fact that it was not from a sun bear or a siamang.

We agreed that the cast should be sent for further examination to the headquarters of the Indonesian National Parks Department, in Bogor. Unfortunately, despite a number of requests, their conclusions have not been forthcoming. Regrettably, I had mistakenly assumed that, since I myself had been able to find a number of tracks of *orang-pendek,* there would be a considerable volume of writings on the subject, and that there would also be plaster casts available. Had I realized at the time that this was not the case, I would have retained the surviving cast, and I also would have taken more care in photographing the actual tracks.

Results

What is the *orang-pendek?* Not, it would appear—in at least some cases—a siamang or a sun bear, although I would expect a proportion of Short Man sightings to be mistaken identifications. I can state that the tracks I followed were not deposited by either a siamang or a sun bear; they appeared to have been made by a bipedal primate. They were essentially human-like in all but the fact that they were too broad.

After a total of seven weeks in Sumatra investigating this animal, I established that it appears to be restricted to the Kerinci region of Sumatra, although it seems likely that a similar form is also found in northeast Borneo.

In reviewing all my evidence, as well as the historical evidence, I assign a probability of about 80 percent to there being an unknown bipedal primate surviving in the high rain forests of southwestern Sumatra. If it is ground-dwelling and elusive, as it is reported to be, this could explain how it has escaped zoological notice, and is known only to the native people. I do not believe that it is more than distantly related to the orang-utan.

Future Plans

I plan to continue my investigation of the *orang-pendek* in Sumatra, and I hope to return to Kerinci for this purpose before long, possibly in 1991. I would welcome any specific advice from ISC members on the next phase of my investigation.

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RECENT ADVANCES IN THE SEARCH FOR THE LIVING GIANT GECKO OF NEW ZEALAND

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INTRODUCTION

Hoplodactylus delcourti is the world's largest gekkonid lizard (Aaron M. Bauer and Anthony P. Russell, 1986a, *Hoplodactylus delcourti* n. sp. [Reptilia:Gekkonidae], the Largest Known Gecko, *New Zealand Journal of Zoology*, Vol. 13: 141–48; Anthony P. Russell and Aaron M. Bauer, 1986b, Le Gecko Géant *Hoplodactylus delcourti* et ses Relations avec le Gigantisme et l'Endemisme Insulaire chez les Gekkonidae, *Mesogée*, Vol. 46: 25–28). It is believed that the *kawekaweau* of Maori folklore is referrable to this taxon (Aaron M. Bauer and Anthony P. Russell, 1987, *Hoplodactylus delcourti* [Reptilia: Gekkonidae] and the Kawekaweau of Maori Folklore, *Journal of Ethnobiology*, Vol. 7: 83–91).

The survival of the species in New Zealand remains in question, although no hard evidence for its occurrence in this century has come to light. Anecdotal and folkloric sources suggest that the former range of this or similar species may have included much of the North Island of New Zealand. The authors (Bauer and Russell 1986a, above) have suggested that the remaining rugged and heavily forested regions of the far north of the North Island might yet harbor populations of these lizards.

During an August, 1988, visit to New Zealand, the authors made a short reconnaisance trip to Northland (Fig. 1) in order to examine likely habitats of *Hoplodactylus delcourti*, and to gather additional information to interpret its probable ecology. W. Gilbert Mair (1873, Note on Rurima Rocks, *Transactions of the New Zealand Institute*, Vol. 5: 151–53) stated that the *kawekaweau* was associated with large forest trees, such as the rata. Given the association of the next largest gecko species, *Rhacodactylus leachianus*, with such forest habitats in New Caledonia, we considered such forested habitats as suitable refugia for the *kawekaweau*, and decided to examine the kauri and rata forest of the Northland region. In particular, the vicinity of the Waoku Plateau had been mentioned as a 19th century locality for this or a similar species (P. Walsh, 1905, A Rare Saurian, *Transactions of the New Zealand Institute*, Vol. 37: 351–52).

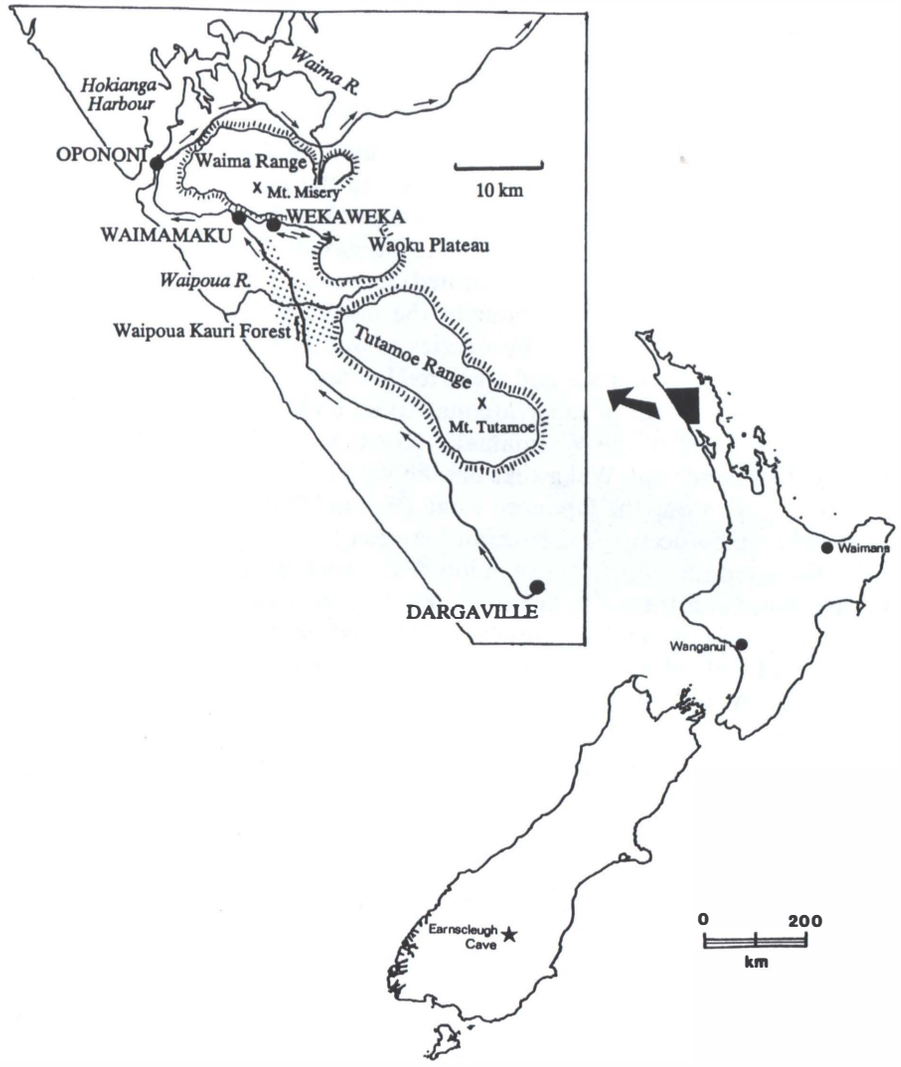

FIG. 1.—Map of west central Northland illustrating the route taken and localities mentioned in the text. The area of kauri forest indicated by the dotted pattern centered around the Waipoua River. Inset shows the region discussed (solid rectangle) in relation to other purported *kawekaweau* localities in New Zealand.

Our goal was to evaluate the area's suitability for supporting the giant gecko, and to assess the degree of disturbance in the region in order to gauge the likelihood of its persistence. No effort was made to actually locate and capture a specimen of the *kawekaweau* because 1) the terrain was difficult, and the most likely sites of occurrence were high in the forest canopy, and

2) minimal activity would be expected during the cool southern winter when the fieldwork was conducted.

Narrative Description

A total of three days was spent in Northland. On August 7, the authors and Grant Macredie, of Auckland, traveled to the region from Auckland. On August 8 and 9 a return trip was undertaken by the authors alone. Departing from Auckland we proceeded to Dargaville, located on the Wairoa River. All sites investigated were located between Dargaville in the south and Opononi at Hokianga Harbour in the north. The large forests of the region are bordered to the east by a series of mountains, from which rivers drain west to the Tasman Sea and north to Hokianga Harbour. The Tutamoe Range lies to the south of the Waipoua River, and the Waima Range (Fig. 2) lies to the north of the Waimamaku River. Between these small ranges and directly inland from Wekaweka lies the Waoku Plateau. From Dargaville we drove north along the Opononi Road (Route 12) to the Waipoua Forest Sanctuary, which occupies the region between the Waoku Plateau and the coast. We continued north through the Waipoua Forest kauri stands to the Wairau River and thence to Waimamaku, located on the river of the same name. We followed the Waimamaku Valley east on the Wekaweka Road to its end, and walked approximately 1 mile (1.6 km) up the Waoku Track towards the Waoku Plateau.

A heavy downpour made progress slow, and few observations could be made; consequently, we returned to Waimamaku and continued north to Opononi and Hokianga Harbor. En route, we made stops in the Waima Forest, near Mt. Misery, in the Waima Range. Our return to Auckland was via Waima, where we crossed the Waima River—site of the lizard observation reported by Walsh (1905, above)—Kaikohe, Kawakawa, and Whangarei. The second trip retraced the first as far as the northern edge of the Waipoua Forest Sanctuary, and observations were concentrated within the sanctuary itself.

The Waipoua Forest Sanctuary occupies 22,500 acres (9,105 ha) in the eastern part of the Waipoua Forest, approximately 30 miles (50 km) north of Dargaville. The area ranges from 600 to 1,987 feet (182 to 606 m) in elevation, and is hilly throughout. Many small streams cross the area, and are drained by the Waipoua and Wairau Rivers, both of which flow westward into the Tasman Sea. The sanctuary contains approximately 6,424 acres (2,600 ha) of kauri (*Agathis australis*), the largest existing stand in New Zealand. Kauris are by far the largest trees in the forest, reaching heights of over 164 feet (50 m) and girths of over 50 feet (15 m) (Fig. 3). The kauris are found in a mosaic of other trees, including rata (*Metrosideros robusta*), taraire (*Beilschmieda tarairi*), towai (*Weinmannia silvicola*), totara (*Podocarpus totara*), rimu (*Dacrydium cupressinum*), kahikatea (*Podocarpus dacrydioids*), and rewarewa (*Knightia excelsa*). The forest is also characterized by the presence of tree ferns, palms, climbers, epiphytes, and mosses (Fig. 4). Similar forests were examined during our visit to the the Waima Forest to the north. The area generally receives more than 71 inches (1,800 mm) of rain per year (W. R. McGregor, 1948, *The Waipoua Kauri Forest of Northern New Zealand,* privately published, Auckland), and mean annual temperatures are mild, ranging from 13° to 16°C (John Ogden and Moinuddin Ahmed, 1989, Climate Response Function Analyses of Kauri [*Agathis australis*] Tree-Ring Chronologies in Northern New Zealand, *Journal of the Royal Society of New Zealand,* Vol. 19: 205–21).

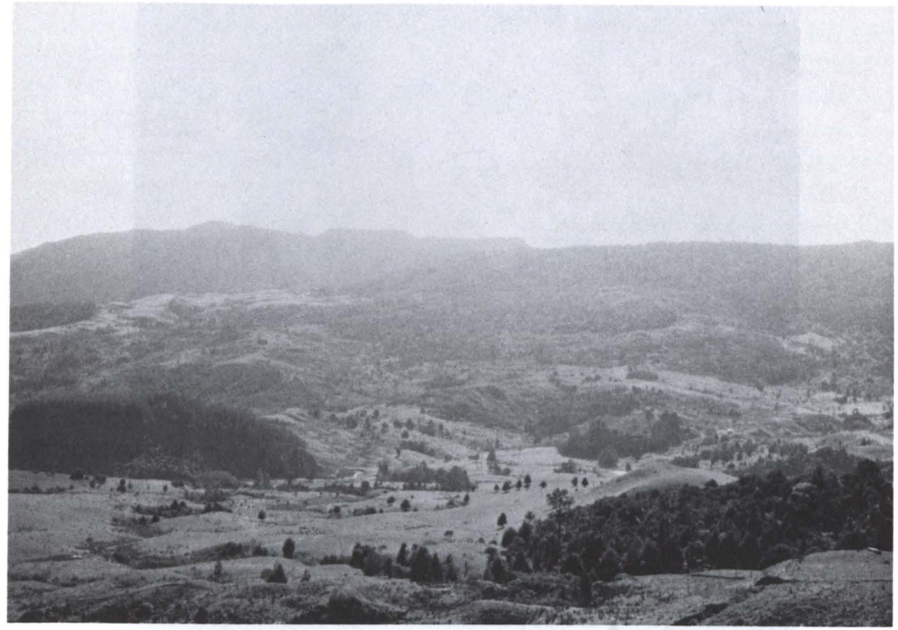

FIG. 2.—View through lowland agricultural land to the western slopes of the Waima Range.

The largest kauri trees have huge columnar trunks—girths of 50 feet (15 m) or more at 15 feet (4.6 m) height—which do not branch below 39 feet (12 m). The spread of the canopy of the largest tree, the "Tanemahuta" is about 11,600 square feet (1,078 sq. m) (Anonymous, 1987, *Waipoua Forest Sanctuary Northland Conservation Park,* Department of Conservation, Auckland).

Results

Mature kauri trees in Northland are several hundred years old (Ogden and Ahmed, 1989, above), and their persistence is evidence of minimal disturbance of the area. The Waipoua Forest has been under government

FIG. 3.—Base of a large kauri tree "Te Matua Ngahere," 53 feet, 10 inches (16.4 m) in girth. Anthony P. Russell (left) and Grant Macredie flank the tree.

control since 1876, and has had the status of a sanctuary since 1952. Prior to 1926, the forest was not accessible by road. Although logging and kauri gum collection occurred in some regions of the forest, much of the region we visited experienced little or no impact from these human activities. This region, thus, appears to be a prime potential refuge for *Hoplodactylus delcourti*. The canopy of the kauri forest constitutes an immense, three dimensionally complex and almost completely inaccessible habitat. Typically, it is such habitats that are the least well studied and understood. Elsewhere in the world, canopy dwelling lizards are only now being discovered and described in detail. While there is no direct evidence to link the *kawekaweau* to the canopy, the connection is plausible, and the canopy is perhaps the most likely zone for locating living specimens. The mountains surrounding the forest, especially the Waoku Plateau, remain sparsely populated and poorly accessible, perhaps providing some hope of the lizard's survival, and some explanation for its successful avoidance of detection.

FIG. 4.—Dense vegetation in the moist understory of the Waipoua kauri forest.

Whether extinct or merely rare and cryptic, the causes for the apparent decline of the *kawekaweau* in European times remain unclear. The authors have previously suggested that the introduction of rats may have negatively influenced populations, as it has in other reptile species (Anthony H. Whit-

aker, 1978, The Effects of Rodents on Reptiles and Amphibians. *In* R. R. Dingwall, J. A. E. Atkinson, and C. Hacy [eds.], *The Ecology and Control of Rodents in New Zealand Nature Reserves,* Department of Lands and Survey, Information Series No. 4, Wellington). However, the size of the *kawekaweau,* and its suggested microhabitat in the kauri or rata canopies, might have afforded some degree of protection from these mammals. Another possibility is that there may have been a natural reduction of suitable habitat. Although authors differ in their interpretations of the optimal growing conditions for kauri, there is evidence of a decline in populations in Northland in the last 2–3,000 years, prior to the arrival of humans and introduced predators in New Zealand (J. R. Dodson, N. J. Enright and R. F. McLean, 1988, A Late Quaternary Vegetation History for Far Northern New Zealand, *Journal of Biogeography,* Vol. 15: 647–56).

Future Plans

Recent interest in the *kawekaweau* has been fostered by the 1990 display of the holotype specimen—the only specimen known to exist—as part of a larger exhibition ("Forgotten Fauna—New Zealand's Amphibians and Reptiles") at the National Museum of New Zealand in Wellington. This display has prompted a number of informants to relate their observations of animals purported to be the *kawekaweau.* During the period February to April, 1990, several sightings were reported in the daily press in New Zealand, raising further the profile of the search for this animal. Many of the recent observations seem to be centered around Gisborne, far to the south of Northland (A. H. Whitaker and B. W. Thomas, 1990, *Large Lizard Sightings in the Gisborne Region: Report on a National Museum Investigation, 7–11 April 1990,* National Museum of New Zealand, Wellington). However, eyewitnesses from the area of the Waipoua Forest and Waoku Plateau have also been located (Grant Macredie, personal communication). The many sightings reported have prompted local herpetologists to investigate and follow up on sightings. While most of these have been dismissed for one reason or another, several appear not to be explainable on the basis of known animals (A. H. Whitaker and B. W. Thomas, 1990, above).

Subfossil remains perhaps referable to *Hoplodactylus delcourti* (Aaron M. Bauer and Anthony P. Russell, 1988, Osteological Evidence for the Prior Occurrence of a Giant Gecko in Otago, New Zealand, *Cryptozoology,* Vol. 7: 22–37) were found at Earnscleugh Cave, in Otago, South Island (Fig. 1). The original bones were not seen by us, but photographs kindly provided by Anthony H. Whitaker, acting herpetologist at the National Museum of New Zealand, who located the original specimens, are consistent with the interpretation that they belong to a very large gecko.

The renewed interest in the *kawekaweau* and the tangible evidence of a large gecko in the South Island of New Zealand lead us to believe that additional evidence of *Hoplodactylus delcourti* may be found in Northland. The habitat investigated in the Waipoua and Waima forests does appear suitable to support a large gecko species. Although we remain skeptical that the species is extant, we feel that every effort should be made to remain vigilant for its possible survival in the areas identified by historical and folkloric sources.

Our future plans include a thorough survey of sightings throughout New Zealand, with emphasis on the Northland District. In particular, we hope to concentrate on the testimony of local Maori residents. Information from native peoples have been shown to be reliable in determining certain aspects of vertebrate distribution, especially in cases where the animals in question were important to the mythology or tradition of the people (Andrew A. Burbidge, Ken A. Johnson, Phillip J. Fuller and R. I. Southgate, 1988, Aboriginal Knowledge of the Mammals of the Central Deserts of Australia, *Australian Wildlife Research,* Vol. 15: 9–39). We also advocate the organization of teams of observers to spotlight in the kauri and rata canopies at night to look for signs of *kawekaweau* activity.

We thank Grant Macredie for suggesting specific sites to us, and for accompanying us on one of the reconnaissance trips. Anthony Whitaker kindly provided us with photographs of the gecko bones described by Hutton, as well as with copies of recent newspaper accounts pertinent to the giant gecko. Lawrence Auerbach furnished additional reference material.

Cryptozoology, 9, 1990, 74–78

THE EASTERN PANTHER ON FILM? RESULTS OF AN INVESTIGATION

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INTRODUCTION

The continued occurrence of the panther or puma (*Felis concolor*) in northeastern North America has been debated for decades (Bruce S. Wright, 1972, *The Eastern Panther: A Question of Survival,* Clarke, Irwin, Toronto; Robert L. Downing, 1984, The Search for Cougars in the Eastern United States, *Cryptozoology,* Vol. 3: 31–49; *The ISC Newsletter,* 1990, The Eastern Puma: Evidence Continues to Build, Winter). The spring, 1990, videotaping in New Brunswick, Canada, of a large felid in the wild brings the question of the Eastern puma (*F. c. couguar*), a subspecies considered extinct by many authorities, into a new dimension. It is possible that the filmed individual represents this very subspecies. Regardless, never before has a free-ranging puma—wild *or* escaped/released captive—been photographed or filmed in the Northeast. I hope that, with this evidence, skeptics will be hard pressed to continue denying the species' presence there.

NARRATIVE DESCRIPTION

In June 1990, Ted Reed, founder and director of the New Hampshire-based Friends of the Eastern Panther, contacted me to discuss my willingness to travel to New Brunswick to meet with provincial wildlife officials and view a privately-owned videotape, made on 15 May, 1990, of an unidentified felid thought by some to be a puma. Having long been interested in the possible existence of the Eastern puma, and having more recently assisted in extensive field work tracking, capturing, and radio-tagging pumas in Yellowstone National Park, I readily agreed.

Reed and I viewed the 10-minute videotape on July 6 at the New Brunswick Department of Natural Resources and Energy (NBDNRE) office in Fredericton. Our host was regional wildlife biologist Gary Moore, who, along with furbearer specialist David Cartwright, the acting Chief of Wildlife Management, had been our primary contact in arranging the screening. Accompanying the video was an impressive array of still and aerial photographs, measurements (made using survey stakes and the known location of the animal, certain trees, and camera at various times), and a reenactment videotape—all assembled by Moore and his colleagues. The latter item, produced using the original camera in its original positions, was especially noteworthy because it featured a 40–50 lb (18–23 kg) chocolate-colored

Labrador retriever (*Canis familiaris*), an animal that compared favorably in general size with the cat in the original film.

After replaying the video several times, Moore, Reed, and I compared notes, discussed our individual thoughts and impressions, and reviewed Moore's on-location tests and trials, as well as the pending work of the Royal Canadian Mounted Police (RCMP) crime laboratory in Ottawa.

From the DNRE facility, we drove to the Waasis, New Brunswick home and workshop of the videotapers, Donna and Roger Noble, where we conducted an hour-long interview. We spent the remainder of the day reconnoitering the area where the video had been taped.

RESULTS

Gary Moore and David Cartwright are to be highly commended for their handling of this incident, from their courteous treatment of the video's makers-owners to their solicitation of outside expert opinion—including that of the renowned RCMP crime lab—as well as their own painstaking and time-consuming measurements and analyses. Their work is a credit to the province.

Filmed in late afternoon along the mixed forest border of a south-central New Brunswick farm field, the videotape is hardly a masterpiece of modern cinematography. Made by Roger Noble while Donna, his sister-in-law—the camera's owner—shouted instructions and looked on from afar, his unfamiliarity with the device—and his breathlessness after seeing the cat, charging from their workshop, and calling for the camera—are evident. Replete with nervous wavering, long-distance shots, poor resolution, and irrelevant aiming, the video reminded me of the famous Patterson Sasquatch film.

Nonetheless, there are several clear sequences of the animal performing a small repetoire of behaviors—walking, crouching, hiding, leaping. The cat's color in the low-angle evening light against a green backdrop is tawny orange. Its underside and inner legs were creamy white. Interestingly, this 2-tone pelage was essentially identical to that of whitetailed deer (*Odocoileus virginianua*) encountered during the visit, the animal that presumably would make up the bulk of the Eastern puma's diet.

The cat's build, particularly in the shoulders, was impressive, its sinewy muscles rippling through its immaculate coat as the animal walked the edge of the grassy field. It was initially unconcerned, apparently, by the two people nearby; the distance between the cat and the witnesses ranged from about 160 to almost 500 feet (~ 50–150 m). The cat's thick, cylindrical tail was ½–⅔ the length of its body, and typically held low, drooping almost to the ground, then curling up at the tip. The animal's sex was not discerned.

The cat's relative proportions were best visualized in a 5–10 second sequence when it stepped out of the patchy 2–5 inch (5–13 cm) grass of the field onto a dirt road. It paused, looking at the camera with its body at a

slightly oblique angle (Fig. 1). It then returned to the forest border. In the forest shadows, the cat's fur took on a more tan hue.

In the final scene of the video, the cat, still in the shadows, leaps upward and out of view into the woods, as if into a nearby tree. It was in that brief footage that the animal's heft, and its lithe and fluid grace, were best exemplified. The effect was impressive and very convincing.

Because of the lack of known-size reference points with which to simultaneously compare the cat during the video, it was very difficult to accurately estimate the animal's size. My impression was of a healthy young puma. Based on their use of film superimposition techniques, geometry, and the dimensions of several trees measured subsequently, experts at the RCMP crime lab arrived at a standing body length estimate of 20–25 inches (51–64 cm). This does not include the tail. The crime lab found no suitable points at which to measure the cat's height. I estimate that 16–20 inches (40–50 cm) from the ground to the top of the walking cat's head would be realistic.

Our search of the area failed to yield any puma sign, but that was not surprising; the incident had occurred 2 months earlier, and in the subsequent time the field had been mowed and the area trampled. The absence of claw marks on the trees seen in the final sequence led us to conclude that the cat had not leapt into a tree, but into the forest and shadows beyond.

As for the witnesses, never have either doubted that what they saw was indeed a puma, and both have been overwhelmed by all the attention they, their quaint farm, and videotape have received. Mrs. Noble, the mother of several young children, was particularly unsettled by the sighting. Clearly, the animal, with its size, grace, and mystique, had left her awed and more than a little frightened.

Moore, Reed, and I all concluded that this tawny, unspotted, long-tailed cat was a puma. Based on experience with western pumas, I placed it at about 8–12 months old. I think the cat was probably a subadult recently separated from its mother and wandering in search of a home range. Contact with humans had presumably been minimal or nonexistent. A statement detailing these conclusions was submitted to the DNRE at Gary Moore's request.

The origin and subspecific status of the cat will likely remain unknown, and in any case may be immaterial. That the cat was alive and apparently healthy, managing to survive in the New Brunswick forests, is sufficient. There are probably others. DNRE biologists continue to receive puma reports from all over southern New Brunswick, just as Bruce Wright did in the 1950's, 1960's, and 1970's.

The original videotape remains in the possession of the Nobles. They have authorized use of parts of it to various periodicals and news agencies. In September, 1990, the DNRE issued an official press release detailing the incident and its handling. A copy of the videotape and related materials are

FIG. 1.—Image of a large felid from the videotape taken in Waasis, New Brunswick, in the spring of 1990 by Roger Noble. The author concludes that the felid was a subadult puma, *Felis concolor*, possibly representing the thought-extinct Eastern puma subspecies, *F. c. couguar*.

available for review at the DNRE office, R.R. 6, Fredericton, New Brunswick, Canada E3B 4X7.

FUTURE PLANS

In cooperation with Friends of the Eastern Panther, a series of puma tracking and sign identification workshops has been scheduled throughout southern New Brunswick and New England for early 1991.

Additionally, a proposal for a puma sign survey in the Fundy National Park area of New Brunswick—a rugged, untamed region that traditionally has held promise as one of the last strongholds of the Eastern puma—has been submitted. It is Ted Reed's hope that this wild and scenic area, long threatened by major development, will ultimately receive recognition and protection as an expansion of Fundy National Park—the Bruce S. Wright Panther Preserve.

I am greatful to Robert L. Downing and Ted Reed for reviewing this report and making helpful suggestions. I also wish to thank Joyce and Jerry Wiland, who continue to perform innumerable administrative functions. Also, I extend special thanks to Margaret Gurnett, of Bozeman, Montana, and Charles

Craighead, of Moose, Wyoming, for providing office space and equipment during preparation of this manuscript.

Postscript: It is with great sadness that I report that, on October 23, 1990, while returning home from a meeting with the provincial Minister of Natural Resources and other interested parties, David Cartwright, 43, died in a auto accident. He will be missed by all who knew him. Regrettably, he will never know the success of what only hours earlier he had helped create—the New Brunswick Panther Recovery Team.

Cryptozoology, 9, 1990, 79–81

LCPI WORK AT LAKE CHAMPLAIN, 1990

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INTRODUCTION

The search for the Loch Ness monster-like animals (Champ) of Lake Champlain by the Lake Champlain Phenomena Investigation (LCPI) in 1990 saw a scaled-down effort compared to the intensive work of the 1987 to 1989 expeditions. More conventional forms of searching, such as surface surveillance with cameras/binoculars and scuba, were emphasized.

This field report reviews the LCPI's fieldwork in 1990, and the reported Champ sightings for that year.

The LCPI's 1990 expedition efforts were directed at: (1) surface surveillance from shore and boat using cameras/binoculars; (2) scuba searches for a Champ carcass; (3) a Klein side-scan sonar search for a Champ carcass; (4) encouraging Champ eyewitnesses to report their sighting(s); and (5) to inform and educate people about this scientific mystery.

NARRATIVE DESCRIPTION

The LCPI conducted 22 days of fieldwork at Lake Champlain during 1990. A Klein 590 Dual Frequency (100/500 kHz) Side-Scan Sonar was provided to the LCPI courtesy of William Key (President) and Garry Kozak (Regional Sales Manager) of Klein Associates, Inc., of Salem, New Hampshire. Douglas Leininger provided the use of his runabout boat for the sonar surveying. All other equipment was provided by the LCPI or by individuals working with the LCPI.

The dates of the 22 days of fieldwork were: June 9, 23–24, 27–29; July 21; August 2, 11–16, 18–24; and September 28. Zarzynski participated in all 22 days of the 1990 LCPI fieldwork. Others who participated were: Bob Benway (June 9, 23–24; July 21); Pat Meaney (June 23, 27–29; August 11–13, 18–20, 22–24); Vincent J. Capone (June 23–24; August 18–19); Douglas Leininger (June 24); Russell Bellico (July 21; August 2, 19, 23); Captain Dan Couture (July 21; August 2, 23); Donald Mayland (August 15, 19); Duncan Mathewson (September 28); Joe Bereswill (September 28); and Jason Bacon (September 28).

Surface surveillance using cameras/binoculars was conducted on all 22 days. Sixteen dives were made at Lake Champlain on: June 6 (2 dives), June 23 (one dive), July 21 (two dives), August 2 (three dives), August 15 (one dive), August 19 (one dive), August 23 (two dives), September 2 (two dives), and September 28 (two dives).

On June 24th, one day of side-scan sonar searching using a Klein 590

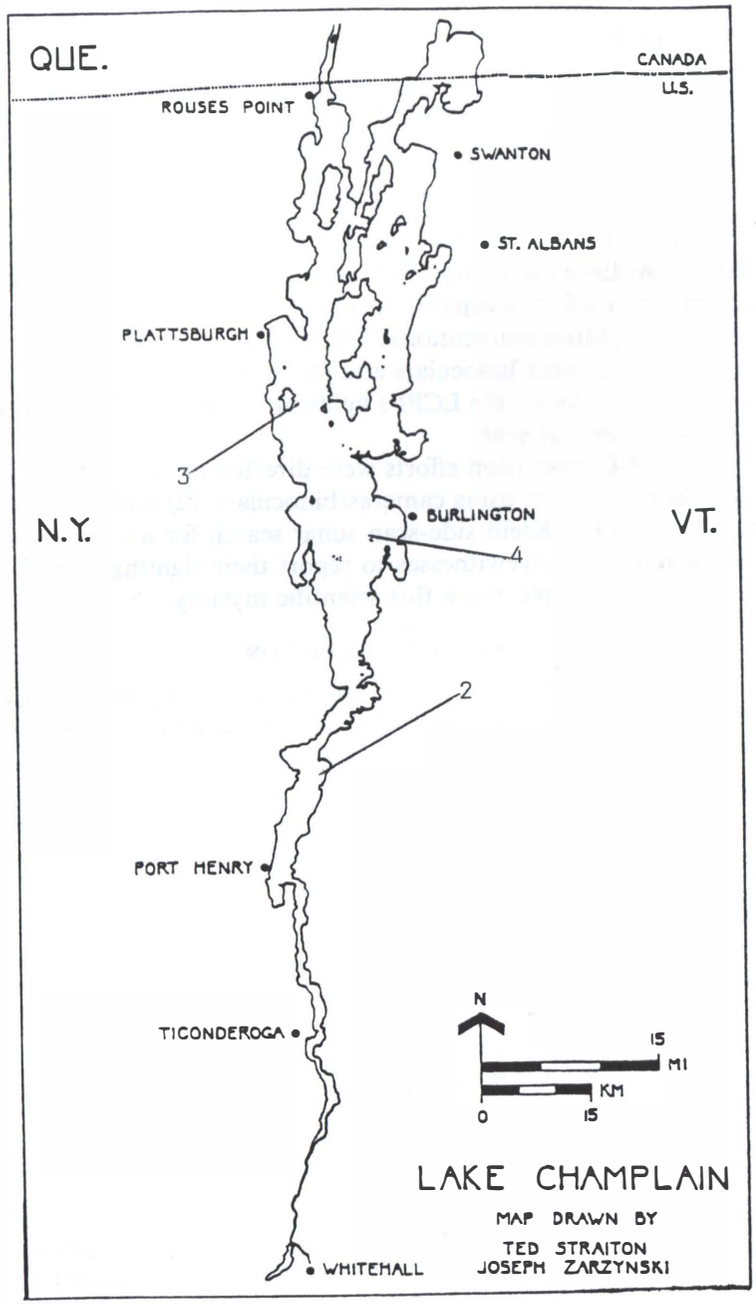

FIG. 1.—Map of Lake Champlain, with numbers indicating the locations of the eyewitness sightings reported to LCPI in 1990. The location of sighting #1 is unknown.

Dual Frequency Side-Scan Sonar was conducted in an attempt to locate a Champ carcass. Vincent J. Capone, President of Marine Search and Survey, of Princeton, New Jersey, acted as primary sonar operator.

RESULTS

No sighting of a Champ-like animal was made by personnel of the LCPI during the 1990 fieldwork.

Four 1990 Champ sightings were reported to the LCPI (Fig. 1). These sightings are listed below:

(1) Summer of 1990 (exact date not recorded): Mr. and Mrs. Charles Weinberg and twin daughters; observed "the phenomenon we call Champ" from their 36-foot (11 m)-long houseboat, *Crane*; no details provided on location, date, time, range, etc.

(2) August 16, 1990: Bob Kambic and family; near Button Bay, Vermont; observed from State Park at Button Bay; saw at a distance of 1 mile (1.6 km) to the south, "three low humps in the water"; each hump was "very long"; Kambic said that big fish or birds were diving around the animal; observed using 7 × 35 binoculars; time of day was 2:30 p.m.

(3) September 5, 1990: Ann and Lewis Wetzel; while sailing between Providence Island, Vermont, and Valcour Island, New York; estimated to be about 15–18 feet (4.5–5.5 m) in length; they observed humps about 2½ feet (76 cm) in length, and about 1 foot (30 cm) high; animal described as "a dark color," with skin "smooth with light texture"; no head seen; it was observed using binoculars at a range of about 150 yards (137 m); when the sailboat—which was under motor—neared the animal "it appeared to be frightened . . . and suddenly disappeared below the surface"; the sighting was estimated at between 3:10 and 3:15 p.m.; the Wetzels noted the surface water temperature at 68°F, and the depth of the water at the sighting spot was 195–205 feet (59–62 m).

(4) September 28, 1990: Stephen and Anita Allen and their 5-year-old son, Daniel; north side of Juniper Island, on the Vermont side of the lake; while boating, the family saw 2–3 parts of an animal's dark-colored body at a range of approximately 200 feet (60 m); they observed no head; parts of the body "not viewed as humps, but rather as different parts of a snakelike body"; calm lake surface; the Allens reported that a lot of fish were jumping at the water surface, and that possibly the animal was feeding; the sighting lasted about 30 seconds, and was made at 6:50 p.m.

FUTURE PLANS

The LCPI will continue its on-going research and fieldwork investigating the Champ phenomenon. The 1991 work will use the same methods as employed in 1990. It is hoped that these efforts will lead to accumulating further evidence to support the hypothesis that Lake Champlain is the habitat of a colony of 15–25-foot (4.5–7.5 m)-long animals still unknown to zoology.

Cryptozoology, 9, 1990, 82–84

SASQUATCH INVESTIGATIONS IN THE PACIFIC NORTHWEST, 1990

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INTRODUCTION

Sasquatch, reported to be a large, bipedal primate by many eyewitnesses in the Pacific Northwest, continues to represent an unresolved scientific problem. I have been interested in such reports for over three decades, and, since my retirement as a wildlife biologist with the Oregon Department of Fish and Game, I have had the time to be more active in fieldwork in an attempt to uncover Sasquatch evidence. This report updates my previous findings appearing in this journal (James A. Hewkin, 1989, Continuing Sasquatch Investigations in the Pacific Northwest, *Cryptozoology*, Vol. 8: 73–74).

Six field trips were undertaken into the Blue Mountains of central and northern Oregon in 1990, three of them involving horse-back travel. In addition, several miscellaneous one-day trips were undertaken throughout the year to investigate certain sites in the Cascade Mountains of Oregon.

NARRATIVE DESCRIPTION

During March 18–20, in the company of Jack Sullivan, I inspected areas in the southern part of the Blue Mountains, in central Oregon. I have reported on this area before (James A. Hewkin, 1989, above). We failed to find any fresh activity that could be categorized as Sasquatch-related. A similar trip was undertaken during June 19–20, also with negative results.

In northern Oregon, I was fortunate to have access to a cabin in a rather unique location in the Blue Mountains—just outside the Mill Creek Watershed, at an elevation of 4,600 feet (1,400 m). The persons who lease the cabin use it during hunting seasons and in the summer as an occasional weekend retreat. The Mill Creek Watershed, the site of numerous Sasquatch-related incidents—some of which have previously been reported in this journal—is off-limits to the public. It lies within the Umatilla National Forest, and is managed by the U.S. Forest Service for the city of Walla Walla, across the border in southern Washington State. The state boundary actually bisects the Watershed. Immediately to the east of it lies the Waneha-Tucannon Wilderness, which includes 176,800 acres (71,548 ha). A major portion of the Umatilla National Forest extends southwards into Oregon. The entire area is characterized by rugged and remote terrain.

I was able to have use of the cabin on three field trips for a total of eight days. The cabin lessees, Mike and Sheryl Jenkins, accompanied me on June

8–10. We rode the boundary trail and other areas outside of the Watershed, and observed considerable tree breakage—snapped tops of small fir trees, and broken limbs and twisted tops on some medium-sized saplings. Much of this tree damage was caused by bears, but there was some damage of undetermined origin. On June 10, the Jenkins' departed, and I continued alone until June 13.

I noticed that two buck deer were acting very "nervous" near the cabin, sensing that "something" was near. The next morning, I found and followed fresh cougar (puma) tracks up a trail for nearly a mile; that settled the question of the "nervous" deer. Another incident of interest was encountering a black bear at very close quarters. I had to actively make my presence known, as it was moving towards me on the trail, and had not detected me. I caught its attention by kicking two rotted logs. The bear halted, turned immediately, and rapidly departed—much to my relief.

The bear was very large and quite black and glossy. Bears in the National Forest are usually cinnamon-colored, with darker heads and feet and lighter shades over the body. Sometimes, they are quite blond. On one occasion, when riding with Sheryl Jenkins, I saw a platinum blond-colored bear with two platinum blond-colored cubs. There are many bears in this area. I observed 11 over a four-day period, all colored in various shades of cinnamon—except for the black one I encountered on the trail.

During June 26–29, I worked from the cabin with Jack Sullivan. We hiked over a considerable area of peaks and canyons, observing a lot of bear sign as well as a few bears. We noted a lot of tree damage, broken logs, and rolled-over rocks. On the ridges, we found that bears spend a lot of time scraping the turf for onion bulbs, sometimes scratching up several square yards of turf. It seems that bears are very active in June scratching for onions. While we could easily observe them during the day—out in the open and away from cover—while scratching for onions, no bears were seen at all in July.

During July 6–8, I rode with Sheryl Jenkins on and off the trail extensively. We examined many broken trees. We collected some blond hair samples from a small white fir tree that had recently been broken at the top. Subsequent microscopic analysis showed that it was bear hair.

During the period of July 21–29, I conducted horse-back fieldwork in the Wenaha-Tucannon Wilderness in the company of Sheryl Jenkins and Wes Sumerlin and his wife. Sumerlin has had much experience in the Wilderness, and claims to have had some Sasquatch experiences over the years. We found plenty of bear sign, and broken trees were mostly on the ridges where low passes make attractive crossovers for game animals. These areas are known to be used extensively by bears. Inclement weather kept us under cover for two days. We saw many species, including deer and coyotes, but encountered no Sasquatch evidence.

Postscript: On January 17, 1991, I received a communication from Vance Orchard, in Walla Walla, Washington, informing me that a new series of tracks had been found by Sasquatch hunter Paul Freeman along Mill Creek. The tracks reportedly began in high snow and wandered down through wheatfields and pastureland. I traveled to Walla Walla the next day, January 18, and did an intensive survey of the situation. The tracks went through a plowed wheatfield, and winter wheat was beginning to sprout. The ground was very muddy, as snow had recently softened and melted.

My inspection and analysis of the site and the tracks indicates that they were faked. The tracks were 13 inches (33 cm) in length, and 6 inches (15 cm) in width at the toes. The stride averaged about 30 inches (76 cm), and the toes pointed outward at each stride. One left print failed to mark; it simply left a gash in the muddy soil as if the foot had broken-down sideways. At a wire-fence crossing, I noted a track under the wire—exactly where I would have placed my own foot when crossing over. A Sasquatch would surely have stood further away from the fence when stretching the other leg over the wire. When the tracks led towards the creek, they would turn back towards the road. A Sasquatch would surely have crossed the creek and entered the protected woods away from the road.

In my opinion, the tracks involved hoaxing, and were purposefully produced using cast material that was attached to usable boots.

Future Plans

Further fieldwork to locate Sasquatch evidence will be conducted in various parts of Oregon in 1991. The results will be reported to this journal.

Cryptozoology, 9, 1990, 85–87

BCSCC REPORT ON OKANAGAN LAKE, 1990

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Introduction

The British Columbia Cryptozoology Club (BCCC) underwent a name change in 1990 when it became the British Columbia Scientific Cryptozoology Club (BCSCC). The reason for the change was more than cosmetic. Some pseudo-cryptozoological groups are springing up across North America, and the Club executive decided that, by stressing the scientific tack that the Club has taken in the past, no connection with groups involved in paranormal or occultic activities would be made.

The Club has recently been involved in fieldwork pertaining to Ogopogo, a supposed "lake monster" which inhabits Okanagan Lake, a body of water 79 miles (127 km) in length in southern British Columbia. In past years, Club members have obtained several sightings and videotape sequences of the animals which, as yet, have not been classified scientifically. In 1990, fieldwork at the lake was conducted from August 4 to 29. The participants were John Kirk, John Kirk Jr., Kenneth Shauntz, Andrew Shauntz, and Kenji Chono—a visiting cryptozoology enthusiast from Osaka, Japan.

Narrative Description

Initial observations commenced at Knox Mountain, Kelowna, where superb views of northern sectors of the lake are possible. Sunny skies and the absence of any significant heat haze allowed the BCSCC team to scan the lake for two hours. At approximately 3:30 p.m., the participants noted a large disturbance in the waters below their vantage point, near Bear Creek. Something below the surface was creating large patches of white water, which were about 15 feet (4.5 m) across. Several boaters in the vicinity also sighted the disturbances, but neither they nor the expedition members were able to see what was causing them. Following this occurence, the team moved to a small beach on the Kelowna lakeshore, where views from the Okanagan Mission in the south to the Okanagan Lake floating bridge in the north are possible. Heavy boat traffic in the area may have prevented any sightings from taking place.

On August 5, the Club participants patrolled the lake surface from a motorboat, taking in the area between Peachland and Kelowna. At approximately 2:15 p.m., John Kirk Sr. and Andrew Shauntz simultaneously spotted a 15-foot (4.5 m)-long object floating about 45 feet (13.6 m) behind another boat some 200–250 yards (180–230 m) away. The sun was shining off the back of what appeared to be a living animal. Its coloration was similar

to that of a peeled log; slight movement was detected. Shauntz gunned the engine of the vessel in an attempt to get closer to the object. However, the object submerged, and, upon arrival at its location, nothing was found. The occupants of the other boat were questioned as to whether they had seen the animal, but they had not been looking in that direction, and had seen nothing. No further sightings were obtained that day.

The following morning saw the team station themselves at Okanagan Lake Provincial Park, just north of Summerland. The field workers spent several hours photographing the lake from a small beachfront. Following this exercise, the team relocated to the Ogopogo viewpoint, across from Squally Point, to carry out more observation tasks. At approximately 11:30 a.m., a large group of waterfowl was seen to be agitated by something just below the surface which was heading towards them from about 100 yards (91 m) away. A large shadow was seen to be approaching the birds, which took off in what seemed to be pure fright. The BCSCC group watched the shadowy mass move further into deep water before it was lost to view. There is a possibility that this was not an Ogopogo-type animal, and could have been a sturgeon. However, the British Columbia provincial government has insisted that there are no sturgeon in the lake. No conclusion has been drawn from this episode.

While most members of the fieldwork group had to return to Vancouver after several days at the lake, Kenji Chono remained for a total of 29 days, but without obtaining any further sightings.

Results

Other than the single sighting of the animal on August 5, the expedition was a rather unsuccessful venture, which was complicated by the large number of boats on the lake; the occupants of these boats haphazardly traverse the surface with little or no regard for others. Large patches of oil were seen from leaking motors, which would no doubt cause a problem in keeping the lake habitable for a population of large animals.

Also at the lake during the summer of 1990 was a team from the Nippon Television Network, which despatched a six-boat flotilla in search of Ogopogo. This team sent divers into the lake with underwater photography equipment, and they kept the surface of the lake under surveillance with several other camera crews. One sequence was acquired which the TV crew took to be Ogopogo, but following scrutiny by John Kirk, Paul Leblond, and other faculty of the Department of Oceanography at the University of British Columbia, it was determined that the footage showed nothing more than a converging wave.

However, the Nippon Television team was accompanied by CKIQ radio news director Mike Gussi, who obtained an image of a 30-foot (9.1 m)-long animate object swimming in 320 feet (97 m) of water on his fishfinder. Following inspection by John Kirk and others, it was thought that the image was not of a fish, but of some unknown animal.

Although Nippon TV and the BCSCC were not blessed with clear sightings, several others were more fortunate. Ann and Richard Kline were looking out of their Kelowna home at 10:30 a.m. on July 17 when they spotted a 30-foot (9 m)-long animal with a horse-like head and several humps swimming across the lake in the direction of Westbank. Ann Kline described what appeared to be ears or protrusions emanating from the animal's head, and described its coloration as dark. Approximately one hour later, Bob Pearson and an unidentified friend also sighted the animal in the same area. It reportedly surface on the south side of the Okanagan Lake floating bridge and swam southwards, with several sections of its body appearing above the surface.

On August 13, Mike and Tina Paskal were relaxing aboard their ski-boat just south of Vernon, near Okanagan Landing, when a 60-foot (18 m)-long animal surfaced very close to their craft. Tina Paskal was so shocked she dropped her baby onto the floor of their boat. Mike Paskal had just enough time to grab his still camera and take several pictures of the animal, one of which reportedly shows a large protrusion which resembles part of a whale's back. Efforts to contact Paskal to obtain permission to reproduce his photograph in this journal have proven fruitless.

Future Plans

Both BCSCC and Nippon Television have begun plans to intensify their searches at Okanagan Lake in 1991. There is the possibility that NTV will enlist the aid of BCSCC members, and make use of their knowledge of areas where the animals are more likely to be sighted. It is envisioned that a subsurface vehicle will be used, as well as several remotely operated vehicles (ROV's) and teams of divers. This would make the venture the largest expedition ever mounted in pursuit of the still-elusive Ogopogo.

Book Reviews

Cryptozoology, 9, 1990, 88–98

Exotic Zoology. By Willy Ley. Bonanza Books/Crown Publishers, New York, 1987 (reprint of 1959 edition). 467 pp. n/p. (c.).

It is difficult to comment objectively on this classic volume which, along with the works of Bernard Heuvelmans, has played a key role in exciting and fostering the cryptozoological interests of two generations. *Exotic Zoology* is a compilation and revision of selected pieces from Ley's earlier books, *The Lungfish, the Dodo, and the Unicorn* (1941), *Dragons in Amber* (1951), and *Salamanders and Other Wonders* (1955). The text is decidedly non-technical, and the casual style of referencing sources makes it difficult for the interested reader to pursue topics further. While this is a major drawback from the viewpoint of scholarly cryptozoology, the anecdotal familiarity of Ley's style was useful in disseminating general cryptozoological information to the public. Further, Ley used not only zoological information, but historical, mythological, and linguistic clues, presaging the modern, interdisciplinary approach to cryptozoology. It is in this role, as a great popularizer, that Ley was most successful.

The subject of *Exotic Zoology* is not cryptozoology per se, but zoological mystery. Therefore, much of the discussion centers on the discovery of fossils or living animals that were difficult to reconcile with conceptions about the natural world prevalent at the time of their discovery. However, since cryptozoology is concerned with a substantial subset of such cases, much of the book is directly relevant to the field. Many of the topics discussed are familiar to readers of this journal, and have since been the topics of one or more books unto themselves (e.g., the *sirrush* = Mokele-Mbembe, mammoths, the okapi), or in other cases (basilisks, krakens, dragons) are the subjects of ongoing research reported in our Society's publications or presented at ISC meetings.

While Ley must be praised as a pioneer and popular advocate of cryptozoology, there are numerous faults in his work. For a practicing systematist, one of the most bothersome aspects of the book is Ley's approach to nomenclature. On at least five occasions Ley refers to synonyms variously applied to the organisms being discussed, and appears to suggest that the current usage is almost entirely whimsical, ignoring the rules of nomenclature

with their emphasis on priority and stability. The taxa involved include the fossil *Chirotherium,* ichthyosaurs, the dodo, the tuatara, and the Namib Desert plant *Welwitschia.*

At times, Ley is not credible. Although many of the stories he relates are supported by tangible evidence, there are some, such as the reports of small furry men from East Africa, that are blithely accepted as indicative of unknown animals without much careful consideration of less exotic alternatives. His acceptance of the statement that whale calves may normally spend the four to six weeks prior to birth with their tails protruding from the vent of the mother suggests not only gullibility, but also ignorance of some very basic biological principles. In other cases, although Ley makes laudable efforts to establish the veracity of his sources, he still errs on the side of nonselectivity. For example, he relies heavily (almost exclusively) on the writings of Leguat regarding the extinct avifauna of Rodriguez in the Indian Ocean. Ley goes to pains to dismiss the criticism that had been leveled at the writings in question (the work had long been suspected of being a fraudulent compilation of previously existing travel tales), and concludes that there was no doubt as to the existence of Leguat—and thus the document's authenticity. However, Percy G. Adams, in his book *Travelers and Travel Liars 1660–1800* (Dover Publications, New York, 1987), concluded just the opposite, that there can be no doubt as to the spurious nature of Leguat's narrative, even if the man himself may have existed and actually traveled to the Mascarene Islands.

Many of the criticisms that can be leveled against *Exotic Zoology,* however, are not faults of the author, but reflections of the fact that the text is over 30 years old and consequently does not reflect the current state of zoological or cryptozoological knowledge. Ley's references to the tuatara imply a close relationship with fossil rhynchosaurs, a connection now known to be spurious. Likewise, *Eunotosauras,* believed by Ley to be the progenitor of turtles, is now known to be excluded from ancestry by virtue of fundamental differences in the pectoral girdle and jaw musculature (as inferred from skull morphology). In his discussion of the Abominable Snowman, the "Thyangboche cap" is mentioned along with the earliest analyses of the hair from it. The cap has since been studied in detail, and revealed to be derived from the skin of a serow (*Capricornis sumatraensis*). Ley provides an extensive section on *Archaeopteryx,* but recent work on the origin of birds and allometry in the known specimens, along with the recent discovery of older avian fossils in China, has made obsolete nearly all of Ley's discussion, except the history of the discovery of the first *Archaeopteryx* fossils. Much the same is true of his discussion of fossil hominids, which predates many of the most exciting African finds over the past 30 years, and reflects many views regarding the biology and anatomy of earlier hominid species now known to be incorrect.

A number of errors—based on the information available in 1959—involving both zoological and historical fact also detract from *Exotic Zoology*. Ley, for example, relates that Darwin returned from the voyage of H.M.S. *Beagle* with the theory of *evolution* (my italics) already conceived. Of course, Darwin did not conceive the notion of evolution, but rather the theory of natural selection, a mechanism by which evolution might proceed. (More recent scholarship has also suggested that Darwin's voyage provided fuel for the theory, but that he had not yet derived the theory upon his return to England). Elsewhere Ley mentions "a Dr. Kaup" as a fairly unimportant player in the field of 19th century zoology. In fact Kaup, based in Darmstadt, was one of the preeminent zoologists of his day.

Discussions of anatomy and distribution also suffer from errors, based perhaps on too heavy a reliance on secondary rather than primary sources. Ley mentions the presence in the coelacanth of a spiral valve (a fold inside the intestine to increase surface area), a structure otherwise present "only among the older types of the living sharks." In fact, the spiral valve is widely distributed among fish groups, and is primitive for jawed vertebrates as a whole, occurring in most groups excluding teleosts and tetrapods. Likewise, Ley's discussion of the lungfishes indicates that air breathing is very rare in fishes; in fact 20 or more genera of teleost fishes regularly use this method to supplement their respiratory requirements—although the "lungs" they use are probably not homologous with those of lungfishes or tetrapods. In his discourse on the plant *Welwitschia,* Ley refers to the Namib Desert extending 20 miles (32 km) inland from the Atlantic coast, and remarks that the bulk of the Desert lies within Angola. In fact the Namib extends inland as much as 120 miles (200 km), and the vast majority of its area lies within Namibia. Ley makes frequent reference to sea snakes, and implies that they are restricted to the Indian Ocean. They do, of course, occur there, but are widespread throughout the western Pacific as well.

On the other side of the globe, Ley credits the waters around Newfoundland with being "infested" with giant squid. While there are several records from the region, the total number of giant squid recovered worldwide is hardly great enough to constitute an infestation! Ley mentions "poisonous mud" as the likely agent responsible for killing large numbers of ichthyosaurs in the Bay of Holzmaden, but also notes that sea lilies occurred in the area. It is unclear to me how such sessile bottom dwellers could survive where Ley specifically claims that worms and crabs could not.

The figure quality in *Exotic Zoology* is highly variable, based largely upon the condition of the original figures from which they were compiled. In general, however, figure quality is moderate to poor. The production quality of the book is typical of that found in titles produced for the discount book trade and, although perfectly serviceable, is not particularly attractive.

Exotic Zoology does not constitute a state-of-the-art overview of cryptozoological problems. It is, however, a classic, wide-ranging account of a variety of zoological mysteries and paradoxes, many of them cryptozoological. Although it is long out-of-date, Ley's work must still be regarded as required reading for all cryptozoologists. *Exotic Zoology* has never been a scarce book, but the reprinting of the 1959 edition guarantees accessibility to Ley's highly readable work for a long time to come. Just as Ley's *Dawn of Zoology* (Prentice-Hall, Englewood Cliffs, New Jersey, 1968) provides a history of natural history, *Exotic Zoology* might, in retrospect, be regarded as reflective of a moment in time at the dawn of modern cryptozoology as a discrete discipline. As such, it not only relates the history of cryptozoology, but is part of the history itself. Despite its many flaws, this is a book that should be on every cryptozoologist's shelf.

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Mystery Animals of Britain and Ireland. By Graham J. McEwan. Robert Hale, London, 1986. 224 pp. £10.95 (c.).

This book has six chapters, the first three of which deal with cryptozoology; namely, British Big Cat reports, sea serpents, and lake monsters. The Big Cat chapter is fairly straightforward, but sheds little new light on the subject. While it is a good introduction for the novice, those who wish to pursue the subject in more depth should acquire Di Francis' 1983 book *Cat Country: The Quest for the British Big Cat* (reviewed in Vol. 4 of *Cryptozoology*), and a more recent volume, *Mystery Cats of the World: From Blue Tigers to Exmoor Beasts,* by Karl P. N. Shuker, published in 1989.

The two chapters on British and Irish aquatic monsters are, in my opinion, more stimulating. Although most of the reports have been published previously, there is also new information. For example, Eric C. F. Bird, a coastal geomorphologist at the University of Melbourne, Australia, observed the famous Morgawr sea serpent of Falmouth Bay, Cornwall, when visiting his sister, Sheila Bird, in 1985. In his signed statement—a rare validation from a mainstream scientist—he hoped that "its role in the ecology of the Falmouth/Veryan Bay area [will soon] be clarified."

Anthony "Doc" Shiels, an outrageous magician who has invoked aquatic monsters—and photographed them—hovers about the proceedings at Falmouth Bay, where three young witches bathed nude and uttered incantations to raise the beast. Shiels' own sighting and photography of Morgawr in 1976 would be easy to dismiss, except for the fact that he was in the company of

David Clarke, the editor of *Cornish Life* magazine. In this chapter, and in later ones, the author discusses recurring problems in trying to obtain good-quality photos of cryptozoological critters. Shiels—and the author, it seems—blames such mishaps on the purposeful intervention of a "cosmic trickster."

I personally found the lake monster chapter to be the most interesting. The first section deals with Irish lake monsters, of which we have seen little in other books. Many old Irish beliefs refer to such lake dwellers as "horse-eels." The animal's head is said to look like a horse, but the body is more like that of an eel. It is also said to have the ability to move amphibiously from lake to lake. (This sounds more like an archaeocete than a plesiosaur.) It is very likely that some of the small Irish lakes might have produced better evidence for monsters than has Loch Ness, with all its publicity and big expeditions. But, of course, Loch Ness is where the limelight and money are. Could anybody have raised corporate funds to search for the Lough Fadda monster in Ireland?

Moving on to Scotland, the author reviews reports in six lochs: Arkaig, Eil, Lochy, Morar, Ness, and Urabhal (Isle of Lewis). No explanation is given why these six were exclusively selected. The 1973 book *The Search for Morag,* by Elizabeth Montgomery Campbell and David Solomon, reviewed evidence for monsters in nine Scottish lochs: Arkaig, Assynt, Canisp, Lochy, Morar, Ness, Oich, Quoich, and Shiel. Combining these two lists gives us a total of 11 Scottish monster lakes. This does not include, of course, lakes with "water-horse" folklore but no modern sighting reports.

The Loch Ness section presents 22 pre-1933 monster reports—blissfully omitting the St. Columba incident. This is a very important point, as some Nessie debunkers have claimed that, prior to the 1933 publicity outbreak, there were no reports at all, demonstrating that there was—and is—no monster. McEwan states that he has documentation on 30 Ness reports from between 1850 and 1933, and quite a few of these were land sightings. Curiously, Nessie is never reported today out of the lake. Too many tourists, perhaps?

The only bothersome thing about these pre-1933 reports is that, as far as I can determine, none of them were actually *made* prior to 1933, the witnesses or relatives or friends of witnesses coming forward in 1933 or later. So, one cannot go to a major Scottish library, open an old, pre-1933 publication, and there find, quite unequivocally, a report of a large unknown animal in Loch Ness. While that would be nice, we should also remember that Ness was a little-visited place in those days, and sightings—if any—must have been made by local people, people who would have found no need to advertise it to the world. In fact, it is quite arrogant and culturally provincial to expect that 19th and early 20th century rural Highlanders would have reacted to such sightings in the same manner as mid- or late-20th century observers.

The author goes on to present a number of post-1933 Ness reports, but no explanation is given concerning the criteria for their selection. For such reports, readers would do better with other books, particularly those by the late Tim Dinsdale. What are lake monsters? This is the title of the last section of the chapter. McEwan doubtfully favors a giant eel. However, he spends far more space on psychic or paranormal possibilities, and we return once more to Doc Shiels' "cosmic trickster."

And this brings us to the second half of the book, the chapters on paranormal black dogs and other bizarre creatures, such as the Owlman of Mawnan, the Brentworth Griffin, the Horse-Man of County Lough, and an assortment of other beings such as fairies, demons, and werewolves. In his conclusions, the author presents three possibilities: hallucinations, entities slipping in and out of our reality from other planes of existence, or physical creatures projected—created—by the human mind.

What I find troubling about the author's approach is his lumping of all cryptozoological and paranormal animals together, forcing them into the same three possible molds. While most of the cryptozoological sightings were simply that—sightings—almost all the black dog and other events were what would more appropriately be called "apparitions" or "hauntings." The only reason they are in the book at all is that they are hauntings—whatever that is—*by animals,* but they would be better placed in a book about ghosts. Such animal ghosts have no more place in a book on unknown animals than human ghosts would have in a book about the Yeti or the Sasquatch.

In reality, then, this book represents two books on two different subjects—cryptozoological animals and animal hauntings—placed together under the title of Mystery Animals. This lumping is accentuated by the fact that the author does not anywhere mention the word "cryptozoology"—or the ISC.

Although other books have been superior in the cryptozoological arena, and certainly *Alien Animals* by Janet and Colin Bord in 1981 is superior in the paranormal animal arena (see review in Vol. 2 of *Cryptozoology*), the book is worth having providing the reader realizes its limitations. There is a chapter-by-chapter bibliography, an index, and also several interesting appendices, including one describing the 17 reports of alleged land sightings of Nessie.

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Thunderbirds! The Living Legend of Giant Birds. By Mark A. Hall. Published by the author (9215 Nicollet Ave. So., 104), Bloomington, Minnesota, 1988. 100 pp. $16.95 (p.).

Although birds are poorly represented by the number of unexpected species in the annals of cryptozoology, the allure of unknown giant birds has long gripped the imagination of both our forebears and contemporary cryptozoologists. In *Thunderbirds! The Living Legend of Giant Birds,* Mark Hall considers reports of giant, diurnal, predatory birds in the United States and Canada—with side references to an unknown giant owl—and advocates that sightings of these birds represent the survival to modern times of a member of the teratorn family, large raptors that soared over Pleistocene America. In 12 chapters, he follows (albeit haphazardly in organization) a standard cryptozoological approach—chronicling modern reports, examining legends and tales of native Americans, discussing extant and extinct birds that could account for such sightings, and reconstructing the reported species. However, the book achieves mixed success in the thoroughness with which each subject is covered.

The compilation and overview of modern reports is thorough, and is supported by extensive citations. This compilation includes giant bird sightings not covered in earlier works, and summaries of sightings treated elsewhere, with little in the way of critical analysis of each report. In this way, the reader must decide for oneself the reliability of any particular sighting or incident. The views of native Americans on this subject are well represented, and include an overview—also supported by extensive citations—of the giant bird traditions of many different tribes across North America. These sections are the strengths of the book.

The ornithological coverage suffers in comparison with the thoroughness of the aforementioned sections. The hypothesis that the Thunderbird is an extant member of the family Teratornithidae—related to the American vultures, but predatory—is advocated. The possibility that the California condor (*Gymnogyps californianus*), the largest U.S. vulture, still exists outside of its historic range is dismissed as an explanation, although the physical description in some reports appears to fit a vulture rather than a teratorn. Some noteworthy scientific papers are missed. These would have expanded or bolstered the discussion of these two taxa, notably the important paper by Campbell and Tonni (*Auk,* Vol. 100: 390–403, 1983) on size—they estimate the largest teratorn, *Argentavis magnificens,* to have weighed 176 lb (80 kg), and to have had a wingspan of 20–27 feet (6–8 m)—and locomotion in teratorns, and work on the distribution, diet, and habitat of the California condor prior to range contraction (e.g., *Science,* Vol. 237: 768–70, 1987; *Quaternary Research,* Vol. 28: 415–26, 1987).

The over-reliance on newspaper reports instead of the primary ornithological literature accounts for this deficiency. Minor gaffs include the lack of consideration of the golden eagle (*Aquila chrysaetos*) as the most likely culprit in the Jarrett, Texas incident (p. 45), using the out-dated *Urubitornis* for *Harpyhaliaetus* (p. 45), and dubbing *Bubo virginianus* the eagle owl instead of the great horned owl (p. 83). The author correctly states that condors (indeed, any American vulture) are incapable of gripping with their feet. The weight carrying capacity of the teratorns is not addressed; Campbell and Tonni (1983, above: 399) state that "the feet were not used for catching or holding prey as in hawks, eagles and owls."

The 28 illustrations range in quality. It is unfortunate that the out-takes from the Huffer film of a putative Thunderbird in Illinois were presented as line drawings of shapes instead of a reprint of the original news photo. Also, no mention of the current disposition of that film is given. The four maps presented are welcome, but would have been more useful if the rivers portrayed had been labelled and county outlines had been provided for intrastate maps. In Fig. 3, Normal, Illinois, is incorrectly placed due east of Bloomington when it should be due north.

The criticism of professional ornithologists is justified in places, but is somewhat misfocused. The lack of interest of most ornithologists in Thunderbirds is probably due to two factors. First, there is the lack of sightings from the legions of competent amateur birdwatchers. As any reader of the journal *American Birds* knows, the number of good birdwatchers scanning the skies of the U.S. and Canada is impressive. Every year, surprising observations of birds far from their normal range are documented, often photographically. How have Thunderbirds escaped their roving eyes? Second, the presence of a large unknown raptor in most areas from which reports emanate seems so improbable. The author states (p. 84) that "living space is what giant birds would require," but does not mention the necessity of an extensive food source. It is generally considered that the extinction of teratorns and contraction of the California condor's range was due to the extinction of the North American Pleistocene megafauna by 11,000 years B.P. What food source could have permitted the lingering of teratorns across such far-flung localities as described in this book? Indeed, the existence of a teratorn in the great corn desert of central Illinois, reports of which cover the first two chapters, strains credulity and points out the lack of consideration of misidentified *known* large birds in this book.

The quote (p. 88) that professional biologists do not pursue the Thunderbird in particular or cryptozoological topics in general because "all the field work has been done," although unattributed, must have come from a molecular biologist! Those of us who are systematists and organismal biologists know well that a complete inventory of species on this planet is far from done, and that basic life history and distribution information is incomplete, even for supposedly well-known North American birds.

The concluding chapter makes sound recommendations for further research on the Thunderbird, which, because of the reasons mentioned above, are most likely to be undertaken by nonprofessionals. Especially important is the recommendation that cryptozoologists without formal zoological training establish ties with natural science institutions. The advice of professionals should be sought and, I suspect, would be more forthcoming in an informal situation than nonprofessionals suspect. The additional recommendation that physical evidence be preserved is one that should be stressed. The current state of molecular systematics makes any piece of physical evidence—hair, feather, blood traces—a source of DNA for analysis; if only the bloody and broken feather recovered from eastern Texas in 1977 (p. 45) were found today!

In summary, *Thunderbirds!* is a useful, although uneven, compendium of information. The thorough citations are to be commended, and make the book of considerable value to anyone interested in pursuing this area of cryptozoology.

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Mysterious Lake Pend Oreille and Its "Monster": Fact and Folklore. By James R. McLeod. Wordcraft Publishing/North Idaho College Cryptozoology Club, Coeur d'Alene, Idaho, 1987. 97 pp. $4 (p.).

In the preface of this booklet, Jim McLeod informs us that he and anthropologist Richard Snyder, with a group of North Idaho College students, formed an investigative organization called the North Idaho College Cryptozoology Club (NICCC) to investigate very persistent, but frequently ridiculed, North Idaho reports associated with two phenomena: the creature popularly known as Bigfoot or Sasquatch, and a "monster" in Lake Pend Oreille. This book confines itself to the latter, and is divided into three parts: The first section introduces Lake Pend Oreille, early lore, and legends about the lake and its alleged denizen. We then are presented with the available evidence—mostly anecdotal reports—beginning in 1944. One is struck immediately by the thoroughness and comprehensiveness of the presented material, and the exhaustive referencing by number which continues throughout the work.

In the second part, we are treated to an analysis of the evidence in a most scientific and scholarly way, and, most importantly, with an objective, open-minded balance. The group's stated objective—of carrying out an impartial

and rigorous investigative process reflecting a spirit of open-minded skepticism—has certainly been realized. The analysis includes comparisons with data from other monster lore lakes, and brings in all conceivable factors, such as the presence of the U.S. Navy's Farragut Naval Training Station on the lake. A chronology of reports from Lake Pend Oreille and other nearby lakes is most useful, as is the chronology of hoaxes and publicity stunts. Other possible monster-related folklore, such as underground channels or caverns, "the dreaded black streak" and other disturbances, mysterious large objects, and the "Great Depth Debate," are described and analyzed. Again, this is done exhaustively with scholarly aplomb—perhaps too exhaustively with regard to the depth of the lake.

The third section includes a detailed discussion of a possible sturgeon explanation, and the book's overall conclusion. A useful list of 16 possible sturgeon reports is included, with dates and relevant information. Based on the available evidence, the group concluded that the presence of sturgeon had not been established, but could not, on the other hand, be ruled out. In the conclusion, careful comparisons are made with possible explanations for monster sightings in other lakes, demonstrating a comprehensive command of relevant published data, coupled with a balanced mind-set when evaluating information. It is refreshing to find such a healthy, balanced approach, rather than the "believer" advocacy on the one hand, and the "closed mind dyed-in-the-wool skeptic" on the other. No firm conclusion is reached, although the overall impression I gained was that the presence of a "popular" type of monster is not very probable.

I was hard pressed to find anything to criticize in this excellent investigation and report. I noted only two typographical errors. In a table of Comparative Statistics of Other Large Lakes with Pend Oreille, Appendix IX, the surface areas for three of the cited lakes, including Loch Ness, are omitted, as not being available. Since great precision is not required for these comparisons, it would have been easy enough to include planimetric scaling area values made from available ordnance maps. In the case of Loch Ness, the area is approximately 21 square miles (54 sq. km).

I find it difficult not to praise this book too highly. It is most excellent, both in content and tone. It can well serve as a model for cryptozoological investigations in general. And at its modest price, it should be on the shelf of every cryptozoologist.

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Champ: Beyond the Legend (Updated Edition). By Joseph W. Zarzynski. M-Z Information (P.O. Box 2129), Wilton, New York, 1988. 240 pp. $12.95 (p.).

Originally reviewed by Henry H. Bauer in *Cryptozoology,* Vol. 5 (1986), Joseph Zarzynski's tome on the Lake Champlain Monster has been updated in a second edition published in 1988. In his original book, Zarzynski chronicled the historical facts concerning Champ, and also laid out chronologies of sightings and fieldwork that he and others associated with the Lake Champlain Phenomena Investigation (LCPI) had undertaken up to 1983.

This new, updated edition also tables sightings and fieldwork from 1984 to 1987. A total of 71 new sighting reports is included, and although a number of photographs taken during these appearances are mentioned, none are included in the book. Sightings are laid out in an easy-to-read typographical style, but are not necessarily in chronological order.

The descriptions of the fieldwork activities are vivid, and meticulous attention to details has once again been displayed by the author. This section should serve as a guide for all contemplating monster-locating activities, as the various methods Zarzynski has employed in his search for Champ are well worth replicating. His methodology has evolved from surface surveillance to underwater searching via the auspices of sonar and underwater photography utilizing remotely operated vehicles (ROV's). Use of sonar has also enabled Zarzynski to branch out into investigating and locating shipwrecks on the bottom of Lake Champlain. This has led to "Zarr" authoring a separate book on the subject, entitled *Monster Wrecks of Loch Ness and Lake Champlain,* which was reviewed by Alastair Boyd in *Cryptozoology,* Vol. 8 (1989).

In concluding this updated edition, Zarzynski expresses the hope that it will encourage others "to work toward conquering this zoological Mt. Everest." Judging by the amount of effort detailed in his latest fieldwork reports, surely it will not be too long before Zarzynski will feel the same exhilaration that Edmund Hilary felt when he went "Beyond The Legend."

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Comments and Responses

This section permits readers to critique or comment on works previously published in Cryptozoology. *The original authors and other readers are encouraged to respond to these critiques or comments. Readers are also encouraged to critique or comment on the works appearing in this issue. All comments are the responsibility of the authors only, and do not reflect any policies established by the Editor or the Editorial Board of* Cryptozoology, *or the Board of Directors of the Society.*

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PALEOCRYPTOZOOLOGY: A MONSTROUSLY GOOD IDEA

(Comment on Adrienne Mayor, 1989, Paleocryptozoology: A Call for Collaboration between Classicists and Cryptozoologists, *Cryptozoology,* Vol. 8: 12–26)

What a monstrously good idea Adrienne Mayor has by the tail—an interdisciplinary project to identify extinct or unknown animals by cross-referencing ancient texts and ancient and modern art, artifacts, bones, and fossils, and corralling this material into cryptozoological classes. At the very least, classicists, archaeologists, art historians, folklorists, paleontologists, and paleocryptozoologists can have a fine Dialogue at Academic Meetings: A Show and Tell, a Scholar's Exchange of *Disjecta Membra* and curiosities. Material that has fallen through the cracks, so to speak, could be resurrected, and those plates at the ends of excavation volumes, filled with dull, miscellaneous fragments—their identifying labels followed by question marks—could rise to become stars of the collections.

The approach, which Mayor briefly demonstrates in identifying the griffin, seems eminently workable. I note the attention she pays, and deservedly so, to the geographic landscape of the animals, their anatomy, and their behavior in developing a typology of each animal. I personally was unaware of the extent of the ancients' interest in fossil hunting.

Gathering evidence for Mayor's project, although daunting, is doable with modern computers. Literary evidence, as she points out, is fairly quickly gathered from the *Thesaurus Linguae Graecae*; artistic evidence can be

gathered from the computerized Beazley Archive at Oxford University, the Perseus Project at Harvard University, *Lexicon Iconographicum Mythologiae Classicae* (LIMC), etc. Mayor's project could be a gold mine for doctoral theses: future topics might be prioritized, finished theses might gain wider publicity, and a systematic approach might bring order, genus, and species to unknown animals. (Relevant past topics at Bryn Mawr College: Katharine Shepard, Ph.D. thesis, 1936: *Sea Monsters in Greek, Etruscan and Roman Art*; Kevin Thomas Glowacki, M.A. thesis, 1987: *Animal Sculpture from the Acropolis.*)

I look forward to testing Mayor's proposed system with my own particular "unclassifiable" ancient felids. In my file, I have a collection of dubious types of "fringe" felids: among them are misdrawn, disjointed, probably real-life, pussy cats; then some which are grossly oversized, dwarfing their masters; some pulling carts and chariots in unfeline behavior; and one unreal, long-nosed, gigantic, spotted animal bounding up on hind feet, attacking a warrior, its tail neatly, but unnaturally spiraled (Copenhagen, Inv. Chr. VIII 275, Lucanian lekythos: *CVA* 6 [6] pl. 240, fig. 3; A. D. Trendall, 1970, *The Red-Figured Vases of Lucania, Campania, and Sicily: First Supplement.* London, no 872, p. 152). Then there are those dog–cat hybrids, odd offspring of some street mating. Are these real animals, monsters, or just monstrous efforts of deficient artists?

Mayor ends her paper on the note that the contribution to both paleocryptozoology and classics from such a projected integration of hitherto unidentifiable animals of literature, art, and archaeology with knowledge of extinct animals could be far-reaching. I agree, but cannot see stopping with the Classical world or the boundaries of the Mediterranean; unidentifiable animals in medieval art, and Scandinavian Celtic art and archaeology, clamor for a similar approach.

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PALEOCRYPTOZOOLOGY AND ARCHAEOLOGY: A SIVATHERE NO LONGER

(Comment on Christine Janis, 1987, Fossil Ungulate Mammals Depicted on Archaeological Artifacts, *Cryptozoology,* Vol. 6: 8–23; and Adrienne Mayor, 1989, Paleocryptozoology: A Call for Collaboration between Classicists and Cryptozoologists, *Cryptozoology,* Vol. 8: 12–26)

As a faunal analyst working on animal bones and shells from archaeological sites in the Mediterranean Basin and Near East, I was delighted to read the recent articles by Janis and Mayor.

In the study of animals exploited in the past by humans, we have various sources of evidence. The best is, of course, the actual remains—the bones and shells excavated on archaeological sites. Next, we have representations of these animals which may exhibit various degrees of artistic license, and may be natural-size, smaller, or larger, and made of terracotta, stone, glass, metal, etc. We have similar pictorial evidence on pottery, stone, coins, textiles, wall paintings, mosaics, etc.

We have good evidence that osteological and malacological remains were shipped far from their source (David S. Reese, 1982, Faunal Remains as Items of "Trade" in Mediterranean Archaeology, *American Journal of Archaeology,* Vol. 86[2]: 281–82). These include hippopotamus and elephant teeth as a source of ivory for carving (David S. Reese, 1985a, Hippopotamus and Elephant Teeth from Kition, Appendix VIII[D] *In* V. Karageorghis, *Excavations at Kition,* Vol. V [Part II], Department of Antiquities, Nicosia: 391–409), North African Hartebeest (*Alcelaphus buselaphus buselaphus*) horncores from the 7th century B.C. Heraion (Temple of Hera), on the Greek island of Samos (Joachim Boessneck and Angela von den Driesch, 1981, Reste Exotischer tiere des Heraion auf Samos, *Mitteilungen des Deutschen Archäologischen Instituts Athenische Abteilung,* Vol. 96: 245–49), ostrich eggshells (David S. Reese, 1985b, The Kition Ostrich Eggshells, in Karageorghis, above: 371–82), Indo-Pacific shells (David S. Reese, 1991, The Trade of Indo-Pacific Shells into the Mediterranean Basin and Europe, *Oxford Journal of Archaeology,* Vol. 10[2]), and fauna from the Nile River (David S. Reese, Henk K. Mienis, and Fred R. Woodward, 1986, On the Trade of Shells and Fish from the Nile River, *Bulletin of the American Schools of Oriental Research,* No. 254: 79–84).

In some cases, it may not be clear whether we are dealing with animals which once existed in the area but are now extinct there, or forms which never existed there and must have been imported. An example of this is the presence of the Nile perch (*Lates niloticus*) on archaeological sites in the Levant. Today, these fish are only found in the Nile River, but they once may have existed in fresh-water areas along the Syro-Palestinian coast (Hanan Lernau, 1986/87, Subfossil Remains of Nile Perch [*Lates* cf. *niloticus*]; First Evidence from Ancient Israel, *Israel Journal of Zoology,* Vol. 34: 225–36; and unpublished remains from Sarepta in Leanon and Hala Sultan Tekke on Cyprus identified by Mark J. Rose).

The correct scientific identification of ancient artifacts with representations

of living or fanciful animals is often difficult. The animal surmounting the famous Kish (Iraq) bronze rein-ring (part of the ornamentation of a four-wheeled chariot) was originally thought to be a tame or domestic deer, based on the heavy halter through the nose which is tied to the right foreleg (Henry Field, 1931, The Field Museum–Oxford University Joint Expedition at Kish—I, *Art and Archaeology,* Vol. 31[5]: 251; B. Laufer, Tamed Deer in Ancient Times, *Field Museum News,* Vol. 1[3]: 1).

Some years later, it was identified as an extinct Pleistocene giraffid form, a sivathere, by Edwin H. Colbert (1936, Was the Extinct Giraffe [Sivatherium] Known to the Early Sumerians?, *American Anthropologist,* Vol. 38: 605–8; Anonymous, 1936, Archaeological Specimen Aids Paleontological Research, *Field Museum News,* Vol. 7[6]: 4; Edwin H. Colbert, 1969, *Evolution of the Vertebrates: A History of the Backboned Animals Through Time,* 2nd ed. John Wiley, New York: 438).

In her paper, above, Janis strongly supports the sivathere identification (see also Christine Janis, 1988, Hurrah for Hyraces! [Response to Tassy, Spassov, and Raynal], *Cryptozoology,* Vol. 7: 104), as do Roy P. Mackal (1980, *Searching for Hidden Animals: An Inquiry into Zoological Mysteries.* Doubleday, New York: xvii–xviii), Robert J. G. Savage and Michael R. Long (1986, *Mammal Evolution: An Illustrated Guide,* Facts on File, New York: 229), Pascal Tassy (1988, Fossil Ungulates and Archaeology: Two Kinds of Evidence [Comment on Janis], *Cryptozoology,* Vol. 7: 100), Nikolai Spassov (1988, A Review of Some Paleocryptozoological Hypotheses [Comment on Janis], *Cryptozoology,* Vol. 7: 101—although he also notes that "the flat and twisted antlers of the statuette from Kish [Iraq] remind one of the antlers of Mesopotamian fallow deer"), and Michel Raynal (1988, Persepolis: A Puzzling Case in Archaeological Cryptozoology, *Cryptozoology,* Vol. 7: 103).

This bronze statuette is in the collection of the Department of Anthropology of the Field Museum of Natural History (cat. no. 236528). It is one of two rein-rings excavated in 1927–28 from Grave 357 (Chariot Burial 2), in the Y Cemetery, and dates to the late Early Dynastic I period, ca. 2800–2750 B.C., not 3500 B.C. as Field and almost all later authors note (Mackal and Savage/Long give the date 2500 B.C.).

In 1977, it was demonstrated conclusively that the animal in question is a Persian fallow deer (*Dama mesopotamica*) and not a sivathere (Anonymous, 1977, 5,000-Year-Old Sumerian Stag Reunited with Antlers, *Field Museum of Natural History Bulletin,* Vol. 48 [10]: 3). This short but important note, which went unnoticed by many, is quoted in its entirety here: "Holding his remarkable discovery, University of Heidelberg graduate student Michael Müller-Karpe, 22, displays the antlers of a 5,000-year-old Sumerian copper stag he uncovered in a small box of dried mud in a Field Museum storeroom. Müller-Karpe was examining hundreds of metal vessels from the ancient Near East when he came across the box with its precious

FIG. 1.—Close-up photograph of the animal on the Kish (Iraq) bronze statuette identified as an extinct Pleistocene giraffid form, *Sivatherium,* by Edwin H. Colbert in 1936. (Field Museum of Natural History, Neg. #70057.)

contents which 'looked like green coral.' He had no idea what he'd found until he recalled a world-famous, mainly antler-less stag decorating a rein-holder elsewhere in the museum. His keen mind suddenly identified the 'coral' as the corroded antlers of the 7½-inch stag. The stag is a very rare example of decorative Sumerian art created 1,000 years before the art of Egypt's Tutankhamun."

I reproduce here (Fig. 1) the close-up photograph of the head taken around 1930 (FM neg. no. 70057; I thank Nina Cummings for locating this in the photo archives), published by Colbert in 1936 (plate 22 left), and used as the basis for a line-drawing by Janis (Fig. 2). I have recently examined the statuette itself, and present here a new close-up photograph with the antlers restored (Fig. 3; Conservation and photograph by Catherine Sease). I think we must now reconsider the initial hypothesis of the 1930's, that we do indeed have evidence for "domestic" (or at least human-controlled) fallow deer in Early Dynastic Iraq.

Sometimes the correct identification of ancient animal figurines is surprising. For example, a 4-cm Gallo-Roman bronze figurine was originally—

FIG. 2.—Line drawing of the head of the animal from Christine Janis (1987), based on drawing by Margaret Matthew Colbert in Edwin H. Colbert (1936).

in 1985—called a wild boar, then properly identified as a rhinoceros (Robert Trier, 1914, Un Rhinocéros Gallo-Romain au Mans, *Revue Historique et Archéologique du Maine,* Vol. 65: 3–7).

The Persepolis (Iran) stone frieze of about 515 B.C. has already received some attention in this journal, and I am here able to add an additional reference (A. Afshar, W. Dutz, and M. E. Taylor, 1974, Giraffes at Persepolis, *Archaeology,* Vol. 27 [2]: 114–17). The dromedary and Bactrian camels shown on the Persepolis friezes have also been studied (Ahmad Afshar, 1978, Camels at Persepolis, *Antiquity,* Vol. 52: 228–231).

The Akrotiri site on the Greek Cycladic island of Thera (Santorini), north of Crete, was destroyed in a violent volcanic eruption around 1500 B.C. Excavations there produced a series of beautiful wall-paintings. Numerous animals are pictured, including North African/Near Eastern gazelles, and frolicking monkeys. J. Ray Porter of New York has been studying these frescoes, and has shown that the monkeys pictured must be the Green monkey (*Cercopithecus aethipos*) of Africa. This has previously been noted by W. C. Osman Hill (1966, *Primates, Comparative Anatomy and Taxonomy,* Vol. 6, Interscience Publishers, New York: 5). These paintings are on display in the Thera Room in the National Museum in Athens, and their pictures have been widely published (for example, Peter Warren, 1975, *The Aegean Civilizations,* Elsevier, Oxford: 115, 118).

To this category of pictorial analysis we can mention the early evidence for a small domestic horse (*Equus domesticus*) (Mary Aiken Littauer, 1971, The Figured Evidence for a Small Pony in the Ancient Near East, *Iraq,* Vol. 33: 24–30), and a few of the more important papers on birds illustrated on archaeological objects (Howard Carter, 1923, An Ostracon Depicting a Red

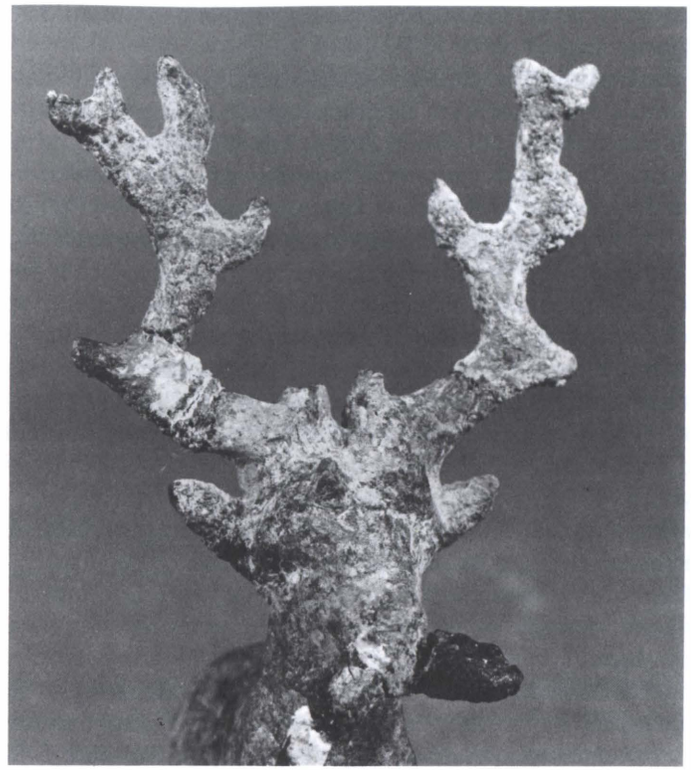

FIG. 3.—New close-up photograph of the animal with the antlers restored. (Catherine Sease.)

Jungle-Fowl [The earliest known drawing of the domestic cock], *The Journal of Egyptian Archaeology,* Vol. 9: 1–4; Percy R. Lowe, 1934, A Further Note Bearing on the Date when the Domestic Fowl was First Known to the Ancient Egyptians, *The Ibis,* Vol. 4 [2]: 378–82; Sylvia Benton, 1959, Birds on the Cup of Arkesilas, *Archaeology,* Vol. 12 [3]: 178–82; Sylvia Benton, 1961, Cattle Egrets and Bustards in Greek Art, *The Journal of Hellenic Studies,* Vol. 81: 44–55; Robert D. Lamberton and Susan I. Rotroff, 1985, *Birds of the Athenian Agora,* American School of Classical Studies at Athens, Princeton).

Burchard Brentjes has contributed much on the zoological identification of animals pictured on ancient Near Eastern/Far Eastern art objects, particularly in a series of poorly-known short papers which appeared in 1969. One of these papers (Brentjes, 1969, Hirsche in Nubien und Äthiopien, *Säugetier-Kundliche Mitteilungen,* Vol. 17: 203–5) includes three ancient illustrations, one of which indicates that the Persian fallow deer existed in Ethiopia until about 1,000 years ago, *contra* the 10,000 B. P. date given by Janis (1987,

A Reevaluation of Some Cryptozoological Animals [Comment on Heuvelmans], *Cryptozoology,* Vol. 6: 117).

Mayor's illustration (Fig. 3G) of the *ketos* sea monster on a clay sealing from the Temple Repositories at Knossos (Crete), dating to the late Middle Minoan III period (ca. 1650–1600 B.C.), has also been interpreted as a hippopotamus (*Hippopotamus amphibius*) (Spyridon Marinatos, 1927–28, Αρχ?τι περί Μινωίχῆς ⟨⟨Σχύλλας⟩⟩ *Arkaologika Deltion,* Vol. 11: 53–54).

Janis (p. 12) notes that the identification of animals on art objects on display in museums are not always correct, her example being an exhibit label of a Roman mosaic picturing a Reedbuck antelope (*Redunca* sp.) from Africa which is referred to as a deer. Roman mosaics are a particular problem in that itinerant mosaicists traveled around the Mediterranean Basin with pattern books from which the mosaic's patron would choose a composition. So it is not really surprising that an Ethiopian or Sudanese animal would be pictured on a mosaic in a country far from the habitat of that animal (J. M. C. Toynbee, 1982, *Animals in Roman Life and Art,* 2nd ed., Cornell University Press, Ithaca). In Roman times, animals were also shipped around the Mediterranean Basin as items of curiosity and sport (Frederick E. Zeuner, 1963, *A History of Domesticated Animals,* Hutchinson, London).

Another good example of the discrepancy between the terms employed by an archaeologist/art historian/classicist and a zoologist is typified in the description of the objects from Tutankhamun's tomb (Charles A. Reed and Dale J. Osborn, 1978, Taxonomic Transgressions in Tutankhamun's Treasures, *American Journal of Archaeology,* Vol. 82 [3]: 273–283; I. E. S. Edwards, 1979, Zoomorphic Anomalies in Tutankhamun's Treasures, *American Journal of Archaeology,* Vol. 83 [2]: 205–6).

I doubt that the 8th century B. C. "gorgon-like" terracotta masks from Tiryns on the Greek mainland (Mayor, Fig. 2, left) find their inspiration in the skull of the pygmy hippopotamus. Pygmy hippopotami remains are only known from the Late Pleistocene/early Holocene on a number of Mediterranean islands (Cyprus, Crete, Sicily, Malta); there are no pygmy hippos on the mainland. Normal-sized hippopotamus bones are, in fact, rather rare on the Greek mainland, only present in Elis and Megalopolis (Ralf-Dietrich Kahlke, 1987, On the Occurrence of *Hippopotamus* [Mammalia, Artiodactyla] in the Pleistocene of Achalkalaki [Gruzinian SSR, Sovet Union] and on the Distribution of the Genus in South-East Europe, *Zeitschrift für Geologische Wiessenschaften,* Vol. 15: 409–414), quite a distance from the southern Argolid location of Tiryns. I do find Mayor's research on the griffin (a more detailed paper by her is now in preparation) much more convincing, and I encourage her and others to continue to bridge the various disciplines of zoology, paleontology, archaeology, history, and folklore.

There is a body of published work on the folklore and legends surrounding the discovery of fossil bones and shells (David S. Reese, 1975, Men, Saints, or Dragons?, *Expedition* 17 [4]: 26–30; Kenneth P. Oakley, 1975, *Decorative and Symbolic Uses of Vertebrate Fossils,* Pitt Rivers Museum, Oxford; Eric Buffetaut, 1987, *A Short History of Vertebrate Paleontology,* Wolfeboro [New Hampshire]: 1–18).

There is also recent literature on a Cypriot archaeological site with extinct endemic late Pleistocene/early Holocene pygmy hippopotami, pygmy elephants, birds, and marine invertebrates associated with man-made chipped stones and stone and shell beads around 8200 B. C. (David S. Reese, 1989, Tracking the Extinct Pygmy Hippopotamus of Cyprus, *Field Museum of Natural History Bulletin* 60 [2]: 22–29; Alan H. Simmons, 1989, Preliminary Report on the 1988 Test Excavations at Akrotiri-Aetokremnos, *Report of the Department of Antiquities, Cyprus,* 1–5). We are very confident that we do indeed have human association with these extinct endemic animals (Bruce Bower, 1990, Cyprus Dig Pushes Back Colonization Date, *Science News,* Vol. 138[23]: 359).

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PALEOCRYPTOZOOLOGICAL COLLABORATIONS

(Response to Ashmead and Reese)

Since its inception, cryptozoology has embraced multidisciplinary studies, both by temperament and necessity. Opening up an avenue for dialogue between the various fields of ancient history and cryptozoology was the purpose of my "call for collaboration"; it was a way to begin identifying some unknown or misidentified animals in ancient Greek and Roman art and literature. As Ann Ashmead points out, many classical scholars come across puzzling bits of material that refuse to be classified, but if these puzzle pieces were collected systematically, a valuable picture might emerge.

Several animals in classical vase paintings have been identified by experts in the past as "cats" or "dogs," but their features and behavior often suggest those of exotic felids and mustelids (for a probable mustelid identified as a felid, see Diana Buitron, 1987, *Antiquities from the Collection of Christos*

G. Bastis, D. von Bothmer *et al.* [eds.], Von Zabern, Mainz: 283; for a probable mustelid—an otter?—identified as a "pug dog," and a probable mustelid carrying young identified as "dog or fox with pup or prey" see A. Kozloff [ed.], *Animals in Ancient Art from the Leo Mildenberg Collections,* von Zabern, Mainz: nos. 148 and 81, respectively; Dietrich von Bothmer, personal communication; Leo Mildenberg, personal communication). Classicist Keith DeVries has recently corrected the identification of an animal in a 6th century B.C. vase painting—which scholars had assumed was a "puppy" or "marten"—as a domestic polecat, a mustelid pet that preceded cats in ancient Greece (1987, Ferreting Out the Truth, *American School of Classical Studies Newsletter,* Spring: 9; see also Adrienne Mayor, 1989, Grecian Weasels, *The Athenian,* Vol. 16 [184]: 22–24).

Ashmead maintains an extensive file on ancient "mystery" felids (personal communication), and has published on exotic cats in 5th century B.C. Athens (Ann Harnwell Ashmead, 1978, Greek Cats, *Expedition,* Vol. 20 [3]: 38–47); I look forward to her possible reidentifications of fringe felids. As Ashmead notes, the recently computerized corpora of ancient literature and vase collections—both extant and planned—should make new paleocryptozoological topics attractive for scholarly dissertations and publications. All of which means more fuel for workers in *many* other fields, including cryptozoologists interested in ancient unknown animals.

David Reese points out that the *ketos* classified as a "sea monster" by most scholars (Fig. 3G in my article) was interpreted by Marinatos as a hippopotamus in 1927. Publications of these two interpretations include artists' drawings of the clay seal; these vary significantly, depending on the interpretation (Fig. 1A and B). The depiction in Fig. 1A is used by those who see a sea monster. The depiction in Fig. 1B is Marivatos' version. This illustrates the need to view actual artifacts or photographs whenever possible. Ashmead also reminds us how subjective the *ancient* artistic representations of animals can be.

As an expert on the distribution of hippopotami in the ancient Mediterranean and Near East, Reese's doubts about the "gorgon-like" masks from Tiryns must be taken seriously. On the other hand, the distance between Tiryns and Megalopolis is about 75 miles (120 km); trade between mainland centers and Cyprus was active since the Late Bronze Age (late second millennium). I used the unidentified masks, and the "unidentified wooden animal" from Samos (Figs. 2 and 4 in my article), mainly as examples to get readers interested in possible relationships between unknown animals in ancient art and fossils, unfamiliar remains, or exotic animals. Two ISC members, Sterling Lanier and David Weaver, wrote to identify the unknown animal from Samos in Fig. 4 as a Hamadryas baboon from Egypt. I agree, even though the baboon's habitat is Egypt—far from Samos, off the coast of Turkey. But the carved wooden head is dated to the 7th–6th century B.C.,

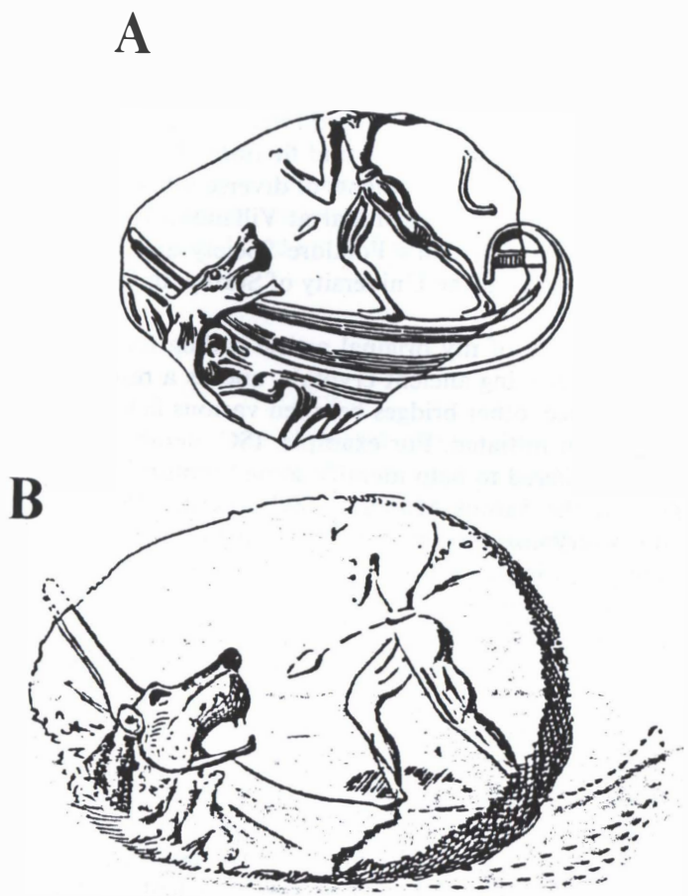

FIG. 1A.—Drawing used by scholars who classify the *ketos* animal as a "sea monster." FIG. 1B.—Drawing accompanying Marinatos' 1927 article identifying the animal as a hippopotomus.

and Samos and Egypt made an alliance in the mid-6th century B.C., which they sealed with an exchange of gifts—perhaps baboons (or baboon artifacts) were sent to Samos then. ISC member Hugh Trotti wrote to suggest a possible connection between the image of the ancient griffin and Scythian "horseback falconry" with trained eagles, a sport unfamiliar to the Greeks.

Reese's ongoing work on pygmy hippos, which were previously thought to have been extinct before humans arrived on Cyprus, is very significant from a cryptozoological viewpoint. It would be useful to know how many other "extinct" animals were actually coeval with humans. Reese mentions the most important works that discuss the connections between fossils and folklore, but they focus chiefly on medieval and later sources; ancient encounters with fossils are still understudied, the most complete study that I

know of being by C.E.N. Bromehead (1945, Geology in Embryo, *Proceedings of the Geologists Association,* Vol. 56, Part 2: 89–134). My research, tracing the griffin legend to ancient observations of Central Asian fossils (Mayor, in preparation), is supported by evidence gathered from paleontology, classics, ancient history, archaeology, geology, and folklore. This cross-disciplinary project has benefited from the comments of diverse scholars and audiences, such as a recent biology-classics seminar at Villanova University, and the joint conference held by Britain's Folklore Society and the International Society of Cryptozoology at the University of Surrey, in Guildford, in July, 1990.

Since the appearance of my original article calling for interdisciplinary collaboration in identifying ancient cryptids, and as a result of the above-mentioned conference, other bridges between various fields and paleocryptozoology have been initiated. For example, ISC member Aaron Bauer, a herpetologist, has offered to help identify some "reptilian" bronze animals (ca. 700 B.C.) in the Samos Museum. ISC member Michael Heaney, a linguist-folklorist-cryptozoologist at the Bodleian Library, University of Oxford, and I are preparing an article on the connections between fossils and ancient griffins, and ancient Arimaspians and the modern Almas of Mongolia. Jeannine Davis-Kimball, director of the new Soviet-US archaeological/ethnological project in Kazakhstan, has promised to photograph ancient petroglyphs of unknown animals near the Scythian tombs around Alma Ata. Other correspondence with classicists and archaeologists working in Greece and Central Asia, and Canadian paleontologists working in Mongolia and northwest China, has begun since the article's appearance.

For readers interested in pursuing paleocryptozoological topics, I also want to mention Liliane Bodson, of the University of Liege, Belgium, a classicist who is an expert on reptiles and amphibians in ancient Greece and Rome; she has published identifications of Mediterranean snakes of antiquity, and directs the Computerized Data Bank on Common Knowledge and Folklore about Reptiles and Amphibians in French-speaking Countries of Europe, which may be of use to cryptozoologists.

Cryptozoologists have sometimes been frustrated by the negative response of scholars in other fields, as well as the media, but I have been encouraged by the many positive reactions of archaeologists, ancient historians, classicists, paleontologists, and art historians to cryptozoological investigations of ancient "mystery" animals. Perhaps this indicates that cryptozoological speculation about antiquity is somehow less "threatening" than modern "mysteries." However, I prefer to think that the growing multidisciplinary enthusiasm demonstrates a new willingness in unexpected quarters to entertain creative, rational speculation about unexpected animals, a sign of what Jean-Paul Debenat has called a rebirth of the old bestiaries of fabulous animals, but in more sophisticated modern terms (Jean-Paul Debenat, 1990,

Fabulous Beasts of our Times, paper presented at the Conference "Fabulous Beasts: Fact and Folklore," held at the University of Surrey, Guildford, July 20-22, 1990). I also hope it reveals an emerging recognition of Heuvelmans' optimistic concept of "biomythology," an inborn human need to use our imagination to confront the unknown (Bernard Heuvelmans, 1990, The Metamorphosis of Unknown Animals into Fabulous Beasts and of Fabulous Beasts into Known Animals, paper presented at the Conference "Fabulous Beasts: Fact and Folklore," held at the University of Surrey, Guildford, July 20–22, 1990).[1]

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SIVATHERIUM DEFENDED

(Response to Reese)

David Reese provides us with some very interesting evidence to show that the Sumerian figurine identified as *Sivatherium giganteum* by Edwin Colbert (1936, Was the Extinct Giraffe [*Sivatherium*] Known to the Early Sumerians?, *American Anthropologist,* Vol. 38: 605–8) was an incomplete specimen in which the horn-like organs had been broken off, and that the addition of the newly-identified missing pieces shows a much more complex pattern of branching antler-like structures. He states that this new discovery demonstrates conclusively that the animal represented in the figurine is, in fact, a Persian fallow deer, *Dama dama mesopotamica,* rather than any type of extinct giraffid.

While I agree that this new discovery is highly interesting, and forces us to reexamine the evidence for a sivathere, I intend to show here that the fallow deer case is far from proven. My argument below will show that this figurine most probably *does* represent a sivathere, despite the newly enforced changes in interpretation of the morphology of the horn-like organs, and that it is highly unlikely to represent a species of fallow deer, despite the apparently similar nature of the recently-discovered "antlers." My argument

[1] This paper appears as the lead article in this issue.—Editor.

will concentrate on three points: firstly, features other than the "antlers" or "horns" of the figurine that suggest sivathere affinities; secondly, the variation in the appearance of sivathere "horns" (properly termed "ossicones" in giraffoids) in the fossil record; and thirdly, the lack of clear homology between the antler-like structures newly assigned to the figurine and the form of the antlers in fallow deer.

Perhaps the most obvious—and the most often cited—feature of the original figurine is that the "horns" (now known to be incomplete) do not resemble the cranial appendages of any living species of hoofed mammal, but look very much like the short, palmated ossicones originally figured for the Late Pleistocene *Sivatherium giganteum* (Edwin H. Colbert, 1935, Siwalik Mammals in the American Museum of Natural History, *Transactions of the American Philosophical Association,* Vol. 26: 1–401). However, other features of this figurine suggest that it is not of a recent living species. The construction of the figurine itself suggests a large animal of considerable bulk, similar in size to a moose or a bison. This is evidenced in the proportions of the relatively short legs and back, the relatively massive neck, and the large head with the short, broad snout. The apparent size and body proportions are very unlike those of any deer—even the similarly-sized moose has proportionally much longer legs for its size—and the length of the neck is not suggestive of any bison or buffalo species. It is, in fact, very similar in appearance to the proportions of *Sivatherium.* Given that one might expect a certain amount of artistic license could give a false impression of absolute size, we perhaps should not weigh this line of argument too heavily. However, a much more distinctive feature is the presence of two small "horns" or cranial appendages of some type over the eyes, anterior to the larger ones.

No deer species has ever been found with such additional structures, although some subspecies of the living giraffid *Giraffa camelopardalis* may develop a single median frontal ossicone in the males. This condition of small anterior frontal cranial appendages and larger posterior parietal ones is only seen in two species of the now extinct sivatherine giraffids, *Sivatherium* and *Bramatherium* (J. M. Harris, 1976, Pliocene Giraffoidea [Mammalia, Artiodactyla] from the Cape Province, *Annals of the South African Museum,* Vol. 69[2]: 325–53). (See Fig. 1A.) It is these little frontal ossicones that stamp this figurine as resembling a sivathere much more than any similarity of the posterior ossicones to the originally illustrated skull of *Sivatherium giganteum.*

My second point is that the ossicones of sivatheres appear to be so highly variable that the fact that the reconstructed figurine no longer bears a close resemblance to Colbert's original description may not be very relevant. It is worth quoting here a more recent description of *Sivatherium maurusium* (= *Sivatherium giganteum*), known from the Late Pleistocene of Africa, from

giraffid expert Rufus Churcher (1978, Giraffidae. *In* V. J. Maglio and H. B. S. Cooke [eds.], 1978, *Evolution of African Mammals,* Harvard University Press, Cambridge: 509-35): "Its skeleton is not particularly variable, but the ossicones are highly variable in conformation. . . . A few [of the ossicones] are so twisted that the planes of parts of the palmation lie in markedly different orientations and the knobs come to resemble tines. The number and distribution of the knobs are also variable, with some individuals having smooth beams and a single protuberance only at the base of the ossicones, others having the knobs evenly distributed along the anterior border, and in those that are highly twisted, one or more lower knobs coming to lie posteriorly" (p. 525). The variability of sivathere ossicones is also taken up by John Harris (1974, Orientation and Variability in the Ossicones of African Sivatheriinae [Mammalia: Giraffidae], *Annals of the South African Museum,* Vol. 65[6]: 189–98), where he distinguishes several different classes of ossicones in sivatheres, and points out that almost all known skulls have ossicones that are longer and less palmate than those in the skull originally illustrated by Colbert.

Many of these skulls have ossicones that extend outwards and backwards, and then tilt forwards again, with flanges and knobs present on both anterior and lateral surfaces. It is not yet clear if these different ossicone "classes" actually represent different sivathere species, or merely variation within a single species, representing, among other things, the secondary bone apposition on the structures with the increasing age of the individual—as also seen in living giraffes. I am at a disadvantage here in citing from secondary literature sources that do not present a full complement of illustrations, and I have not yet had the opportunity to examine these skulls myself. However, it is clear that the variability described in *Sivatherium* warrants a much more thorough investigation of the range of variation in the species before the revised form of the figurine can be dismissed as "unlike a sivathere."

My final point concerns the actual condition of the antlers of the fallow deer, which may bear a superficial resemblance to the recently discovered structures of the figurine, but do not match up in precise detail. The living fallow deer (*Dama dama*) is defined by having a four-tined antler morphology: comparison of juvenile and adult animals makes it clear that the palmated portion of the antler in the older animals is homologous with the single tine A3 in the younger ones (Colin P. Groves and P. Grubb, 1987, Relationships of Living Deer. *In* C. M. Wemmer [ed.], 1987, *Biology and Management of the Cervidae,* Smithsonian Institution Press, Washington, D.C.: 21-59). (See Fig. 1B and C.) The Persian fallow deer (*Dama dama mesopotamia*) had antlers that were reduced over this condition, with highly reduced "brow" tine (A1) and a narrow palmated region represented by posteriorly pointing tines only (A. Lister, 1987, Diversity and Evolution of Antler Form in Quaternary Deer. *In* C. M. Wemmer [ed.], 1987, *Biology*

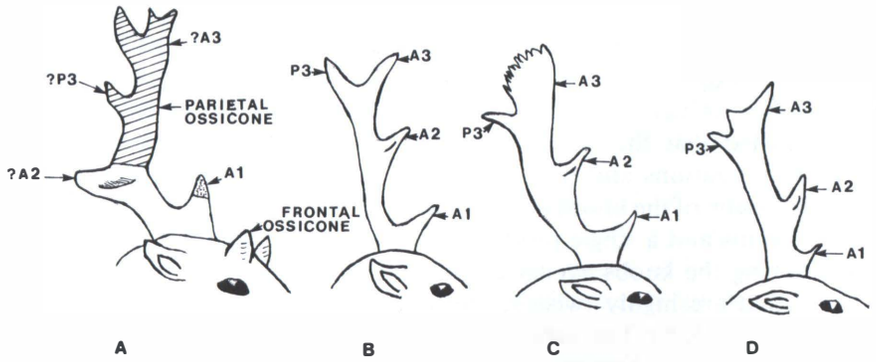

FIG. 1.—Sketches of heads and cranial appendages of animals mentioned in the text. A. "Sivathere" figurine (based on photographs in above Comment by Reese). (Shaded portion represents newly added part of artifact, stippled portion represents part of artifact present in the original 1936 depiction by Colbert, but absent from new photograph supplied by Reese.) B. Juvenile European fallow deer (*Dama dama*) (from Groves and Grubb 1987). C. Adult European fallow deer (from Groves and Grubb 1987). D. Adult Persian fallow deer (*Dama dama mesopotamica*) (from Lister 1987).

and Management of the Cervidae, Smithsonian Institution Press, Washington, D.C.: 81–98). (See Fig. 1D.)

In Figure 1, I have done my best to trace possible homologies between the form of the ossicones of the figurine and the antlers of fallow deer, but it can be seen that they do not show a clear homology. Although I am again at a disadvantage in making a sketch from the specimen illustrated by Reese, and have not seen the original, it is clear that the second tine (? A2) on the figure points *postero-laterally,* which is diametrically opposed to the A2 tine of the fallow deer that points antero-medially (this I have checked with an actual specimen). Additionally, while a superficial resemblance exists between the figurine and the European variety of fallow deer, it bears much less of a resemblance to the Persian fallow deer with its reduced antlers. (The specimen figured by Reese appears to have a reduced brow tine A1, but in comparing this photograph with the original one figured by Colbert, the tip of this tine appears to have been broken off since 1936.)

In summary, the new reconstruction of the "sivathere" figurine raises some questions about the original assignation by Colbert, but does not invalidate it. Other aspects of the figurine suggest sivathere status. The ossicones of *Sivatherium* were so highly variable that these new antler-like structures cannot be definately stated to disprove the sivathere affinities, and the newly restored, more complex ossicones cannot be homologized with the antlers of the fallow deer, especially with the Persian variety of this species.

I would also like to take this opportunity to provide an update on my original claim for the depiction of a fossil hyrax, *Pliohyrax,* on a Chinese

figurine (Christine Janis, 1987, Fossil Ungulate Mammals Depicted on Archaeological Artifacts, Cryptozoology, Vol. 6: 8–23; and 1988, Hurrah for Hyraces! [Response to Tassy, Spassov, and Raynal], *Cryptozoology,* Vol. 7: 104–06). Karl Shuker has brought to my attention that I was far from the first person to have made this suggestion, and he has sent me a copy of a picture of a different bronze figurine from China that greatly resembles the one that I figured, but is clearly not the same specimen. It was assigned to *Pliohyrax* by F. M. Duncan (1935, A Chinese Noah's Ark, *The Field,* Vol. 166: 1286–87.) While I feel a slight pang of regret at being deposed as the originator of archaeological evidence for this large, semiaquatic extinct hyrax, I am delighted that further evidence for its possible survival into historical times exists and has been noted elsewhere.

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MORE ON THE OKAPI-PERSEPOLIS LINK

(Comment on Robert G. Tuck and Raul Valdez, 1989, Persepolis: Nilgai—Not Okapi, *Cryptozoology,* Vol. 8: 146–49)

Tuck and Valdez state that Henry M. Stanley saw a live okapi in Africa, and, when he carved his name at Persepolis, this resulted in the only real okapi-Persepolis link. Stanley carved his name at Persepolis when he was in Persia as a young man reporting for the *New York Herald,* just before he set out to find Livingstone (Henry M. Stanley, 1895, *My Early Travels and Adventures in America and Asia,* 2 Vols., Charles Scribner's Sons, New York).

We know that Stanley was in the Ituri forest during his expedition to relieve Emin Pasha in 1887–1889. However, he did not personally see an okapi, but gathered evidence from pygmy natives about what he thought was an unknown wild ass called *atti*—thus, his evidence was second-hand (Henry M. Stanley, 1890, *In Darkest Africa, or the Quest, Rescue, and Retreat of Emin Governor of Equatoria,* 2 Vols., Charles Scribner's Sons, New York: Vol. 2, Appendix B, p. 490).

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The Yahoo: An Improbable Hypothesis

(Comment on Malcolm Smith, 1989, Analysis of the Australian "Hairy Man" [Yahoo] Data, *Cryptozoology,* Vol. 8: 27–36)

Smith's examination of historical material about the Yahoo is well directed, but he fails to pursue it with sufficient rigor or in enough depth. For instance, his analysis of physical details omits mention of tree climbing or the possession of claws, facts which have a significant bearing on his ultimate conclusion. Similarly, in his case-by-case analysis Smith is able to write off Osborne's long, detailed account as a hoax only because he (Smith) fails to mention confirmatory material referring to color, tree-climbing, and the shape of feet. And it does not help that he misquotes Harper: the sighting took place *not* at a distance of twenty yards, but at "not twenty yards." The creature did *not* keep to the outer limits of the firelight so that Harper "could not possibly" have seen what he described. In fact, the animal was reflected in firelight (a "big blaze") which illuminated the scrub for some distance around.

The image suggested, however, is not wasted because Smith, throughout his analysis, keeps to the outer limits of the evidence he examines. His arguments abound with "coulds," "might haves," and "probablies," while the evidence is made to come from slow-witted, credulous fools with poor eyesight and heads stuffed full of preconceptions. The result is certainly a mundane conclusion; but is it the correct one?

The fact is that there is an extremely large number of references to the Yahoo in the 19th century, more than in any other comparable case I can think of. These contain a considerable amount of consistent detail, and nothing which is incontrovertibly a contradiction. This issue was examined previously in this journal (Graham Joyner, 1989, The Yahoo and the Nature of Zoological Discovery [Comment on Groves], *Cryptozoology,* Vol. 8: 136–139).

Smith can scarcely avoid the same conclusion, but instead inexplicably finds "a marked lack of detail" with "marked inconsistencies." The first is simply wrong, and the second seems to confuse inconsistencies with differences. But that is not all, because "inconsistency" somehow is elevated to "contradiction," and it is these supposed—but actually non-existent—contradictions which Smith uses to justify deliberate discounting of the evidence. A more reasonable view is that the problem lies in the analysis and not in the evidence at all. Smith correctly points out that, in the face of the unknown, perceptions are influenced by expectations, but he forgets to add that, here, this applies most of all to Smith himself.

The hypothesis he puts forward, that reports of the Yahoo were caused by sightings of particularly hairy Aboriginal outcasts, is open to several serious objections. First, the argument is invalid. Even if we accept that there were both wild tribal outcasts and extremely hairy individuals, it does not thereby follow that there were extremely hairy tribal outcasts. Secondly, the probability of such a combination occurring would be so low as to be negligible. It could not account for the large number of references in the geographical area and timescale actually found. Thirdly, it is not in accordance with the evidence. For example, of the four most comprehensive first-hand accounts, Smith badly misrepresents one, unjustifiably calls another a hoax, refers to a third as "aberrant," and quite overlooks the fourth.

Fourthly, one would expect to find evidence for hairy outcasts throughout Australian Aboriginal society, not only in the coastal forests and ranges of New South Wales. Fifthly, such a solution was never suggested by contemporaries. Sixthly and lastly, the Aborigines themselves, who could scarcely have been mistaken, had traditions about the Yahoo as an animal entirely separate from themselves.

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The Yahoo: Clarifying the Hypothesis

(Response to Joyner)

This controversy has the potential to generate more light than heat, but in view of Joyner's comments it would appear that my hypothesis needs clarification.

What I am hypothesizing is the type of popular delusion or craze which makes people misinterpret quite mundane events. It is a common phenomenon which, in 1987, resulted in both a naked lunatic and a fully-clothed hermit being reported as "ape-men," which places sinister connotations on every sound heard in a "haunted" house, and which repeatedly results in the planet Venus being reported as a flying saucer. (This does not mean, of course, that I reject all reports of hauntings or UFO's.)

In this instance, the focus of the delusion was not a sub-population of hairy tribal outcasts. They were individuals at the outer range of racial variation seen in special, but not abnormal circumstances, i.e., when roaming the bush by themselves and unwilling to communicate with outsiders. In that sense, they were anti-social; they need not have been rejects from their own society. Seen in a native camp, they would not have attracted comment,

any more than would a more normal Aborigine seen by himself in the bush. Nor would similar sightings made in areas where there was no myth to confuse perceptions. After all, Venus is never reported by people who *know* it is not a flying saucer.

I take the point that Harper's visitor was "not twenty yards" from the fire rather than exactly twenty yards. But, seriously, how relevant is this, given normal difficulties in estimating and remembering distances? The point is that the light of even a big fire does not go far at night, and a real wild beast would be unlikely to walk into the full glare of a fire. According to Harper, he was able to see a lot of detail, but caught only occasional glimpses of its feet. What is quite impossible is that he could have seen the color of the creature's eyes at that range, let alone their piercing quality. Those who have any doubts, should try it in broad daylight.

Of course, everybody knows that there is individual variation within a species, and variation among observers as to what features they observe and how well they remember. In this regard, I did list all references to shape of feet and hair color. Readers can decide for themselves whether they are mutually consistent and within the range of human variation. As for tree-climbing, that hardly rules out the Aboriginal hypothesis. Nevertheless, some described features are so striking that, if they were genuine, they ought to have been mentioned more than once. Notable are the arms of the Bombala beast, which were not merely long, but reached almost to its ankles. Even more exceptional was Harper's visitor, whose abdomen hung like a sack halfway down its thighs, which thighs were also much longer than its calves, and must have resulted in a very inefficient gait. It is best to regard them as misperceptions and exaggerations.

Osborne's animal had two features which ought to have been noticed by at least one other witness: a tan-colored streak from neck to abdomen, and feet twice as long as those of a similarly sized man—which must also have made it very clumsy. The Braidwood beast possessed neither of these traits. It was, however, surprisingly light for its size, and its face was abnormally broad and shaped like a polar bear's. The journalist who examined its carcass could not be certain it was not a wombat.

It would be unlikely that every single report was genuine. The reason I reject these last two is not that they fail to conform to my pet theory, *but that they fail to conform to the other data.* To accept them would imply that there are (? were) three species of Yahoo, two of which were seen only once! The method of cryptozoology is to seek the common factor in reports, and discount what clearly does not fit. When this is done to the Yahoo data, they are found to fit both my theory and Joyner's. Mine, however, obeys the rule of parsimony: it does not require the existence of either an improbable Australian bipedal primate, or an improbable primate-like marsupial.

Joyner mentions Aboriginal traditions, but what exactly do they say? It is not easy to interview members of another race without speaking their language or understanding their mind-set. One tends to hear what one expects to hear, and be told what one wants to be told. Those with most contact with the Aborigines, and who provided the most detailed reports, told of something that can best be translated as "ogre." And such beings exist in the folklore of many cultures worldwide without having any basis in fact.

For myself, I will be ready to change my views when I see evidence comparable to that surrounding the North American Sasquatch (Bigfoot). However, my opinion is irrelevant. People differ in the weight they place on eyewitness testimonies. Readers now have a summary of the facts and two rival theories. They can make up their own minds.

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ENLARGING ON SOME FOOTPRINT DETAILS

(Comment on Donald Baird, 1989, Sasquatch Footprints: A Proposed Method of Fabrication, *Cryptozoology,* Vol. 8: 43–46)

Baird suggests a method for making simulated Sasquatch feet, complete with dermatoglyphics, by soaking latex molds of human feet in kerosene to cause a 50 percent expansion. Thick copies of these could then be pounded into the ground while guarding against leaving one's own footprints. He does not claim to have made such "feet," nor to have left "footprints" with them. His contention is only that a fabrication of this sort could be made, and that, consequently, any realistic-looking tracks might not be authentic.

It is indeed possible to make fake Sasquatch tracks that duplicate many characteristics of those of claimed authenticity. I have devoted considerable time and effort to determine exactly what can and cannot be produced by artificial means. I have also experimented with the techniques proposed by Baird, and found that these will not duplicate certain critical items, such as differential widening and shifting of parts relative to each other. Furthermore, there are several other problems that merit some discussion.

1. Wetting a latex mold with kerosene will expand it in all directions, along with all of its included detail. Wetting just part of the mold will expand

that part—also in all directions equally. If one treats the edge of a foot-shape it gets wider, but that edge also elongates by a similar percentage. This throws the mold into convolutions that can be flattened out only with some difficulty. The result is not a wider "foot," but instead a fanned-out effect along the treated edge.

2. Sasquatch footprints have certain adjacent parts that are shifted in relation to each other, as compared with a human foot. The required cut-and-paste alterations would break the continuity of dermatoglyphic patterns, and this is demonstrably not the case. (I will not specify this item any further because it is one of two traits of Sasquatch footprints that have been kept confidential as a guard against successful faking.)

3. The most adhesive design of primate friction skin is with dermal ridges spaced apart at a constant 0.5 mm, regardless of the size of the hand or foot. Thus, a gorilla will have up to ten times as many ridges on a digit than does a small monkey. Baird's expectation that Sasquatch should have more widely-spaced ridges is understandable, but incorrect.

4. Rubber tracks certainly can be pounded into the ground by a human hoaxer, and it is possible that some of them were made this way. When the tracks occur in moist dirt or a dust-covered road, however, no amount of foot padding can conceal the hoaxer's own tracks. I have seen one such trail; investigators John Green and Bob Timus have examined and photographed others.

John Bodley, an anthropologist in my department, devised a similar procedure to produce a track with introduced dermal ridges. He made an actual cast (just one) from a ground impression of his device. I showed this cast to five different fingerprint experts, along with one of my "wild" specimens of similar quality. All of these experts independently identified Bodley's track as a fake within one minute, and could find nothing to question the authenticity of the other in as much as an hour's study.

My recommendation is that any suggestion about how such tracks might be faked should be accompanied by an actual demonstration.

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PROVIDING THE LACKING ELEMENT

(Comment on John Green, 1989, The Case for a Legal Inquiry into Sasquatch Evidence, *Cryptozoology,* Vol. 8: 37–42)

In my capacity as a practicing attorney interested in the Sasquatch phenomenon, my reaction to Green's article is basically a qualified *positive* one.

Green implores the legal sector to both advocate governmental inquiry into the Sasquatch phenomenon, and then sit on and/or assist inquiry panels in evaluating the evidence. Since most politicians are attorneys, it is perceived that there is an inherent appeal in such a notion. There should be incentive for practicing attorneys and politicians alike to endorse and sponsor this endeavor. They work with evidence and testimony all the time, and hence this is their bailiwick, as opposed to scientists, who, although knowledgeable—and, in fact, indispensable—in the field, are uncomfortable in that particular forum.

I most certainly agree with the premise that the legal and political community should be the avenue of appeal in promoting a Sasquatch inquiry. I do not believe, however, that simply calling upon contact with politicians by generally eloquent individuals or individual attorneys is a very constructive way to go about it. I think it is too myopic of an approach to the situation.

First of all, although most politicians indeed do have a legal background, once in elective office, the politician's constituency base is far greater than an attorney's counterpart, his clientele. As a practicing lawyer, my interest is limited to that narrow group that hires me for a particular purpose as expressed in a lawsuit on file in a court of law. If I were to be elected to office, that interest would become territorial, and a vast array of occupational interests would become encompassed within those boundaries. In short, I would have to be assured that my entire constituency's majority interest would be promoted, and not just that of a narrow, albeit educated, minority.

I realize that what I am about to state now is extremely self-serving, but I think it is nevertheless valid. I would like to openly advocate that Bigfoot (Sasquatch) supporters form a national, non-profit public benefit corporation, not only for tax purposes, but to better organize the vast resources of expertise that are available but collectively untapped. Ultimately, this would provide the one element that has been lacking in Sasquatch research to date—*legitimacy.* To put it into the context of the International Society of Cryptozoology, the organization could just as easily be *inter*national in scope.

As an individual attorney, it would be much easier for me to try to promote the efforts of the International Sasquatch Society, Inc. (that is an arbitrary name posed for illustrative purposes only) than to abstractly call for a Sasquatch inquiry. If I called for government support for microbiological re-

search, I might not get very far. It could very well come off as sounding too narrow and arbitrary.

However, if I asked for help funding the microbiology department of the American Medical Association (AMA), I might get a far more positive—and much quicker—response. The AMA has that kind of reputation. Their name imbues the cause with a "stamp of legitimacy." Likewise, it is far easier for me to campaign for the promotion of the goals of a distinguished portion of the scientific community than it is for me to push for the, at least, *perceived-to-be,* very narrow interests of an amorphous group of individuals. It would seem that politicians could use that same rationale to justify an inquiry because the promotion of the cause of such distinguished scientists, along with the lay interest that is out there, would be in the best *majority* interest of their constituencies. A non-profit identity would give them the "stamp of legitimacy" they would seek to make such a public appeal.

In sum, I agree in principle with Green's appeal to the legal and political realms, but I respectfully disagree with the means of motivating their support. I urge all Sasquatch advocates to act upon the non-profit alternative. I continue to regard it as the single most effective means of promoting the Sasquatch cause.

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LEAVE SASQUATCH TO CRYPTOZOOLOGISTS

(Comment on John Green, 1989, *Cryptozoology,* Vol. 8: 37–42)

Let me initially assert that I sincerely respect all that Green has done in his endeavors to prove the existence of the Sasquatch. I have eagerly read most of his books, and first met with him at his home in Harrison Hot Springs in 1974. During that time, I was engaged as counsel for Robert W. Morgan's expeditions into the Mt. St. Helens area of southwestern Washington. I also talked with him in 1978 at the Manlike Monsters on Trial Conference held at the University of British Columbia.

I was bemused over Green's suggestion that we enlist lawyers and politicians to set up some sort of ongoing fact-finding commission designed to take testimony of alleged eyewitnesses, and then sort out the truth after grueling cross examination. I may not be as familiar with the financial situation in Canada, but I do know that in the United States this would be absolutely prohibitive from a cost standpoint. What with the failure of our

savings and loans associations, the decline and eventual demise of Social Security, the lack of fair treatment of our own native Americans, the national debt, and now the Gulf War—please, no more lawyers, commissions, or tax-supported studies!

Actually, no one really wants to affiliate with lawyers if they can be avoided, whether it be Shakespeare in his admonition to "Kill All the Lawyers" or Biblical references to our counterparts, the scribes and the pharisees. We should remember that lawyers are trained to represent the "truth" in a light best suited to their clients. I believe Buddha once stated that "the truth is as one perceives it," and only how the truth is perceived by the trier of fact, whether it be judge, jury, or Congressional committee, is all the attorney will care about. One week later, he or she may be engaged on the opposite side of the fence, glibly convincing someone else *au contraire.* A Clarence Darrow once convinced some of us that we were descended from apes and monkeys, while a William Jennings Bryan almost convinced us that God made the Heaven and the Earth in but six days. Even the attorney in the motion picture *A Miracle on 34th Street* "proved" the existence of Santa Claus; but, more recently, all of King Richard's barristers and all of his solicitors couldn't put Humpty back together again after Watergate, leaving egg on the collective faces of our legal establishment.

Please, let us leave Sasquatch in the capable hands of the cryptozoologists. Their opinions are given much more credence, and they know how to better stretch a buck without depletion of our national treasury. I happen to concur with Grover Krantz that the only way to prove their existence is by killing one, capturing one, or finding its remains. Easier said than done, obviously, but far easier than retaining a cumbersome legal staff at perhaps $150 an hour per capita, for starters.

Once our creature's existence is established, then let the legal community mobilize around movie rights, protective legislation, constitutional rights, and cultural aid for their betterment. Then perhaps even I'll dust-off my old backpack and open a branch office in some remote valley in the realm of Sasquatchery.

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Lawyers and the Sasquatch: Choosing the Right Technocrat

(Comment on John Green, 1989, *Cryptozoology,* Vol. 8:37–42)

Green's paper is a clear indication that he is a victim of the obfuscation and propaganda that has made lawyers the high priests of the reigning political religion. (The term "political religion" itself is a redundancy. Both politics and religion are belief systems in which members take oaths, adore certain symbols, subscribe to certain rules, and are sanctioned for failure to follow the rules.) Green has more faith in the ability of lawyers to discern reality than do I. Lawyers are not the appropriate technicians to resolve scientific disputes. The scientific facts will evolve as they may to prove or disprove the existence of the Sasquatch.

Green argues that politicians should hire lawyers to interview those who claim to have seen Sasquatch. His postulate that "inquiry counsel could subject all [Sasquatch] witnesses to lie detector tests and to skillful cross-examination, and could investigate their testimony to their heart's content" fails to recognize that legal practitioners are only technocrats of the political religion in which they are licensed to practice.

Some lawyers are trained and skilled in the art of asking questions that do not allow the person questioned to weasel out of the question. (The term weasel is a spiciest form of speech named after a hairy, four-legged beast—thought by some to make a "pop"-like sound—considered to be a figment of the imagination before it was accepted as an authentic animal long ago.) That does not mean that lawyers are necessarily interested in the truth. The vast majority will reach whatever conclusion they are paid to reach, either in money or promises of heaven.

Two examples of inspired legal fiction, one from recent history and one from distant past, tend to mellow the apparent cynicism of this statement. In the United States, it was lawyers who ascended—there is a controversial etymology that links this word to a vulgar word for a raised buttock, and looking at the postures of some political-religious leaders lends credence to this connection—with the help and appointment from other lawyers to judgeships on the United States Supreme Court, and who declared in 1896 that separate facilities for members of different racial groups were equal under the United States Constitution (*Plessy v. Furgeson,* Supreme Court of the United States [1896] 163 U.S. 537). Yet, the same Supreme Court, with a new membership 58 years later, determined that separate facilities for the races were inherently unequal (*Brown v. Board of Education of Topeka,* Supreme Court of the United States [1954] 347 U.S. 483). In each case lawyers interpreted, under the color of law, what they believed were the proper social mores of the time. They were *not* deciding facts.

The attempts by those in power to set the standards the rest of us may or must believe is not new to the world. Most people now accept the notion

that the world—planet Earth—revolves around the sun. Yet, Galileo was excommunicated by 17th century political-religious pundits of the Roman Catholic Church for supporting Copernicus's heliocentric theory. It was not until the last decade that the soul of Galileo was "pardoned" by the new pundits who claim to hold the keys to the gates of the ecclesiastical heavens.

This, of course, might all be seen differently if an international gathering of lawyers and judges were to occur in the northern woods, and were to be visited by a Sasquatch!

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Lawyers, Evidence, Shakespeare, and Sasquatch

(Comment on John Green, 1989, *Cryptozoology,* Vol. 8: 37–42)

Green's suggestion that the existence of the Sasquatch might be established "as an accepted fact" by means of a legal inquiry into the evidence surrounding the phenomenon expresses an excess of confidence in the capacities of law and the legal profession, on the one hand, and in the level of public confidence in lawyers and their works, on the other.

Putting aside the question of whether a properly constituted tribunal *could* be persuaded to come to any useful conclusion on this vexed and vexing topic—a question to which I will return—I am not convinced that such a conclusion would be afforded significant weight. One has but to review the reception of the Warren Commission Report on the Kennedy assassination to be impressed by the extent to which those with strong beliefs (reasoned or otherwise) about a controversial subject are likely to be unpersuaded by the outcome of any complete and objective forensic inquiry running counter to those beliefs. More crucially still, data on the reputation for trustworthiness of the legal profession suggest that large segments of the informed public may be unwilling to accept that it is *possible* for lawyers to conduct a truly "complete and objective" inquiry.

And they may be right—not, I hasten to say, because lawyers deserve their collective reputation for venality, opportunism, and superficiality, but because the applicability of the legal model of dispute resolution to, for example, the Sasquatch debate is severely limited. The model of legal process which has been devised as a social mechanism for the resolution of various interpersonal and intergroup disputes is characterized by a variety of features

which do not lend themselves to the consideration and determination of arguments featuring historical and scientific evidence, both of which figure prominently in the "file on Sasquatch." One obvious example, which would tend to bias the outcome of any legal inquiry against the conclusion that Sasquatch exists, is the so-called "hearsay rule" of the law of evidence.

In essence, the hearsay rule provides that only the live testimony of "eye-witnesses"—first-hand accounts of phenomena under investigation—will be considered by a court or other tribunal; second-hand reports of the experiences of persons other than the witness generally will not be considered as proof that those experiences actually occurred. The rationale of the hearsay rule, of course, is that it assures that witnesses whose testimony is weighed will be subject to cross-examination, a legal mechanism for ascertaining "truth"—in which Mr. Green expresses, I think correctly, considerable faith. Unfortunately, however, the case for the existence of Sasquatch depends, in no small measure, on historical records and accounts of witnesses whose presence in court could not be guaranteed. And although the hearsay rule is subject to a number of exceptions, including one which applies when the person whose experience is being recounted second-hand is himself or herself "unavailable," there is no guarantee that any of these would extend to the bulk of the accumulated anecdotal reports bearing on the question of the existence of Sasquatch.

Obviously, an inquiry to be staffed and conducted by lawyers nonetheless could be so conceived and organized so that the bar to the admissability of evidence otherwise posed by the hearsay rule was eliminated—by the simple device of waiving the applicability of that rule. To a great extent, however, such a maneuver would serve to undercut the value of the enterprise by limiting the extent to which the inquiry could be usefully characterized as a "legal" one.

Even assuming, however, that these threshold difficulties could be overcome, there remains another which I suspect would prove insurmountable. Strange as it may seem, the processes of law are not actually very well-suited to resolving cases involving disputes over facts in which there is considerable evidence, of one kind or another, on each side. For all its elaborateness, legal procedure remains something of a blunt instrument where the determination of "truth," as that term might be used in a scientific context, is concerned. At the same time, society has no particular use for legal proceedings which yield inconclusive answers, or no answers at all. Thus, law has coped with its inability to deal with cases where the evidence on an issue is in something like equipoise by creating so-called "burdens of proof."

In many criminal cases, for example, there is some evidence to suggest the defendant's guilt, and some to suggest (contrariwise) his or her innocence. But the result in such a case will not necessarily be seen as a particularly close call—because the state is required to prove the defendant's guilt "beyond a reasonable doubt." In civil cases, the party seeking relief has a somewhat lesser burden to carry in establishing the version of the facts that undergirds his or her case: proof "by clear and convincing evidence" or "by a preponderance of the evidence" are typical formulations. Whatever the phraseology, however, the meaning is the same: To succeed in a legal inquiry, one party will be required to show that his or her version of the "truth" is significantly more likely to be true in fact than that espoused by the other party.

Who then, as between the proponents of the existence of the Sasquatch, on the one hand, and those who profess disbelief, on the other, would be likely to be assigned the burden of proof in a legal inquiry on the subject? Obviously, the answer to the question might be crucial to the outcome, and just as obviously, there is no precise precedent which would dictate the decision which the judges (or other decision-makers) making up the tribunal would be required to make on this "threshold" issue. If I were to predict, however, I would suggest that the party who challenges the "status quo" or accepted wisdom is, as a general matter, most likely to be assigned the burden. If I am right, the forensic model may well not be the best suited to fulfill Green's objectives.

Some years ago, I had the experience of representing the position of those who argue that the plays and poems generally attributed to William Shakespeare were in fact the work of his contemporary, Edward DeVere, Earl of Oxford, in a forensic argument presided over by three Justices of the United States Supreme Court (Boyle, Jaszi *et al.*, 1988, In Re Shakespeare: The Question of Authorship, *The American University Law Review,* Vol. 37: 617–823). I lost big, and granting that the case against Shakespeare is considerably less sympathetic than the case for Sasquatch, there nonetheless is a lesson to be derived from that loss. In a sense, it was a foregone conclusion from the moment that the Justices determined how the burden of proof would be allocated, and assigned me the task of proving my case "by clear and convincing evidence, not simply by a preponderance of the evidence" (Boyle, Jaszi *et al.,* 1988, above: 819). The same inertial influences which went into that determination also would be in play in any legal inquiry relating to the Sasquatch phenomenon.

Green suggests that legal thinking is somehow different from, and superior to, ordinary thinking. Although—on behalf of my professional colleagues—I gratefully acknowledge the compliment, I fear it is undeserved. Law and lawyers are as prone as science and scientists to be impressed by long-standing orthodoxies—at least as likely, that is, to give extra weight to conventional wisdom, and impose additional burdens on those seeking to upset it. As much as I would like to believe that law has something unique to offer to the resolution of this fascinating and important debate, I do not think that the case has been made.

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SASQUATCH INQUIRY: ONLY ONE FINDING POSSIBLE

(Response to Pincher, Ernst, Vogel, and Jaszi)

I am pleased that four members of the legal fraternity within ISC have taken my "Case for a Legal Inquiry into Sasquatch Evidence" seriously enough to comment on it, but I am left with some concerns about my competence in my own profession of journalism, since there seem to be several points on which I failed to make my meaning clear.

I am quite certain that I did not implore the legal sector to advocate governmental inquiry, since no such idea occurred to me. My suggestions were directed to everyone who has an interest in seeing the evidence investigated. Perhaps Pincher was too eager to get on with his advocacy of a nonprofit public benefit corporation to take in what I actually said. Since I happen to agree with his proposal, however, I am glad to have been able to give him the opportunity to present it.

It was good to hear from Ernst again, even indirectly, but I cannot agree with his contention that a legal inquiry would be prohibitively expensive—despite the fact that the lawyers involved would, as always, be overpaid. Considering the importance of this subject, and the kinds of things that governments already pay for, I think it would be an unusually prudent use of the taxpayers' money. And while I cannot give him the exact source, I suggest that he will find that, when Shakespeare penned the words "kill all the lawyers," he put them in the mouth of a criminal, not a responsible citizen.

Both Vogel and Jaszi, and perhaps Ernst as well, understood me to have expressed a high regard for the abilities of members of their profession, but with some outstanding exceptions—my father was a lawyer—I am not of that opinion. I subscribe to the sentiments expressed in the riddle:

Q. "What do you call 10,000 lawyers at the bottom of the sea?"

A. "A good start."

What I intended to convey was that, whether we like them or not, lawyers are the people who take seriously the kind of evidence—eyewitness testimony—which makes up the bulk of the case for the existence of the Sasquatch; and that, however much one may deplore the fact, lawyers are the people who, in the main, control our legislative bodies, and thus the allocation of government funds for scientific research.

In response to Jaszi, I did not intend to indicate that a legal inquiry could establish the existence of the Sasquatch "as an accepted fact." He has missed a step in the process. I meant to convey that, since lawyers must necessarily take eyewitness testimony seriously, and are indeed practised in evaluating it, a legal inquiry would have to conclude that Sasquatch exist; and that this could lead to lawyer/politicians making the funds available, probably during the inquiry as well as subsequent to it, for the sort of investigations that could produce proof acceptable to scientists. The sort of inquiry I suggested is not normally conducted on adversarial lines, although the witnesses are cross-examined, and I certainly did not advocate waiving the hearsay rule. I am entirely in agreement with that rule, and recognize fully that most of the classic Sasquatch stories would be of no value to a legal inquiry because the witnesses are dead.

The main point underlying my paper, which I seem not to have made sufficiently clear, is that the public attention aroused by such an inquiry would bring out so many current witnesses that, no matter how many might be found wanting, either in themselves or in the extent of what they could describe, there would still be too many with unshaken credibility and adequate substance for the lawyers conducting the inquiry to report any finding other than that the existence of the Sasquatch was proven.

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