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Special Guest Article in Celebration of the 50th Anniversary of the Discovery of the Coelacanth

REMINISCENCES OF THE DISCOVERY OF THE COELACANTH, *LATIMERIA CHALUMNAE* SMITH

Based on Notes from a Diary Kept at the Time

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Republic of South Africa

The Editor is pleased to present this special guest article on the discovery of the coelacanth by the discoverer herself, Marjorie Courtenay-Latimer, retired director of the East London Museum. Miss Courtenay-Latimer, Hon. Doc. Phil., an Honorary Member of the International Society of Cryptozoology since its founding in 1982, graciously consented to prepare this article, based on notes from the diary which she kept at the time. We are privileged to be able to publish this account—written in her own words—50 years after what many have called the zoological discovery of the century.—Editor.

This Article Is Dedicated to the Memory of Captain Hendrik Goosen, Who Died on January 5, 1990.

Could this day, December 22, 1988, really be 50 years since the greatest trauma in my life's career had occurred? Oh, yes! Reading through my old diary, I am again woken to the facts revealed therein.

I therefore pen these facts from my diary as a tribute to my friend and colleague Captain Hendrik Goosen, to whom the world owes a debt of gratitude; for without his interest and energy, there never would have been a coelacanth to astound the world of natural science.

I was appointed and commenced duty on August 24, 1931, to the small East London Museum. There was little to display. But the Latimers were collectors, and their objects and interests—in ethnology, ornithology, botany, and archaeology—and a fairly comprehensive collection of marine seashells,

FIG. 1.—Captain Hendrik Goosen and the author celebrating the 50th anniversary of their discovery of the coelacanth fish—seen at right. This photograph was taken in the Coelacanth Hall of the East London Museum on February 16, 1989, exactly 50 years after J. L. B. Smith confirmed that the "strange fish" was, in fact, a coelacanth. (J. Richard Greenwell.)

formed the nucleus of the exhibitions on view. Captain Goosen became interested, and he frequently visited the Museum to see how we were progressing. Thus a lifelong friendship was formed.

During the first 5 years of building the East London Museum, I had taken no leave, and by 1936 considerable leave was due me. I asked the East London Museum Board of Trustees to grant me leave to go to Bird Island to collect specimens. Bird Island was 40 miles east of Port Elizabeth, and my parents and I were taken there by packet boat, laden with supplies for us and for the small community of people living on the island.

We sailed from Port Elizabeth at 5 a.m. on September 2, 1936, and remained on the island for 6 months. For me, this was heaven on earth, for, wherever I looked, specimens were to be seen and collected. Sea birds of all species, nests and eggs, fish, seashells, mammals, and much more, came to a happy end for preservation in my hands. During this time, Captain Goosen, then skipper of the trawler *Aristea,* used to stop by the island to get fresh rabbits for a change of diet for his crew, and to watch the preparation of the specimens I had collected.

Towards the end of my sojourn on the island, he called in again, and I mentioned to him that I had collected 15 packing cases of specimens, but did not know how I would get them back to the Museum. He thought for a

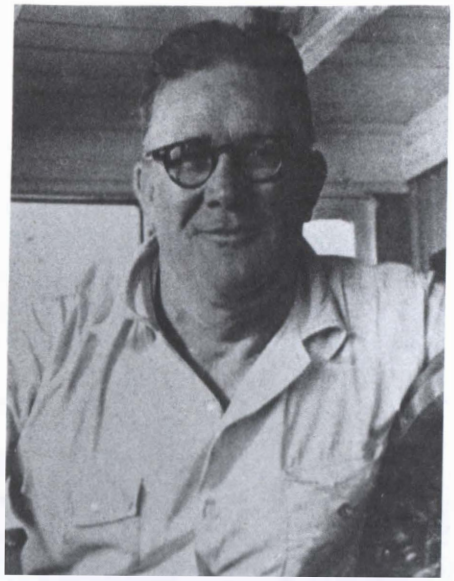

FIG. 2.—Captain Hendrik Goosen in the 1930's, when his nets brought up the "strange fish" which was saved for the author. (East London Museum.)

while, and then said: "Well, if you don't mind the wait, I can bring in one case at a time as we return to port."

How grateful I was to him. True to his offer, this is what he did. Each time the *Aristea* docked, a case was brought to the Museum, and then when he changed to the trawler *Nerine,* he finally delivered the last of the 15 cases of specimens.

Captain Goosen then said that, if we wished, he would continue collecting for the Museum, and could also collect live specimens for the Aquarium, which had been opened on December 2, 1931.

So, Captain Goosen, Mr. G. G. Smith (manager of the Modern Engineering Works and Vice President of the Museum), and I then designed a tank for collecting. This container was to keep specimens for the Museum, and there was another tank to hold water for live specimens for the Aquarium.

These tanks were then placed on the trawler *Nerine* through the kind permission of Messrs. Irvin and Johnson, who owned the trawler fleet. Thus Captain Goosen began his collecting, with our first consignment arriving on April 22, 1937.

The marine collection at the East London Museum grew; many beautiful specimens of coral and other treasures of the deep continued to be brought in. Unfortunately, we had little equipment at the Museum, so I had to

personally skin, cast, mount, and preserve all material brought to me as quickly as possible.

I had met Dr. J. L. B. Smith in the early 1930's, and he would often visit the Museum. In 1937, he became interested in the marine specimens being brought in by Captain Goosen, and he said he would be pleased to have any duplicates of fish specimens we did not need. Dr. Smith was then lecturer of chemistry at Rhodes University, in Grahamstown, but he was also very knowledgeable on South African fishes. I approached the Railway Superintendent, a Mr. van der Spuy, for permission to send such fish specimens over by motor transport to Grahamstown, and he agreed to do this.

And so, the stage was set for the discovery which was to follow. The notes below are extracted from my diary entries at the time.

December 22, 1938. A phone call at 9:45 a.m. (A new phone, our very first, had only just been installed on November 20, 1938): "Miss Latimer? This is Jackson of Irvin and Johnson speaking. There is a ton and a half of fish specimens for you on the trawler *Nerine*. Are you interested?"

Oh dear! I was so busy completing the display of the large fossil reptile *Kannemeyeria wilsonii* Broom, which we had excavated at Tarkastad, and for which Mr. Robert Wilson had given us a special display case.

I thought of how good everyone at Irvin and Johnson had been to me, including, of course, Captain Goosen and his crew. So, it being so near to Christmas, I thought the least I could do would be to go down and wish them the compliments of the season. I then phoned for a taxi, and took Enoch, my 20-year-old Xhosa assistant (who constituted my entire staff), and a grain sack, and set off for the Irvin and Johnson wharf.

When we arrived, I went into the office, and Mr. Jackson told me that all the specimens were still on the trawler *Nerine*. As I left the office, he called out after me, saying: "Miss Latimer, not quite a ton and half, but a Happy Christmas to you!"

All the crew had gone ashore except for an old Scotsman, who told me the specimens were on the fo'c'sle deck. He helped me up, and there I found a pile of specimens: sharks, rays, rat-tailed fish, sponges, gorgonian corals, etc., all looking much like what we already had in the Museum collection. I told the Scotsman I would not be taking any of them, but I nevertheless went through them carefully. I then noticed a blue fin sticking up from beneath the pile. I uncovered the specimen, and, behold, there appeared the most beautiful fish I had ever seen. It was a pale, mauvy blue, with faint flecks of whitish spots; it had an iridescent silver-blue-green sheen all over. Just on 5 feet long.

I gazed at the fish. What could it be? A voice broke the silence: "Yes, Miss, it is a strange fish. I have been trawling for over 30 years, but I have never seen its like. It snapped at the Captain's fingers as he looked at it in the trawl net. It was trawled with about a ton and a half of fish, and what

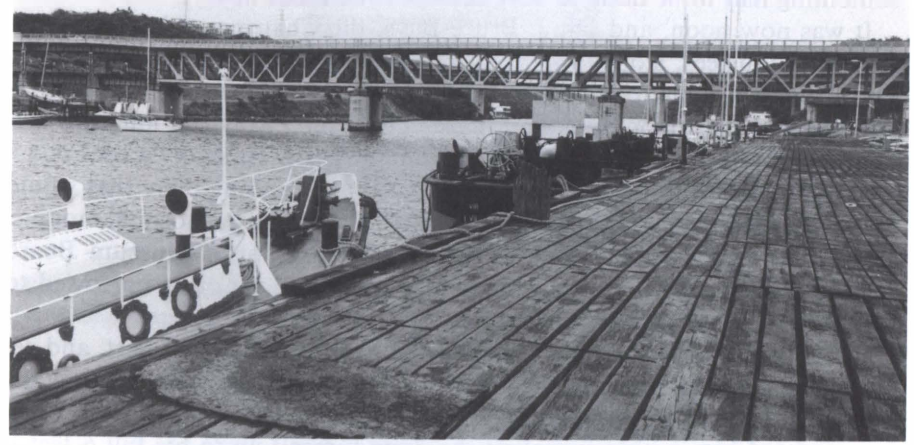

FIG. 3.—A 1989 photograph of the exact spot at the port of East London where Captain Goosen used to dock his Irvin and Johnson trawler, the *Nerine*, and where the author found the first coelacanth specimen. (J. Richard Greenwell.)

you see here." I told him that I would definitely take this one back to the Museum.

I called Enoch, and he and I carefully placed it on the grain sack and took it down to the waiting taxi. To my surprise, the taximan said: "I refuse to take any stinking fish in my car." I said: "It is not stinking. It is perfectly fresh, and if that is the case, you can go and I will get another taxi. I brought you here to collect fish for the Museum." With that, he relented, and stood with his hands on his hips while Enoch and I loaded it carefully into the boot [trunk] of the car.

At the Museum, we placed it on the table where I had just been working on the fossil reptile display, and then I began a literature search. I had little information on fish. I had *The Fish of South Africa* by Dr. Barnard, of the South African Museum, but little else. At the back of my mind I kept thinking of lines I had written at school: "A ganoid fish is a fossil fish." Could this be a fossil fish? I also had an old book, *The Study of Fishes,* by Albert C. L. G. Gunther, given to me by a Dr. J. Innes-Brownlee. Perhaps that might help.

I looked up ganoid fish on page 47. It stated that ganoid fish are hard and bony, and covered with a layer of enamel. So was the fish now lying on my table. Could it be? I continued searching, and on page 365 traced it, uncertainly, to Coelacanthidae. No, I thought, this is impossible. A ganoid fish is a *fossil* fish. This is a *live* fish! It cannot be a fossil fish! In any case, I knew something had to be done to save this specimen; but how?

It was now noon, and Dr. J. Bruce-Bays, the Chairman of the Museum Board of Trustees, stopped by. I showed him the fish with great glee. He said: "Oh, yes, a big rock cod." "No," I said, "not with these strange limb-like fins and that puppy-dog tail in the caudal fin." Maybe a lungfish? But then there were those hard, rugose scales! "Oh, yes," he said, "a rock cod." I then hurried off to the public library, but found nothing there that would help. I decided that a good place to preserve it would be the hospital morgue, and I sent Enoch with a note to Mr. W. E. Sargeant, a Museum Board member, asking for the use of his hand cart, which we often borrowed to fetch heavy supplies from town.

Enoch and I then carefully reloaded the fish onto the hand cart, and set off for the hospital, much to the annoyance of pedestrians walking along the pavement [sidewalk]. At the morgue, I asked to see Mr. E. Evans, but when I confronted him with my request, he drew himself up to his full 6 feet—his eyes almost popping out of his head—looked down his nose, and said: "I have never heard of such an iniquitous request! What would everyone think?" "Oh," said I, "they're all asleep anyway, and it would be just until I could get someone to classify it for me." "No! Most definitely no!" said he. Dejected, I went back to Enoch and the fish, and said: "Now let's go to the Cold Storage Commission." "Yes, Miss," answered my faithful assistant.

It was now 2:30 p.m. On arrival at the cold storage, I asked to see Mr. Latimer (no relation of ours). I asked him if we could keep the fish in the cold storage. He came out and looked at it, but said: "No, most definitely not—no stinking fish in the cold storage!" It was perfectly fresh, no odor about it at all. After a heated argument, I left, now even more despondent. I then decided to take it to Mr. Robert Center, an old Scotsman, who was a good taxidermist, but not very skilled at mounting fish. He agreed to save it, but said he needed formalin to preserve it for a few days. I went to Mr. J. Forbes, a chemist who had always been kind to me, and, although formalin was in short supply at the time, he agreed to spare me some.

My next stop was home, where I asked my mother, Mrs. E. Courtenay-Latimer, for a bed sheet. Back at Mr. Center's, we tore the sheet into strips and bound the fish with them, pouring diluted formalin all over it. We then covered it with newspaper. I returned to the Museum and tried to reach Dr. Smith on the phone, but Dr. J. Rennie, who was the lecturer on paleontology at Rhodes University, told me that Dr. Smith was out; he agreed to give him the message. I then wrote Dr. Smith a detailed letter, telling him about

FIG. 4.—Robert Center, the taxidermist to whom the author went in desperation to save the first coelacanth specimen in December, 1938. (East London Museum.)

the fish and the steps I had taken to save it, and requesting that he come and classify it for me. I delivered the letter to the Post Office, and then caught the bus home. Thus ended an exciting day!

December 23, 1938. I went down to Mr. Center's, and we checked the fish. It had now turned a very dark gray in color. I made a rough sketch, and wrote a *second* letter to Dr. Smith, enclosing the sketch. The early summer heat was intense. Time went by.

December 27, 1938. 9 a.m. I went down to Mr. Center's. He had kept the newspapers covering the specimen damp with formalin, but he told me he did not think we would be able to keep it much longer—it had been 6 days now—in the intense heat. I said: "Well, I'll return at 5 p.m. By then, we should have a letter from Dr. Smith." But by 4:30 p.m. there was still no news. I told Mr. Center we could not risk letting the fish rot, and that it would be best to skin it and save what we could. I suggested that he cut it under the belly to save the scales, rather than on one side—as most fish mounts were done in those days. I watched as he skinned it. The flesh was greeny-blue, and as he cut into the backbone, oil spurted out. I quickly gathered a bottleful. It was smooth and fine; it did not feel greasy, nor did it smell after 6 days. It had a very fine texture, and was pale yellow in color. He then cut the hard bony tongue out, which I took home to examine.

December 28, 1938. 5 p.m. I went to see Mr. Center again. He was cleaning the skin, and said: "Don't forget to bring the tongue back." Back home, I discovered that my mother had thrown it away! In desperation, I searched through the rubbish and found it! How close it had been to being lost forever.

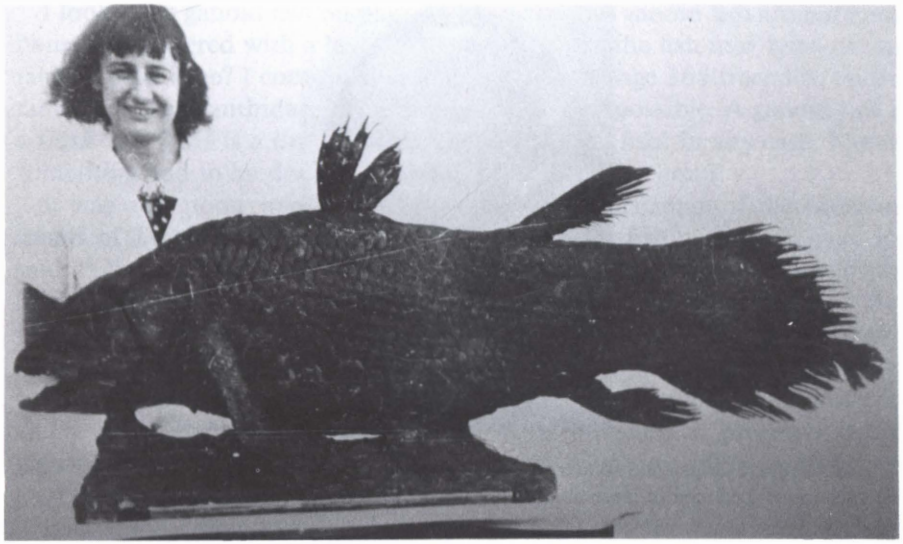

FIG. 5.—The author 50 years ago with the just-mounted first specimen of the coelacanth, later named *Latimeria chalumnae* in her honor by J. L. B. Smith. (East London Museum.)

December 29, 1938. 5 p.m. I took the tongue to Mr. Center. He was busy making a mannequin to put the skin over—the skin was very difficult to handle—in order not to disturb the scales.

January 9, 1939. Today, joy of joys, first contact from Dr. Smith—a wire reading: "SAVE VISCERA FISH INTERESTING SMITH." I hurried to Mr. Center, whom I had asked to keep the internal organs and tissues, but, alas, he said that, with the heat and length of time, they had been discarded after 12 days. He was also having difficulty with the skin, which was very oily and now a dark brown in color because of the formalin.

January 19, 1939. At last, a phone call from Dr. Smith saying he would come over to see the fish on February 16, at about 10 a.m.

January 24, 1939. A letter from Dr. Smith castigating me for the loss of the internal organs. Had he come when I first tried to contact him, on December 22, there would have been no loss at all.

January 25, 1939. Mr. Center brought the fish, which he had mounted as best he could, to the Museum. The Board of Trustees still did not seem interested. I had the mounted specimen in my office, on the fossil table, as we had no case big enough to enclose it. Wires, phone calls, and letters went back and forth between myself and Dr. Smith.

February 16, 1939. I walked to work at 6 a.m. Such excitement! My thoughts in a turmoil. Perhaps I had been wrong. Perhaps it was a rock cod, as Dr. Bruce-Bays had said. Oh, no, forget that thought! At 10:30 a.m., Dr.

FIG. 6.—J. L. B. Smith with his wife Mary Margaret in the 1930's. Upon seeing the coelacanth for the first time, he told the author: "Lass, this discovery will be on the lips of every scientist in the world." (J. L. B. Smith Institute of Ichthyology.)

Smith walked into my office, and I showed him the fish mounted on the table. He walked twice round the table looking at it, and confirmed that it was a coelacanth. He then said: "Lass, this discovery will be on the lips of every scientist in the world. I always thought someday one of these primitive fish would appear."

I had previously told the East London *Daily Dispatch* that Dr. Smith would be arriving on this date to see the fish. So a reporter and photographer came by. Dr. Smith said: "No photographs." I pleaded with him to let them take some pictures. I had previously taken photographs of the fish on the *Nerine* and at the Museum, but they were all ruined when the photographer, Mr. R. Kirsten, dropped the spool in the mud. "Oh, well," said Dr. Smith, "one photograph." That photograph went round the world, and Mr. Adams, the photographer, made a neat pile. The East London Museum had to buy back a copy from him for two guineas!

I asked Dr. Smith all kinds of questions, which he answered only vaguely. He sat down and said: "To think a fish of this age could exist." I asked: "How old is it?" He counted the rings on the scales, and said it was about 33 years old, but its primitive origins were more like 70 million years old. My mind boggled. So I had been on the right track. It was a living fossil!

February 22, 1939. Dr. Smith phoned to ask for the loan of the fish for

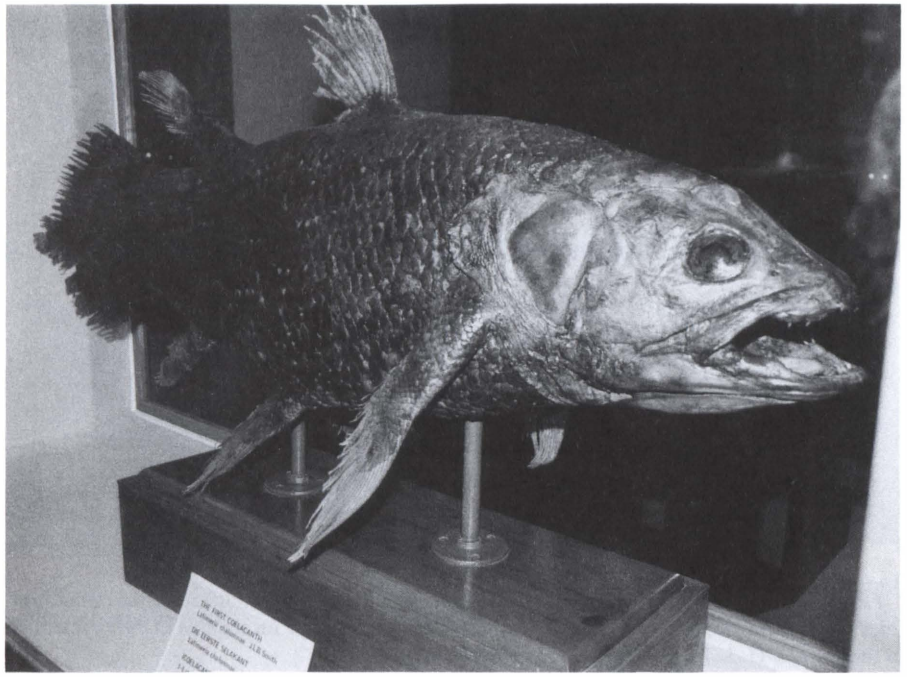

FIG. 7.—The first coelacanth—and the type specimen of *Latimeria chalumnae*—as it may be seen today in the East London Museum, South Africa, 50 years after its discovery by the author. The previous most recent coelacanth was a fossil genus dated at about 80 million years B.P. (J. Richard Greenwell.)

study. The Board agreed, but we were then pestered with visitors wanting to see it, and we had to tell them that the fish was being worked on by Dr. Smith in Grahamstown.

March 29, 1939. Dr. Smith phoned to say that he had named the fish *Latimeria chalumnae,* after me and the Chalumna River, in the mouth of which it had been caught at 40 fathoms. I said I thought it should have been named after Captain Goosen, who had brought it in for me; without him, there would have been no coelacanth. "But you ultimately saved it for science," he said.

Time went by. Finally, the Board decided that the fish should be returned to the East London Museum.

May 3, 1939. The fish was duly sent back to the East London Museum, but it was in sad need of proper mounting.

Dr. E. Gill, Director of the South African Museum, in Cape Town, then wrote to say that their taxidermist, Mr. J. Drury, would undertake the work if we sent it to them. South African Railways very graciously gave me a van

and free passage to Cape Town. The fish and I left East London on August 24, 1939, and I handed the specimen over to Mr. Drury at the South African Museum.

I then met Sir John Wedgewood, who said he would like to strike a Wedgewood commemorative plate of the coelacanth. War intervened, and years passed before he and I finally came to a decision about the plate: the coelacanth in the center, with my favorite marine seashell, the *Pecten,* around the rim.

December 2, 1939. The reconstructed coelacanth arrived back in East London in all its glory. It was a beautiful piece of work by Mr. Drury, together with a very handsome cast of the coelacanth. In thanks and gratitude, we gave the South African Museum a complete colored cast.

December 5, 1939. A wonderful day. The East London Museum received 300 pounds from the Carnegie Trust Fund. This was a godsend to us. We had two cases made to house and display the original specimen and the colored cast, costing a total of 200 pounds. The remaining 100 pounds the Museum sent in gratitude to Captain Goosen.

The coelacanth was now safely housed in the East London Museum. The Board of Trustees then decided to adopt it as the special Museum emblem. Stationery was printed with an impression of the coelacanth, all its crockery bore the emblem, and there was the beautiful Wedgewood plate specially struck to commemorate this remarkable discovery of December 22, 1938: the first coelacanth, *Latimeria chalumnae* Smith, the type specimen, weighing 127 lbs.

And a very, very happy young Museum director decided all the trauma had been worthwhile.

Cryptozoology, 8, 1989, 12–26

PALEOCRYPTOZOOLOGY: A CALL FOR COLLABORATION BETWEEN CLASSICISTS AND CRYPTOZOOLOGISTS

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ABSTRACT: Paleocryptozoologists should become aware of the extensive body of ancient textual and archaeological evidence for Mediterranean crypto-animals. A cross-referenced catalog of interdisciplinary sources concerning ancient cryptids in art, literature, and archaeology is needed to make the material accessible. A selected source-list of ancient texts shows how Heuvelmans' (1968) chronology of sea monsters could be extended back to about 700 B.C. No previous identification of the griffin by cryptozoologists or classicists accounts for all of the relevant evidence. Ancient interest in paleontology has been underestimated by modern scholars. Modern Aegean and Asian archaeological and paleontological field reports of "anomalous" remains could also contribute to classical cryptozoology.

INTRODUCTION

According to Roy Mackal, "It is the concern of the cryptozoologist to . . . determine whether or not a particular case represents something solid, real, and, perhaps, unique"; that is, whether the evidence suggests an imaginary creature, a "relict species," or a "mistaken identification of an ordinary animal." As Mackal notes, it is the responsibility of "those who have the requisite resources" to make them available for "the development of science" (Mackal 1980: xii). In this paper, I want to draw attention to an extensive body of neglected cryptozoological evidence in ancient literary and artistic sources, and in archaeological collections and publications.

If cryptozoologists are serious about the need to address "professional skepticism" due to "fragmentary or inadequate evidence" and the need to make our work "a legitimate and recognized scientific activity" (Mackal 1980: xii), the use of these well-documented scholarly resources would go far to enhance the credibility of paleocryptozoological research.

Since the Renaissance, classical scholars have documented literally thousands of texts and artifacts depicting Greco-Roman "unknown" animals. Classicists usually analyze this textual and physical evidence for artistic, historical, or symbolic significance only. This vast, diverse resource is rarely consulted by paleocryptozoologists. Moreover, as paleomammalogist Christine Janis has commented, "There may be a wealth of . . . examples of fossil animals represented in archaeological artifacts or artwork awaiting a concerned researcher in cryptozoology," and "the significance of such artifacts is more likely to be noted by paleontologists than by archaeologists because of [paleontologists'] familiarity with animals represented in the fossil record"

(Janis 1987: 10, 22). Indeed, modern archaeological field reports refer to "exotic" faunal remains in many Mediterranean sites, but survey teams tend to pay little attention to anomalous or extinct remains (Reese 1985). If these obscure reports were cataloged, interesting cryptozoological patterns might emerge.

The lack of a systematic bibliography of archaeological, literary, and artistic sources has meant that cryptozoologists trying to identify unknown animals of antiquity have had to rely on incomplete citations of traditional (and sometimes medievally corrupted) authorities and on a few supposedly "typical" artifacts. Those who want to determine whether observations of "extinct" animals in historical times might have inspired ancient narratives or depictions of unknown creatures need to know about the relevant artifacts in museum collections. Illustrations, dates, and provenience do appear in classical publications, but these are often hard for non-classicists to track down. And none of the ancient material is indexed in cryptozoological terms.

The task of collecting and classifying the immense corpus of Greco-Roman artifacts—and then correlating this evidence with all the known written accounts from a cryptozoological point of view—is daunting, but the benefits would be far-reaching. Three major problems need to be overcome: (1) the original sources are written in ancient Greek and Latin; (2) many of the artifacts and remains are in far-flung or obscure museum collections, or only described by the original excavators in brief notes and appendices; and (3) descriptions and discussions are usually published in journals read only by classical scholars. Obviously, the task requires ongoing collaboration between cryptozoologists and interested specialists in classics.

Fortunately, most of the literary portion of the project—gathering references to unknown animals in written sources—could be accomplished fairly quickly using the recently completed *Thesaurus Linguae Graecae* (*TLG*) IBYCUS computer system. The *TLG* contains on a single laser disk all ancient Greek literature (the collection of Latin texts is under way); it is available in many university classics libraries. Using IBYCUS, anyone who knows ancient Greek can call up from the *TLG* every mention of a given crypto-animal in minutes. For example, a search of ancient Greek literature for the word *gryps* ("griffin" in ancient Greek) turned up over 200 passages written by more than 60 authors. A cryptozoologist working with a classical scholar could undertake similar searches for words or phrases denoting "sea monster," for example, and produce a printout of every passage in Greek literature that refers to marine monsters. The passages would then need to be translated, analyzed for relevance, and classified by type.

Gathering the artistic and archaeological evidence is more challenging, but, again, a cooperative effort by classicists and cryptozoologists makes the project feasible.

This article points out the extent of the evidence for ancient crypto-animals, and suggests how a cross-disciplinary approach could advance studies in cryptozoology, classics, art history, folklore, archaeology, and other fields. I include a selected bibliography of ancient sea monster sources as an example, and some representations of classical crypto-animals not ordinarily consulted by cryptozoologists appear as Figs. 1 through 4. I hope this article will stimulate cryptozoologists and scholars in classics and related fields to contribute to this interdisciplinary project.

Unidentified Crypto-animals of Antiquity

The many "unknown" animals in ancient texts and artifacts are carefully recorded by classical scholars according to date, mythological or artistic style, and place of origin. The characteristics of the narratives and artifacts could be keyed into established or new cryptozoological classes. These types could then be cross-checked against other classification genres. For example, the *sirrush* dragon of the Babylonian Ishtar Gate (ca. 600 B.C.) has many features similar to those of Greek griffin-types and the (Persian) Persepolis monster; and some characteristics of the crested/maned Aegean sea serpent are similar to those of crested Scythian griffins. Yet, these similarities are typically attributed to *artistic* influence alone by art historians (on *sirrush* see Ley 1948: chap. 6, Heuvelmans 1972: 286–88, Lum 1952: 24, Costello 1979: 106).

The Griffin

Hundreds of quadruped predators with bird- or lizard-like features exist in Mediterranean and Middle Eastern artifacts, although cryptozoologists are aware of only a few frequently published illustrations of "griffins." Ancient Greek and Latin descriptions of griffin habitat, behavior, and appearance are datable and detailed; unfortunately, only a few translations are familiar to cryptozoologists. Classical scholars assume that these creatures—classed as "griffins" despite anatomical variations—are either imaginary or misrepresented real animals. Few non-classicists realize that a great variety of unknown animals appear on Scythian gold, bronze, wood, and leather objects from 3000 to 100 B.C. (Metropolitan Museum of Art 1975, Basilov 1989). Illustrations and photographs of the extensive variety of griffins in collections are available in publications that are often inaccessible to non-classicists (Archaeological Museums of Samos, Delphi, Olympia, Pella, Corinth, and Athens, Greece; Bisi 1965, Bouras 1983, Flagge 1975, Toynbee 1982: 287, 290, 291).

Cryptozoologists and folklorists attempting to identify the *gryps* or "griffin" tend to cite the well-known ancient authorities (Aristeas, Ctesias, Herodotus, Aelian, Aechylus, and Pausanias), and the standard depictions of griffins

are arbitrarily assumed to be "typical." Consequently, they have concluded, along with classical scholars, that either (1) the griffin was a misidentified real creature, such as an eagle, vulture, or other large raptor, or a Tibetan mastiff, or (2) the griffin was an imaginary invention symbolizing vigilance or the difficulties of gold-mining in the Asian steppes (Jennison 1937: 115, Bolton 1962, How and Wells 1928: vol. 1, 307, with references, Liddell and Scott 1940: sv *gryps,* Ball 1883, Costello 1979: 71–82 and references pp. 193–95). But the first conclusion is undermined by the fact that other representations of real raptors and mammals from the same time and place are anatomically accurate. As for the second conclusion, symbolic creatures of antiquity tend to be obviously imaginary hybrids, such as mammals with wings or human heads.

Identifications have depended on selecting only some attributes, and excluding others, to suit a particular theory. Neither the "mistaken identity" thesis nor the "symbolic animal" idea attempts to account for all of the factors (anatomy, habitat, behavior) associated with the griffin in antiquity. Neglected are the thousands of lesser-known depictions of four-legged predators with raptorial or reptilian features, large claws, scales, crests or manes, and distinctive knobbed heads (Fig. 1).

The *geographic landscape* of Central Asia where the legend of the griffin originated has not been considered, nor has the significance of the artifacts suggesting *behavior*. Griffins are shown attacking their prey (horses, mountain goats, elk, geese, deer, and humans); fighting with lions, humans, or other griffin-types (pedimental sculpture, Temple of Apollo, Delphi Museum, Greece; Bolton 1962: 87, Rudenko 1970: 235–36); being captured (along with zoologically accurate known animals) in a baited trap ("Great Hunt" mosaic, Toynbee 1982: 29, 40–41); and even with their young (bronze metope covering, Archaeological Museum, Olympia, Greece).

An adequate working hypothesis for identifying the griffin should accommodate all of the information in the ancient texts and artworks, and should incorporate ancient and modern discoveries of large prehistoric bones in the griffin's traditional habitat (Mayor, in preparation). The carefully documented work on the possible identification of the Egyptian animal god Set is a model of the kind of cross-disciplinary approach that could be undertaken for the griffin (Swords 1985).

Wild Men, Scythian Warriors, Pygmy Hippos

The accounts (e.g., Pausanias, Pliny, Apollodorus) of "hairy wild men and women" encountered in antiquity have not been analyzed from a cryptozoological point of view (Costello 1979: 62). Analysis should consider literary sources in relation to artistic representations; for example, the Scythian mirror-back that shows shaggy men battling a griffin (Bolton 1962: 89–91, Metropolitan Museum of Art 1975: plate 4).

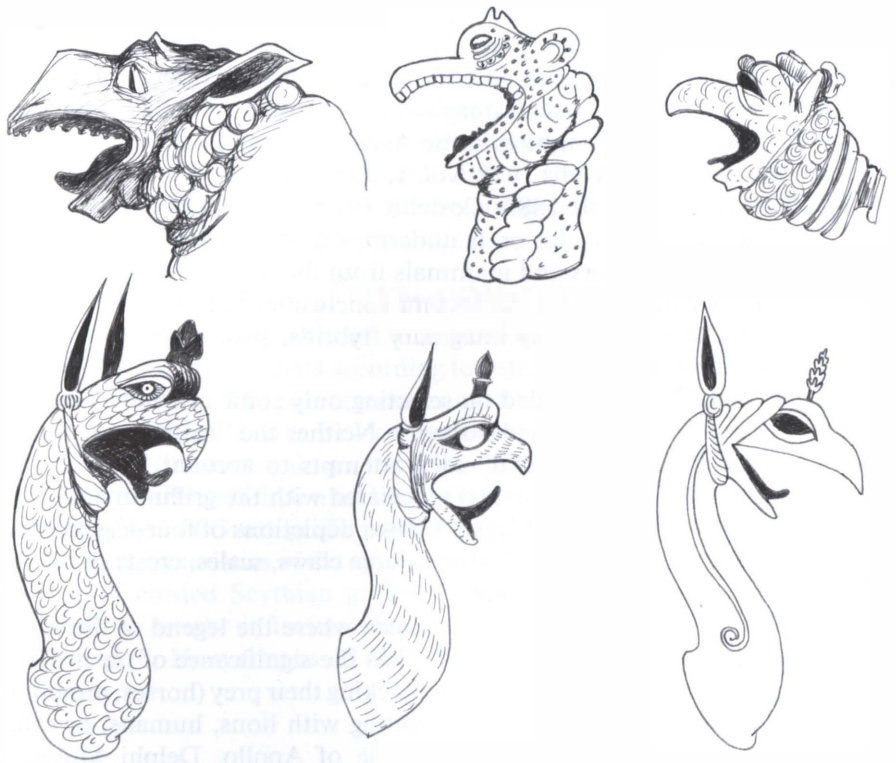

FIG. 1.—Six examples of the scores of Archaic bronze "griffin" heads from the Temple of Hera, ca. 7th–6th century B.C., Samos, Greece. They range from brutish and bulky, apparently "realistic" depictions, to elegant renditions in which some details have evolved into decorative motifs; yet certain anatomical features remain consistent. (Scales vary; drawings by Adrienne Mayor.)

Most classicists and cryptozoologists are surprised to learn that the mummified bodies of 5th century B.C. Scythian warriors, discovered in frozen tombs in central Asia, are covered with tattoos of realistically-drawn known and unknown animals. The tattoos themselves may have shamanistic meanings; interestingly, many of the unknown animals conform to griffin-types (Rudenko 1970). Moreover, the steppes where the griffin legend arose has extensive fossil remains revealed by erosion, raising the possibility of a connection between observation of petrified bones and artistic depictions of "strange" animals (Mayor, in preparation).

In 1926, several huge terracotta heads with knobs, tusks, ridges, and bulging eyes were unearthed (Karo 1934) from two ca. 8th century B.C. sites in Greece (Archaeological Museum, Nauplia, Greece). Certain features of the "gorgon-like" masks call to mind the skull and teeth of the hippopotamus

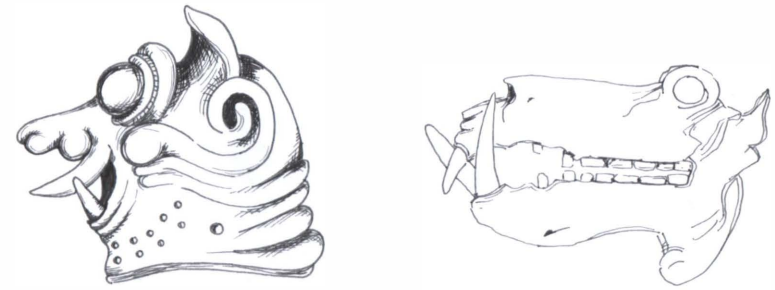

FIG. 2.—Left: one of the "grotesque" terracotta masks (large enough to fit over a human head) from Tiryns, ca. 8th century B.C., Archaeological Museum, Nauplia, Greece. Right: a possible model—the skull of a prehistoric hippopotamus. (Drawing by Adrienne Mayor.)

(Fig. 2), whose canines and fossil bones are common in the Aegean. The pygmy hippopotamus was once believed to have become extinct in the Aegean long before the appearance of humans. But recent (and controversial) archaeological discoveries indicate that humans may have hunted the pygmy hippopotamus to extinction on Cyprus 10,000 B.P. (Simmons 1988, *Science News* 1988). The grotesque heads could have been based on fossil discoveries or observations of the pygmy hippo before its extinction.

SEA MONSTERS

Hundreds of detailed depictions of ancient Mediterranean sea monsters are fully documented, dated, and typed according to artistic style and place of origin. Yet no scholar has classified these depictions from a cryptozoological standpoint. Existing cryptozoological discussions of Mediterranean sea monsters (Heuvelmans 1968: 45–46, 48, 82–84, 86–87, 135, 241, 593, Ley 1948: 92–94) could be supplemented with ancient accounts of lesser known but significant—and datable—reports of "out of place" and "unknown" sea creatures that washed up on coasts, remains or models displayed in Rome and Greece, and sailors' encounters with unidentified marine creatures (Mayor 1985).

Besides the extensive and diachronically consistent evidence for two distinct sea monster types (Fig. 3), the *ketos* ("dog-headed sea dragon") and the *hippokampos* ("mer-horse"), ancient texts written between 700 B.C. and 500 A.D. also describe giant eels, sharks, octopuses, sea turtles, snakes, and squid; the Hydra; "dragons"; and mer-people (see Selected Annotated Bibliography). Using the IBYCUS system, every mention of unusual marine creatures could be gathered, and then cross-checked with representations of sea monsters in vase paintings, mosaics, sculpture, bronzes, jewelry, ivories, seals, gems, and coins (Shepard 1940, Thompson 1947, Boardman 1987) to develop a typology of anatomy, habitat, and behavior, structured along the

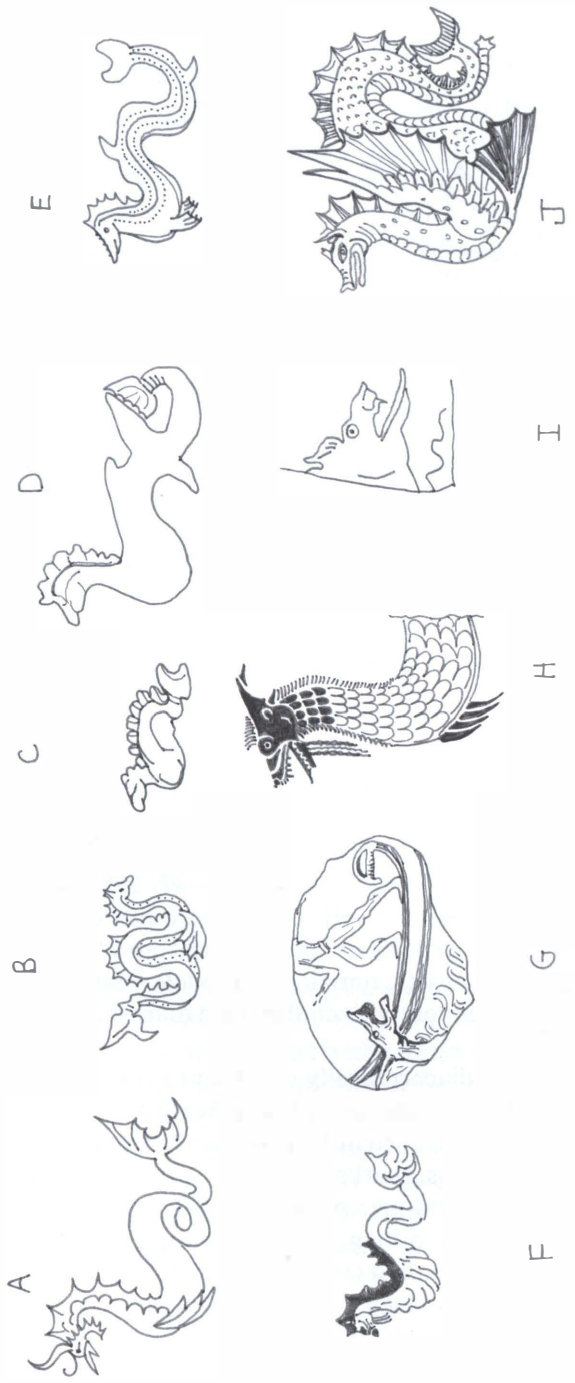

FIG. 3.—Ten examples of representations of the *ketos* sea monster type. A. Greek vase painting, ca. 500 B.C., Louvre, Paris. B. South Italian vase painting, ca. 4th century B.C., Wurzburg. C. Scythian (from Crimea) ornament-pin, Birmingham, England. D. Marble relief, from Melos, National Museum, Athens. E. South Italian vase painting, ca. 4th century B.C., Czartoryski Museum, Goluchow. F. Sculpted relief from Temple of the Via San Leonardo, Orvieto. G. Clay seal from Minoan Knossos, ca. 3000–1400 B.C., Archaeological Museum, Herakleion, Crete. H. Painted plaque, from Corinth, ca. 7th–6th century B.C., Berlin. I. Vase painting, from Corinth, head of sea monster that menaced Andromeda, 7th–6th century B.C., Berlin. J. Gold ornament, Scythian, 4th century B.C., Hermitage, Leningrad. (Scales vary; drawings by Adrienne Mayor.)

lines of the "annotated checklist" by Heuvelmans (1986). Preliminary research suggests that the features of sea monsters and encounters with them in antiquity are remarkably congruent with the exhaustive chronology by Heuvelmans (1968), which begins with the year 1639. When that chronology is extended back to antiquity, the record of classifiable sea monsters should become even more compelling.

Ancient and Modern Paleontology

"Exotic" faunal remains excavated by archaeologists at Greek sites include such anomalies as non-Mediterranean shark vertebrae and teeth, fossils, and shells; unexpectedly large cattle, horse, and human bones; extinct ostrich eggs; and Pliocene and Miocene lion, elephant, and pygmy hippopotamus bones and teeth (Reese 1984, Rapp 1978: 69–70). Such findings could be fruitfully pursued by cryptozoologists. Archaeologists rarely follow up on these "unclassifiable" items, and only recently have they begun to include experts in animal remains on survey teams (Reese 1985). Zooarchaeologist Simon Davis (1987: 20–21, 118–25) points out that his field only began to develop as a true discipline with the founding of the International Council for Archaeozoology (ICAZ) in 1976. (This organization publishes a newsletter, as well as lists of addresses, research topics and other items of possible interest to cryptozoologists. The address is: ICAZ, Biological-Archaeological Institute, University of Groningen, Poststraat 6, 9712 ER Groningen, Holland.) David Reese, of the Field Museum of Natural History, in Chicago, is one of the few scholars who publishes research (1976, 1981, 1984, 1985) combining Mediterranean paleontology, classical archaeology, and folklore.

No single source collects and analyzes the Greek and Roman accounts of the discovery of fossils in the Mediterranean and Asia Minor. Although modern scholars underestimate the ancient interest in paleontology, many ancient sources describe fossil hunting by Greeks and Romans in locations now confirmed by modern paleontological work as fossiliferous. Greece and Central Asia have very rich fossil beds yielding extinct mammal material, including *Samotherium* and other giraffids, the immense *Dinotherium* and *Baluchitherium, Hipparion, Tragocerus, Ictitherium, Dinocyon, Hyaenarctos, Metarctos, Helladotherium, Pliohyrax, Hystrix, Orycteropus, Chalicotherium, Ancylotherium,* and an assortment of other forms, including felids, rhinos, hippos, elephantids, gazelles and antelopes, a giant tortoise, and a giant ostrich (Mayor 1983, 1984, Woodward 1901 with notes, Bate 1905, Melentis 1961, 1982, Wendt 1968: 237–43, Andrews 1926, Davis 1987, Osborn 1921: 200–71 with notes, Gore 1982: 704–06).

Modern archaeological excavations show that prehistoric skulls, teeth, eggs, and fossils were collected or acquired by trade in antiquity (Reese 1981, 1985), and stored in treasuries and shrines along with other valuables (Rapp

1978: 70, Davis 1987: 118, Andrews 1926: 268–98). Although some scholars have raised the possibility that "fabulous" animals in narratives and artifacts could have been based on discoveries of fossils (Carrington 1957: 71, Bolton 1962: 84, Lum 1952: 25, 48, Ingersoll 1928: vi, Thenius 1973: 32–38), the acceptance or rejection of this theory for ancient crypto-animals tends to be arbitrary and contradictory.

It is well known among cryptozoologists that U. Vogelsang, who sculpted the dragon of Klagenfurt, Austria, in 1590, based the statue on the skull of an Ice Age woolly rhinoceros which had been dug up in a nearby quarry in about 1000 A.D. (Thenius 1973: 37–38, Ley 1948: 53–54). Paleontologists and folklorists generally accept that the legend of the one-eyed ogre Cyclops was inspired by observations of elephant skulls, common in the Mediterranean region (Thenius 1973: 37, Ley 1948: 47–51, Wendt 1968: 19, Carrington 1956: 50, Swinton 1966: 21–22). Nevertheless, the idea that the ancient Greeks and Romans might have *consciously* attempted to reconstruct an unusual animal from its fossil remains has been rejected "because the ancients were not paleontologists," and classical scholars dismiss their interest in fossils as "accidental," "never deliberate" (Phillips 1964, Heuvelmans 1972: 286, Costello 1979: 76, Ley 1948: 54, 162, Swinton 1966: 20).

In fact, classical texts attest to the planned excavation and rational identification of the relics of past cultures and "extinct" flora and fauna from fossil beds. Theories of evolution and extinction had been developed in Greece as early as the 6th century B.C. (Matthews 1962: 144–45, Osborn 1929: 39–102). Reliable ancient sources relate that, when fossils were discovered in antiquity, they were transported with great care, identified, preserved, restored, and sometimes traded. Reconstructed models or the remains of "unknown" species were displayed in Greece and Rome, as were skins or actual specimens of living "unknown" animals (Halliday 1928: 207–09 with references). Identifications were proposed for fossil remains, based on a relatively sophisticated understanding of history, geography, natural history, and anatomy (Phillips 1964: 177). Although the Greeks and Romans certainly were not "paleontologists" in the modern sense, numerous passages (for example, in Herodotus, Pausanias, Aelian, Suetonius, Plutarch, Philostratos) support the theory that some representations and descriptions of crypto-animals in antiquity were based on reconstructions from skeletons of living or extinct animals (Mayor, in preparation).

Some artifacts, such as the group of large "grotesque" terracotta heads in the Nauplia Museum (Fig. 2), seem to be good candidates for the reconstruction theory. Working with specialists in other disciplines, paleocryptozoologists could integrate the artistic record with the ancient texts and the modern excavation reports to determine (1) whether individual or combined features from prehistoric fossils were used as models, or (2) whether "living fossils" might have been observed in historical times.

Interdisciplinary Approach

The Greek island of Samos exemplifies the richness of the unanalyzed data discussed in this article. Ancient literature, folklore, and artifacts, and modern archaeology and paleontology intersect in ways that could benefit from the kind of collaboration suggested here. Since at least the 5th century B.C., huge bones unearthed from red gullies of central Samos were thought to be the remains of giant monsters called Neides whose roars were said to cause earthquakes. In Hellenistic times (3rd–1st centuries B.C.), the bones were identified as the skeletons of a herd of Indian elephants that died on Samos while being transported to Greece by the god Dionysus. Alternatively, the bones were regarded as marking the burial ground of (giant) Amazons slain in battle on Samos (Halliday 1928: 208).

The Samos Archaeological Museum has a superb collection of bronze heads of unknown animals excavated from the Temple of Hera. All are identified by art historians as 7th century B.C. "griffins," despite salient differences (see Fig. 1). The Museum also contains a small carved wooden head of an unidentified animal also excavated at the Temple. Its small eyes, snout, and fangs bring to mind features of the wolf, baboon, or shark/dolphin (Fig. 4).

Indo-Pacific giant clamshells, ostrich eggshells, hippopotamus canines, and North African antelope horns were also retrieved from excavations in the ancient Temple area (Reese, personal communication, Boessneck and von den Driesch 1981). And in Mytilini, a little-known paleontological museum displays the fossils of the giant *Samotherium* and other large prehistoric mammals excavated in modern times from the red gullies of the Nekrotafion Zoön ("animal graveyard"), where huge bones had come to light in antiquity (Melentis 1982). Few art historians who study the Samos griffins are aware of the island's paleontological deposits. Cryptozoologists interested in identifying the griffin should note this coexistence of possibly related resources. The island of Samos is just one example of a site where classical texts and legend, ancient artifacts and fossils collected in antiquity, and modern paleontology coalesce. The region of ancient Scythia is another.

Conclusion

This brief survey indicates the wealth of ancient material that could be integrated into existing or new cryptozoological classes. The material is wide-ranging, from ancient Greek and Roman literary and artistic works to modern archaeology and paleontology. Collecting the scattered and sometimes obscure evidence into one accessible resource would require a long-term and close collaboration between classicists and cryptozoologists, but there is no question that it would prove to be *mutually* beneficial. If the many "unidentifiable" creatures depicted in artifacts were integrated with all of the ancient written references to "unknown animals," and checked against an-

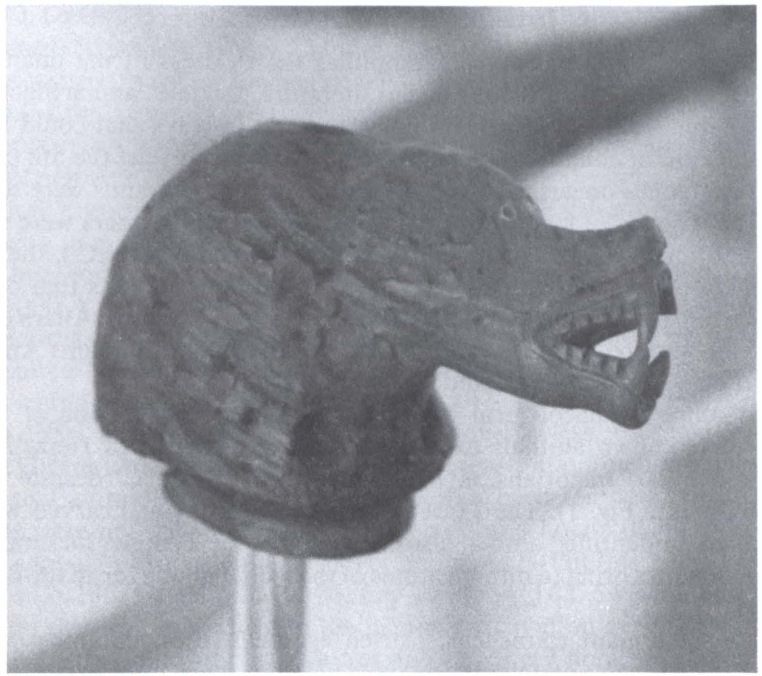

FIG. 4.—Carved wooden head of unidentified animal, 6th–7th century B.C., Samos Museum, Samos, Greece. About 5 inches high. (Photo by Adrienne Mayor.)

cient and modern discoveries of extinct or unknown animals, the contribution to both paleocryptozoology and classics could be far-reaching.

ACKNOWLEDGMENTS

Helen Michaliades of the Samos Museum, Greece, encouraged my early interest in the origin of griffins; the late Professor Eugene Vanderpool of the American School of Classical Studies in Athens told me about the bone beds of Greece; and David Reese of the Field Museum of Natural History, Chicago, generously shared his extensive files on "exotic" Mediterranean archaeology and paleontology. It was Josiah Ober, Professor of Ancient History, Montana State University, who first invited me to Greece and spurred me to write it all down.

SELECTED ANNOTATED BIBLIOGRAPHY FOR SEA MONSTERS OF ANTIQUITY AS AN EXAMPLE OF SOME IMPORTANT PRIMARY SOURCES

Sources (700 B.C. to 550 A.D.) for the sea monster (a complete list of every ancient Greek source can be produced by the *TLG* IBYCUS system):

Aelian 13.21, 15.19 (pickled triton and Damostratos' description), 14.23 (*ketos*)
Apollodorus 2.5.9 (Hesione and sea monster), 2.4.2–3 (Andromeda and sea monster)
Aristophanes *Frogs* 556, *Thesmo.* 1033 (*ketos*)
Aristotle *HA* 4.7.12, 8.26.8
Dio Cassius 75.16.5 ("whale" at mouth of Tiber)
Diodorus Siculus 2.54.3 (*ketos*)
Euripides fragment 121 (*ketos*)
Eustathius 294.16 (*ketos*)
Greek Anthology 6.38 (*ketos*), 7.275 (sea beasts), 7.506, 9.371
Herodotus 6.44 (Athos peninsula monsters)
Hesychius (sv *ketos*)
Homer *Od* 5.421, *Id* 20.147 (*ketos*)
Josephus *BJ* 3.442
Juba II
Laevius *Poet.* 21 (fragment ca. 300 B.C., earliest use of *hippokampos* to denote sea monster)
Lycophron 954 (*ketos*)
Manilius *Astronomica* 5.600 (*cetus*)
Moschus 2.116, 2.119 (*ketos*)
Oppian *Haleutica* 5.113 (*ketos*)
Pausanius 2.10.2 (huge sea creature's skull at Corinth); 9.20.4–21.1 Levy note 105 (pickled triton); 4.35.9 (Andromeda and monster), 2.1.8 (sea monster statue), 1.44.12 (giant man-eating turtle), 8.2.7 (triton), 1.4.1 (Atlantic sea monsters), 2.34.2 (marine monsters and sharks), 5.17.11 (statue of river monster), 5.25.3 (Adriatic sea monsters), 4.34.2 (river monsters), 10.12.1 (killer of sea monsters), 10.4.4 (immense sea-giant washed up at Cadiz), 9.26.5 (giant serpent killed with fishhooks)
Pliny *NA* 5.69 (Andromeda); 36.26 (*hippokampos*), 9.5 (orca beached at Ostia used in games under Claudius), 9.4 (displayed remains)
Procopius *History* 7.29.9–16 (45-foot "whale" stranded at Constantinople after 50 years of trouble)
Sargon of Akkad inscription
Strabo 8.7.2, 16.2.17 and 28 (Beirut monster and Andromeda)
Suetonius 2.72 (Augustus' fossil museum)
Varro *Sat Men* 406 (*cetus*)

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ANALYSIS OF THE AUSTRALIAN "HAIRY MAN" (YAHOO) DATA

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ABSTRACT: Reports of "hairy men" (Yahoos) in Australia for the period 1871–1912 are examined. The relatively small number of eyewitness accounts are characterized by a low level of detail, conflicting descriptions, and a high level of second-hand reportage. The analysis of individual sightings suggests that most can be explained by encounters with isolated Aboriginal males interpreted in the light of a developing settler myth. The evidence for the existence of an unknown primate in the area is very poor.

INTRODUCTION

Groves' (1986) paper on the historical Australian hairy bipeds (Yahoos) has provoked a certain amount of controversy. Rejecting the unknown primate hypothesis, he explained the stories as frontier myths on a par with the Tantanoola Tiger and the moa legends of New Zealand. An outsized wombat was suggested as the basis of one report, an outsized kangaroo for another, and the rest were dismissed as "a hotchpotch of shooters' campfire tales, unidentified apparitions seen at dusk, and various hairy horrids." Bayanov (1987) and Joyner (1987) thereupon took him to task for attempting no detailed analysis of the reports. Groves (1987) countered by saying that the reports tended to be inconsistent, and contained a high proportion of "noise" to "signal." The following year, he published an analysis of six variables in the original reports, and came to the same conclusion (Groves 1988).

The present paper is intended to provide a more thorough examination of Joyner's first (1977) publication, on which most of the controversy is centered, and to raise an explanatory hypothesis. The data consist of articles from various publications, mostly newspapers, which Joyner has numbered from 1 to 29. Three areas are covered: Aboriginal beliefs, European settler beliefs, and purported eyewitness reports.

The Aboriginal beliefs are recorded, by and large, by people whose knowledge of Aboriginal culture and experience in dealing with Aborigines is not stated, but we must expect a certain amount of cross-cultural confusion to have intruded. Also, as Groves (1986) pointed out, the traditions are extremely varied, some referring to tribal renegades and others to fantastic creatures with mythological overtones. In short, although the existence of an unknown primate would go a long way towards explaining the traditions,

they could just as easily be the result of the human imagination's well-known propensity to create manlike monsters.

The settler beliefs are also of limited value. They are presumably dependent on original sightings, but the information content would have deteriorated in the transmission. The beliefs would also have created a climate of expectation, which, in turn, would have clouded the interpretation of any genuine sighting.

It is to the twelve eyewitness accounts, therefore, that we must turn for information. Two methods of analysis are presented here. The first is to abstract the descriptive details from the reports to highlight the similarities and differences. The second is to analyze the reports one at a time, and attempt to provide a mundane explanation where possible. In all cases, the numbers quoted are those used by Joyner for his articles. This practice was also adopted by Groves (1988), and allows easy cross-referencing.

Analysis of Physical Details

The first thing one notices about most of the reports is the remarkable lack of detail. Many consist of just a sentence or two, and even longer descriptions are vague. In the case of the Braidwood beast, for example, where a dead female body was available for inspection, the witnesses neglected to record such an elementary detail as whether or not it was a marsupial. Apart from being a poor basis on which to form an hypothesis, this suggests a lack of critical standards by the population involved, and a concomitant tendency to jump to conclusions. Be that as it may, the following details can be culled from the accounts. Behind each detail is the number of the article from which it has been abstracted. The Braidwood beast [10] [11] is excluded at this stage because it is clearly aberrant.

Height: 5 feet [1]; "about the same as her grandfather" [4]; "somewhat larger than a man" [9]; over 6 feet [12]; "stature of a man" [13]; 7 feet [17]; between 5 feet, 8 inches and 5 feet, 10 inches [19].

Build: slender proportions but chest well developed [1]; "an old man whose back is bent" [4]; "the appearance of a huge monkey or baboon" [9]; "huge animal in an erect posture" [13]; "like a blackfellow" [15]; "like a black man" [15]; "man-like" [16]; "a great trunk all one size from shoulders to hips" [17]; human shape, enormous body frame, with a stomach hanging like a sack halfway down thighs [19].

Body Surface: "every particle of the body except the feet and face was covered with black hair, with a tan-coloured streak from the neck to the abdomen" [1]; "thick coat of hair" [4]; "covered with long hair" [12]; "like a blackfellow with a blanket on him" [15]; "coat was as hairy as that of a gorilla" [16]; "a tall man with a fur cap and a pea jacket" [18]; "body, legs, and arms were covered with long, brownish red hair," shorter on the arms, while the hair on the shoulders and back was jet black and long [19].

Head and Face: "back of the head straight, with the neck and body, but the front of the face projected forward, with monkey features" [1]; "head sunk into chest" [16]; "face . . . like that of an ape or man, minus forehead and chin" [17]; very small, very human, eyes large, dark, piercing and deep set, horrible mouth with canines which protruded when mouth shut [19].

Arms: long, with well developed muscles [1]; nearly reached ankles [17]; "extremely long and large, and very muscular" [19].

Hands: "nails of tremendous length" [4]; hand print showed little finger set like thumb [17].

Legs: "like a human being" [1]; fibula much shorter than a man's, but femur very long, out of all proportion to the rest of the leg [19].

Feet: "18 inches long, and shaped like an iguana, with long toes" [1]; footprints "resembling an enormously long and ugly human foot in the heel, instep and ball, had only four toes—long (nearly 5 inches), cylindrical, and showing evidences of extreme flexibility" but hallux not opposable [17]; metatarsals very short, phalanges very long [19].

Sounds: bellowed like a bullock when it hurt its foot [12]; howling and yelling [13]; bellowed [16]; growled, grimaced and thumped its chest [19].

Some of the accounts described the animal as being erect, and nearly all implied it, though nos. [1], [17] and [19] also mentioned quadrupedal locomotion. The emphasis on hairiness implies that the creatures were otherwise man-like, and the estimated heights are the same as in humans. A tail is never mentioned, and in two cases its absence is specifically noted. Neither genitals nor female breasts are ever mentioned, though in Victorian times this would not be unusual.

Nevertheless, it is obvious that a marked lack of detail coexists with marked inconsistencies, particularly in the form of the hair, legs, and feet. Furthermore, some witnesses reported striking details which should not have been missed by others. Groves' (1987) criticism on this point is certainly valid.

Theoretical Considerations

What is to be made of all this? Before this question can be answered a number of factors must be considered.

The first is the quality of the evidence. Experience shows that, in the face of the unknown, people's perceptions are often heavily influenced by social expectations, and are frequently sensationalized. Readers can no doubt provide examples of their own. I myself can remember how the night-time sighting of a "lion" south of Brisbane once initiated a string of "lion" reports until the feral dogs actually responsible were identified. Similarly, I know of a case in southeast Queensland in which an animal was shot that was described as being the size of a large dog, with the skin of a pig, mane of a lion, gait of a kangaroo, and the external attributes of both sexes. It turned

out to be a large, male feral dog which had lost all its hair, except on the neck, to mange mites, which had also infected its nipples, causing them to swell like those of a lactating bitch. Not only had its admittedly unusual features been described in the most sensational terms, but a completely extraneous feature, the "gait of a kangaroo," had somehow been introduced into the story.

With these experiences in mind, we should be prepared to discount some of the sensational aspects of the Yahoo stories. To add to our caution, it should be noted that two of the witnesses were children of unstated ages, and at least four of the accounts are second-hand—even discounting the cynical view that the retelling of a story by a journalist automatically makes it second-hand. Certainly, the witnesses' precise words are recorded in only a few cases. A fairly high level of reporter error must therefore be expected.

Secondly, there is the sheer improbability of an unknown primate existing in Australia, where the only other eutherians are bats, rodents, and the dingo, which was itself introduced by man. Moreover, the eucalypt forests of this region form a habitat totally different from that of other primates, and one in which it would have faced serious competition from the much more efficient Aborigines for at least 40,000 years. Furthermore, the accounts all emanate from the years 1871 to 1912. If a real unknown animal were involved, sightings would have become more and more common as the Anglo population increased. Footprints should be even more numerous. (This is an area where few people would go barefooted.) This has been the case with the North American Sasquatch, but, despite more recent sporadic rumors of "Yowies," this is not the case in Australia. There are strong grounds, therefore, for expecting a mundane solution to the problem.

The hypothesis explored in this paper is that reports of "hairy men" refer to just that: hairy men, isolated Aboriginal males whose physical features were sufficiently striking to confuse the credulous Anglo settler. At a time when tribal society was breaking down under the impact of Anglo settlement, degraded and antisocial individuals are likely to have been fairly common.

It is important not to read into the accounts more than is actually stated. The phrase "covered with hair" usually brings to mind the thick pelage of an ape, especially to those raised on tales of the Bigfoot and Yeti. It may, however, mean something quite different. On our own beaches are often seen individuals whose hairiness is striking: thick, curly hair covering not only the chest but also the abdomen, shoulders, and part of the back, not to mention the arms and legs. When the same individual sports a scruffy beard and mop of hair, and belongs to a race different from the observer's, his unexpected appearance may well be misinterpreted. Anthropologist Frank Poirier was, after all, mistaken for a Wildman when in China (Poirier, Hu, and Chen 1983), and the Australian Aborigines, as a race, are just as hirsute as Caucasians. Also, those in southeast Australia tend to be more heavily

built in the shoulders and chest than their northern relatives. Even the sounds attributed to the creatures would be made by humans in similar circumstances.

What about the references to "ape-like" features? I mean no offense, but the fact is that all human races possess "ape-like" characteristics which draw the attention of those who do not share them. In the case of the Australian Aborigines, these include heavy brow ridges, receding foreheads, broad, flat noses, and a marked prognathism. Any exaggeration of such features might well be classed as "ape-like" by people who had never seen a real ape or—more than likely—even a good photograph of one. Even so, the words "ape" and "monkey" appear more often in the articles on settler beliefs than in the eyewitnesses' testimonies.

Every year, a number of North Americans are mistaken for deer by overenthusiastic hunters, sometimes resulting in their demise. Nevertheless, many people may still find it completely preposterous that a human being could be mistaken for an ape. It would therefore be best to quote two recent newspaper reports.

The first is from the Brisbane *Sunday Mail* of February 22, 1987.

DESERT FAMILY STICKS TO "MONSTER" CLAIM

Alice Springs.—Relatives of a family chased by a huge "half-man, half-beast" in the central Australian desert are convinced they saw a monster, despite police having arrested a man nearby.

Alice Springs woman Phyllis Kenny, her four grand-children and friend Frank Burns, were chased by what they described as a terrifying "yowie-like" creature eight days ago at an isolated bore on Yambah Station, 50 km north of Alice Springs.

Mrs Kenny said the creature leapt from an empty tank at the bore, chased and caught hold of their utility truck before disappearing into the bush.

The man-like animal was covered with hair and stood at least 2m tall.

The two adults and four children were rabbit-shooting and decided to stop at a favorite camping spot, Top Bore, for water and tea.

It was 5 p.m. and still fully bright.

A large fig tree was beside the bore. They had some kangaroo tail with them, Mrs Kenny was passing around food and the two elder boys, Daniel Kenny and Ronald Dodd, were some distance away.

Daniel was the first to see the creature. "He was in the tank," he said. "He leapt out, not climbing out, but just straight out of this big tank."

Daniel and Ronald ran towards Mrs Kenny and Mr Burns. "Then, Nanna (Mrs Kenny) saw it and shouted out to jump in the ute" [the utility truck], Daniel said.

Ronald said: "It was coming towards us, not making any noises or anything or waving, just walking towards us."

Mrs Kenny said yesterday she had not slept properly since the sighting and was too scared to leave the house.

"When I saw that thing coming towards me, I nearly died," she said.

"You can tell the difference between a man and a beast—and that was no man."

"There are some things that you just can't explain, that are just scary, and that was one of them."

Daniel said: "It had a sort of ape's face. It had big eyes, a large forehead and it was all red around its mouth."

Ronald said: "It ran like a gorilla. Its arms hung down at its sides and it just sort of loped along."

But Mr Burns said: "I think it was a man. He looked like a man to me."

Police said yesterday a patrol arrested a large naked man in the area the day after the Kenny family's fright.

Constable Sean Sandry, of Alice Springs police, one of two officers to arrest the man, said he was "definitely not a yowie."

"We had a call from Yambah Station and we went out to see what was going on," he said.

"We found this bloke sitting on the side of the road completely naked."

"He was a great big bloke, about six foot eight, and I reckon he would have weighed about 20 or 25 stone" [280–350 lb].

"But he didn't give us any trouble. He just came along peacefully and hopped in the back of the van."

Police took the man to Alice Springs Hospital. He was admitted.

The race of the "apeman" was not mentioned in the above dispatch. In the next report, from the Brisbane *Telegraph* of July 28, 1987, he was definitely European and fully clothed.

"APEMAN" TURNS OUT TO BE "RAMBO" THE HERMIT

Recluse's Bizarre Forest Lifestyle

Sydney.—Police hunting the Woronara apeman have been told the "creature" is a Yugoslav hermit known as "Rambo" who has been living in a national park for years.

"It's not an apeman, it's 'Rambo,' " the locals of the Heathcote Inn said today.

The "Rambo" tag refers to nomadic hermit Franjo Jurcevic, a 53-year-old Yugoslav who has lived in the Heathcote National Park for as long as anyone can remember.

But his bizarre lifestyle is not totally isolated. On pension days he comes out to get liquor supplies at Heathcote, including beer, whisky and port.

"He comes up about 10 am on pension days and gets his supplies," Heathcote Inn barman, Buck Rogers, said.

"I can see why people would think he's an apeman. He has hair everywhere and wears a bandana round his head."

"But he's not 7 ft tall and he doesn't seem ferocious."

Locals have told police "Rambo" wears jungle boots, stained jeans and a murky-colored shirt.

"He never talks to anyone when he comes in," publican Ces Partland said. "He just takes his grog, along with fruit and veggies, and off he goes. He may be a bit short with people but he's certainly no Rambo."

But police fear they may gain no new information on the wild man of Woronara because he often lies low for very long periods of time. Officers at Engadine, who investigated sightings last Saturday night, said the creature may not be seen for years.

"It might be a while before he is sighted again."

Included in the report was an artist's impression of how a witness saw the "apeman" carrying an animal carcass.

Case by Case Analysis

Let us now examine each of the eyewitness accounts, beginning with the simplest.

[9] A four line article which simply states: "The animal, if such it be, has the appearance of a huge monkey or baboon, and is somewhat larger than a man." This description is too bare for any conclusions to be reached.

[15] Two second-hand reports, both with a minimum of detail. The first states: "Joseph says it was like a blackfellow with a blanket on him." The second tells of a creature killed by Aborigines at some unspecified date in the past: "It was like a black man, but covered all over with grey hair." Nothing in these accounts would suggest that the subjects were anything but human.

[18] This consists of a single sentence from a letter, and it is uncertain whether it is a first- or second-hand report. "It appeared to be a tall man with a fur cap and a pea jacket, and running with a peculiar attitude." No doubt he was wearing some cast-off European clothing.

[4] A little girl "asserts that she has met an old man, whose back is bent, and body covered with a thick coat of hair—in height (to use the girl's words), about the same as her grandfather. The strange being in question had nails of a tremendous length . . ." The child was probably telling the unvarnished truth. The amazing thing is that any adult was amazed at it.

[12] A boy—of unstated age—saw a "wild man or gorilla" which appeared from behind a tree and ran away from him. His description was limited to two sentences: "The boy states that he appeared to be over six feet in height and heavily built. He describes it 'as a big man covered with long hair.' " No doubt that is what it was. Its behavior was also quite in keeping with that of a human.

[13] [16] Here we have two second-hand reports from the pen of a Mr. John Gale. One concerns a Mr. Cox, who saw "a huge animal in an erect posture tearing through the undergrowth" making a noise between a howl and a yell. The complete description of the animal is covered by that short phrase. Obviously, no conclusions can be based on anything so vague. The second refers to a sighting by the Webb brothers. It took place during the short Australian twilight, when visibility would be limited. No indication is given of the lapse of years between the initial sighting, its report by the Webbs, and its retelling by Gale, but it is probable that it was long enough for distortions to creep in. Certainly, the story told by Gale in 1927 is different from the 1903 version. In the earlier story, the creature was merely "hirsute," and left no trail of blood after being shot at, but 24 years later its coat was "as hairy as that of a gorilla" and it left "distinct traces of blood." Furthermore, in the later version, the brothers waited till morning to examine the trail, and a herd of stampeding cattle had been introduced, perhaps from

the story of Cox's encounter. It is not often that we have such an opportunity to observe the evolution of a legend.

The following is a summary of the description from both versions: hairy; of similar size, stature, and gait to a man; head set very deep between its shoulders; loud vocalizations; no response to the brothers' challenge; and man-like footprints with the long, spreading toes of one who normally goes barefooted. It would not take much to make a man out of these data.

[17] This is the Bombala beast, which Groves considered to be a large kangaroo. It is another second-hand story, but probably more reliable. The sighting took place at midday, little more than a week before being reported. The creature appears to have been less than 150 yards from the witness, but the height of 7 feet may still have been overestimated.

It was described as being covered in grey hair, with the face of an ape or man, minus forehead and chin, trunk all of one size from shoulders to hip, and arms nearly reaching the ankles. This is very different from a kangaroo. However, if the length of the arms is exaggerated—and this feature is not emphasized in any other report—it could easily have been a big, hirsute Aborigine.

Its behavior was also man-like. It was drinking on all fours; i.e., the man was probably kneeling at the water's edge, or, more likely, squatting. If he had his knees drawn up by the sides of his body, it would explain why the witness initially mistook him for a kangaroo. Afterwards the creature picked up a stick and walked away like a man.

The following day, the writer examined the creature's tracks. The hands were said to have the little finger set like a thumb, while the feet had only four toes, all very long. This pattern is very unusual for a primate, and it would be interesting to know how clear the prints were, and the nature of the soil in which they were found. It would not be difficult for human hands and feet to leave such marks.

[19] This refers to the creature seen by Charles Harper in the Currickbilly Range. At first glance, it might be considered the ideal report: a detailed description written by the eyewitness himself. In fact, it has many defects.

In the first place, Harper gives no indication as to how long before the sighting had taken place. If the time gap had been short, he would probably have mentioned it. It is more likely that many years had elapsed, a period during which his memory had accentuated the strange and dramatic elements of the experience.

Secondly, the sighting took place at a distance of twenty yards, which is quite a long way at night, even with a large campfire. It can be safely assumed that the visitor had kept to the outer limits of the light. Yet, Harper gives a detail which he could not possibly have seen: "The eyes were large, dark and piercing, deeply set."

Let us take the most unlikely feature first: "The fibula bone of the leg was

much shorter than in man. The femur bone of the thigh was very long, out of all proportion to the rest of the leg." It is hard to believe that *any* biped could be built like that and still walk efficiently. No other report mentions such a peculiarity, and it throws a cloud over the whole of the rest of the description. Since Harper claims to have caught only occasional glimpses of the feet, the extreme length of the toes was also probably an exaggeration.

Another unique feature was the abdomen, which "seemed like a sack hanging halfway down the thighs." Gross obesity would have been rare among Aborigines at that time, but not unknown, and this one was probably wearing a sack-like garment.

The rest of the description would fit a hirsute, heavily-built Aborigine. The hair on the shoulders and back appeared jet black, as distinct from the reddish brown body hair. This suggests a mop of typically human head hair. "The head and face were very small, but very human," though some of its teeth protruded. In departing, it walked a few yards erect, "then at a faster gait on all fours through the low scrub"; i.e., it left the field of vision by crawling or stooping under the low foliage.

The last two reports do not fit the hirsute Aborigine theory.

[1] The details given by George Osborne may be summarized as follows: 5 feet high, general primate shape, covered with black hair except for a tan streak on the ventrum, with feet 18 inches long, shaped like an "iguana's" (i.e., an Australian monitor lizard, *Varanus* sp.). "It walked quadruped fashion, but at every few paces it would turn around and look at me following it, supporting the body with two legs and arm, while the other arm was placed across the hip." No human being would act like that, but it is also certain no native mammal of similar size could climb down a tree as this did.

The best explanation I can produce for this story is that it is a hoax. To dispose of inconvenient data in this manner may seem like special pleading, but the fact is that practical jokes do occur. Furthermore, the credibility of this story is compromised by the inclusion of striking features unsupported by other reports, such as the ventral streak and the outsized and outlandish feet.

[10] [11] The Braidwood beast, killed by A. Marrin, was definitely aberrant. The cardinal points are as follows: female; weight over 7 stone (98 lb); 4 feet from top of head to rump; no tail; face like a polar bear's; 11 inches across the forehead; tan colored with strong hair. Also: "Its four legs were shaped just like a man's arm and about the same length, and the feet were shaped like a man's hand with the palm precisely similar and toes which had a close resemblance to fingers with overgrown nails." It was not noted whether or not the animal was a marsupial, though one would have thought the presence of a pouch or the position of the nipples would have been worthy of note. No mention was made of such important characteristics as the teeth, ears, or shoulders.

Clearly, this is not a primate. The shape and size of the face precludes that, and the weight is low for even a female ape with such an elongated torso. Despite the uncharacteristic behavior of standing on its hind legs, I am inclined to accept the interpretation that it was a wombat (Groves 1986, 1988). The journalist who saw the carcass was not prepared to rule this out. One thing is certain: the animal was quite different from all the others previously described.

Conclusions

When a house has a reputation for being haunted, every creak, bump, or shimmer takes on sinister connotations. Similarly, when "hairy men" are rumored to lurk in the forest, every stray, antisocial native is likely to become the subject of anecdote. My analysis of the southeast Australian data strongly supports Groves' (1986) interpretation of the stories as frontier myths which faded when the first generation of settlers had passed away.

Critics might argue that I have deliberately discounted much of the evidence. This would be a valid criticism under normal circumstances, but when the evidence is contradictory, much of it *has* to be discounted. The reports cannot all be accurate, and I think I have sufficiently demonstrated that the witnesses were quick to jump to conclusions, slow to provide concrete details, and that, all too often, what details we have are second-hand. The kernel of truth that is left is subject to quite mundane explanations.

Researching old and obscure documents is a very time-consuming job, and by undertaking it Joyner has rescued from oblivion an important part of his country's cultural heritage. However, unless the stories are supplemented by more modern ones of a more reliable nature, they should be considered the province of the folklorist rather than the cryptozoologist.

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THE CASE FOR A LEGAL INQUIRY INTO SASQUATCH EVIDENCE

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Abstract: Consistent failure to persuade zoologists or anthropologists to investigate Sasquatch phenomena may be explained by the fact that most of the available evidence consists of eyewitness testimony, which scientists are not trained or accustomed to deal with. The professionals who *are* trained to evaluate testimony, and who take it very seriously, are lawyers. Since lawyers also dominate the political scene, where the purse strings of scientific research are held, the best way to achieve a scientific investigation may be to convince the lawyers first. To this end, a campaign for a judicial or legislative inquiry into the existing Sasquatch evidence is advocated.

Introduction

Individuals get involved with Sasquatch investigations for a variety of reasons. To some, it is just a matter of personal interest, independent of any consideration of the attitude of the society around them. At the other extreme, some see it as a chance to achieve fame, fortune, or both. To the first group, the question of whether the existence of the Sasquatch is established in their lifetime, or ever, may be of no concern, while in the second group, each individual is competing with all the others to produce the final proof him/herself. For anyone else to beat him or her to it may seem to represent absolute failure.

Between these extremes, however, are a number of individuals who, for whatever reasons, would like to see the existence of the Sasquatch established as an accepted fact. They slant a lot of their efforts towards that end, but, after all these years, they have very little to show for it. What has been done may not have been altogether wrong, but I would like to suggest that an alternate approach, one which could be a lot more productive, has been ignored.

What has been done is to try to persuade members of the scientific community, such as zoologists and anthropologists, that they should take the Sasquatch seriously and do something about it.

This is not an unreasonable approach. If the Sasquatch phenomenon does not involve a very interesting unknown animal, then it has to involve some exceedingly strange human behavior that has been going on worldwide and throughout history.

Either possibility is surely far more significant for humankind than many of the matters to which some members of the scientific community devote their careers, or at least substantial time and resources. It is only logical to

argue that the mass of Sasquatch material laymen have assembled should be sufficient to interest at least some scientists in taking up the matter. After so many years, however, it should be obvious that they are not going to do so. Therefore, why not try something else?

One of the basic problems faced in dealing with scientists is that the primary type of evidence that has been assembled on the Sasquatch, eyewitness testimony, is not the sort of data they are used to dealing with. They cannot replicate it. They cannot examine it in their laboratories. So they write it off as having no scientific value.

The Lawyers

Perhaps eyewitness testimony is not of much value. There are grounds for argument about that. But the important point here is that a certain professional segment of society does *not* consider it worthless. These people deal with testimony all the time, and, furthermore, consider it very important indeed. And this segment of society is not by any means insignificant or impotent. On the contrary, it is the most potent of all.

I refer, of course, to the legal profession. Lawyers set great store by testimony, and they use it to determine matters of the most vital importance, even to the extreme of deciding whether someone will live or die. A person can be executed on the basis of eyewitness testimony. Such testimony can also bankrupt a corporation, even a huge one, or send an individual, no matter how important, to the calaboose. Testimony is very powerful stuff.

But there is even more to it than that, because lawyers do not just run the court system; they usually run much of the government as well. A very high proportion of politicians are lawyers.

If enough politicians are convinced that Sasquatches should be taken seriously, they are in a position to tell the scientists: "Do something about this"—and some of them will do it gladly. Perhaps some scientists might find that last statement a bit offensive, but at least the politicians can tell the scientists: "Do something about this, and your research will be well-funded." I think it comes to about the same thing.

For that matter, the politicians could also tell the armed forces: "Take some of those fancy toys we are forever having to buy for you, and go and find one of these things." With the sophisticated sensing devices designed to monitor the movements of enemy soldiers, it ought to be relatively easy to track down an unsuspecting Sasquatch to his resting place; if there didn't happen to be a Sasquatch in the area at the time, it would still be a good training exercise for the troops.

The first question which may arise is: if the politicians can do such things, why are they not doing them already? After all, a lot of voters are known to take an interest in the Sasquatch, as well as in other kinds of cryptozoological reports. When the Smithsonian Institution co-sponsored a television program on the search for "monsters" some years back, it drew the top audience of the week; and when Harrison Hot Springs, a village of a few hundred people, proposed to stage a search for the Sasquatch, the whole world took notice (Green 1978: 50–51).

I suspect that the reason politicians have done nothing so far on the Sasquatch problem is that they have been getting bad advice. They have been asking the opinions of scientists instead of considering the matter themselves, not realizing that this is a subject which they are well equipped to deal with, while scientists are not.

An Official Inquiry

Maybe it is time to do something to change that. What I would like to propose is that a government, any major government, hold an official inquiry into the possible existence of the Sasquatch within its territory. A commission composed not primarily of scientists (although it would not hurt to have a few on the staff as technical advisers) but of judges, as we usually do in Canada, could be appointed; or there could be a committee of inquiry run by the politicians themselves, as seems to be favored in the United States.

There would, of course, be competent legal counsel assigned to the inquiry staff, and they could evaluate all potential submissions beforehand, assessing all the witnesses, whether they are volunteers or supplied from the files of the Sasquatch investigators, before putting them on the stand. I can guarantee there would no lack of witnesses. In fact, provided that their expenses are paid, I am sure the inquiry could be provided with a continuous supply of witnesses until they asked for the flow to stop.

The inquiry counsel could subject all witnesses to lie detector tests and to skillful cross-examination, and could investigate their testimony to their hearts' content. They could also have full evaluations by recognized authorities of any physical evidence that witnesses may submit, such as hair or fecal samples, or dermal ridges on footprint casts (Krantz 1983). They could have photographs, including the Patterson film, computer-enhanced and otherwise thoroughly analyzed by appropriate experts. They could do all the things Sasquatch researchers have wanted to have done for years, but could never afford.

When certain accounts indicate that there is an area where it might be possible to find further evidence, that could also be investigated. At the same time, if there are people who want to make a case that Sasquatches do not exist, or to challenge any particular testimony, they could also testify—after being put through the same wringer as the other witnesses.

Presumably, such a process would weed out a lot of accounts and a lot of people, either because an account did not stand up to thorough scrutiny or

because a witness had not seen anything that seemed sufficiently significant. There would still be plenty of witnesses left, thoroughly tried, tested, and evaluated, to keep the committee members listening until they tired of it.

And they would have to conclude that Sasquatches do exist. They could not reach any other conclusion. At least they could not do so without admitting that the processes by which they make so many other important decisions are so flawed that they should not be used at all.

Other Factors

This would all cost money, of course; a lot of it. But would anyone reasonably argue that it would be money wasted? For those who think it would be a waste, would it be as much of a waste as the inquiry held in Canada recently which attempted to determine whether top athletes use steroids? That commission heard testimony for months, and it finally found someone willing to testify about what everyone has surely known all along: that most world-class, medal-winning athletes have used steroids, because if they did not, very few of them would be winning medals! Could a Sasquatch inquiry be more of a waste of money than that?

Whatever government decided to hold this inquiry would be sure of getting something of real value for its money. It would get publicity. Officials in the Washington State government in Olympia already know that the Sasquatch is good value for publicity, or they would not have adopted it as the official State Animal for the 1989 Centennial Year.

We can be sure that any government that sets up an inquiry into the existence of the Sasquatch could count on worldwide attention. And if they were not capable of turning that attention to good use by convincing people that a place which may be inhabited by Sasquatches is an interesting place to visit, then they could bring in some advisers from Scotland's Loch Ness to show them how.

There is, of course, a fairly obvious reason why politicians might be reluctant to get involved in the Sasquatch question, and that is because they would fear the effect of ridicule by their political opponents. There are at least two ways to get around that. One would be to persuade both the major political parties to commit their support. Another would be to involve more than one government, either by lining up two American states or Canadian provinces (preferably with governments on opposite sides of the political fence), or by making it an international inquiry, with both Canadian and U.S. jurisdictions involved.

Actually, I do not think there would be any political risk. I think too many people would be really interested in the testimony, and in the outcome, for the responsible politicians to have anything to fear. In this connection, the International Society of Cryptozoology's symposium "Sasquatch Evidence: Scientific and Social Implications," hosted by Washington State University

in June of 1989 (at which this paper was originally presented) did not result in any ridicule for that institution. Neither did that happen in 1978 when the University of British Columbia sponsored the conference "Sasquatch and Similar Phenomena," which, in fact, resulted in the publication of an important volume (Halpin and Ames 1980).

I realize that what I am proposing may seem peculiar to many readers. I can only emphasize that I know what I am talking about. As a newspaperman assigned to cover courts, I spent a lot of time with lawyers; and as a party politician I have had a close look at how government works, and, in Canada at least, I know a lot of the people who make it work. Their titles may be different in the United States, but I am sure that the people are much the same.

How to Start It?

This approach is practical. It is far more practical, in my opinion, than trying to interest some zoologist—who never sees animals of any sort except through a microscope, or who only simulates their behavior or ecology through computer models—who knows he will be frowned on by his superiors and perhaps ridiculed by his colleagues if he gets involved.

How does one go about getting this started? Perhaps I am giving it something of a start right now. Beyond that, eloquent individuals could contact state, provincial, or national representatives and sound them out on the subject. If they show any interest at all, one could get them together with some solid Sasquatch witnesses.

If they do not help, one could go up the line, trying for a meeting with a whole caucus, or with the ministers, or deputy ministers—or secretaries or undersecretaries—of the departments most concerned. Those responsible for promoting tourism might be more ready to listen than those dealing with science, but whoever one tries, *one should ensure that one is talking to lawyers.*

If the direct approach does not work, one could try the media instead. A favorable column or editorial could do a great deal to ease any fears the politicians might have. One might even be able to get a few scientists to speak out in favor of this approach, since it does not really require them to do anything.

This approach may not succeed right away, but all it would take would be persistence. For 30 years, Sasquatch researchers have attempted to get scientists to pay attention to eyewitness testimony, something they just do not feel comfortable with. It should not take even a small fraction of that time to find lawyer/politicians somewhere who are completely at home with testimony, and who know the value of being in the spotlight. Perhaps such an approach could get the Sasquatch question settled once and for all.

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SASQUATCH FOOTPRINTS: A PROPOSED METHOD OF FABRICATION

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Abstract: Giant anthropoid footprints, attributed to the Sasquatch (Bigfoot) and showing dermatoglyphic sculpture on the soles, are not, as claimed, incapable of being faked. They can be fabricated by enlargement of human footprints using the von Fuehrer latex expansion technique.

Readers of this journal need no introduction to the Sasquatch (Bigfoot), an elusive large, bipedal primate that is said to haunt the woods of the northwestern United States, neighboring Canada and elsewhere. Most of the evidence that has been advanced to prove the existence of such a creature—sighting reports, still and motion pictures, hair and fecal samples—is either insufficiently documented, dubious, irrelevant, or spurious. The legitimate investigators of the phenomenon concede the inadequacy of such testimonials, but they point to what they consider to be hard evidence in the form of giant footprints, observed *in situ* and preserved by plaster-casting. Some such footprints, which have been most carefully documented by Krantz (1983), display what all competent observers agree to be genuine dermatoglyphic "fingerprint" patterns and sweat pores of the type found in higher primates.

Proponents further assert that it would be impossible to fake such footprints. This assertion, I submit, is mistaken. To make an impressively convincing Sasquatch track by enlarging a human footprint is not a difficult trick, nor is it a new trick. I learned the enlargement technique in 1939 from its inventor, staff artist Ottmar F. von Fuehrer, of the Carnegie Museum in Pittsburgh, who published it that same year in *Museum News* (Fuehrer 1939). Paleontological applications of the method were later published by me (Baird 1951, 1974). The technique was also mentioned on page 393 of the *Handbook of Paleontological Techniques* (Kummel and Raup 1965); thus, it is hardly a trade secret, at least among paleontologists. The present paper is intended for anthropologists and cryptozoologists who may not be aware of it.

Let it be clearly understood that, in presenting this information, I cast no aspersion on any person, nor do I presume to judge (without first-hand examination) the genuineness or otherwise of footprints that have been

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attributed to the Sasquatch. I merely wish to describe how such footprints can be fabricated by anyone who has such an inclination.

The following materials are what are needed:

1. A pair of human feet to serve as models.
2. Natural latex molding compound (about 1 quart).
3. Kerosene or other light oil.
4. Plaster of paris, preferably the type sold as molding plaster.
5. Silicone rubber molding compound (optional, about 1 quart).

The latex molding compound required is a thick (about 72% solid) suspension of prevulcanized latex particles in water, having about the consistency of cold cream. In small quantities (1 quart to 1 gallon), it has been sold by jobbers at different times and places under a variety of names, including "Von Fuehrer Compound," "Permamold Latex Molding Compound," and "E & R Latex Mold Rubber." In seeking a local supplier, one should describe the product and specify that one wants "sweet latex" that is only slightly alkaline. Note that this is a very different product from the so-called "sculptors' latex," a watery compound that pours like restaurant maple syrup and reeks of ammonia. I cannot recommend the latter product for any purpose, except, perhaps, for pouring down bee-nests in the garden.

Molding compound is applied to the model in thin layers with a brush or with fingers, taking special care in the first application to work the compound well into the surface and eliminate air bubbles. Each coat needs several hours to dry, though this may be speeded-up by applying mild heat (as from a light bulb) or a breeze (as from an electric fan). A thickness of two to three millimeters will require four to six coats. Obviously, this will not do for duplicating the soles of a human subject (other than a medical cadaver or a hospital patient in traction), so the first impression of the feet should be taken in a tray of plaster of paris. Heat generated by the plaster as it sets may cause some discomfort, and the subject may need to be reassured that plaster is very different from cement. The feet can be withdrawn after 20 minutes or so, and the plaster should be allowed to dry thoroughly. Molding compound can then be applied to the impressions as described above, producing rubber casts of the soles.

Now comes the trick. A latex mold or cast, when treated with kerosene or some similar light oil, will swell and expand by 40% to 50% of its original linear dimensions *while retaining its surface detail*. As the oil-treated rubber loses its tensile strength and elasticity, only a single plaster casting can be made from it; the rubber will disintegrate as it is pulled off. When the plaster is quite dry, a second and larger rubber cast can be made as before, and, if further enlargement is desired, this cast can be oil-treated like the first. The process can be repeated indefinitely.

In the late 1930's, von Fuehrer used to impress the audiences at his Carnegie Museum lectures by holding up the wing of a dragonfly, then an enlarged rubber replica of it, then another, and another, each half again the size of the last, each showing plainly the venation and other surface features of the original. Finally, to delighted applause, he would reach down into his bag and haul out a rubber dragonfly wing nearly two meters long!

In making that series of enlarged replicas, the oil had been applied uniformly to the rubber so that no distortion of shape resulted. However, as von Fuehrer discovered, it is possible to induce distortion deliberately by applying varying amounts of oil to different parts of the cast. In the case of a footprint, one could differentially increase the width or the length, widen the splay of the toes, or lengthen some toes relative to the others. Through all this manipulation, the dermatoglyphic pattern of the toes and sole will be faithfully reproduced in enlargement, along with sweat-pores, wrinkles, and other surface features.

As Krantz (1983) has correctly pointed out, the latex-and-kerosene technique will expand the ridge spacing of the skin pattern to the same extent that it enlarges the overall dimensions of the sole. In his opinion, the result would be "easily recognizable as abnormal." On this point I beg to differ. A footprint that is, say, double the normal human size will strike any viewer as grossly abnormal, but doubling the ridge spacing—an effect measurable in fractions of a millimeter—is a much more subtle effect. With all respect to Krantz, I believe the enlargement would not be perceived as being extraordinary. On the contrary, it seems to me, a Sasquatch-sized footprint with a human-sized dermatoglyphic pattern should give us greater grounds for suspicion.

Once the desired size and shape have been achieved, one can then make a pair of soles, left and right, with which to imprint trackways in the field. To make footprints with rigid plaster casts would be crude and unsubtle; it would be better to cast the soles in flexible rubber. For this purpose, one could use the same latex as a casting medium, building it up in successive coats with layers of cheesecloth incorporated as a filler and stiffener. On the whole, however, it would be simpler and more satisfactory to cast the soles in silicone rubber, which can be poured to the desired thickness (about 1 cm) in one operation.

To duplicate the long (1.2 m) stride and the deep impressions of a Sasquatch trackway, it is not necessary to fasten the rubber soles to one's feet and go leaping through the wilderness while wearing several hundred pounds of handicapping weights—as advocates of the Sasquatch claim a faker would have to do. Instead, one may simply lay the soles on the ground in the appropriate sequence and tamp them down, in turn, with a mallet and stake.

By differential pounding in different areas of the sole, one can make the

footprints deeper in the heel area, or the toe area, or in between. Also, while performing these shenanigans, one will want to bag one's own feet with padding so as not to leave Smallfoot tracks alongside those of the Bigfoot.

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Field Reports

Field Reports are not reviewed by members of the Editorial Board of Cryptozoology *or other outside referees. Reported descriptions or results of fieldwork are the responsibility of the authors only, and are subject to future criticism in the Comments and Responses section.*

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FURTHER INVESTIGATIONS INTO THE REPORTED *YEREN*—THE WILDMAN OF CHINA

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Introduction

In October and November, 1989, the authors undertook a personal investigation into the possible existence of the reported Chinese Wildman, known as *yeren.* Besides the cities of Beijing and Shanghai, they visited areas in the provinces of Hubei, Sichuan, Guangxi, and Yunnan.

Like Sasquatch (Bigfoot) reports in North America and Yeti reports in the Himalayan region, Wildman reports have been the subject of much discussion and speculation in the West over the past decade. One of the authors (Poirier) had already gained considerable field experience in China during the course of studying the primates of that country, the third largest on Earth. This afforded him the opportunity to be the first Western scientist to evaluate the question firsthand (Frank E. Poirier, Hu Hongxing, and Chung-Min Chen, 1983, The Evidence for Wildman in Hubei Province, People's Republic of China, *Cryptozoology,* Vol. 2: 25–39). However, due to political, logistical, and linguistic factors, no western investigators had ever gone to China specifically for the purpose of examining the Wildman evidence in an attempt to reach a scientific determination. This, in fact, was our purpose.

The expedition to China was the subject of a documentary film production by a British firm, The Filmworks. The film is currently being edited and is due to be broadcast in 1990 by Channel 4 television in Britain, and by the Public Broadcasting System (PBS) television network in the United States. It will also be broadcast in Western Europe and Australia.

Narrative Description

After meeting in Beijing, we proceeded to Shanghai, where a visit was made to Liu Mingzhuang of the Hua Dong Teachers University. Professor Liu, a biologist, is secretary of the Chinese Wildman Research Association. He showed us much of the massive material he has accumulated over the years, which set the stage for our own investigations in the interior of China. We then flew to Wuhan, the capital of Hubei Province, an area with a high concentration of Wildman reports. In Wuhan, contact was established with Cheng Lian Sheng, a witness to a close-up sighting of a Wildman in May, 1976. The incident, which had occurred at night when the animal was observed by Cheng and five other cadres—all forestry workers—in the headlights of their truck, resulted in a large 1977 expedition into the Shennongjia region of Hubei province sponsored by the Chinese Academy of Sciences.

We interviewed Cheng at length, and were able to uncover details which had not previously been revealed. The animal, which had been "cornered" against the road embankment, was covered with reddish hair and had a very large, round belly. We were also able to determine that it *only* stood erect when it was attempting to scramble up the embankment. At other times, including when it finally escaped down a gully, it moved quadrupedally. Although it was nighttime, the witnesses had the truck's headlight beams on the animal, and they got within 6 feet (2 meters) of it, affording them an excellent view.

Mr. Cheng was thought to be reliable. His temperament was rational and sensible. He did not make any wild claims. We also determined that he was acquainted with known Chinese wildlife. At the conclusion of the interview, we suggested to him that the animal may have been an orang-utan (*Pongo*), which existed in China in the Pleistocene, and probably persisted there into historical times (they are now only found in restricted areas in Sumatra and Borneo, but not on mainland Asia). Mr. Cheng had seen orang-utans on film, and he stated that the animal he saw was not that ape. However, he admitted that it was more like an orang-utan than any other known animal.

From Wuhan, we embarked on a river boat for a 2-day journey up the Yangtze River, during which one of us (Poirier) became sick with fever; unfortunately, the associated ailments persisted throughout much of the expedition. The river journey took us through Hubei into Sichuan Province. We disembarked at Wan Xian, and the following day proceeded by Land

Cruiser over rough mountain terrain for 12 hours to the town of Wuxi. At Wuxi, we met many local dignitaries, including the local Party secretary and members of the Wuxi Science Committee. We were housed in the Science Committee Center. We held discussions with the Wuxi Science Committee, whose members are convinced the Wildman exists, and who supplied several hairs for analysis. Unfortunately, most of the other evidence they have collected is of little scientific value.

We also met several more Wildman witnesses, who had traveled from different parts of Sichuan to be interviewed (prior arrangements had been made for their arrival in Wuxi to meet us). In all cases, the witnesses were questioned intensively through the use of professional interpreters. A brief summary of their reports follows:

- Ye Wan Chou, male, and Chen Zong Chun, female. May, 1981. While tending sheep, the witnesses (8-year-old children at the time) observed a large red-haired "man" squatting down. They were frightened and left, but the animal followed them relatively slowly for some time. Its locomotion was described as bipedal, but leg-bending was also described. A search party later failed to find the animal. Closest distance: 25–30 feet (7–10 meters). Duration: 10/"a few" minutes.
- Zhou Xian Chun, female. April, 1981. The witness observed a large animal covered with red hair leaning bipedally against a tree. It then dropped to a quadrupedal stance and was observed for some time ascending the nearby mountain. The witness stated it was not a bear or a monkey. She was reportedly terrified by the sighting. A search by villagers failed to find the animal. Closest distance: 10 feet (3 meters). Duration: 20 minutes.
- Wu Ying Cai and Liu Gou Zhao, males. October, 1964. The first witness, a mute, observed a large, hair-covered, bipedal animal while cutting wood. It left by climbing a nearby mountain. (It must be admitted that interviewing a mute witness through the use of hand signals was not very productive.) The second witness saw it at a greater (300-foot/100-meter) distance. The animal was described as having robust human proportions. Closest distance: 100 feet (30 meters). Duration: 30 minutes.

After our stay in Wuxi, we proceeded by Land Cruiser further to the northeast—through rugged, mountainous areas of spectacular beauty—towards Shennongjia, a forest preserve on the Hubei-Sichuan border which is closed to foreigners (author Poirier has for years unsuccessfully sought a permit to enter Shennongjia to study golden monkeys). Shennongjia is said to be the site of a high incidence of albinism, such as in bears, is home to the endangered snub-nosed golden monkey, and is also where many Wildman reports have originated. After a 3-hour drive, the unpaved and narrow road ended near a forestry lodge, where we were housed, and from which we made forays into the montane forests. This was done in the company of

Li Guo Hua, a Wildman investigator with whom we had established both contact and rapport. Unbeknownst to Western cryptozoologists, Mr. Li has been quietly seeking the Wildman for 10 years.

Mr. Li spends most of the year living in the mountains searching for the Wildman and studying other wildlife; having habituated golden monkey groups, which he documented to us with photos, he probably has had more experience with those rare primates than any other human. Because of his intense, almost full-time search for the Wildman in very rugged terrain, he is without doubt the most active Wildman investigator in China. However, even though he has become an expert tracker and forest survivalist, and although he claims to have found evidence such as footprints and sleeping "nests"—and had three distant sightings—he has been unable to bring the search to successful fruition.

The authors found the steepness of the mountains to be a serious impediment to fieldwork in China, at least by those not thoroughly accustomed to mountain foot travel. Although this was already known to Poirier, these mountains, rising abruptly at angles of 75–80 degrees—literally forcing the trees and vegetation to grow sideways—allowed only slow-moving penetration of the forest with minimum weight carrying (Fig. 1). Essentially, this prevented deep excursions near Shennongjia, which was the original aim, and which was still about 60 miles distant. No direct physical evidence of the Wildman was found by the authors during these forays.

We then returned overland to Wuxi, and finally back towards the Yangtze River, flying out of the town of Liang Ping to Chengdu, the capital of Sichuan, and then back to Shanghai. In Shanghai, we visited Zeng Xianzhou and his colleagues in the Department of Nuclear Science at Fudan University to discuss the analysis of Wildman hair which had previously been conducted in their laboratory. Using a sophisticated Van der Graff accelerator, and more recently a tandem accelerator, Professor Zeng and his associates have been perfecting an elemental analysis technique called "particle-induced X-ray emission" (PIXE). This technique, which has a precision of within 5%, involves the bombardment of a sample by high-speed proton emissions, and is finding applications in biology, medicine, and even archaeology and art (Zeng Xianzhou, Wu Xiankang, Shao Qiyun, and Yao Huiying, 1989, An Improved On-Demand Beam Pulsing System and Its Application to Art and Archaeology, *Vacuum,* Vol. 39: 91–95).

With the Wildman hairs, which had come from different parts of China, the Fudan scientists consistently found that their average content ratio of Fe/Zn (proportion of iron to zinc) was about 50 times that found in human hairs and about 7 times that found in known primate hairs (Fig. 2). (These results will be published shortly in: Zeng Xianzhou *et al.,* 1990, Biomedical Applications of PIXE at Fudan University, *International Journal of PIXE,*

FIG. 1.—Typical view of montane forest in eastern Sichuan, with mountains rising at 75–80 degree angles. The steepness of the mountains was found to be a "serious impediment" by the authors.

Vol. 1[1]). During our conversations, the Fudan University physicists did not attempt to speculate on the implications of these results, and we ourselves are uncertain as to their full implications. Nevertheless, it does appear to be an instance in which modern analytical science supports the contention that some specific Wildman hairs derive from a higher primate not yet known to zoology. (Since our return from China, we have learned that Professor Zheng has analyzed another Wildman hair sample—with the same results.)

We also visited Cao Hanmin and his colleagues in the Department of Biology of East China Normal University. Professor Cao had analyzed Wildman hairs using a scanning electron microscope (SEM), and had found some interesting internal structural differences when comparing them to human and other primate hairs. Differences were found when comparing cuticular scales, medulla, and pigment granules (Cao Hanmin, Zhou Jinpeng, Jiang Fumin, and Liu Mingzhuang, 1987, Comparative Studies of Ultrastructure of Hairs of Some Unknown Animals, *Journal of the East China Normal University,* Natural Science Edition No. 3: 98–103).

Interviewed intensively about his findings, Professor Cao stated categorically that the hairs were not of known great apes or humans. Nevertheless, the hairs came from a higher primate (ape or human), and their closest

FIG. 2.—Zeng Xianzhou (below) indicating to author Frank Poirier the results of his Fudan University team's analysis of Wildman hairs using a technique known as Particle-Induced X-ray Emission (PIXE). The Chinese nuclear physicists found that the average content ratio of Fe/Zn (proportion of iron to zinc) in the hairs was about 50 times that found in human hairs, and about 7 times that found in known primate hairs. The PIXE analytical technique has a precision of within 5%.

morphological affinity is to humans. Professor Cao therefore concludes that they came from an unknown higher primate—presumably the one the local people call *yeren.* As before, the implications of Professor Cao's findings are difficult to assess, but, again, they support the notion of an unknown higher primate.

While in Shanghai, further discussion was held with Professor Liu of the Chinese Wildman Research Association, as well as with Association members Xu Yong Qing of the Anthropology Research Section of the Shanghai Museum of Natural History, and Zhang Hong Kui of the Institute of History of the Shanghai Academy of Social Sciences.

After Shanghai, we flew west again, this time to Guilin, in the southern province of Guangxi. This more tropical area, just a few hundred miles from Vietnam, is where the famous karst mountains dominate the landscape, rising abruptly like fingers. Here we teamed up with Wu Xinzhi, a world-renowned paleoanthropologist at the Institute of Vertebrate Paleontology and Paleoanthropology (IVPP), in Beijing, who had flown south to meet us.

Our plan was to visit the famous Liuzhou Cave with Professor Wu, who had worked in it many years before. The cave is where the three *Gigantopithecus* fossil mandibles were discovered in the 1950's (since then, two other mandibles from a different species have been found in India). We proceeded down the Li River in a hired riverboat to Yangshuo, after which we drove to the village of Liuzhou and the nearby Lengzhai Mountain, a karst formation, in which the cave is located.

Because of the Wildman's proposed relationship to *Gigantopithecus,* as well as the historical significance of the fossil discovery in itself, we were very desirous of entering the cave, which is located halfway up the mountain. The cave was closed during the Cultural Revolution, and afterwards was essentially abandoned. Nobody had entered it in about 25 years. Furthermore, we were surprised to learn that we were to be the first Westerners to ever enter the cave at all, which added some romance to the adventure.

Because part of the ascent involved an 80-foot cliff, a rope ladder was made for us by the local villagers. This facilitated our climbing the cliff and reaching the cave, although in the end Professor Wu opted to remain below. The interior had various chambers and corridors, and we were able to observe where previous excavations had been conducted. We felt that much more excavation could still be done in the relatively brittle limestone walls, and that new *Gigantopithecus* fossil material—perhaps even the long-sought skull and limb bones—could be uncovered with sufficient effort and patience. Furthermore, there are probably thousands of other such karst mountains in Guangxi Province containing similar caves, many of which might yield profitable results if excavated diligently.

After returning to Guilin, we flew to Kunming, the capital of Yunnan province (and where we heard that, some time before, a tiger had entered the town and eaten two people!). We visited the Kunming Institute of Zoology, and were hosted by Professor Ji Weizhi, the deputy director, who runs the Institute's Division of Primate Research (author Poirier had spent a month at the Institute in 1987). We were able to observe and handle the rare southern subspecies of the snub-nosed monkey (*Rhinopithecus roxellanae bieti*), two rare macaque species (*Macaca assamensis* and *Macaca thibetana*), and the white-browed gibbon ape (*Hylobates hoolock*), all three of which have, at different times, been mistaken for a Wildman by eyewitnesses.

Like most Chinese scientists, Professor Ji was skeptical about the Wildman. We also queried him on the possibility of living orang-utans in China as the explanation for some of the Wildman reports. He had not heard of this hypothesis before, and was doubtful, but he promised to remain alert to such a possibility. Author Greenwell also visited the Kunming Zoo, where much of China's rare fauna is to be seen, including the giant panda, the takin, and various subspecies of large cats.

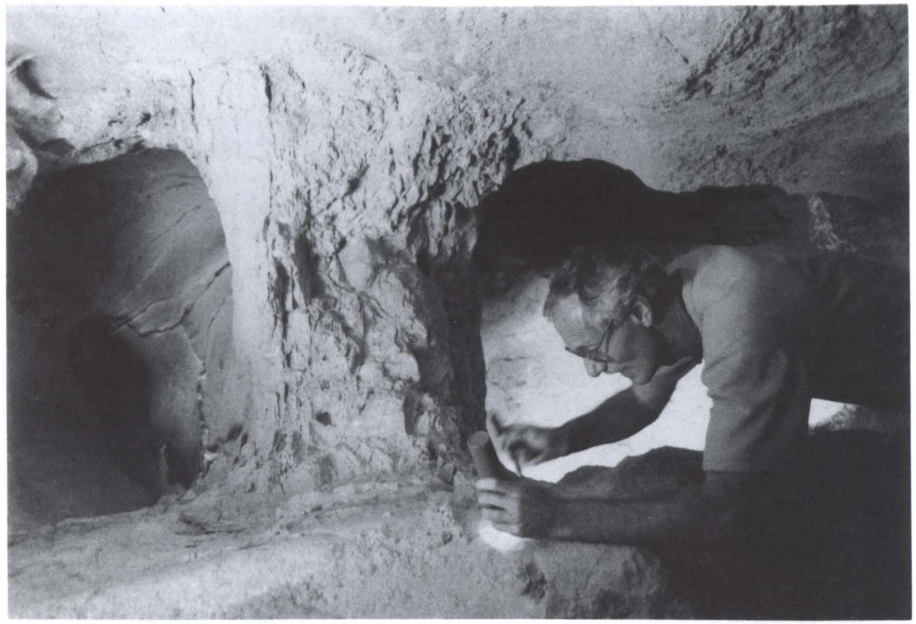

FIG. 3.—Author Frank Poirier examining the limestone interior of the famous Lengzhai Mountain Cave, in which the two *Gigantopithecus* fossil mandibles, and many teeth, were found in the 1950's. The cave, which had been abandoned for about 25 years, was located halfway up the mountain, and reaching it included negotiating an 80-foot cliff with a rope ladder. The authors feel that new *Gigantopithecus* fossil material could still be uncovered in the cave.

Upon returning to Beijing, a visit was made to IVPP for further discussion with Professor Wu, and acquaintance was also made with Professor Li Youheng, who had worked in the *Gigantopithecus* cave before its abandonment. Author Greenwell also visited Zhou Guoxing, of the Department of Anthropology at the Beijing Natural History Museum. Professor Zhou, who also serves as deputy director of the Museum, had recently opened a new and controversial exhibit on human evolution, which includes a Sasquatch/Wildman display. Greenwell held extensive discussions with Professor Zhou, who has investigated the Wildman question for many years (Zhou Guoxing, 1982, The Status of Wildman Research In China, *Cryptozoology,* Vol. 1: 13–23), and who serves on the Board of Directors of the International Society of Cryptozoology (ISC). Professor Zhou is not fully convinced of the Wildman's existence, but he believes the matter deserves continued investigation. He was skeptical of the possibility of living orang-utans in China.

By coincidence, visiting Beijing at the same time as the authors—to conduct research at IVPP—was Nikolai Spassov, a mammalogist at Bulgaria's National Museum of Natural History, and also an ISC Board member. The opportunity was taken by one author (Greenwell, who serves as ISC Secretary) to discuss with Spassov cryptozoology in general and the Wildman situation in particular. Dr. Spassov was sympathetic to the possibility of living orang-utans in China.

RESULTS

The authors did not discuss their personal evaluations of the evidence between themselves until near the end of the expedition. This was done partly to accommodate the requirements of the filming project, but also because of a legitimate desire not to unduly influence one another. Upon comparing notes, however, it was found that both of us assigned a higher than 50% probability to some kind of primate or primates "inconsistent with the known fauna of China" being responsible for Wildman reports. Of particular interest was the shift that had occurred in our assessment of the Wildman's possible existence since our arrival in China.

Initially, author Greenwell, based on his prior knowledge of the subject, had assigned a 30% probability to the unknown animal hypothesis, and author Poirier, who was far more skeptical, only about a 5% probability. At the end of the expedition, in order to arrive at some sort of objective, quantifiable conclusion, we ranked on a scale of 1 to 10 the value of the different kinds of evidence we had been exposed to, thus: existing habitat, eyewitness reports, footprint casts, and hair analyses. Habitat and hair analyses received high marks, eyewitness reports moderate marks, and footprint casts poor marks.

Upon tabulating the totals, it was found that author Greenwell assigned a probability of 60%, and author Poirier a probability of 52%. It is of interest that, while Greenwell's probability assignation had doubled, and was still the higher, Poirier's had increased tenfold. It was also determined, in the course of further discussions, that our subjective "beliefs" in the Wildman approximated the above objective probability assignations.

We will now present our overall conclusions in plain English. First, it can be stated with a high degree of certainty that the umbrella-term *yeren* (Wildman) has encompassed a wide variety of known animals, such as bears, gibbon apes, and macaque and golden monkeys (even one of the authors, Poirier, was once mistaken for a Wildman—which almost resulted in his demise—when villagers who had not previously seen a hairy Westerner encountered his almost-nude body napping by a river!). An extreme conservative opinion would probably "solve" the Wildman question by attributing *all* reports to such known animals, as well as hoaxes. We do not concur with such an assessment.

Our investigations indicate that, while a number of known animals (and probable hoaxes) have been involved, the umbrella-term *yeren* also includes animal forms which cannot be so easily explained away. In fact, such hard-

to-dismiss reports fall into two discrete categories, indicating the possibility of two distinct animal species being involved: a giant, hair-covered and bipedal primate, and a smaller (but still large) hair-covered and quadrupedal primate. While the imagery of the former morph is now well-known to cryptozoological investigators, some of whom have proposed that, like the American Sasquatch, the fossil genus *Gigantopithecus* could be involved, the latter morph, particularly its quadrupedal mode of locomotion, has not been previously reported. This is probably because the information has not passed accurately though the sociocultural and linguistic filters which exist on the route between the eyewitnesses' personal descriptions and their interpretation in subsequent publications, particularly when translated into European languages in Western publications.

The descriptions of a large and unknown quadrupedal morph in modern China add a new and interesting dimension to the Wildman problem. We propose that the descriptions of this second morph may result from observations of orang-utans (*Pongo*), either the known species or, more likely, a related species—perhaps even a fossil form—populations of which might still survive in rugged and isolated pockets of the country. When observed, such large, red-haired primates could well be referred to by the generic term *yeren* by the local inhabitants. We should remember that, before this mythical ape was "discovered" by science, the original meaning of the Malay term *orang-utan* was (and is) "wild man of the forest."

Serious questions could surely be raised concerning habitat and ecology, as well as the nutritional requirements of such Chinese orang-utans. Concerning habitat, after visiting the mountains of Sichuan—and we were not able to penetrate the truly remote areas—we are convinced that large, unknown species could persist there indefinitely almost undetected. The ecological and nutritional problems presented are also not insurmountable. However, we do not propose to dwell on the subject at length here, as a longer paper specifically addressing this question is planned for the future.

As for the proposed relationship between the bipedal Wildman reports and the fossil genus *Gigantopithecus,* such a giant primate did exist in China until relatively recent times, between 300,000 and 500,000 years ago (although there is still uncertainty as to whether it was a biped). In fact, *Gigantopithecus* is the longest-living primate genus known (at least in the last 30 million years), having survived a period of about 8 million years. It takes only a little "push" to propose its survival another half-million years to the present time, particularly as some of its associated *Ailuropoda-Stegodon* fauna, such as the giant panda (discovered by science as recently as 1869 and not brought back alive by Westerners until the late 1930's), persist to the present time.

However, we should also emphasize that, although *Gigantopithecus* may be an attractive candidate for the bipedal Wildman, this does not by any means make its present survival an established fact. Further physical evidence of the reported animal, and preferably also of the fossil species, would need to be found and compared before such a determination could be reached. There is also the possibility—albeit less likely—of another large primate, unknown paleontologically and zoologically, being responsible for such Wildman reports.

In conclusion, it is our opinion that numerous *known* primates (including *Homo sapiens*) have been labeled "Wildman" (*yeren*); that there is a high probability that one and possibly two different genera of unknown or cryptic primates may also be involved (one of them being *Pongo,* an orang-utan); that the matter remains unresolved; and that further investigation is certainly warranted. Due to the difficulties of terrain and logistics, as well as sociocultural, political, and linguistic factors, such future investigations will not be easy to conduct.

We would like to thank numerous individuals in China who made the expedition possible, and who collaborated with help and information: Cao Han min, Ji Weizhi, Li Guo Hua, Liu Mingzhuang, Shan Yu-Sheng, Tang Jia-Yong, Wu Xinzhi, Yi Guangyuan, Zeng Xianzhou, Zheng (Edward) Xu, Zhou Guoxing, and all the members of the Wuxi Science Committee. We also wish to express our appreciation to producer Geraldine Easter and Bernadette Drury of The Filmworks, and director Ian Duncan, cameraman Michael Coles, and soundman George Hitchins, all of London, England, for their extensive administrative, logistical, and artistic efforts, as well as for their inspiration and participation in the project.

Future Plans

The authors currently have no plans to conduct further Wildman investigations in China. Author Poirier will be returning to China in the future in connection with other primatological work, and attempts to acquire new information will be made as a matter of course. We plan a future paper outlining in more detail the possibility of living orang-utans in China.

Cryptozoology, 8, 1989, 58–63

AAS UNDERWATER ELAPSED TIME CAMERA SILHOUETTE PHOTOGRAPHY EXPERIMENTS AT LOCH NESS, 1989

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INTRODUCTION

The possible existence of large, unidentified aquatic animals in Scotland's Loch Ness still intrigues investigators, including members of the Academy of Applied Science (AAS), which is resuming studies which were intensively carried out in the 1970's, and earlier summarized in this journal (Robert H. Rines, 1982, Summarizing a Decade of Underwater Studies at Loch Ness, *Cryptozoology,* Vol. 1: 24–32).

This report, preliminarily drafted by Harold "Doc" Edgerton before his sudden death in early January, 1990, describes the final research activity and Loch Ness fieldwork of Doc's long and illustrious career. The fieldwork was centered on improving upon earlier underwater elapsed time camera equipment of his design; this was specially adapted for making use of the sun's rays and the long, summer daylight hours of northern Scotland as the source of back illumination for silhouette imaging of underwater objects passing over an upwardly-looking camera.

Earlier designs and tests of this nature were previously reported in this journal (Robert H. Rines, Harold E. Edgerton, and Robert Needleman, 1984, Activities of the Academy of Applied Science Related to Investigations at Loch Ness, 1984, *Cryptozoology,* Vol. 3: 71–73). These, in turn, were sparked by some interesting earlier silhouette shapes, photographed by an Edgerton 16mm camera of 1975 vintage, that continued elapsed-time frame exposures after the strobe batteries had been exhausted (Robert H. Rines, Charles W. Wyckoff, Harold E. Edgerton, and Martin Klein, 1976, Search for the Loch Ness Monster, Figs. D and F, *Technology Review,* Vol. 78[5]: 25–40).

The continued objective was to deploy the camera floating underwater above an anchor, with no ropes or cables to the surface, thus leaving the open space over the upwardly directed camera lens unobstructed.

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[2] Now at Brewster Academy, Wolfboro, New Hampshire.

FIG. 1.—The Edgerton camera cylinder on ground, attached to the two bouyant suspension spheres. Justice Rines (left) and Harold "Doc" Edgerton are seen relaxing on Temple Pier, Urquhart Bay, Loch Ness, on July 6, 1989.

NARRATIVE DESCRIPTION

The camera, unlike the earlier above-mentioned versions, consisted of a pair of in-line 35mm film spools separated by a sprocket drive powered from a 6-volt battery—parts put together by Bill MacRoberts, of the Strobe Laboratory of the Massachusetts Institute of Technology (MIT), from earlier National Geographic Society projects. The camera operated in a continuous mode at a very slow rate (about 10 seconds between frames), with the sprocket driving a switch that discharged a capacitor into a lens shutter-operating magnet, producing a $\frac{1}{25}$ second exposure time for each frame. The film spools were No. 10 daylight loading spools of 125 feet of thin Estar base Type 2475 film, providing for 2,000 separate pictures, each measuring 1″ × ¾″. The board carrying the spools and the fixed focus f/4.5, 35° lens was inserted within a steel cylindrical housing with a watertight sealed glass window adjacent to the lens.

The camera was deployed in Urquhart Bay, Loch Ness, in July, 1989. Participating in these preliminary experiments was Adrian Shine, a resident

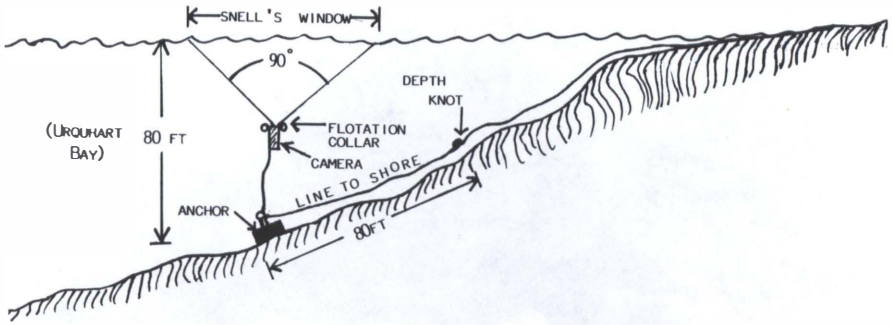

FIG. 2.—The deployment technique of the underwater floating silhouette camera in Urquhart Bay, Loch Ness.

researcher who had conducted the earlier Operation Deepscan sonar survey of the loch, in 1987, among other studies. Shine's ingenuity and assistance resulted in a simple and effective floating suspension system comprising a collar formed by a pair of bouyant spheres lashed to the camera cylinder at its lens end (Fig. 1).

A description of the deployment technique is in order. It involved securing the camera to an anchor with a line, which positioned the camera window 40 feet below the water surface (Fig. 2). A line to shore was also secured to

FIG. 3.—Shore line being carried by boat. Charles Wyckoff (left) and Adrian Shine in boat.

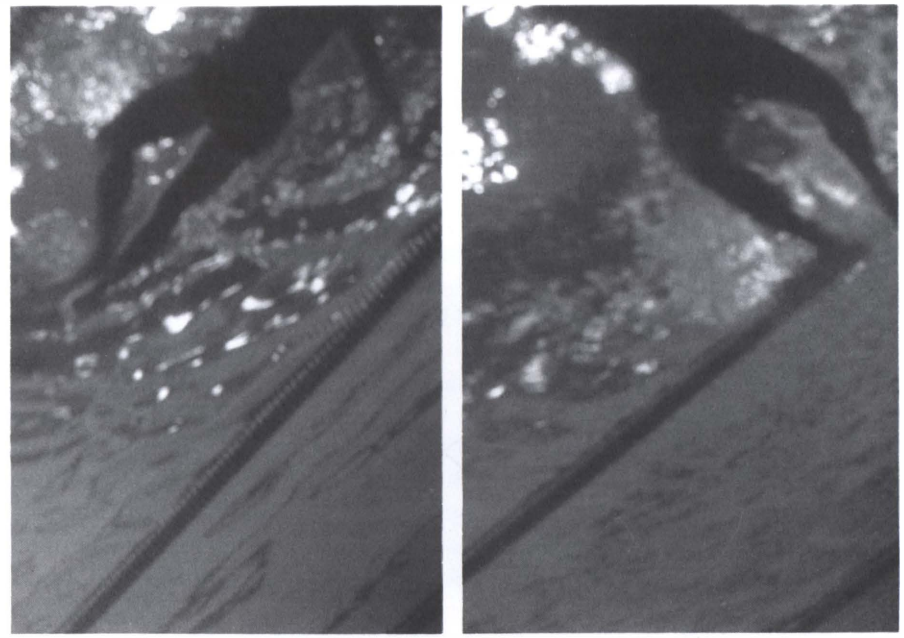

FIG. 4.—Sun-lit photographs obtained in swimming pool tests at the Massachusetts Institute of Technology.

the anchor; it had an identification knot spaced 80 feet from the anchor, with the line allowed to follow the bottom contour as it led to shore and its attachment on the beach. When deploying the camera, this shore line was carried by boat to the approximate location (Fig. 3), and then, while the identification knot was held by hand at water surface level, the boat was moved outward or shoreward until the anchor hit bottom.

Since the identification knot was 80 feet from the anchor, the depth of the anchor on the bottom was accurately determined, and thus the camera lens was known to be 40 feet below the surface. A reverse operation was used in retrieving the camera. Several exposures were made in this mode. Additional exposures were made at different surface-to-camera depths in order to verify the developed film density predictions.

RESULTS

The initial focusing and calibration tests at the MIT Strobe Lab and swimming pool were recently humorously described by Trude E. Bell (1989, 'Doc' Edgerton: An Electrical Engineer for All Seasons, *IEEE Spectrum,* Vol. 26[9]: 52–57). These tests resulted in encouraging sun-lit photographs (Fig. 4).

However, swimming pool water and the waters of Loch Ness are quite dissimilar. Our studies at the loch, while not detecting anomalous swimming

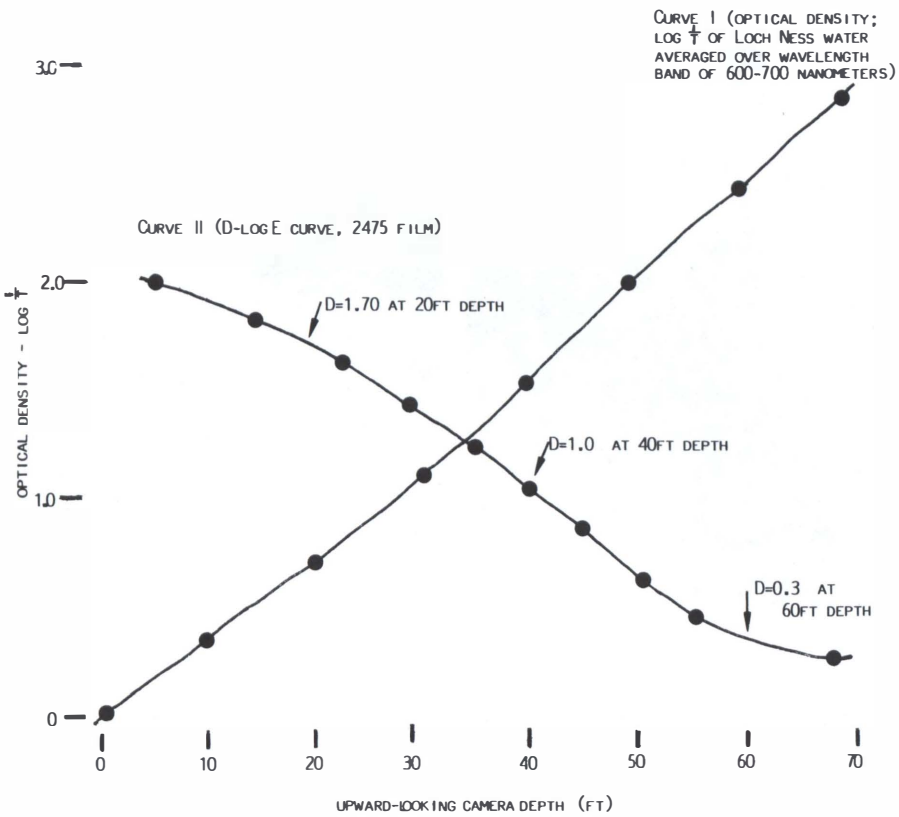

FIG. 5.—Underwater silhouette camera exposure conditions at Loch Ness: optical density as a function of upward-looking camera depth. Kodak recording film 2475; extended red sensitivity; $^1/_{25}$ sec., f/4.5; overhead sun-colored by Loch Ness water.

objects—which would have been an unexpected bonus for this equipment testing program—did result in a better knowledge of the underwater film exposure conditions at Loch Ness. This now enables the publication of a calibration curve (Fig. 5), plotting optical density as a function of upward-looking camera depth. Curve I provides a ready reference for the amount of light absorption in Loch Ness for water path lengths from 0 to 70 feet. For example, over a water path of 70 feet, only a tenth of a percent of the light will be transmitted. The other 99.9% is absorbed or not available for practical use.

Curve II is basically a D-log E characteristic curve of the film Type 2475 when exposed to direct overhead sunlight in the silhouette camera at varying depths. The data were adjusted to produce a developed density = 1.0 at a camera depth of 40 feet. According to Curve II, when the camera was at a depth of 60 feet, the developed film density was 0.3, and produced useful

exposures. When the camera was only at 20 feet, the developed film density had a value of 1.7, but again produced useful exposures.

Curve II, moreover, is also useful in a somewhat different way: to predict the length of daylight time during which useful exposures can be made. Let us assume, as an example, that the camera is at a depth of 20 feet, with the sun directly overhead. This would produce a developed film density of 1.7. As the sun deviates from being directly overhead, during either early morning or late afternoon, resulting in a fall-off in light by approximately 30 times, the camera still produces useful silhouettes with a developed film density on the order of 0.3. A greater drop in light value, however, does not record a satisfactory silhouette image.

Four separate rolls of film were used in the camera during the fieldwork. Only two of these ran for the entire 125 foot length. During the exposure tests, a 10-foot-long rowboat was allowed to enter the field of view of the camera for several exposures. During this time, a temporary target was suspended at different depths in order to determine the clarity of objects coming between the camera and the water surface. During most of the several hours of recording time, however, there was nothing in the field of view except Snell's Window (see Fig. 2).

While a 6-volt camera battery should last for several days of operation, one battery was exhausted after the exposure of only 2,000 frames. It was discovered that the camera shutter synchronizer was causing a momentary short circuit across the battery every time the shutter operated, which quickly drained the battery. This was remedied on the last day of operations. This mishap represents a typical example of the many problems encountered during debugging procedures in field tests.

FUTURE PLANS

The 35° angle of the lens proved far too small to take advantage of the 90° provided by Snell's Window (see Fig. 2), and a wider-angle lens will be substituted in the future.

During the last day, we attempted to obtain exposure data at various depths, but, because of high wind conditions, it was very difficult to stabilize the camera to make certain that it was aimed up at the center of Snell's Window. We suspect it was not, because the silhouette of the boat never appeared in any of the exposures. Thus, the validity of the resulting exposure tests for that run remains in doubt, and must be repeated.

It is hoped that this silhouette camera will be used as one of several future imaging tools, should the more comprehensive continual sonar survey of the loch, now planned by AAS, provide interesting targets for identification.

The only sad note is that Harold "Doc" Edgerton will not be with us in person—though his spirit will inspire us to try to find the ultimate answer to the zoological mystery in Loch Ness.

Cryptozoology, 8, 1989, 64–66

RESULTS OF CHIMPANZEE PHEROMONE USE IN SNOWMAN (WILDMAN) FIELD INVESTIGATIONS

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INTRODUCTION

This report summarizes the results of a recent expedition which attempted to obtain evidence of the Snowman, the reported Soviet hominid or hominoid, in the Chimkent and Dzambul regions of the western Tien Shan mountain range of Soviet Central Asia. The expedition was conducted in July and August, 1989, and was headed by Alpinist Gleb Isaenkov, with the author serving as scientific leader.

The program involved what we consider to be some original strategies for the detection of the Snowman. One of these strategies appears to have met with success. The strategy in question involves the use of a sexual pheromone from the African chimpanzee, *Pan troglodytes*. The chemical compounds were obtained from the urine of a young female chimpanzee, 3 years of age, at the Institute of Physiology of the Academy of Sciences of the U.S.S.R., in Leningrad. Sterilized pieces of cloth were saturated with such pheromones.

NARRATIVE DESCRIPTION

Experiments were conducted in the River Aksu valley, at an altitude of 2,500 meters (8,200 feet). Snowman sightings have been reported in this area. The valley is difficult to penetrate, and we were the first humans to enter it in 1989. The surrounding mountains reach a height of between 4,000 and 5,000 meters (13,000 and 16,500 feet). We placed the target cloths on easily visible trees. The trees were also selected according to the surrounding soil conditions, with the idea of track preservation.

During the night of August 1, heavy footsteps and smashing sounds were heard by my colleagues. In the morning, we found large, bipedal footprints about 50 meters (160 feet) from camp, and 25 meters (80 feet) from a target cloth. The length of the footprints was 32–33 centimeters (12.5–13 inches) (Fig. 1), and the stride was 105–110 centimeters (41–43 inches). From this, a height of 2.2 meters (7 feet, 2.5 inches) was determined. The weight of the animal involved was calculated to be at least 250 kilograms (550 pounds).

At 6:30 a.m. on August 9, we heard footsteps again. Upon leaving our tents, we heard the rapid motion of a large body moving through the bush. We followed rapidly with cameras, but no sighting of the creature itself was made.

Upon examining the area around the camp, we determined that the Snow-

FIG. 1.—One of the footprints left on the night of August 1, 1989, after being filled with plaster. The footprints measured between 12.5 and 13 inches.

man had approached the first cloth and torn it into strips. This tearing was done by hand, not with teeth. The Snowman had also broken a branch from the tree, in a way similar to how I have observed hamadryas baboons (*Papio hamadryas*) doing in the field. The same procedure had been followed with the second cloth. We were able to determine that the creature had approached to within 3 meters (10 feet) of our camp, right up to the camp's clothesline. Upon our arising, it rapidly departed. At one point, it left a fist-track on the ground. The tracks from the August 1 incident were identical to those from the August 9 incident. No further incidents took place during the expedition.

Results

Based on our fieldwork, we conclude that the Snowman is present in the Tien Shan region. We also conclude that ape pheromones serve as good sexual attractants for locating the Snowman.

Future Plans

We hope to undertake further fieldwork in search of the Snowman. We hope that the use of ape pheromones will again prove successful.

Cryptozoology, 8, 1989, 67–72

LCPI WORK AT LAKE CHAMPLAIN, 1989

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Introduction

In 1989, several U.S. senators announced their intention to support legislation seeking to make Lake Champlain the U.S.A.'s sixth "Great Lake," and thus free federal funding for research and cleanup. Though Lake Champlain is rather clean compared to the Great Lakes—Superior, Michigan, Huron, Erie, and Ontario—it has problems like soil erosion, phosphorous pollution, pesticide runoff, and a growing population of parasitic sea lampreys. All of these threaten the lake's ecosystem, and thus probably jeopardize its reported colony of Loch Ness monster-like animals dubbed "Champ." As the Lake Champlain Phenomena Investigation (LCPI) continues its ongoing search for evidence of Champ, it supports efforts like increased environmental funding to insure a healthy lake environment; this will allow Champ, although undescribed by science, to subsist and breed.

The 1989 LCPI fieldwork season was a continuance of a high-tech approach to solving the Champ problem, ushered in during 1987 when Klein side-scan sonar and an underwater robot called a ROV (remotely-operated-vehicle) were deployed. That sophisticated equipment was used again in 1988 and 1989 in an attempt to find a carcass of Champ. Beside the high-tech approach to fieldwork, conventional forms of lake monster searching were used again in 1989, such as surface surveillance with cameras and binoculars, and scuba searches.

LCPI fieldwork in 1989 was focused on: (1) trying to locate a carcass of Champ using Klein side-scan sonar surveys; (2) scuba searches looking for Champ; (3) surface surveillance using cameras/binoculars from boats and shore sites; (4) further documentation, analysis, and publication of reported Champ sightings and Champ-related news; (5) encouraging lay and professional people to become involved in the search for Champ; and (6) providing assistance to serious individuals and groups involved in Champ-related research and fieldwork.

Narrative Description

The LCPI conducted 32 days of fieldwork at Lake Champlain during 1989. As in 1987 and 1988, donated gear and personnel helped make the expedition well equipped. Donated equipment and personnel came from: Klein Associates, Inc., Salem, New Hampshire; Kaselaan & D'Angelo Associates, Inc., Haddon Heights, New Jersey; Jones' Aqua Sports, Willsboro, New York; and Marine Explorer, Shelburne, Vermont. Martin Klein, Founder and for-

mer President, Klein Associates, Inc., and Vincent Capone, formerly of Kaselaan & D'Angelo Associates, Inc. and now with Steadfast Oceaneering, Inc., acted as primary Klein side-scan sonar operators, with Joseph W. Zarzynski assisting. Kaselaan & D'Angelo Associates, Inc. donated the use of their MiniRover Mk II ROV. It is equipped with a VHS video camera, a manipulator arm for object retrieval, and a 35mm camera for underwater still photography. Kaselaan & D'Angelo Associates, Inc. also provided corporate insurance on the sonar. Providing technical and logistical support were William Key, President, and Garry Kozak, Manager—Field Operations, Klein Associates, Inc., and Chip D'Angelo, President, Kaselaan & D'Angelo Associates, Inc.

Four boats were used during the LCPI fieldwork: a 13-foot-long Avon inflatable (LCPI), a 25-foot-long cabin cruiser (Marine Explorer and Captain Dan Couture), a 21-foot-long runabout (Jones' Aqua Sports and Doug Jones), and a 15-foot-long Boston Whaler (Ralph Veve and Donald Mayland). M. Patram Meaney and Joseph W. Zarzynski donated various camera gear, plus provided lakeside housing/meals to LCPI personnel.

Zarzynski participated in all 32 days of LCPI fieldwork in 1989. Others participated on these dates: M. Patram Meaney (June 11, 24–30, August 5–10, 12–13, 17–19, 23–25); Vincent Capone (August 14–18, 20); Russell Bellico (August 7, 9, 15, 22, 24, 28); Captain Dan Couture (June 11, August 9, 15–17, 22, 24, 28, September 16, October 21); Bob Benway (June 11, 25, September 16); Martin Klein (August 16–17); Jack Sullivan (June 11, August 9); John Farrell (June 11, September 16); Doug Jones (August 7, 14, 20); Don Mayland (July 22, August 17–18); Dave Van Aken (July 6, October 21); Denny Pajak (October 21); Peter Hess (October 21); Gino Bardi (August 9); and John Brewer (October 21).

Special thanks go to Richard Smith and Bonnie Clonan for notifying me of 1989 Champ sightings.

The 1989 LCPI fieldwork consisted of: daytime surface surveillance from shore and boat using 35mm cameras, telephoto lenses, camera tripods, and binoculars; scuba monitoring; scuba diver-held underwater photography; side-scan sonar; and a ROV on location for use should the sonar detect a Champ carcass-like target which needed to be eyeballed.

Lake surface surveillance and most LCPI activities were based from Kimball Dock, Vermont. Numerous surface surveillance sessions from boats were launched based out of: Kimball Dock, Vermont; Burlington, Vermont; Malletts Bay, Vermont; Town Farm Bay, Vermont; Willsboro, New York; and Westport, New York. The side-scan sonar surveys were from boats chartered from businesses owned by Doug Jones and Captain Dan Couture.

The dates of the 32 days of fieldwork were: June 11, 24–30; July 6, 22; August 5–10, 12–20, 22–25, 28; September 16; October 21. Daytime surface surveillance from shore or boat(s) was conducted during all 32 days; boat

deployed side-scan sonar surveying was done during five days; the ROV was on site for seven days, but was never deployed as no significant sonar-discovered target was located in deep water to ROV dive and identify; and 26 scuba dives were conducted, 16 with diver(s) using a Nikonos underwater camera.

The 26 scuba dives were made as part of the LCPI's ongoing underwater reconnaissance in search of a carcass of Champ. The dates of these scuba dives, the diver(s), and the number of dives were: June 11 (Benway, Farrell, Sullivan, and Zarzynski—2 dives); June 25 (Benway and Zarzynski—2 dives); July 6 (Van Aken and Zarzynski—2 dives); July 22 (Benway, Mayland, and Zarzynski—1 dive); August 7 (Bellico, Jones, and Zarzynski—2 dives); August 9 (Bardi, Bellico, Meaney, Sullivan, and Zarzynski—2 dives); August 15 (Couture and Zarzynski—1 dive); August 17 (Couture—1 dive); August 18 (Capone, Mayland, and Zarzynski—1 dive); August 22 (Bellico, Couture, and Zarzynski—3 dives); August 24 (Bellico, Couture, and Zarzynski—1 dive); August 28 (Bellico, Couture, and Zarzynski—3 dives); September 16 (Benway, Farrell, and Zarzynski—3 dives); and October 21 (Brewer, Hess, Pajak, Van Aken, and Zarzynski—2 dives).

Results

No visual sighting of a Champ-like animal was made by LCPI personnel during the 1989 fieldwork season.

However, our side-scan sonar surveying using a Klein 500 kHz unit did produce some "targets" to eyeball. On August 15, the sonar noted a target which we thought was inanimate in nature, possibly some type of shipwreck. Captain Dan Couture and Joseph W. Zarzynski dived the target site, but it was identified as a log. On August 17, another side-scan sonar target was noted. Like the target of August 15, this was shallow enough to use a scuba diver to eyeball rather than the MiniRover Mk II ROV, which was at our base camp should we have needed it to examine deep water targets. Captain Dan Couture dived the August 17 sonar target. He reported visibility at about three feet, but was able to identify the target as a 1960's-era automobile with the trunk open. He got the license plate number, we buoyed the site, and we reported our unusual discovery to the Vermont State Police. Since there was low visibility, Captain Couture was unable to determine if the vehicle contained the remains of a body. To date, we have not been notified by Vermont law enforcement officials if their dive squad examined the vehicle and its contents.

Our five day side-scan sonar surveying also picked up a possible shipwreck target in deep water on the New York state side of the lake. Thus, our remote sensing surveying, though not finding a Champ carcass, as was our intention, did once again demonstrate that cryptozoological expeditions can turn up fascinating and noteworthy non-cryptozoological finds.

The year 1989 was a rather quiet one for reported Champ sightings. Only four sightings were reported to the LCPI. They are listed below in chronological order (Fig. 1):

(1) July 6, 1989: Renee Chagnon and several other people; in Malletts Bay, Vermont, near Clay Point; while boating, about 7:30 p.m.; the group saw an animal they at first thought was a log; it then showed two humps, and lifted its head up about 12 inches (head described as twice the size of a human head); black in color; animal was 15–20 feet long; when the boat went over the site, the boat's fish-finder sonar picked up a large object 53 feet down in 83 feet of water (however, they were unable to freeze the sonar screen to save the record); witnesses about 100 feet from animal; no camera aboard; sighting lasted 5–7 minutes.

(2) July 8, 1989: Robert H. Hemming; between Chimney Point, Vermont, and the Crown Point State Historic Site, New York; 10:45 p.m.; animal described as "three tube-like humps protruding out of water 12 to 15 feet in length"; "no head seen"; "very dark in color"; range about 1,000 yards; sighting lasted 10–15 minutes; no camera at hand.

(3) Week of July 17, 1989: Ronald and Paula Young; between Burtons Island, Vermont, and Hathaway Point, Vermont; on very calm waters they both saw a "large black object" about 24-feet-long at a distance; no range given; weather was hot and humid; no time of sighting given; the Youngs stated: "The only strange thing, which we have heard before on the ocean, was the sound of a mammal's blow hole, as a whale or porpoise would make when surfacing"; no photographs taken.

(4) August 5, 1989: Shirley and Joe Marinello; sighting was "60 to 80 yards due east of the Pines Restaurant on Route 9, Village of Port Henry, New York"; sighting was at 8:05 p.m., lasting for 20 minutes; Joe Marinello described the sighting as: "We saw what appeared to be a series of fins sticking up out of the water and moving slowly to the northeast; suddenly the 'fins' separated, and one set moved rapidly to the east about 100 yards from the first. This set disappeared after a brief time. They appeared to descend in a stepping motion under the water. The first set continued to move northeast at a very slow rate of speed. During the whole sighting, I saw what I took to be a shadow in the water which preceded the fins to the north by 10 or 15 feet and seemed to follow by 20 or 30 feet"; no photographs taken.

Future Plans

The LCPI plans to continue its fieldwork at Lake Champlain in 1990 doing conventional camera surveillance and scuba diver reconnaissance, plus more high-technology work involving side-scan sonar and a ROV. We also hope to be able to continue field tests of John Becker's video digitizer computer monitoring system (VDCMS), which was used at the lake in 1987 and 1988.

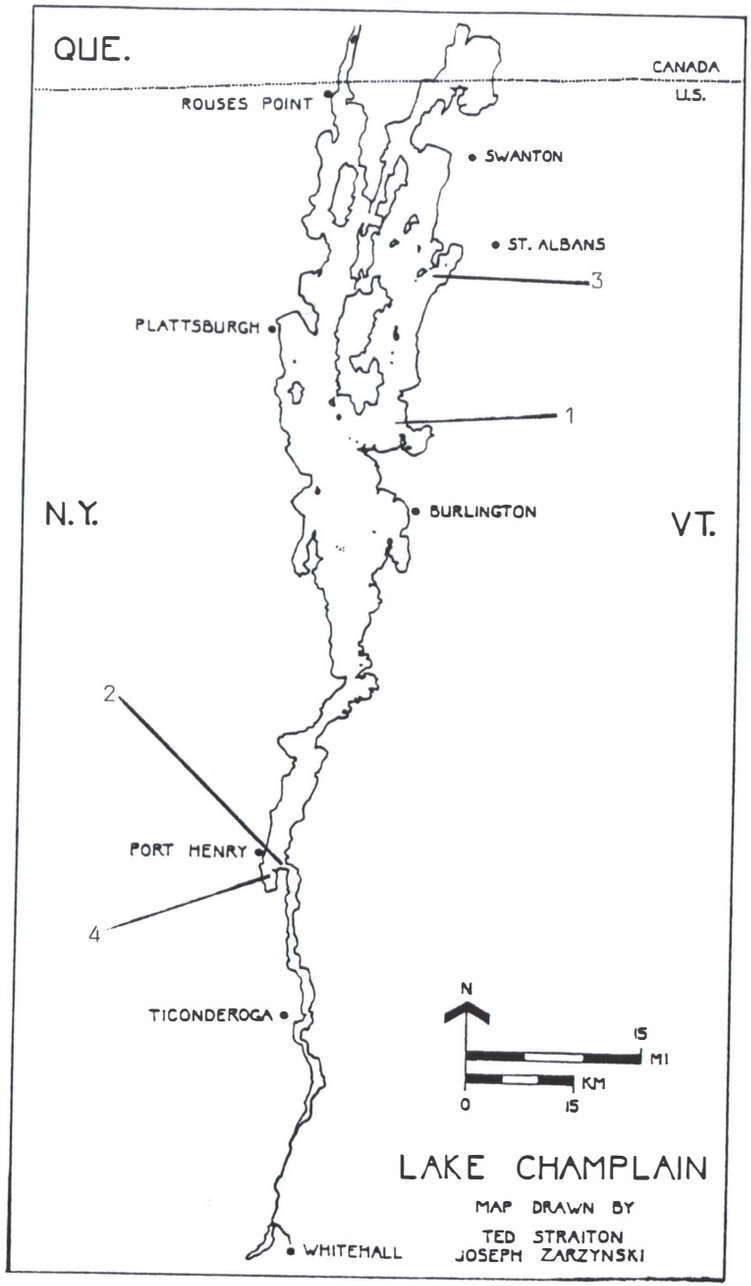

FIG. 1.—Map of Lake Champlain, with numbers indicating the locations of the eyewitness sightings reported to LCPI in 1989.

We believe the carcass search strategy, though a definite "long shot," still offers the best potential results: positive proof. However, LCPI urges a passive approach to securing a carcass of Champ, and not one in which such a cryptid is hunted down to secure the specimen. The LCPI also asks its ISC colleagues to write to us with suggestions on the carcass search strategy, so that we can better maximize our limited time with the high-tech equipment.

Cryptozoology, 8, 1989, 73–74

CONTINUING SASQUATCH INVESTIGATIONS IN THE PACIFIC NORTHWEST

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INTRODUCTION

The author has had a continuing interest in Sasquatch reports for about 30 years. As a wildlife biologist, he has conducted extensive fieldwork in many parts of the Pacific Northwest, and, since 1973, has actively sought Sasquatch evidence. A summary of these activities was published previously in this journal (James A. Hewkin, 1986, Investigating Sasquatch Evidence in the Pacific Northwest, *Cryptozoology,* Vol. 5: 27–37). The purpose of this report is to summarize his 1989 Sasquatch fieldwork.

NARRATIVE DESCRIPTION

In March of 1989, an employee of the U.S. Forest Service (USFS) informed me about large Sasquatch tracks he had examined in light snow. The tracks had been reported to him by cedar shake cutters. On March 23, I went to the area in question, along the Clackamas River, in the Mount Hood National Forest, and found a scattered log and a scratched-up area. The snow had, by then, melted away. The tracks were described by the USFS employee as about 17 inches in length, and the track stride "greater than that of a man."

Two months later, on May 11, 1989, also in the Clackamas River drainage, I located a large log with the bark freshly torn off (a slide of this log was shown during my presentation at the ISC's Symposium "Sasquatch Evidence: Scientific and Social Implications," held at Washington State University in June, 1989). There were no claw or definite nail marks on this log; there was a possible claw mark at two locations. A small footprint, about 10 inches long and with a narrow heel, was found a few feet from the log.

During July 19–21, together with Jack Sullivan, I spent some time in the Blue Mountains of central Oregon in an attempt to find more evidence in the area where we had searched last year (James A. Hewkin, 1988, Sasquatch Snapped Saplings [Or Bigfoot Broke Branches . . .], *Cryptozoology,* Vol. 7: 130–31). We hiked up an adjacent drainage, and noted several torn-up logs; some appeared old, and some quite fresh. We closely examined this evidence, and found definite nail marks at two locations and saw bear claw marks on one log. However, we suspected that a good deal of the evidence was due to Sasquatch activity.

During August 8–11, 1989, together with Jack Sullivan and Francis Williams, I investigated a suspected Sasquatch area in the Blue Mountains on the Oregon-Washington border. The area in question is in the Wenaha-

Tucannon Wilderness, within the Umatilla National Forest, and is adjacent to the Mill Creek Watershed of the Walla Walla system, which was the focus of a previous field report in this journal (Lonnie Sommer, 1987, New Signs of Sasquatch Activity in the Blue Mountains of Washington State, *Cryptozoology,* Vol. 6: 65–70). We were successful in locating about 15 stumps and logs that had been recently disturbed. There were good nail and finger marks on two logs. Bear claws were noted at one location and suspected at another. We were very impressed with the Wenaha Wilderness; it is very rugged, and holds a large amount of large game (elk and deer).

We also talked with a rancher who knows the area well. Over the years, he has hunted it consistently with a pack string of mules and horses. In 1986, he reported, his hobbled horses became very agitated one night while camped on a headwater tributary of the Wenaha Wilderness. The following morning, while investigating the cause of the disturbance, he and his team discovered the tracks of what were believed to be three Sasquatch individuals. According to the tracks, one individual was quite small, and another was crippled. In the spring of 1989, they rode the area and searched thoroughly, but could not find any new tracks.

Another interesting item pertaining to the subject was found at the nearby Tollgate Restaurant. The proprietor has photos of supposed Sasquatch tracks tacked to a wall, and he showed me a photo album, which included a photo of a cougar (puma) which was supposedly found dead with its head bashed in. Obviously, until I locate the person who found the animal and took the photo, it does not carry much weight. However, I believe that there have been reports of Sasquatch feeding on cougar kills. If this is the case, a cougar defending its hard-won kill could very well end up with a bashed head!

Results

The evidence uncovered during the past year gives additional support to the possibility that old logs and stumps are important Sasquatch grubbing sites. Such stumps and logs contain many rodents as well as insect larvae. It is also possible that large game animals are important for Sasquatch survival in rugged habitats. The reported dead cougar may be a relevant lead.

Future Plans

I plan to continue searching for Sasquatch evidence in the Pacific Northwest during 1990. I also hope to pursue the question of the dead cougar.

Cryptozoology, 8, 1989, 75–79

BCCC REPORT ON OKANAGAN LAKE, 1989

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Okanagan Lake is a large body of water in south-central British Columbia, a western Canadian province. Since early settler times, the lake has been the source of many reports of large, unidentified animals or "lake monsters," known generically as Ogopogo, as well as Indian mythology of a large monster. This is also the case with numerous other North American lakes, most notably Lake Champlain, located between the U.S. states of New York and Vermont, and jutting north into the Canadian province of Quebec.

The British Columbia Cryptozoology Club (BCCC), founded in May of 1989, carried out its first two field expeditions to Okanagan Lake during the summer of 1989. The objective of the first expedition was to determine the best vantage points for lake surveillance. The second expedition was to conduct further surveillance, as well as to deploy a remotely operated vehicle (ROV) in the lake. The principal participants were the author and James Clark, assisted by John Kirk, Jr., and Barbara Clark.

Narrative Summary

On July 29, Messrs. Kirk and Kirk carried out fieldwork at Peachland, which affords sweeping views of the lake in an area that has produced a plethora of sighting reports. Observation was made simple by the absence of wind and cloud in the area, and visibility was excellent for a distance of 12 miles north and 8 miles east. Kirk Sr. noted that the famous Ogopogo marker constructed by the British Columbia government on the east side of Highway 97 served as an observation platform which afforded excellent viewing of the lake (Fig. 1).

That afternoon, the western side of the lake was investigated for possible observation posts, but few high-level sites were suitable, and those by the lakeshore only offered limited viewing because of the headlands and points which are numerous in the area. Fintry, at the northwest end of the lake, was examined to determine why Ogopogo supposedly frequents the area. The elusive lake monster has reportedly been spotted by several boaters in the area, but no noticeable large fish population was detected in the vicinity.

The focus of investigation turned to Summerland, and a particularly good vantage point was located at Peach Orchard Beach, Lower Summerland, on July 30. All four members of the investigating team were stationed at various points on the beachfront when, at 3:55 p.m., a most extraordinary occurrence took place. A large patch of white water materialized close to a headland at the southern end of the beach, drawing the attention of the BCCC observers.

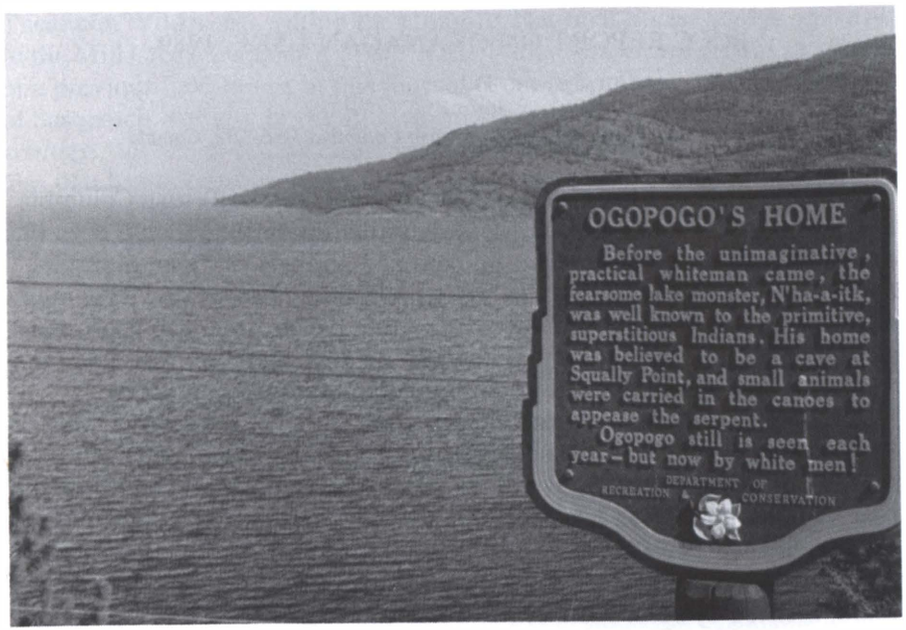

FIG. 1.—The Ogopogo marker off Highway 97 offers an excellent view of Okanagan Lake.

It was about 1,000 feet distant at this point, and it was clear that a large animal was swimming in a northerly direction against the prevailing wind and slight swell. At a distance of about 600 feet, Kirk Sr. was able to see clearly through a Bushnell 40× telescope that this was the classic Ogopogo, with its humps well above the water level. Both Clarks were also able to see the object clearly through binoculars. The animal displayed, variously, five and sometimes six humps.

Kirk's telescope allowed him to see that the animal's skin was whale-like, and that there were what appeared to be random calcium-like deposits under the skin which appeared to be similar to barnacles in shape. All the team members agreed that the animal was between 30 and 35 feet in length, and was almost 3 feet above the surface at its highest point—that being the middle hump. The sighting was confirmed by a local couple who refused to give their names. These other witnesses were located about 300 feet behind the BCCC contingent, but they were still able to see the animal clearly.

Mr. and Mrs. Clark and Kirk Jr. were all able to see the tail, which protruded to a height of about 12 inches above the water. Kirk Sr. was unable to see the tail, as his focus was on the animal's center hump, which sank to a height of 1 foot, and then alternately rose again in a series of seemingly lazy undulations.

Further viewing of the animal was prevented by the convergence of two motorboats just meters away from the animal, which caused it to take cover by submerging vertically as the crafts came closer. The total sighting lasted 105 seconds. Drawings of the animal were made by all four members of the team (Fig. 2).

Rattlesnake Island has long been reputed to be the home of Ogopogo, so it was targeted for investigation on August 1. Mention had been made that the animal had previously been seen "sunning" itself on a ledge on the island. As Jim Clark had suffered from severe sunburn during previous outings, he and Barbara Clark were unable to join the Kirks, who were taken to the island aboard a CKIQ Radio cruiser. The cruiser was piloted by broadcaster Blaine Pudwell, and we were also accompanied by Angela Yielding, of Thornhill, Ontario.

No discernible ledge was located on the island or the nearby shoreline, but many optical illusions which appeared to resemble a lake monster were noted. A variety of standing, reflected, and refracted waves produced artificial humps, and at times strongly resembled a head just above the surface. Investigation of the great bend of the lake southeast of Peachland failed to produce any sign of the legendary animal or its habitat.

At approximately 3:15 p.m., Pudwell, a firm disbeliever in the Ogopogo phenomenon, turned the attention of the other occupants to the west side of the lake. Traveling through the water at a speed of about 15 miles per hour was a large animal, heading south. The two Kirks noted that, during the sighting 2 days earlier, the animal had been locomoting at a speed of only about 5 miles per hour.

Pudwell and Yielding were at the front of the boat, and were able to see clearly between five and six humps, which the Kirks also confirmed. No head was visible, as the object moved from a distance of 600 feet to less than 300 feet from the bow of the boat, directly in front. At their highest, the animal's humps were 3 feet out of the water and undulating very slightly.

Approximately 2 minutes of videotape were shot of the animal's appearance, and although the image is jittery, several sequences show a series of humps moving rapidly through the water. The entire sighting lasted 2 minutes, 45 seconds.

Having succeeded in sighting the Ogopogo on two occasions, the BCCC was approached by Robotic Systems International (RSI), of Sidney, British Columbia, to participate in a joint expedition from August 25 to 29. On this expedition, RSI was able to provide an ROV (remotely operated vehicle), which included a JVC 8-millimeter videocamera, and a 100-watt spotlight mounted inside a propeller-driven underwater sled.

On the morning of August 26, John Kirk Sr. was investigating a potential viewing site at Bertram Creek Regional Park when, at 10:45 a.m., his attention was drawn to a large object just beyond a point to the west. On

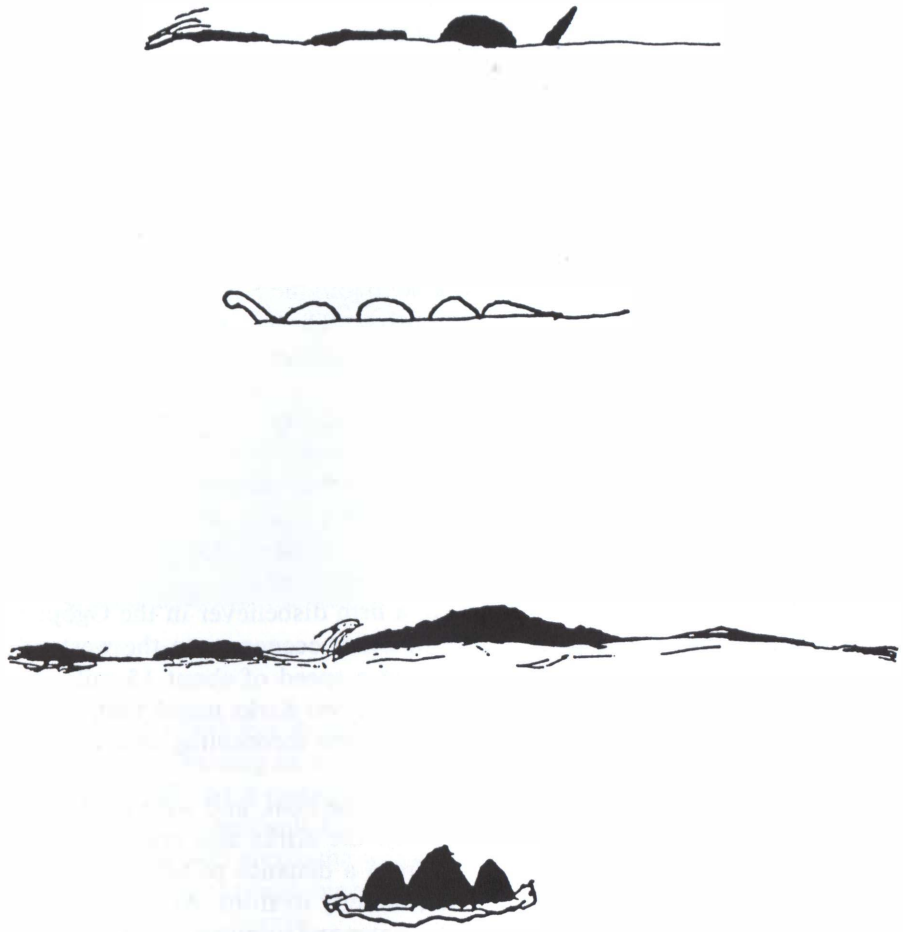

FIG. 2.—Drawings of the unidentified animal observed in Okanagan Lake by the BCCC team on July 30, 1989. From top to bottom, drawings are by James Clark, Barbara Clark, John Kirk, and John Kirk, Jr.

climbing up to the point, Kirk was able to observe 10–14 humps about 1,000 feet offshore, spread across about 120 feet of water. These represented not one animal but two, as one group of humps appeared to be swimming at a slightly different angle from the second group.

The humps remained in view for a minute before disappearing from view. Half an hour later, Kirk was able to see four large protrusions across the lake on the west side, near the earlier sighting, and noted that the protrusions covered a total length of 60 feet. Another sighting was obtained by Kirk that evening as he drove to Penticton. A large black hump measuring 25 feet in

length was spotted by Kirk close to Rattlesnake Island. The hump was at least 4 feet out of the water, and moving northward very slowly. Kirk was able to shoot 20 minutes of videotape, which shows the hump submerging, surfacing, and splashing.

On 29 August, the ROV was launched by Chris Roper, Marine Division manager of RSI, from the shores of Rattlesnake Island. The objective of the search at this phase was to locate any possible caves under the island in which the animals are alleged to dwell. The ROV functioned perfectly, and sharp images were returned to the monitor on shore. No trace of a cave, or of an Ogopogo, was found. An earlier test run at Okanagan Lake Bridge also failed to locate one of the animals or its traces.

After returning to Shelter Bay Marina to regroup, Kirk noticed something moving towards Okanagan Lake Bridge at 3:45 p.m. He was able to attract the attention of three tourists from Ontario, who also sighted the animal. Three black humps measuring about 20 feet in length were moving at approximately 8 miles per hour. The three tourists, Rob Turnbull, Tricia Plancher, and a third individual who wished to remain unidentified, confirmed Kirk's sighting. As details of the sighting were being recorded, something large began pushing water in the opposite direction of the animal seen moments before. Seconds later, a black hump 3 feet high rose out of the water before the same group of witnesses. Kirk ran to get his camera and alerted Roper, who also saw the animal. Turnbull, Plancher, and the third, unidentified Ontario tourist noted that the animal's tail was visible. Kirk did not see the tail, as he was searching for his camera at the time.

RESULTS

The first fieldwork at Okanagan Lake conducted by BCCC resulted in several visual sightings of large, unidentified animals. The interpretation of these visual sightings is continuing. Also, for the first time, an ROV was deployed in the lake and operated successfully.

Following the spate of sightings experienced during the past summer, BCCC has decided to undertake further fieldwork in the summer of 1990. At present, plans are in the initial phase, but it is anticipated that more technology will be employed in the next expedition. Tragically, James Clark succumbed to a heart attack on September 3, 1989. His participation was an invaluable asset to the team, and he will be missed.

Book Reviews

Cryptozoology, 8, 1989, 80–97

An Odd Bestiary: Or, a Compendium of Instructive and Entertaining Descriptions of Animals, Culled from Five Centuries of Travelers' Accounts, Natural Histories, Zoologies &c. by Authors Famous and Obscure, Arranged as an Abecedary. By Alan James Robinson (illus.) and Laurie Block (comp.). University of Chicago Press, Chicago, 1986. 73 pp. $29.95 (c.).

This is indeed an odd book, but then *Cryptozoology* is an odd journal. In the first place, no text or illustrations appear on the back sides of the volume's pages, which remain blank. Perhaps this was done to give the volume a medieval flavor. Furthermore, there is no pagination. Thus, although I counted 73 pages, only half of these are "used." The volume covers 26 animals, one for each letter of the alphabet, from A to Z. The animals are represented by referenced first historical accounts, some of them sad, some of them charming. Each is accompanied by two illustrations, one a linecut and one a wood engraving, all by artist Alan James Robinson. The book's bibliography contains background texts on each of the historical accounts, including biographies on the original authors.

To give the reader a better idea of the book's range, perhaps we should review the 26 animal entries included: armadillo, basilisk, camelopard (giraffe), dodo, elephant, flying fish, griffin, hippopotamus, ibex, kangaroo, loris, manatee, narwhal, ostrich, platypus, quetzal, rhinoceros, sloth, tortoise, unicorn, vampire bat, walrus, xiphias (swordfish), yak, and zebra. The reader will observe that a few of the animals are truly mythical, and although that seems quite obvious and straightforward to us today, no clear distinctions between "real" and "mythical" animals were perceived in the Middle Ages. Thus, it is interesting to read, in the first written natural history of the elephant, by Richard Eden in 1577, that this animal has "continual war against Dragons, which desire their blood, because it is very colde: and therefore the Dragon lying awaite as the Elephant passeth by . . ."

In her introduction, Laurie Block reminds us that, in the Middle Ages, "men saw all living things as symbols, moral allegories, of the feudal hierarchy: God reigned supreme over angels, angels over the stars, the stars over men, men over Noah's Ark. There were people who ventured out and glimpsed what life was like beyond the stone walls surrounding the medieval community, and when these travelers returned home they brought word of an

earth so large, so full of splendor, so remote from the experience of those at home, that their tales altered the people's dreams."

And after the Middle Ages, the splendor and remoteness continued to surprise us. There were new continents to be discovered and explored, and new animals to be found and talked of. Indeed, exploration has not ended to this day—Block points out that "zoology begins with travel"—and neither has the discovery of new animals. Some keep reminding us that romance no longer plays a role in modern zoology. Perhaps that is so, but I suspect that those who continue to emphasize this point would probably never recognize romance in the first place.

The early medieval bestiaries began in the 12th and 13th centuries, based essentially on an even earlier Latin work, the *Physiologus,* compiled by an unknown monk sometime between the 2nd and 5th centuries, which, in turn, probably had its roots in Greek writers, such as Herodotus, Aristotle, and Pliny. Such pagan material was, of course, cloaked in a new religious and moral context, one that was to last in Europe for a thousand years. Indeed, such texts were considered practically holy scripture, not to be doubted or abused.

Over time, the bestiaries grew in size, "becoming," as Block states, "jigsaw puzzles of the animal world; their pieces were cut and glued together from scrambled bits of hunting lore, travelers' tales, mythology, etymology, theology, and artists' fancies." And, a twist not completely unknown in the 20th century, "a mere spelling error by a scribe could result in the creation of an entirely new species, which then inspired rapid flights of etymological imagination." On the other hand, Block also remarks on the high number of real animals—90 percent—the bestiaries contained. "In the millennium when human memory was the greatest storehouse of information, it would seem more probable that animals living thousands of miles away from Europe's monasteries would vanish from the record. That the Bestiaries' mosaic base retained so much unverifiable but real zoological data is a tribute to those who worked in the great peace of the abbey libraries . . ."

As Copernicus enlarged the Heavens and Columbus the world, noblemen learned how to read and write and education began to flourish outside the walls of the Catholic Church, leading, inevitably, to the Renaissance. In England alone, the number of books printed annually between 1500 and 1630 increased from 45 to 460, and English gradually replaced Latin as the reading language of laymen. The first bestiary in English was Edward Topsell's 1607 *Historie of Foure-Footed Beastes,* based on Konrad Gesner's earlier Latin text *Historia Animalium* (see review of a modern Topsell edition by David Heppell in Vol. 2 of *Cryptozoology*). However, Topsell stayed faithful to the old thinking, and, in fact, greatly enlarged the menagerie of mythical animals. Like Gesner before him, he had little or no understanding

of the concept of ecology or of the interrelation of living things, even in their most rudimentary forms.

As England and the other European powers navigated, explored, and conquered most of the rest of the world, the stories of new animals came pouring in, but, as Block points out, "there were simply no models for objectively observing, then describing, the appearance, character, or behavior of real creatures living in the wild. The standard vocabulary of anatomy, chemistry, and physiology were not yet part of the English language . . . To our ears, the results of the travelers' efforts sound like verbal caricatures, but . . . their accounts enlarged the known world by enlarging the world of actual knowledge."

Before concluding this review, let us examine a few of the alphabetized accounts. We learn from John Leo, in his *Geographical Historie of Africa* (1600), that, in shape, the hippopotamus "resembleth an horse, and in stature an asse . . . Barkes and botes laden with wares and sayling downe the river of Niger are greatly endangered by this sea-horse, for oftentimes he overwhelmeth and sinketh them."

Of particular interest to this reviewer were the first accounts of Australasian fauna. Sir Joseph Banks described the kangaroo for the first time thus: "June 22, 1770: . . . the people who were sent to the other side of the water to shoot pigeons, saw an animal as large as a greyhound, of a mouse colour, and very swift . . . June 25th: I had the good fortune to see the beast so much talked of, though but imperfectly; he was not only like a greyhound in size and running, but had a tail as long as a greyhound's; what to liken him to I could not tell, nothing that I have seen at all resembles him . . . July 27th: This day was dedicated to hunting the wild animal. We saw several and had the good fortune to kill a very large one weighing 84 lbs . . . called by the natives Kangaroo . . . it may, however, be easily known from all other animals by the singular property of running, or rather hopping upon only its hinder legs, carrying its fore-feet close to the breast."

And, finally, the platypus was first reported by David Collins in his 1788–1801 *An Account of the English Colony in New South Wales*: ". . . an amphibious animal, of the mole species . . . in size it was considerably larger than the land mole . . . but the most extraordinary circumstance observed in its structure was, its having instead of the mouth of an animal, the upper and lower mandibles of a duck . . ."

The first scientific examinations and descriptions of the platypus are a curious story in themselves, and, although not addressed in the volume under review, will be of interest to readers of this journal. George Shaw, of the British Museum, was the first to receive a specimen from Australia, in 1798. He named it *Platypus anatinus*. However, unknown to Shaw, the generic name *Platypus* had already been used in 1793 to describe a beetle, rendering it unusable. Meanwhile, the German anatomist Johann Blumenbach described the animal in 1800 as *Ornithorhynchus paradoxus*. However, by the international rules of zoological nomenclature, Shaw's specific name *anatinus* had precedence; thus the animal is known today as *Ornithorhynchus anatinus* Shaw.

Zoologists always want a specimen as proof of an animal's existence, especially when such a peculiar anatomy as this—a mammal with a duck's bill—is reported. Shaw got his specimen, and he could barely believe the evidence of his own eyes. He wrote: ". . . it naturally excites the idea of some deceptive preparation by artificial means . . . nor is it without the most minute and rigid examination that we can persuade ourselves of its being the real beak or snout of a quadruped."

Indeed, I have heard that, if one closely examines the original specimen today—which is still in the British Museum (Natural History)—one may see Shaw's scratch marks around the bill, evidence of his initial desperate attempt to demonstrate that the unbelievable animal was a fraud.

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The Southern Ark: Zoological Discovery in New Zealand 1769–1900. By J. R. H. Andrews. Century Hutchinson New Zealand Ltd., Auckland, 1986. 237 pp. NZ$85.00 (c.).

New Zealand is well known zoologically for its depauperate terrestrial fauna. Long considered "disharmonious" because of the preponderance of birds and the absence of non-volant mammals, New Zealand's animals reflect the long isolation of the islands. Since the formation of the Southern Ocean and the opening of the Tasman Sea some 80 million years B.P., New Zealand has remained, as the title of this book suggests, an ark—carrying a cargo of endemic and often archaic plants and animals.

For more than a century after its discovery, New Zealand remained a biological mystery. A country more distant from European civilization than any other on earth, it attracted few visitors until the late 18th century, and it was not until the mid-1800's that settlement began in earnest. It is this period that saw the birth and growth (in starts and stops) of natural history in New Zealand. In this well-written and engrossing volume, J. R. H. Andrews traces the history of zoology in New Zealand from the first explorers, through missionaries and museum collectors, to the early development of the scientific community of native-born researchers.

The twelve chapters of *The Southern Ark* are organized roughly chronologically, but overlap in time when necessary in order to follow a particular theme within a single chapter. Andrews presents a balanced treatment of the historical development of all branches of systematic zoology, although vertebrates, insects and molluscs dominate the discussion, as they did in the research of the biologists of the time. My comments here are limited to those portions of the book that are of particular pertinence to cryptozoology.

During settler times, the Maori people knew the *kiwi, takahe,* and *tuatara* firsthand, and the *moa* at least by myth and subfossil remains. Indeed, only the native frogs of the genus *Leiopelma* were new to them when discovered by Europeans. Thus, the pattern of biological investigation in New Zealand was, strictly speaking, cryptozoological; Europeans encountered an entire fauna of scientifically undocumented but "ethnoknown" animals (sensu J. Richard Greenwell, 1985, A Classificatory System for Cryptozoology, *Cryptozoology,* Vol. 4: 1–14). As Andrews demonstrates, the earliest period of discovery in New Zealand was limited geographically to the coastal regions, principally in Fiordland, near Dusky Sound, in the southern part of the South Island. From their first contacts with the Maori, Europeans were made aware of the existence of a diverse fauna in the interior, but it was not for many decades that even a fraction of these forms was collected.

The cryptozoological history of New Zealand begins with Captain Cook's first voyage to the islands in 1769, when a large bird with an enormous tail was spotted flying overhead. On Cook's second voyage, unknown mammals resembling dogs, jackals, or cats were reported, and on his last voyage, in 1777, Cook was told of giant lizards and snakes that inhabited the country. None of these animals have since been positively identified, and although they may represent missightings, or the stuff of Maori myths, some, like the giant lizards, might loosely be based on living or recently extinct taxa.

Terrestrial zoology in New Zealand was, and still is, dominated by ornithology. The long isolation of New Zealand and the absence of most predators has produced a high level of endemism among the local birds, and the author devotes fully three chapters to ornithological problems of cryptozoological interest. In chronological order, these deal with the discoveries of the kiwi, the moa, and the takahe. In each case, Andrews draws into the story the personalities of the parties involved, and he stresses the process of discovery from the first anecdotal or partial physical traces to the capture of fresh specimens and their ultimate scientific description.

The discovery of the moa in particular engendered as much argument and innuendo as do modern cryptozoological problems. It embroiled leading New Zealand and European biologists and paleontologists in fierce debates. Not only Sir Richard Owen, the greatest anatomist of the day, but Colenso, Mantell, Taylor, Haast, Hutton, and Hector were all intimately connected with the discovery of these giant birds. As Andrews relates, the first moa

remains reached Owen in London in 1839, nearly a decade after the first New Zealand settlers became aware of their existence, and of the Maori traditions associated with them. Owen's accurate reconstruction of the bird from a single fragment, and his declaration that the bone was of recent origin, stimulated cryptozoological interest in the moa that continues to this very day, with the debate over the possible survival of *Megalapteryx* or other moas into historical times, or even to the present (Roy P. Mackal, 1980, *Searching for Hidden Animals: An Inquiry into Zoological Mysteries,* Doubleday, New York).

The takahe (*Notornis mantelli*) was first discovered in the form of subfossil bones in 1847. Only two years later, the first skin was obtained, but by the turn of the century only three more birds had been found and the animal was believed extinct. The rediscovery of this species in the 1940's falls outside the chronological scope of *The Southern Ark,* but represents one of the most significant cryptozoological discoveries of the last 50 years.

The discussion of reptiles is limited and scattered throughout the book, but the discovery of the tuatara (*Sphenodon*), and the recent discovery of the giant gecko, *Hoplodactylus delcourti* (undescribed at the time of the book's publishing) are outlined briefly. Mention is also made of the role of crocodiles and other reptiles in Maori mythology, and the possible bearing of myth on zoological discovery. Andrews speculates that the only known specimen of the giant gecko may have been mounted by William Williams, of the Paihia Mission, and returned to France surreptitiously with one of the officers or crew of the *Astrolabe* under the command of Dumont D'Urville. For more information on the cryptoherpetology of New Zealand, another new publication, *New Zealand Lizards: An Annotated Bibliography* (Tony Whitaker and Bruce Thomas, 1989, Ecology Division, DSIR, Lower Hutt, New Zealand), gives complete references to dozens of papers and books discussing Maori knowledge and mythology relating to lizards.

An underlying theme of *The Southern Ark* is the effect that scientists and others had on the ecology of New Zealand during the period of first contact and later settlement. It highlights the fact that many cryptozoological problems exist because of the thoughtlessness of our predecessors. In contrast to the modern catch-phrase "take only pictures, leave only footprints," the early European visitors to New Zealand were somewhat more intrusive. Scientists and explorers generally collected judiciously, but a few cases of excess were recorded. More importantly, however, these men brought with them the seeds of the destruction of the unique and precariously balanced ecology of New Zealand. Beginning with the second voyage of Cook in 1773, Europeans began to release exotic species in New Zealand. Cook himself left pigs, goats, geese, chickens, and, inadvertently, rats.

The success of these and other immigrants was spectacular, and occurred at the expense of the native fauna, compounding the effects that the intro-

duction of the Maori dog and Polynesian rats had on the indigenous forms hundreds of years earlier (Joan Druett, 1983, *Exotic Intruders: The Introduction of Plants and Animals into New Zealand,* Heinemann, Auckland). The effects of these introductions were already obvious to naturalists in the early 1800's, who noted the declining populations of certain birds. This trend was to culminate in the formation of acclimatization societies later in the 19th century, which systematically introduced foreign mammals, birds, and fish. These effects were clearly responsible for the extinction of many bird species, and may be implicated in the decline of reptile species as well (Aaron M. Bauer and Anthony P. Russell, 1987, *Hoplodactylus delcourti* and the *kawekaweau* of Maori Folklore, *Journal of Ethnobiology,* Vol. 7: 83–91).

From a cryptozoological perspective, a major drawback of the book is that the question of the "New Zealand otter"—known to the Maori as *waitoreke*—is not discussed. Although a number of the protagonists of *The Southern Ark,* including Walter Mantell, were involved with the history of this cryptid, it receives no mention whatsoever. Neither are supposed lizard-like animals, such as those known to the Maori as the *kumi* or *ngarara,* discussed at all. However, it must be remembered that such omissions are to be expected in the context of a general zoological book, given that no material evidence of these creatures exists.

The Southern Ark is beautifully illustrated by 88 drawings, paintings, and engravings from the time period covered, including a number of manuscript illustrations never before published. These figures, most in color, are ideally suited to the subject matter, and reflect the changing interests and sophistication of students of New Zealand zoology over more than a century. In addition, segments of handwritten text from important letters and manuscripts dealing with New Zealand zoology are also figured. Of particular interest are Walter Mantell's sketch of the takahe and the plates from Richard Owen's original works on *Notornis* and *Dinornis.*

The book reads well, and is accessible to the non-specialist. It is liberally referenced, with over 400 works cited, and hundreds of footnotes placed in the margins provide additional information, parenthetical or technical. Treating, as it does, both history and science, the book is an invaluable aid in bridging the literature gap between the two fields. For the zoologist, it serves as a reference tool to access not only the rare, older New Zealand literature, but also as a guide to the voyages, journals, and manuscripts pertinent to the zoology of the region. These sources are typically the realm of archivists and historians, and as such are beyond the reach of most biologists. The book is fully indexed by both general terms and zoological names.

The Southern Ark might easily have been overlooked by most cryptozoologists, as it has no designated American distributor. Indeed, a common

trend in New Zealand is to underestimate the demand overseas for such works. The price may deter some potential buyers, but the cost is well in line with the high production quality of the book. An exceptional book that is both a useful research tool and an attractive general account, *The Southern Ark* will be appreciated by all with an interest in the history of zoology or the fauna of New Zealand. It should be required reading for those with special interests in the Pacific region or in avian cryptozoology. The retrospective analysis of the unraveling of crypto-ornithological puzzles of the last century will prove both interesting and instructive to modern workers in the field.

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King Cheetah: The Story of the Quest. By Lena Godsall Bottriell. E. J. Brill, Leiden, 1987. 241 pp. £17 ($24.95) (c.).

Lena Bottriell and her husband Paul have a passion for king cheetahs, and managed to communicate this passion to enough financial backers to enable them to travel to the Botswana–Zimbabwe–South Africa border area to search for them. This book tells how they fared; they set up camp in the Tuli salient of Botswana (where they didn't find any king cheetahs), then traveled through Zimbabwe and parts of South Africa (where they tracked down some skins), made a flying visit to Namibia to inspect a reputed king cheetah skin (which turned out to be a serval), visited the Kruger National Park (where they think they saw a live one), and ended up at the De Wildt Cheetah Breeding Station and Research Centre, where they were able to examine living spotted cheetahs at leisure.

The story is told breathlessly, colloquially; full of misplaced apostrophes ("it's" meaning "of it") and idiomatic grammatical infelicities ("to Paul and I"); and so full of incidents tumbling over each other that the reader sometimes loses track of exactly what happened when, to whom, and in what order. Never mind; it is an interesting tale, and communicates the author's excitement about the beast and the quest. There are plenty of photographs, including some in color: 22 of king cheetahs (including their skins), 18 of the author and/or her husband (4 of these depicting king cheetahs *and* one or more Bottriells), while ordinary cheetahs figure in 11 (8 with kings or Bottriells, only 3 on their own). Among the other photos, 20 in number, micrographs on p. 172 of cheetah hairs (both normal and king) are of special

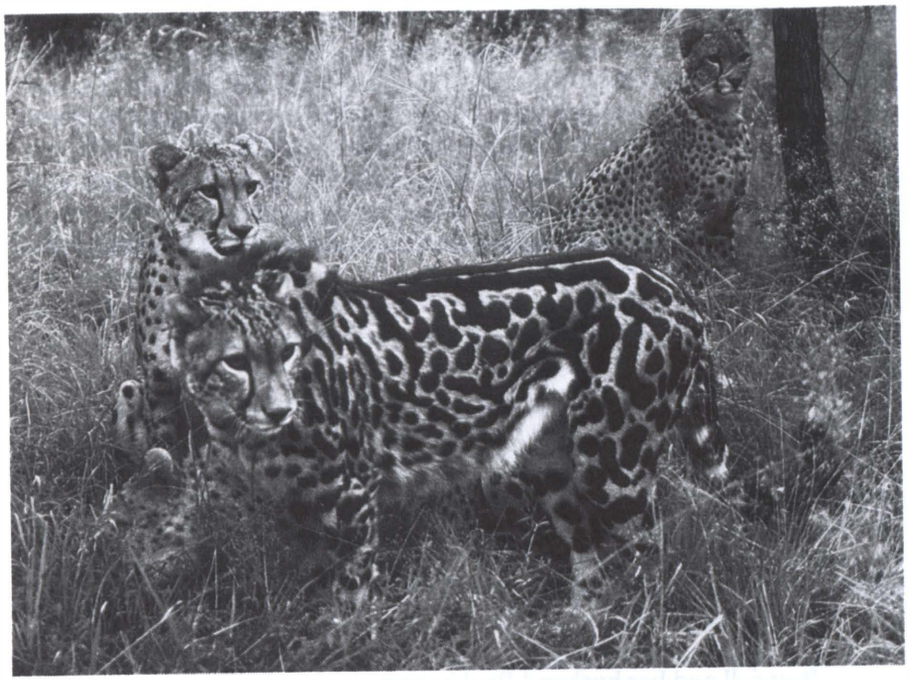

FIG. 1.—The striking appearance of the king cheetah when seen in the company of "ordinary" spotted cheetahs.

interest, while those who (like myself) are interested in the history of mammal taxonomy will be delighted to see Reginald Pocock himself on p. 33.

So what is the king cheetah, and why all the excitement? Described as a new species, *Acinonyx rex,* by Pocock in 1927, this cheetah with blotched flank pattern and longitudinal stripes along the back (Fig. 1) was later deemed by its describer to be but a morph of the one and only species of living cheetah, *A. jubatus.*

Other specialists, such as A. Roberts (1951, *The Mammals of South Africa,* Central News Agency, South Africa) demurred. Species or just morph, the king cheetah's beauty and rarity have combined with its enigmatic taxonomic status to fascinate many up to the present day. Lena Bottriell recounts this history in the Prologue and her first chapter ("Legend"), and returns to the question of "What is it?" in her last chapter ("Ex Africa Semper"). So this is what we must concentrate on now.

The king is not just a morph, she maintains. It differs not just in the different color pattern (p. 228): the stripes and blotches are raised, as if embossed; the hair is longer, silkier; the tail has a continuation of the dorsal stripes on its proximal part; the marks are boldly black on a creamy ground;

and the mane is longer. These differences, she stresses, "are hardly comparable to a single colour, or . . . pattern change. . . .": not like a black jaguar or ginger leopard, and there are no intermediates as between small-spotted and large-spotted servals. The cuticular pattern on the hairs is of the (leopard-like) petal type, not mosaic as in the normal cheetah. The animals seem to predominate in thornbush and woodland country, not the normal cheetah habitat. She sees this as "evolution in action," a new taxon caught in the process of evolution.

"It is argued," she writes on p. 231, "that King Cheetahs cannot be awarded sub-species status as they do not occupy a separate geographic area. But as far as the naming of the species and races is concerned, the definition of separate geographic areas is as yet not precise. The species of rhino particular to the island of Java is as separated, specifically, from the smaller Sumatran species, as the two islands are by the Sunda Strait. Few natural barriers in Africa can so precisely and so completely delineate and isolate one geographic area from the other. No better is this demonstrated than with the geographic areas ascribed to the seven species and sub-species of zebra. These have all been named and described from a portion of the African continent; and if the mountain zebra of the Cape had not been exterminated throughout most of its former range their ranges would still widely overlap."

I have quoted this paragraph in full because it exhibits so many conceptual misunderstandings (and factual errors) that logical argument cannot proceed unless they are cleared up:

1) Species are reproductively isolated—they do not exchange genes under natural conditions (though there may be occasional hybridization, and indeed in captivity they may hybridize freely, but that is another matter). Subspecies are geographic segments (populations) of a species which differ morphologically from one another in some consistent way; by *definition* they cannot occupy the same geographic area because (again by *definition*) they are not reproductively isolated.

2) Javan and Sumatran rhinos are distinct species not because they happen, today, to be separated by the Sunda Strait, but because all kinds of evidence indicates that they were reproductively isolated; indeed, until well after the turn of the century, the Javan rhino lived alongside the Sumatran in Sumatra, Malaya, Thailand and Burma—areas from which it has been exterminated, while the Sumatran survives there (in fact, Javan rhinos have recently been found still hanging on in Vietnam).

3) Mountain zebras (*Equus zebra*) and plains zebras (*Equus quagga*) still live today in the same areas in northern Namibia, i.e., they are reproductively isolated; Grevy's zebra (*Equus grevyi*) lives in the same areas of northern Kenya as plains zebras. But the plains zebra is divided into (about six morphologically differing) subspecies, some alas now exterminated, which simply replace or replaced each other progressively in different parts of Africa

and—not being reproductively isolated—intergrade where they pass into one another. For that matter, the mountain zebra is also divided into subspecies; as Lena Bottriell states, the species has been eliminated from most of its former range, so the two living subspecies are now geographically isolated from each other and have no chance to intergrade.

So the king cheetah *cannot* be a subspecies. It is found in the same areas as ordinary cheetahs; it is not the geographic representative, in any region, of the species *Acinonyx jubatus.* It is a species, or it is a morph. The matter is quite clear-cut; there is no subspecies *Acinonyx jubatus rex.*

Nor is it a distinct species of cheetah. On p. 166 is footage from a film of a king and a normal cheetah walking side by side. Not proof that they are actual or even potential mates, but suggestive. Several anecdotal observations in the book record kings and normals interacting, and, decisively, there is the De Wildt experience—two normal females, mated to the same male, gave birth to king cubs. So the king cheetah is not a species; there is no species *Acinonyx rex.*

Under normal circumstances, an observation of two normal individuals mating and producing an oddity would give rise to just one explanation: the oddity is due to a recessive allele—at the very least, a series of closely linked alleles. Lena Bottriell seems to dispute this: "Genotype aside," she writes on p. 228, "the King Cheetah is essentially different. . . ." I don't see how the genotype can be pushed aside; the breeding record is perfectly clear. A normally colored male was mated successively to two normally colored females, and each of them produced a king cub. If one mates two normally colored leopards, and if each of them happens to be a heterozygote for the black allele, there can be black cubs in the resulting litter.

On pp. 227–8, the author mentions the hypothesis that the king cheetah is due to a gene homologous to "blotched tabby" in the domestic cat; but she dismisses the idea—it's not a simple color variant like white tigers or the white lions of Timbavati; tabbies are universally distributed, not localized. . . . She does not know that genes are very commonly pleiotropic (affecting several different morphological and behavioral features, to differing degrees), nor is she aware of the complexities of cat genetics. As shown by R. Robinson (1978, Homologous Coat Colour Variation in *Felis, Carnivore,* Vol. 1: 68–71), almost all the color variants recorded in wild felids can be plausibly referred to mutations homologous with those known from domestic cats; the king pattern in cheetahs is indeed the "blotched tabby" mutant at the T locus.

There is another simple color mutant in the cheetah: the fluffy grey-white form with red-brown spots, named *Felis lanea* by P. L. Sclater. Bottriell refers to it on pp. 38–40: ". . . cats that sound suspiciously like forerunners, or at least immature examples, of king cheetahs . . . inhabited the Lebombo Mountains dividing Kruger Park from Mozambique." Robinson (1978, above) ascribes this morph to the Chinchilla allele at the C locus, like the white tigers of Rewa; it is, needless to say, nothing whatever to do with the king morph genetically. There are three skins of the king morph in the British Museum (Natural History); two of them (including the type of *lanea*) are from Beaufort West, in southern Cape Province (the third just from "South Africa"). Bottriell's mention of Lebombo, incidentally, appears to stem from a misreading of R. Lydekker (1894, *The Royal Natural History,* 6 vols., Frederick Warne, London), which she quotes out of context; he merely mentions the morph, then, returning to the question of the distribution of the cheetah in general, mentions the "Bombo Mountains."

Even within the species, then, it is not quite true to say that the king cheetah is "essentially different"; when we turn to a species like the leopard, we are confronted with an astounding variety of color morphs, which begins to approach that of the domestic cat. Like king cheetahs and the Beaufort West "woolly" cheetah, some of the leopard's morphs are restricted to particular regions: thus, the very peculiar tawny-orange form, with its small spots and black dorsum, described as "var. *melanotica*" by A. Gunther (1885, Note on a Supposed Melanotic Variety of the Leopard, from South Africa, *Proceedings of the Zoological Society of London,* 1885: 243–45), is known only from the Albany and Grahamstown districts, from just six specimens (C. G. Shortridge, 1934, *The Mammals of South West Africa,* William Heinemann, London). What is a little surprising, perhaps, is not the occurrence of a wild felid morph with the "blotched tabby" allele, but its occurrence in a species as genetically monomorphic as the cheetah.

In the last few pages of the book, the author speculates on the evolutionary significance of the king cheetah—a species (or subspecies!) in the making, perhaps? I do not completely discount it, but we need a great deal more evidence than is presently available; in particular, is it spreading, and replacing the common morph? On p. 236 is given a useful list of all known king cheetah skins, and their localities and dates; plotting these on a map might yield some information, but, then again, it might be the distribution of white hunters rather than of kings! And it does seem, from the reports in the book, that the king has been known to particular African communities since the last century at least. And those De Wildt breeding records are very clear; there is no indication, no sign whatever, of any developing reproductive isolation between the king and normal morphs.

Certainly, the book has its value; it is a good read, and yields interesting information in some sections. But it would have been still more useful if it contained an index, and if the author had given us more descriptive data; the full genealogies of the De Wildt births recorded on p. 237, for example, would have been of genetic value. And the entire final chapter is most ill-advised. It is as if, confronted with the breeding data, the author had felt the value of her quest being put in question, and felt obliged to justify it.

The justification ends up being muddled and based on inaccuracies. But a morph is not "just" a morph. Just because the king cheetah is not a distinct taxon, it is not deprived of significance.

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A Living Dinosaur?: In Search of Mokele-Mbembe. By Roy P. Mackal. E. J. Brill, Leiden, 1987. 340 pp. £17 ($24.95) (c.).

Mokele-Mbembe is the name used by natives of the northern Likouala region of the People's Republic of the Congo for a large, amphibious animal that is unknown to the scientific world, but is said to live in remote swamps of the region. Local people say the herbivorous animal has a bulky torso, stout legs, small head, and a long neck and tail. Because this description is similar to that of a sauropod dinosaur, some have proposed that Mokele-Mbembe is a small surviving member of that group, which otherwise became extinct 65 million years ago. As a vertebrate paleontologist with a special interest in dinosaurs, I found this book, an overview of Mokele-Mbembe and other "hidden" animals (cryptozoons) of Central Africa, very interesting—but ultimately unsatisfying.

The book is composed of several sections, including an eight-page, laudatory foreword by Bernard Heuvelmans, seven appendices of reports on various expeditions to Central Africa in search of Mokele-Mbembe and other unknown animals reported in the region, a bibliography of references mentioned in each of the 24 chapters, and a comprehensive index. The photographs and line drawings are of generally good quality, and the type is clear and dark. However, the publisher made a mistake in not indenting the first line of each paragraph. This makes reading the text or finding a particular section more difficult than usual. The references cited are often out of date, such as the 1907 and 1948 papers on ceratopsian dinosaurs that were used as the main sources of information on that group. It is clear that Mackal did not make an exhaustive search of current literature, especially in relation to his attempt to identify the various cryptozoons of the Congo.

A majority of the text is devoted to either the history of reports on Mokele-Mbembe or to the preparation and execution of Mackal's two expeditions to Africa in search of the beast (Fig. 1). Thus, the book will please readers mainly interested in an adventurous travelogue to the Congo spiced by the tantilizing—although remote—possibility of finding a beast unknown to science. However, the book will be disappointing to readers primarily interested in hard evidence in support of cryptozoons of the region. The vast majority of the evidence presented by Mackal consists of unconfirmed sightings, often second hand or worse. The physical data presented, such as the alleged tracks of Mokele-Mbembe, is extremely limited and unconvincing. What is needed to prove to the world that this or other "hidden" animals exist is part or all of a body, living or dead, or at least an unambiguous still or motion picture record. Mackal's book does not provide such evidence.

FIG. 1.—Roy Mackal, fourth from right, leading his team into the Likouala swamps in 1981 in search of Mokele-Mbembe evidence.

Besides the supposedly sauropod-like Mokele-Mbembe, a variety of other undocumented animals have been reported from Central Africa. For all of them, however, the problem is the virtual absence of physical evidence to support even the existence of the animals, much less their identity. Eyewitness reports or legendary accounts by native informants is the main evidence for most of these creatures; yet, even in these reports, there is often a lack of uniformity in the descriptions of what is assumed to be the same animal.

One of Mackal's previous books, *The Monsters of Loch Ness* (1976), is

one of the most comprehensive and balanced reviews of the evidence for, and interpretations of, a cryptozoon. I had hoped that his latest book would do for Mokele-Mbembe what that book did for Nessie. Unfortunately, this is not the case.

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Monster Wrecks of Loch Ness and Lake Champlain. By Joseph W. Zarzynski. M-Z Information (P.O. Box 2129), Wilton, New York, 1986. 111 pp. $8.95 (p.).

Joseph Zarzynski's second book is a slimmer volume than his first—*Champ: Beyond the Legend,* published in 1984—and, as the title suggests, does not deal exclusively with cryptozoological subject matter. The book is divided into nine sections, covering aspects from ship and aircraft wrecks to the various items of hardware that have been employed in the search for Nessie and Champ. Each section is divided into smaller subsections, so that the book is basically a collection of short pieces of, on average, a couple of pages in length. There are twenty nine of these, an index, plus notes and a bibliography, giving the reader ample opportunity to follow up the stories more fully. The text is accompanied by thirty seven black and white illustrations.

The book is, in the main, carefully researched, as one would expect of Zarzynski, a pioneer fieldworker at Lake Champlain who has made several visits to Loch Ness in order to keep in touch with investigations there. It is an informative guide to various curiosities from the two lakes, and the format makes it handy for reference. Some of the subjects which the author describes as "challenging mysteries" and "alluring enigmas" do not fit these classifications in quite the same way that Nessie and Champ do, but readers may find them interesting nonetheless, if only for their association with the two "monster" lakes. The author explains in his introduction that "most of the stories in this book are not the typical rewrite about Nessie and Champ sightings," and, with the exception of a small number of sighting accounts and sonar contacts, he includes nothing in the way of evidence to support the case for the existence of either Nessie or Champ. However, the book is written from the perspective of a strong belief in both, which suggests that it is targeted at those who are already familiar with, and have accepted, that evidence.

The longest individual section in the book is a detailed account of the raising of the wartime Wellington bomber from Loch Ness in 1985 (see *The ISC Newsletter,* Summer, 1986, p. 10). Zarzynski is a member of the Loch Ness Wellington Association, and he travelled to Scotland to witness the event. He describes the bomber, discovered by Martin Klein in 1976 after a side-scan sonar search for physical remains of Nessie, as "undeniably the greatest by-product result of the monster quest at Loch Ness."

He goes on to describe several other discoveries made at Loch Ness and Lake Champlain which were also spin-offs from sonar searches of a cryptozoological nature. Among these are two others made by Martin Klein at Loch Ness in 1976. One was several ring-shaped heaps of stones in 10 and 25 meters of water off Lochend, named "Kleinhenge I & II" after the discoverer, and the other was a 10-meter-long target on the loch bottom off Fort Augustus, dubbed "The Average Plesiosaur." In 1976, this was described by Klein as "a carcass-like shape with a long neck-like projection."

A certain amount of realism has been applied to both these cases since that heady summer of the New York Times/Academy of Applied Science expedition. In a letter to Zarzynski in 1985, Klein conceded that " 'The Average Plesiosaur' could be most anything. I feel it is most likely to be geology." Although Zarzynski is still happy to refer to the Lochend stones as "a possible megalithic site," after Martin Klein and Charles Finkelstein's speculation that the structures "were built on land, perhaps thousands of years ago," possibly to function as a giant clock or calendar, at least he includes John Mills' more mundane explanation that "the ring shaped heaps of stones at the bottom of the north end of Loch Ness are almost certainly the result of dumping the morainic material dredged from the nearby River Ness during the construction of the [Caledonian] canal from barges."

It is important for the credibility of cryptozoology that misjudgments such as the identification of the Lochend stones as ancient stone circles should be publicly corrected, or myth can quickly become piled upon myth to the point of total absurdity. Consider Janet and Colin Bord's suggestion that "the Loch Ness complex of stone circles could have been erected either to originate or control the monster by giving the builders control over the earth energies it utilized."

Zarzynski's report of the death of John Cobb, whose speedboat disintegrated during his attempt to break the world water speed record at Loch Ness in 1952, sails a little too close to the wind of myth creation. He records that "one popular theory suggests that the *Crusader* [Cobb's speedboat] . . . hit the beastie or waves from the monster." He then quotes a newspaper item titled "Killed by a Monster?" which reported Cobb as saying: "I wouldn't be wildly surprised if I did not see the monster one of these days," before he draws the conclusion that "Possibly Cobb did, as his *Crusader* ripped along the waters of Loch Ness that September 29, 1952 day!" Zarzynski

should be aware that there is a quite straightforward and unsensational explanation for the ripples which caused Cobb's death. According to two eyewitnesses to the accident, interviewed by Ronald Binns, "what actually happened was that Cobb was signalled to proceed when the wake from one of his own guide boats was still travelling across the loch. Cobb sped forwards and smashed into the wake sideways."

Naturally, newspapers are always going to print the sensational, but the cryptozoological researcher is expected to sort out the wheat from the chaff in an effort to find the truth and make it known.

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Ogopogo: The True Story of the Okanagan Lake Million Dollar Monster. By Arlene Gaal. Hancock House, Surrey, British Columbia, and Blaine, Washington, 1986. 128 pp. C$6.95 (p.).

The Indians called it *naitaka,* the dweller of Okanagan Lake, that had to be appeased with animal sacrifices. In the 19th century, European settlers heard about it, and even caught a glimpse of it now and then. The popular press dubbed it "Ogopogo," after a music-hall song of the 1920's. In the Okanagan Valley of British Columbia, Canada, Ogopogo has by now become a household name, as well as a solid touristic asset. Arlene Gaal lives in Kelowna, the main town of the Okanagan Valley; she tells the story of the Ogopogo phenomenon in this fact-filled little book.

Okanagan Lake is about 300 miles (500 kilometers) from the sea, in the dry, mountainous interior of British Columbia. About 60 miles (100 kilometers) long, up to 800 feet (240 meters) deep, this narrow trench-like lake is similar to, but larger than, Scotland's Loch Ness. It communicates with the sea through the Columbia River system, past a series of dams constructed within the past 50 years. The area was heavily glaciated up to about 10,000 years ago, and all its fauna must have migrated in from the sea or other lake systems since that time.

Although represented on the cover of the book as a mean-looking serpentine creature, Ogopogo is described by witnesses as an animal with a heavy snake's body, 20 to 50 feet long, with a well-bearded horse's or goat's head. Most witnesses, however, only catch a glimpse of a rounded back, or of two or three humps breaking the surface.

Gaal starts by putting the subject within the perspective of other observations of aquatic cryptids. She then reviews observations chronologically, starting with Indian legends, continuing with those of early European settlers, and bringing the reader to the modern days of film and photography. Reproductions of some of the best shots of Ogopogo are presented, including one from the Folden film, claimed to be the real "proof" that some large animate creature inhabits Okanagan Lake. The story is also supported with a variety of newspaper clippings which convey to the reader some of the semi-serious attitude about Ogopogo which prevails in the Okanagan. There is also a very useful chronological list of over two hundred sightings presented in an appendix.

The "Million Dollar Monster" label was added by the publisher to what is really an expanded and updated version of Arlene Gaal's first (1976) book on Ogopogo: *Beneath the Depths.* It arises from a reward of that amount offered by the local tourist association, and underwritten by Lloyd's of London, to anyone who could catch the creature with a rod and line and hold it alive for inspection and verification by zoologists from the University of British Columbia. The offer was valid for a year from February 1, 1984. There were no serious claimants.

Arlene Gaal's book is an excellent introduction to the story of Ogopogo, and a good example of how dedicated amateurs can make valuable contributions to cryptozoological research.

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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.*

Cryptozoology, 8, 1989, 98–149

Cryptozoology: Science or Pseudoscience?

(Comment on Bernard Heuvelmans, 1988, The Sources and Method of Cryptozoological Research, *Cryptozoology,* Vol. 7: 1–21)

In his remarkable article—a landmark in cryptozoology, and even in epistemology—Heuvelmans analyzes the sources and method of cryptozoological research, giving to the scholar a very useful "theoretical weapon."

All the same, when he states: "The more witnesses who can be produced . . . the stronger the case," I would like to draw attention to the fact that the *quality* of the reports of hidden animals is more important than their *quantity* (ideally, of course, quality *and* quantity combined are even better): hundreds of Sasquatch reports of "a big hairy man," "a huge ape-man," "a true King-Kong"—nothing more—have no value at all for cryptozoologists. On the other hand, many unknown hominoid reports from the USSR (particularly from the Caucasus, thanks to Marie-Jeanne Koffmann's remarkable field work), do make sense, as they contain a lot of anatomical, ecological, and behavioral details which appear consistent to zoologists and anthropologists (Marie-Jeanne Koffmann, 1984, Enquêtes Récentes sur des Hominidés Inconnus dans le Caucase, Paper Presented at the Third Annual Membership Meeting of the International Society of Cryptozoology, University of Paris VI, June 9, 1984).

The same can be stated about the "physical evidence" for some unidentified animals: for instance, many of the photographic and even sonar track

records, considered by some to be hard evidence, are, at the very least, highly questionable (Steuart Campbell, 1986, *The Loch Ness Monster: The Evidence,* The Aquarian Press, Wellingborough, England; Rikki Razdan and Alan Kieler, 1986, Sonar and Photographic Searches for the Loch Ness Monster: A Reassessment. *In* Kendrick Frazier [ed.], *Science Confronts the Paranormal*. Prometheus Books, New York: 349–58). Though I am certainly not a debunker, I must emphasize that one good piece of evidence (such as the Walla Walla track casts showing dermatoglyphs) is better than dozens of poor ones. Many documented Walla Walla events will be necessary to convince the scientific establishment, because the more unlikely an event is assumed to be, the more the quality and quantity of evidence required. This can be regretted, but it cannot be ignored.

This leads me to my main point: according to some, cryptozoology, like astrology, UFOlogy, parapsychology and *tutti quanti,* is not a science at all, but simply a kind of wild good chase or, to say it all in one word, a pseudoscience (Daisie Radner and Michael Radner, 1982, *Science and Unreason,* Wadsworth Publishing, Belmont, California: 33). As there is no simple and satisfactory definition of *science* (Henry H. Bauer, 1987, What De We Mean by "Scientific," *Journal of Scientific Exploration,* Vol. 1[2]: 119–27), it is easier to state what a *pseudoscience* is (thus, what a science is *not*): its theory is subjective, with aspects only accessible to "initiates"; its formalism is poor, involving few or no mathematics and/or logic; it claims unverifiable hypotheses, or even false ones; it does not use notions from other disciplines, and does not encroach on other disciplines; its doctrine is always the same (whereas science increasingly enriches itself); and its conception of the world is in contradiction with the laws of physics, if not with common sense (James A. Alcock, 1981, *Parapsychology: Science or Magic*? Pergamon Press, Oxford).

Heuvelmans' review of methodology, as well as his previous theoretical articles, clearly include cryptozoology in the realm of science. Objectivity, verifiability (importance of the bibliography), interdisciplinarity (cryptozoology calls upon zoology, of course, but also paleontology, psychology, archaeology, linguistics, etc.), etc., characterize the work of true cryptozoologists. And, finally, the existence of still undescribed animal species or subspecies is continually confirmed by new discoveries, and these do not at all violate any established scientific laws.

Another difference between science and pseudoscience is that the former, besides its methodology, is also characterized by its results, as science is continuously progressing; the strength of a scientific explanatory system is not only in accounting for what is known at a given time, but also in predicting new discoveries. For instance, Albert Einstein's relativity not only accounted for the Michelson-Morley interferometry experiment on the isotropy and invariance of c (the speed of the light), but it also foresaw several

new facts (such as time dilation when the speed approaches *c*), later confirmed experimentally.

Similarly, Alfred Wegener's theory of continental drift not only accounted for the resemblance of the fauna and flora of Europe and North America during the Mesozoic Era, as well as for the shape of the continents—which curiously seemed to fit together like a jigsaw puzzle—but it also foresaw the existence of mineral deposits in Argentina, in relation to the well-known ones in South Africa, and these were subsequently discovered. Both of these revolutionary theories were ahead of their time. Continental drift, now known as plate tectonics, was only accepted by the scientific establishment as late as 1968—a loss of 50 years in the earth sciences!—not really for scientific reasons, but, rather, for subjective ones.

Regarding this ability of predicting new facts, Heuvelmans recently listed 110 to 138 unknown animal forms relevant to cryptozoology (Bernard Heuvelmans, 1986, Annotated Checklist of Apparently Unknown Animals With Which Cryptozoology Is Concerned, *Cryptozoology,* Vol. 5: 1–26). Since this list was submitted for publication, at least three of these forms have been spectacularly confirmed by autoptical evidence (a specimen), and the case for several others has been strengthened by new evidence:

- The *onza,* a long-legged, puma-like cat from the Western Sierra Madre of Mexico, had been reported for almost 5 centuries (Heuvelmans 1986, above: 22); in 1986, a specimen was shot and preserved, and it is currently being studied in the United States (Richard Greenwell, 1987, Is This the Beast the Spaniard Saw in Montezuma's Zoo? *BBC Wildlife,* July: 354–59). The identification proposed by Helmut Hemmer, that of a surviving Pleistocene cheetah from North America (*Acinonyx* [*Miracinonyx*] *trumani*), was wrong, but otherwise its existence, appearance, geographical range, etc., were cryptozoologically predicted and subsequently confirmed. If not a new species, it might turn out to be a genetic anomaly, or a subspecies in the making, as suggested for another felid, the king cheetah (Lena Godsall Bottriell, 1987, *King Cheetah*: *The Story of the Quest.* Brill, Leiden).

- The existence of unknown lemurs in Madagascar was also listed by Heuvelmans (1986, above: 17). Not surprisingly, a new species has since been discovered and described as *Hapalemur aureus,* the golden bamboo lemur (Bernhard Meier, Roland Albignac, André Peyriéras, Yves Rumplar, and Patricia Wright, 1987, A New Species of *Hapalemur* [Primates] from South East Madagascar, *Folia Primatologica,* Vol. 48: 211–15). The following year, a second new species, the golden-crowned lemur, was scientifically christened as *Propithecus tattersalli* (Elwin Simons, 1988, A New Species of *Propithecus* (Primates) from northeast Madagascar, *Folia Primatologica,* Vol. 50: 143–51); this new lemur, with white fur and a golden-orange crown on its head, is so conspicuous that it can be seen in treetops from a distance of half a mile.

- I myself predicted the existence of an unknown flightless bird, a rail on Hiva-Oa (Marquesas Islands) related to the *takahe* from New Zealand (*Porphyrio* [formerly *Notornis*] *mantelli*), from observations by Polynesian natives and Western explorers; e.g., Thor Heyerdahl, the hero of the *Kon-Tiki* expedition (Michel Raynal, 1980–1981, *Koau,* l'Oiseau Insaisissable des Iles Marquises, *Bulletin de la Société d'Etude des Sciences Naturelles de Béziers,* Vol. 8[49]: 20–26; Heuvelmans 1986, above: 24). This hypothesis has just been confirmed, as 1,000-year-old bones from a new rail species, *Porphyric paepae,* have been found on two islands of this archipelago, to wit: Tahuata and Hiva-Oa (David W. Steadman, 1988, A New Species of *Porphyrio* [Aves: Rallidae] from Archaeological Sites in the Marquesas Islands, *Proceedings of the Biological Society of Washington,* Vol. 101[1]: 162–70).

- The survival of probable subspecies of the thylacine or marsupial wolf (*Thylacinus cynocephalus*) on mainland Australia—besides the survival of the Tasmanian form—again listed by Heuvelmans (1986, above: 23), seems to have been confirmed. An Australian Aborigine, Kevin Cameron, has taken photographs of what is believed to be a live specimen (Athol M. Douglas, 1986, Tigers in Western Australia?, *New Scientist,* Vol. 110[1505]: 44–47).

- The survival of Neanderthal Man (*Homo sapiens neanderthalensis*) into the 18th century in the Pyrénées was conjectured by several European authors (Paul Ormières, 1974, *Les Néandertaliens dans les Pyrénées,* Narbonne, unpublished ms: 1–9; Jose-Manuel Gomez-Tabanera, 1978, La Conseja del Hombre Salvaje en la Tradición Popular de la Peninsula Iberica. *In Homenaje a Julio Caro-Baroja,* Centro do Investigaciones Sociologicas, Madrid: 471–509; Heuvelmans 1986, above: 12). This has now become more likely, since a Neanderthal mandible has been found at Boquete de Zafarraya, Spain, in a level of late Wurm III[1] (Jean-Jacques Hublin, 1989, Le Dernier Néanderthalien, *Pour la Science,* Vol. 138: 12–13), placing it much later than the normally accepted date of extinction of Neanderthal in Western Europe.

- Heuvelmans (1986, above: 13) comments on a still undescribed subspecies of wildcat in the Ile du Levant. Coincidentally, two specimens of wildcat (*Felis silvestris*) were shot in Corsica in 1986, where its presence had always been doubted (J. Arrighi and M. Salotti, 1988, Le Chat Sauvage [*Felis silvestris* Schreber, 1777] en Corse: Confirmation de sa Présence et Approche Taxonomique, *Mammalia,* Vol. 52[1]: 123–25). Thus, its existence in the nearby Ile de Levant would now not be surprising at all.

- Finally, Heuvelmans (1986, above: 5) stated that unknown beaked whale species remain to be discovered: three cetologists actually sighted and photographed an unidentified species of *Mesoplodon* off the Pacific coast of Mexico, a discovery which was discussed at the 1986 meeting of the Inter-

[1] The Wurm III interstadial ended about 11,000 B.P.—Editor.

national Whaling Commission (Robert L. Pitman, Anelio Aguayo L., and Jorge Urban R., 1987, Observations of an Unidentified Beaked Whale [*Mesoplodon* sp.] in the Eastern Tropical Pacific, *Marine Mammal Science,* Vol. 3[4]: 345–52). Incidentally, another species of beaked whale, found in Peru, has recently been described as *Mesoplodon peruvianus.* Though none of these species is the one sighted by P. H. Gosse, the existence of still undescribed forms gains new support.

If only because of these predictions, and their remarkable confirmation in a brief 3-year period, the existence of cryptozoology is certainly warranted.

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CRYPTOZOOLOGY AS A CASE IN LAW

(Comment on Bernard Heuvelmans, 1988, *Cryptozoology,* Vol. 7: 1–21)

Heuvelmans gives an excellent overview of how to approach research in this field. He rightly stresses that it requires not only a thorough grasp of the zoological sciences, but also a certain familiarity with a number of unrelated fields, such as mythology, linguistics, archaeology, and history. He also emphasizes that, because of the wide scope of multidisciplinary competence required, virtually anyone, depending on his skill, competence, and inclination, can contribute to the progress of cryptozoology.

Heuvelmans gives the following definition of cryptozoology: "The scientific study of hidden animals, i.e., of still unknown animal forms about which only testimonial and circumstantial evidence is available, or material evidence considered insufficient by some." He goes on to describe the aim of cryptozoology as "the scientific description and naming of . . . 'hidden animals' as recognizable species or subspecies, which could eventually receive legal protection if threatened with extinction." Cryptozoology aims further at a systematized search for specimens of these "hidden" animal forms which, if successful, would make them pass from a cryptozoological to a zoological status.

Heuvelmans goes on to present, in the form of guidelines, a comprehensive but concise description of the cryptozoological method: Collect evidence

from all sources, analyze, compare, sift, make all elements complementary, and then produce a coherent, consistent synthesis of the evidence.

It is most unfortunate, in view of the thorough and comprehensive nature of this paper, and its undoubted utility, that Heuvelmans then proceeds to contradict both his definition and his stated aim of cryptozoology.

According to the cryptozoological method given by Heuvelmans, research can build up an "identikit" picture of the animal sought, including the anatomical traits which characterize its morphology, as well as giving information on its physiology, behavior, ecology, and geographical distribution. This, Heuvelmans states, allows the animal to be identified with some precision, so as to allow it to be classified with some accuracy within the categories of the natural system—all of this with no scientific, experimental evidence of any sort; nothing for scientific (laboratory) examination; no sample of the animal, or of its bones, or hair, or tissue, or whatever. According to Heuvelmans, this is where proper cryptozoological work ends. Yet, by his own, above definition, the animal still remains hidden, and thus within the domain of cryptozoology.

Heuvelmans then rightly states that, with the cryptozoologist's conclusions in hand, any competent layman will have a clear idea of where, how, when, with what, etc., to search for the animal in question. This, of course, if successful, would presumably lead to the collection of a specimen of the animal sought, which could then be studied, measured, analyzed, etc., by qualified experts and, finally, identified unambiguously and classified within the natural system. It is at this point, if we follow the above definition given by Heuvelmans, that the animal is no longer hidden, and thus passes from the realm of cryptozoology to that of zoology.

Heuvelmans then compares the process of cryptozoological research with trial proceedings in a court of law: The unknown animal is on trial to determine its existence, with the cryptozoologist acting as counsel for the defense. The defense must produce evidence—such as witnesses, documents, circumstantial evidence, and material proof if available—argue any objections raised, and cite precedents. Ideally, if the case is not dismissed for lack of sufficient evidence, the cryptozoologist should be able to assign reasons for all relevant phenomena, introduce suggestions as to the identity of his "client," and offer a plea for its reception by science into the community of known animals.

According to Heuvelmans, the case is to be built primarily on testimony and circumstantial evidence, with the introduction of material evidence only if available. The circumstantial evidence is to be reinforced by testimony from experts, and may include the introduction of elements from fields such as linguistics and mythology. Ultimately, however, all such elements, including testimony, may be regarded as circumstantial. Furthermore, indirect material evidence, such as tracks, droppings, hair samples, photos, etc.,

likewise falls into the realm of circumstantial evidence. The approach erected is thus totally circumstantial, and there is no insistence on incontrovertible experimental proof, i.e., a specimen of the animal.

Unfortunately, as Heuvelmans also points out, indirect material evidence can be faked. This is known to be a particularly acute problem in connection with Sasquatch research in North America. One sample purported to be Sasquatch hair, collected in the Blue Mountains of the Washington/Oregon border area, has been shown to be synthetic fiber (Lonnie Somer, 1989, Morphological Analysis of Possible Sasquatch Hair Recovered From the Blue Mountains of Washington State, Paper Presented at the Eighth Annual Membership Meeting of the International Society of Cryptozoology, Washington State University, Pullman, June 24, 1989).

Heuvelmans is doubtless correct when he states that even a single, whole specimen risks being dismissed as representing an individual variation or a freak. However, if the cryptozoological guidance which led to collection of the single specimen was sound, additional specimens could presumably be collected by continuing the effort.

Finally, the approach of drawing a parallel between cryptozoological research and the conduct of a legal case before a trial court appears flawed by an underlying error: It involves comparing an inexact science, law (if law may even be called a science), and an exact science, e.g., the natural sciences, such as physics, chemistry, biology, zoology, etc.

In law, one may admit as meaningful that which is truly unscientific, such as ocular testimony, observations and reports which cannot ultimately be verified, and the like—all circumstantial. In an exact science, a conclusion should only be accepted if it is based on sufficient experimental evidence. Furthermore, an essential characteristic of acceptable experimental evidence is reproducibility. If published results cannot be reproduced by other recognized, competent scientists, then the original finding is in serious question. In this regard, witness the current furor over high-temperature superconductivity, and, especially, the controversy over what has been termed "cold fusion." Plainly, if circumstantial evidence is faked, it can be reproduced only by the author of the fake, and not by other and more reputable scientists.

For the reasons set out above, I believe that there are two weaknesses in Heuvelmans' proposed method of cryptozoology:

1) He strays from rigorous scientific methodology when he proposes to base a scientific investigation on indirect and largely unverifiable circumstantial evidence, as in trial proceedings before a court of law.

2) He does not carry the cryptozoological method far enough. If we start from his own basic definition, then I would see it as extending through the collection of sufficient material evidence, probably in the form of more than one complete specimen, and, finally, the study and classification of these specimens by recognized zoologists. Only at this point—and not before—

can the animal in question be recognized as "known." It is thus no longer "unknown" or "hidden," and so passes from the field of cryptozoology to that of zoology.

In summary, if I were a judge ruling on the inference that a court trial approach can yield acceptable scientific proof, my finding would be: "Case dismissed."

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CRYPTOZOOLOGY, EPISTEMOLOGY, AND ETHICS

(Response to Raynal and Winn)

To clear up any misunderstandings, I first want to state that I have the greatest respect for scientists who investigate seriously such phenomena as extrasensory perception (ESP), unidentified flying objects (UFO's), cosmic influences, homeopathy, etc., which cannot be explained in accordance with the laws of nature, at least as they stand nowadays. I admire their curiosity and their courage, the two main assets of pioneering in science. And I think it takes far more courage to delve into such highly controversial matters than it does to deal with still-unknown animals, which, after all, are just ordinary flesh-and-blood creatures, even if sensation-seeking journalists and misinformed people call them "monsters" or find them "strange" or "bizarre." I would never think of automatically branding the research work of such scientists as "pseudoscience."

All the same, it is probably not superfluous to remind even ISC members that cryptozoology is not some cryptic or mystic doctrine—and thus a pseudoscience—but simply a branch of the natural sciences, based on an original, revolutionary method, and intended to precede—and complement—traditional zoology. Thus is Michel Raynal, incidentally, one of the most active cabinet cryptozoologists in the world, to be congratulated for his careful and detailed restatement of the question.

All I can do in response to it is to enrich his list of the most recent successes of cryptozoology with two more cases: one concerning a tiny sea animal, the other an unrecognized form of the largest of all land animals. The first one will serve to emphasize once more that cryptozoologists are not just after

huge, horrific "monsters," and the second one will demonstrate that the wildest hopes can still be cherished by them.

The midget, a coastal fish, is a species of yellow-colored blenny of the Mediterranean, whose existence has been resting for several decennia on hearsay only; it has finally been traced through cryptozoological channels, and collected off Yugoslavia by French ichthyologist Francois Charousset, who has thus kindly dedicated it to me: *Lipophrys heuvelmansi* (1985, Un Nouveau Poisson Trouvé en Méditerranée, *Clin d'Oeil,* November: 10–15).

The giant is, paradoxically enough, the pygmy elephant, described in 1906 by Noack as *Loxodonta pumilio,* but repeatedly debunked since. Quite recently it has even been dubbed "the sea serpent of zoology" (a rather clumsy and misleading expression) by French naturalist Pierre Pfeffer, in his monograph *Vie et Mort d'un Géant: l'Eléphant d'Afrique* (1989, Paris: 25, 34–39). After having studied the matter for 30 years, both with museum specimens and in the field, Pfeffer has concluded that pygmy elephants simply do not exist, and are nothing but immature young of the forest elephant (*Loxodonta africana cyclotis*) forming momentary herds.

In *On the Track of Unknown Animals* (1958, New York and London), I myself suggested that the pygmy elephant might represent, besides the larger savannah (bush) elephant and the smaller forest elephant, a third, dwarfed subspecies, adapted to a darker and more moist habitat. This now turns out to have been a rather conservative hypothesis, since Martin Eisentraut and Wolfgang Böhme, of the Museum Alexander Koenig, of Bonn, West Germany, have recently established on morphological, craniological grounds, and on new biological evidence (pygmy elephants, which shun forest elephants, have been sighted with their own young), *that the pygmy elephant is, in fact, a full species, quite distinct from the two forms of the main species.* Once again, although it took more than 80 years, the cryptozoological method has successfully been tested.

In diametrical opposition to Raynal, Edward B. Winn does not consider cryptozoology—at least not *my* cryptozoology—a true science. He accuses me, indeed, of straying "from rigorous scientific methodology" because I base cryptozoological investigation on what *he* calls "indirect and largely unverifiable circumstantial evidence." He reproaches me for disregarding "scientific experimental evidence" or "incontrovertible experimental proof"—that is, what in his own mind can be subjected to "scientific (laboratory) examination," and can even be reproduced to order, since "an essential characteristic of acceptable experimental evidence is reproducibility." He moreover deems it an error to draw "a parallel between cryptozoological research and the conduct of a legal case before a trial court," because one cannot compare "an inexact science, law (if law may even be called a science), and an exact science, e.g., the natural sciences, such as . . . zoology."

Well, the first thing I would urge Winn to do, to the suspension of all other business, is to read Henry H. Bauer's magnificent article: "What Do We Mean By Scientific?," already cited above by Raynal. I am afraid that by headlessly juggling with words like "scientific," "unscientific," "evidence," "circumstantial," and "exact," of which he does not seem to know the meaning, Winn only shows that his ignorance of epistemology, the philosophy of science, is as great as his ignorance of the subtleties of law.

To begin with the latter, Winn is obviously not very well versed in jurisprudence. He believes, for instance, that even testimony "may be regarded as circumstantial." Not being a lawyer myself, I will simply remind him that a true expert on the subject, John Henry Wigmore, pointed out in an authoritative work (1935, *Code of Rules of Evidence in Trials at Law,* 2nd ed., Boston), that there are only three possible kinds of evidence: *testimonial, circumstantial,* and *autoptical* (that can be seen by one's own eyes). Needless to say, the three are quite distinct and are not interchangeable.

Winn states, further, that "indirect material evidence, such as tracks, droppings, hair samples, photos, etc., likewise falls into the realm of circumstantial evidence." Now, even if these various items were to be considered "indirect material evidence"—which *all* are certainly not!—then an enormous, probably the greatest, part of all our scientific data, and more especially zoological data, is also based on such evidence.

One can see for oneself:

Tracks. As I stressed in my article, several fossils accepted by the most conservative paleontologists are nothing but hollow prints or casts, "negatives" in a sense, and ichnology, the science of tracks, is unanimously recognized as such.

Droppings. Since every living animal can generally be characterized by its specific, stereotyped constellation of parasites, notably the internal ones, feces, which transport these, often reveal the exact zoological identity of their producer, as surely as fingerprints identify a human individual.

Hair samples. If it is true that the analysis of hair seldom helps to identify its owner, even up to the level to its zoological order—actually only Microchiroptera's hair is readily recognizable—anatomical fragments such as teguments cannot reasonably be held as *less material* than teeth or bones.

Photos. Photographs, sound, sonar, and radar tracks, etc., are, just like footprint tracks: recorded prints of what emanates from material objects—not from ghosts, mind you—and, as such, are widely used in scientific research.

To make a long story short, what Winn rejects in an off-hand manner as "indirect and largely unverifiable circumstantial evidence" turns out to be, on the contrary, good autoptical (i.e., material) evidence.

I have a feeling Winn speaks of "circumstantial evidence" as if it were a synonym for "suspicion," the way the locution is used in popular literature

and conversations. He states, for instance: "In law, one may admit as meaningful that which is truly unscientific, such as ocular testimony, observations and reports which cannot ultimately be verified, and the like—all circumstantial." We disposed already of Winn's confusion between testimonial and circumstantial evidence, but he actually holds both in equal contempt. Is that sensible?

The issue of a trial, based essentially on such evidence, is certainly more important for a man accused of murder in the first degree than the result of any physical or chemical experiment for a scientist, because the former's life may be at stake. If the final sentence really depended on the "unscientific" evidence (meaning, of course, unsure, ambiguous, weak, scanty, and mystifying evidence) that Winn pooh-poohs so much, nobody could ever feel secure any more in our society. Fortunately, the comprehensive evidence used in law cases serves most of the time as the Rock of Ages, and it is not just because legal investigations are often founded on the experimental science of criminology, but actually, as Edwin M. Borchard clearly demonstrated in his book *Convicting the Innocent* (1932, Yale University Press, New Haven), because circumstantial evidence is by far the strongest and most reliable kind. So much for Winn's forensic rambling.

Let us turn now to the more strictly scientific side of his indictment. Here, the fact comes to light that Winn's formation as an electrical engineer, intended to work in the field of applied physics, has so radically biased his understanding of science that he goes so far as to include biology and zoology among the "exact sciences." A flabbergasting step indeed, but one which explains his being literally obsessed with "experimental evidence," "experimental proof," "laboratory examination," and "reproducibility." Now, everybody knows that experimental science is but *a very small part* of science in general, and more particularly of the descriptive sciences which are—I apologize for having to remind the reader of these primers—the sundry natural sciences: geology, mineralogy, botany, zoology, etc. In these sciences, one indeed resorts occasionally to experiments, but certainly not always.

To take an admittedly extreme example, ethology, the science of behavior, when applied to wild animals, rests almost entirely on the sole testimony of the observers, now and then supported by photos and, rarely, by film—thus, on testimonial evidence, supplemented with what Winn calls "indirect material evidence." In the much broader field of the life sciences in general, the situation does not appear much brighter for the die-hard experimentalist. For how could a biologist verify *through experiments* the mechanics of such a natural and common phenomenon as evolution? And how could a paleontologist reproduce *in a laboratory* the fauna of past ages, the birth, for instance, of an entirely new form of life?

Even in some so-called "exact sciences," namely in physics and astronomy—as a matter of fact, only mathematics is indisputably exact!—many

conditions or events cannot possibly be measured or reproduced artificially, to wit the state of matter (temperature and pressure) in the core of our own Earth, or of distant planets and stars. And who, except a god, could duplicate the famous "Big Bang," which has nevertheless become the revelation of our new cosmogony? To shift from the infinitely great to the infinitely small, may I remind the reader that, according to one of the principles of contemporary physics—Heisenberg's Principle of Uncertainty—it is actually impossible *a priori* to make an extremely fine measurement, since one cannot ascertain the relative position and speed of an elementary particle without altering both by the impact of the grain of light one must shoot at it to make it visible.

Winn accuses me not only of unorthodoxy in the scientific method; he also states that my cryptozoological practice contradicts both my definition of the discipline and its stated aim. I challenge him to specify in which way my proceeding is in contradiction to my very strict definition. But it certainly contradicts *his* definition of the aim of cryptozoology, which haunts him to such an extent that he ascribes it to me. I actually wrote: "Cryptozoology aims *accessorily* at a systematic search for specimens . . . ," which he distorted in paraphrasing me, consciously or not, by stating that it "aims further" at this particular search. That is not exactly the same thing.

It is perfectly true that, in proper cryptozoological research, "there is no insistence . . . on a specimen of the animal," which Winn naively considers "incontrovertible experimental proof." This is precisely what distinguishes cryptozoology from traditional zoology; it has to be stressed once more.

In my personal perspective, the ultimate aim of the new discipline is to be able to do *without* specimens; that is, to describe new animal forms as completely as possible without having to kill one of their representatives. There is nothing wrong with an animal staying a little longer—or even much longer—in its own cryptozoological community before vanishing. When we are familiar enough with the species, we will certainly have a better chance of collecting individuals that died from natural causes, and which will eventually yield the necessary anatomical data.

Killing and captivity should be avoided, not only for obvious ethical reasons, but because corpses and stressed individuals, psychologically disturbed by an abnormal environment, are not absolutely indispensable for our scientific enlightenment. This new discipline of zoology I have gradually built up during the past five decennia has indeed a revolutionary nature, as it actually endeavors to replace concrete (autoptical) evidence with a sufficient amount of both testimonial and circumstantial evidence (just as in the medical sciences, researchers of high morals are currently perfecting alternatives for the degrading practice of animal experimentation).

It should also be borne in mind that, from a strictly scientific viewpoint, the diligent and long-continued observation in the wild of chimpanzees by

Jane Goodall and gorillas by George Schaller, and later Dian Fossey, has taught us immensely more about these fascinating cousins of ours than the former slaughter and capture of hundreds, even thousands of individuals. And what is true for man-apes is naturally true also for sharks, toads, dinosaurs, moas, and whales—and why not? Ape-men, sub-men, or just men. All of them can be studied peacefully, without being harmed: this is the great lesson we owe to ethology, another relatively new discipline of science, whose impact on our own social life becomes increasingly perceptible.

By slowing down and hopefully putting an end to the wanton collecting of specimens, which is already threatening with extinction innumerable forms of life, cryptozoology could be instrumental in the adoption of new ethics in zoological research. Morals of this sort have now become imperative, owing to the dizzying destruction and disappearance of much of our planet's wildlife, a tragedy which could someday not only render zoologists—not to mention cryptozoologists—obsolete, but herald the twilight and doom of our own species.

Of course, I should not, in a scientific journal, set forth the mere respect for life, which is part of my private Buddhist philosophy. But owing to what I just had to stress, I cannot let the opportunity pass without emphasizing that, in my opinion, the ISC "Policy Statement,"[1] conceived somewhat after Pilate's fashion—we wash our hands of those innocents' blood—is contrary to the very spirit of cryptozoology as I originally conceived it.

As to Winn's aim, it is clear enough: he avowedly wishes to bag or shoot animals of which there is not, in his opinion, the slightest acceptable scientific proof of their existence. Is that a cryptozoologist's program? It sounds more like the strategic plan of a ghostbuster.

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[1] See *The ISC Newsletter,* Autumn, 1984, p. 9, for the text of the Policy Statement.—Editor.

COLARUSSO'S LINGUISTIC CRYPTOZOOLOGY: A MODEL

(Comment on John Colarusso, 1988, *Waitoreke,* the New Zealand "Otter": A Linguistic Solution to a Cryptozoological Problem, *Cryptozoology,* Vol. 7: 46–60)

Just as I previously stressed the exemplarity of Christine Janis' contributions to an archaeological approach to cryptozoology (Christine Janis, 1987, Fossil Ungulate Mammals Depicted on Archaeological Artifacts, *Cryptozoology,* Vol. 6: 8–23; Bernard Heuvelmans, 1988, The Sources and Method of Cryptozoological Research, *Cryptozoology,* Vol. 7: 1–21), I would now like to take John Colarusso's philological analysis as the perfect example of another approach to it.

As perfection is simply to be enjoyed and praised, and cannot thus be criticized, I will try to justify this otherwise superfluous intrusion into the *Comments and Responses* section by merely complementing Colarusso's work with some historical data on the Waitoreke, and more precisely, the bibliographical milestones which led to the clarification of its zoological identity. Just as in all other sciences, one should never, in cryptozoology, forget to render unto Caesar the things which are Caesar's. . . .

The German biologist Wilhelm Bölsche (1861–1939) was the very first, in 1896, to suggest that the Waitoreke could be a monotreme: "Möglich also, dass der *Waitoteke* [sic], den bisher leider noch kein Zoologe erbeutet hat, ein Schnabeltier oder gar ein noch älterer Rest aus der Welt der Ursäuger ist." ("Possibly also, the Waitoteke, which alas! no zoologist has yet captured, is actually a duck-billed animal [*Ornithorhynchus*] or even an older remnant of the primitive mammals' world.")

Half a century later, ISC Honorary Member Ingo Krumbiegel, after having briefly mentioned the Waitoreke in two previous publications (1949, Wunderinsel Neuseeland, *Arche Noah,* Vol. 1: 171; 1950, *Von Neuen und Unentdeckten Tierarten,* Kosmos, Stuttgart: 70–71), finally devoted a whole study to it (1952, Das "Waitoreki," ein angeblich neues Säugetier von Neuseeland, *Zeitshrift fur Säugetierkunde,* Vol. 18: 110–15). In this article, Krumbiegel reviewed some of the literature mentioning sightings of the unknown otter-like animal, and quoted Taylor (1855), Haast after Hochstetter (1863), Bölsche (1896), Lendenfeld (1900), Sievers and Kückenthal (1902), and Heck's edition of Brehm (1912). There were actually, at that time, at least fourteen more references available, which reported other significant testimonies of eyewitnesses.

Then, in my own 1955 book *Sur la Piste des Bêtes Ignorées* (Heuvelmans, Plon, Paris: Vol. 1: 330–38), published in English in 1958 as *On the Track of Unknown Animals* (London, Rupert-Hart Davis; New York, Hill and Wang: 246–50), I endeavored to ground the monotreme hypothesis on pa-

leontological and zoogeographical arguments; that is, on circumstantial evidence:

> There is not the least doubt that the marsupials spread and flourished before the placentals. But practically nothing is known of the monotremes' ancestors. The most one can say is that there was a great crop of reptilian mammals (or mammalian reptiles) in the Jurassic period, and that the monotremes were probably among them. During the Age of Reptiles they could have spread across the world from Asia to Australia and thence to New Zealand, which were then all connected, without being noticed much, although the reptile empire would still have been able to crush any rising of true mammals. If there *is* an indigenous mammal in New Zealand which has been there ever since the islands were cut off, there is a good chance that it is a monotreme. And as it is supposed to look like an otter—that is to say like a platypus—the most likely theory is that it is one of this creature's close relatives. The distribution of the three groups of mammals would then fit in perfectly with the geological changes of the past. The main expansion of the monotremes would have been in the Jurassic period. Then in the middle of the Cretaceous the rising tide of marsupials swept them off the earth except for a few rare species in Australia and New Guinea, and one, the *waitoreke,* which escaped when New Zealand was cut off. Finally the placentals, at the end of the Cretaceous and the beginning of the Eocene periods ousted all the marsupials but a few in South and Central America and the Southern States of the U.S.A., from everywhere except the now isolated countries of New Guinea and Australia.

Gunter G. Sehm (1988, the Waitoreki of New Zealand—Marsupial or Monotreme? *Tuatara,* Vol. 30: 62–65) has recently contributed to the fulfillment of this prophetic reconstruction of the geology and paleontology of past ages—without, however, mentioning its originator—by producing the last piece of the Waitoreke puzzle: "Recently (1985) Australia's first fossil Mesozoic mammal, a platypus-like monotreme, was described [Michael Archer, Timothy F. Flannery, Alex Ritchie, and Ralph E. Molnar, 1985, First Mesozoic Mammal from Australia--an Early Cretaceous Monotreme, *Nature,* Vol. 318: 363–66]. It was discovered in early Cretaceous sediments in New South Wales and was named *Steropodon galmani* (n. gen. and sp.). The fossil find represents an early ornithorhynchid-like (platypus-like) monotreme of rather large size. The fact that distinctive badger-sized monotremes were present in Australia in the early Cretaceous (when New Zealand was presumably still part of that land mass) indicates not only an antiquity of this group hitherto unheard of and certainly great enough to have settled New Zealand *without* having to brave an open sea, but also testifies to an evolutionary trend which obviously favored this group of Monotremata."

John Colarusso's brilliant linguistic demonstration finally succeeds in casting out the nines in this identification procedure, based essentially on testimonial and circumstantial evidence, which characterizes cryptozoological research.

BERNARD HEUVELMANS

MORE ON MONOTREMES

(Comment on John Colarusso, 1988, *Cryptozoology,* Vol. 7: 46–60)

Research on the cryptofauna of New Zealand is apparently enjoying a renaissance. Reference to *Cryptozoology* and *The ISC Newsletter* of recent years attests to this fact. Two papers published in 1988 (which, coincidentally, I received on the same day) have addressed the problem of the well-known Waitoreke, or New Zealand "otter." Each concluded, as did Bernard Heuvelmans (1986, Annotated Checklist of Apparently Unknown Animals with which Cryptozoology is Concerned, *Cryptozoology,* Vol. 5: 1–26), that the animal is, or was, a monotreme. The two new papers differ, however, in their approach. That by Colarusso (1988, above) employs linguistic evidence, while the second (Gunter G. Sehm, 1988, The Waitoreki of New Zealand—Marsupial or Monotreme?, *Tuatara,* Vol. 30: 62–65) relies on inference from known monotreme biology.

Colarusso's linguistic approach, though somewhat complex for the uninitiated, provides a possible solution to a problem which has tantilizingly few biological clues, but which, nonetheless, has become one of the most well-known cryptozoological cases in the Pacific region. A similar approach was used by John Becker (1985, Towards an Etymology of Maori *Waitoreke, Crypotzoology,* Vol. 4: 28–36), who concluded that the animal was mythical. I, on the other hand, find Colarusso's linguistic information compelling, and am led by his arguments to the logical conclusion that the Waitoreke is—or was—a member of the Monotremata. Certainly, his refutation of the reptilian nature of the beast is justified, and the correspondence between linguistic and anecdotal sources argues for some material basis for the Waitoreke. By the same token, however, certain biological implications of his work give me concern.

Colarusso relies heavily on the interpretation of the Maori word *waitoreke* as "water-diver (with a) quill or knob"; he suggests that the quill may imply a poisonous spur like that of the male platypus, and that the Maori may have feared the animal because of this feature. This spur, however, is relatively small in the known monotremes, and is certainly not the most striking feature of these mammals. Nor is it likely that the mildly venomous secretions of the associated crural gland would induce fear, as envenomation is rare and never fatal—this despite ample human contact with the platypus during the era of fur trapping in Australia.

A second potential difficulty involves Colarusso's interpretation of the pelage of the Waitoreke. He interprets the contrast in hair type among the living monotremes (soft hair in the platypus, and spiny projections in echidnas) as perhaps reflective of a dichotomy in the evolution from a therapsid "proto-hair" type. In fact, echidnas, although spiny, posseses small, soft,

typical mammalian hairs between their spines. The spines themselves must be regarded as secondary modifications in this lineage. Indeed, because monotremes are the sister group of other mammals, and not the ancestors, it may be assumed that the most recent common ancestors of all mammals primitively possesed rather typical hair. If, then, the Waitoreke is a monotreme, the "gristly hair" reported for it must be a secondary characteristic, or, as I believe more likely, a largely irrelevant feature, perhaps stemming from the matting of wetted hair. As a further, functional consideration, quill-like hair provides mechanical protection but little insulation, hardly beneficial for an aquatic mammal in the cold, temperate climate of South Island, New Zealand.

Sehm (1988, above) adds one bit of information to the solution of the problem, by noting that the recent discovery of Mesozoic monotreme fossils in Australia (Michael Archer, Timothy F. Flannery, Alex Ritchie, and Ralph E. Molnar, 1985, First Mesozoic Mammal from Australia—an Early Cretaceous Monotreme, *Nature,* Vol. 318: 363–66) allows for the possibility of an early mammalian invasion of New Zealand before the opening of the Tasman sea, approximately 80 million years B.P. Unfortunately, the rest of Sehm's paper is pure conjecture, and adds little to the more detailed analysis of Colarusso.

I am impressed by the novel utilization of linguistics in the attempt to solve the problem. In the work of both Becker (1985, above) and Colarusso (1988, above) I see illustrations of the interdisciplinary nature of cryptozoology at its best.

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THE WAITOREKE: LINGUISTICS VERSUS SCIENCE

(Comment on John Colarusso, 1988, *Cryptozoology,* Vol. 7: 46–60)

Becker's linguistic analysis of the Maori words *waitoreki/waitoreke* and *kaurehe/kaureke* for a nondescript, supposedly otter-like animal of New Zealand resulted in regarding the animal as a purely mythical part of Maori lore (John Becker, 1985, Towards an Etymology of Maori *Waitoreke, Cryptozoology,* Vol. 4: 28–36). Colarusso's linguistic analysis of the same words, on the other hand, leads him to conclude that New Zealand's South Island harbored or still harbors "a species of monotreme of amphibious habits" (p. 59), or a therapsid reptile; he wavers between a "basal monotreme" (whatever that may mean zoologically) and an "advanced therapsid" (a likewise unspecified coining).

The fact that we now have two different linguistic analyses giving two contrasting results only goes to show that the discipline called linguistics is—contrary to claims made by Schelsky and other founding fathers—*not* an exact science, but one among many methods of interpretation, and consequently belongs, like all language studies, with the arts. From this, it will have to be deduced, though perhaps reluctantly, that linguistics can in no way substitute for a scientific approach, nor serve as an auxiliary discipline for solving a scientific problem. In science, for instance, any laboratory test, and the result by the experimenter, may be repeated by others using the same materials and employing the same methods. In the present case, the same material (Maori words) were subjected to the same method (linguistic analysis); nevertheless, the authors arrived at diametrically diverging results.

Therefore, a scientific problem calls for a scientific approach. Whether or not the Waitoreke may possibly exist, or could have existed in recent times, will have to be tackled by a paleontological approach. Paleontology can well serve to show whether a therapsid reptile or an early mammal (monotreme) could possibly have survived in New Zealand. Once the possibility has been established by strictly scientific means, everyone given the same data may reiterate the logical line of thought, and arrive at the very same conclusion.

The necessary prerequisites are: (1) a record of fossil finds of therapsid reptiles and monotremes of Australia and New Zealand; (2) the time-scale of therapsidian and mammalian evolution and radiation; and (3) the time-scale of the segregation of New Zealand from the Australian land mass. Becker and Colarusso did not provide these data, so their theories lack the realistic framework into which they must fit. To give an example: should New Zealand have separated from Australia long after therapsid reptiles died out (with no ecological niche for them to survive over the ages), and long before mammals had radiated (with no logical possibility for them to have evolved before that "deadline"), then the possibility of Waitoreke reports and rumors being related to an unknown therapsid reptile or a nondescript monotreme is, logically, nil, and a linguistic analysis redundant. This is, in fact, the traditional position of those who claim that the Waitoreke is but a mythical animal of Maori lore, or the result of confusion with other known animals. Cryptozoology should seek a logical fault in the premises themselves, one that aims at establishing, in the first place, the likelihood of the existence of an unknown creature before going out into the field to find it. So much for criticism of the theoretical approach.

To make up for the missing framework of a reasonable geological-paleontological time-scale in the first place, it may be pointed out that, according to the *communis opinio* of geologists, New Zealand had separated from Australia by the end of the Cretaceous period; i.e., some 70 million years B.P. On the other hand, there is a nearly complete record of therapsid reptiles from the late Permian to the late Triassic, when the thecodonts, ancestors of dinosaurs, took their place, i.e., occupied the complete set of their various ecological niches, so that no later (younger) finds can be expected. Thus, therapsids had been extinct for some 120 million years when New Zealand separated from Australia, and the therapsid hypothesis should not have been advanced at all (incidentally, it is not in line with, and invalidates, the intricate puzzle-work of Colarusso's reasoning).

There are those who hold that, although therapsids died out completely, one of their early small-sized lineages evolved by various intermediate stages rather "directly" into present-day monotremes (echidnas and the platypus), at least independently from other mammals that also go back to therapsid ancestors (Robert T. Bakker, 1977, Dinosaur Physiology and the Origin of Mammals, *Evolution,* Vol. 25: 636–58; Adrian J. Desmond, 1975, *The Hot-Blooded Dinosaurs: A Revolution in Paleontology,* Blond and Briggs, London: 103). So what about monotremes? As no fossil finds of comparably modern monotremes have been dated at before the Tertiary, paleontologists have assumed that they evolved from their ancestral group in that Period; that is, *after* the segregation of New Zealand. Whether the Monotremata evolved in Australia or invaded the Australian land mass via adjacent Asian tectonic plates still unseparated from Australasia, they were, in any case, "too late" in geological time to reach New Zealand, whose plate had already drifted apart. As monotremes do not have any marine adaptations, and the "vegetation raft" idea is no longer considered convincing, monotremes did not have any possibility of reaching and settling New Zealand. Similarly, the even "later" immigration of Marsupialia from South America via the connecting bridge of Antarctica (which still clung to Australia on one side and Patagonia on the other) took place too late in geological time to involve New Zealand, which had segregated earlier. Placental mammals (except bats, marine mammals, and those introduced by man) never reached Australia, as they radiated still later. These points lend no probative force to any arguments favoring the existence of a monotreme in New Zealand, as there is no logical flaw in the logical deduction. Or so it seems.

However, the premise (no earlier fossil finds than Tertiary ones in Australia) leads to an assumption based solely on the *presently available* fossil facts. This assumption may be tackled scientifically; that is, logically—this is basically the creative method of lateral thinking that Heuvelmans has perfected. In this case, one may argue that a lineage as "old" as the monotremes should have evolved and radiated at a considerably earlier age than

marsupial and placental mammals that radiated in the Tertiary. For already in the Cretaceous the regression of the epicontinental seas caused by a considerable fall in sea level had made various new terrestrial (and fresh-water) niches—that were not occupied by saurians nor by evolving birds—available to animals of small size, insectivorous habits, or specializing in a diet of small-sized invertebrates. Consequently, it is but logical to hold that monotremes evolved and radiated in the Cretaceous, when New Zealand was still part of the Australian land mass. Convincing though this argument may be, it is considered unconvincing as long as it fails to produce a fossil monotreme of Australian origin and Cretaceous age. Alas, You Must Produce the Body, says the Habeas Corpus Act. This oxymoron—to put forth a logical, strictly scientific line of thought and still be asked for "bodily" proof—is what cryptozoology has to bravely endure instead of evading into the no-trespassing realms of nonscientific disciplines.

In the case of the Waitoreke, cryptozoologists may rejoice. For it should be added here as a postscript that a very recent find of a fossil ornithorhynchid (platypus-like) monotreme from early Cretaceous strata of New South Wales, Australia (Michael Archer, Timothy F. Flannery, Alex Ritchie, and Ralph E. Molnar, 1985, First Mesozoic Mammal from Australia—An Early Cretaceous Monotrome, *Nature,* Vol. 318: 363–66), has indeed changed the premise of the questioned assumption (Gunter G. Sehm, 1988, The *Waitoreki* of New Zealand—Marsupial or Monotreme? *Tuatara,* Vol. 30: 62–69). This new piece of evidence has now made the presence of a similar platypus-like monotreme in New Zealand not just possible, but highly likely.

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Platypuses, Proof, and Possibilities

(Response to Heuvelmans, Bauer, and Krumbiegel and Sehm)

Heuvelmans' detailing of the literature surrounding the Waitoreke is a welcome addition and corrective to the bibliography of my linguistic study. With characteristic intelligence, he appreciates the scientific force of linguistic argument. He is also astutely aware of the critical importance of independent lines of investigation leading to the same conclusion. Initially, I set out to show that the Waitoreke was either mythical or, at best, another name for the tuatara, in effect building on the work by Becker (1985, Towards an Etymology of Maori *Waitoreke, Cryptozoology,* Vol. 4: 28–36) by re-examing the terms for the supposed creature, and determining what the exact names were, since there seemed to be linguistic confusion on this matter.

My goals, therefore, were modest philological ones. Quite against my expectations, I was inexorably drawn in the course of my work to the conclusions that the Maori term *waitoreke* refers to an amphibious animal with a spur or quill, and that *kaureke* refers to an animal with many quills. These conclusions provided a striking confluence with other, independent lines of evidence (anecdotal, paleontological, and geological), and make the existence of the Waitoreke highly likely.

In their comments, both Heuvelmans and Bauer cite a recent paper on a Cretaceous "platypus-like monotreme" (Michael Archer, Timothy F. Flannery, Alex Ritchie, and Ralph E. Molnar, 1985, First Mesozoic Mammal from Australia—An Early Cretaceous Monotreme, *Nature,* Vol. 318: 363–66). I will only add here that the significance of this jaw has also been taken up by Z. Kielan-Jaworowska, A. W. Crompton, and F. A. Jenkins (1987, The Origin of Egg-Laying Mammals, *Nature,* Vol. 326: 371–73; for further discussion, see also Robert L. Carroll, 1988, *Vertebrate Paleontology and Evolution,* W. H. Freeman, New York: 420–21).

Bauer, as did Heuvelmans, appreciates the compelling character of independent lines of analysis pointing in the same direction. My paper owes a great deal to his insightful criticism as one of the original (although at the time anonymous) referees.

He expresses doubt that a Maori would make much of the poisonous spur on a platypus or platypus-like animal, particularly since, in the platypus, this spur does not seem to be effective at delivering venom. I can only refer to the statement by one Maori: "We are afraid of them" (my paper, p. 57, with reference therein). Bauer adds that such a spur would not be a very conspicuous feature, especially given the other oddities of the platypus. In a fauna dominated by lizards and birds, however, a mammal with a beak might not seem very odd, so that the spur might seem one of its most striking features.

Because of the concurrent designation, /kau-reke/, 'very-spiny' (or the like, see p. 55), I lean strongly towards the conclusion that the animal had a spiny or very coarse pelage, and that the spur interpretation was, at best, not at odds with the dominant sense of the name *waitoreke.* This leads to Bauer's second comment.

Bauer doubts that the "gristly hair" of the Waitoreke may be primitive, as monotremes, marsupials, and placentals all exhibit similar types of hair, implying that the most recent common ancestor did as well. He is particularly bothered by an amphibious mammal with quills, and dismisses the /-reke/ part of both names as merely referring to matted wet hair. I do not believe that such a transient feature as matted wet hair would form a crucial part of both native names for an animal. I think that, if the animal exists or existed, it had spiny, coarse hair, and that this is a genuine biological problem given what we know of amphibious mammals. The whole matter vanishes if we reject the spiny interpretation and stick with the spur, but then the name *kaureke* remains enigmatic. So, accepting the problem, I saw "proto-hair" as one solution, attributing a pelage of vibrissae or even proto-trichia to a primitive monotreme, seeing in the spines of the echidna some vestige of this.

Even assuming that, since all mammals have hair, their common ancestor must then have had hair, and thus ignoring the possibility of parallel evolution (possible for a structure as simple as hair), the transition from a scaly integument with sensory proto-trichia to one of hair must have been made at least once in the therapsid–mammal lineage; and such a transition may well have entailed "gristly hair," perhaps merely the Maori way of describing a pelage made up largely of vibrissae. In an aquatic mode, vibrissae, as opposed to quills, might offer far less resistance to water and thus enable an animal to be a competent swimmer. This would imply that the Waitoreke was more primitive than a monotreme, a possibility noted in the first paper on the Waitoreke by Wilhelm Bölsche: "gar ein noch älterer Rest aus der Welt der Ursäuger ist" (cited in Heuvelmans' Comment). But this takes me into matters that are better dealt with in my response to Krumbiegel and Sehm.

It is unfortunate when a Comment bears two names, one of which (Krumbiegel) has my unqualified respect, for it is difficult to determine exactly whose voice one is refuting. Thus, I must refer to Krumbiegel and Sehm as "they." I shall start with their attack on the discipline of linguistics.

I never realized that, because two workers in a field disagree, as Becker and I do, that the field was therefore "non-scientific." The implication here is that paleontology—but not linguistics—is a science, and, therefore, that paleontologists do not disagree. A most peculiar proposition. The on-going debate regarding dinosaur physiology, involving for the last twenty years many of the leading figures of vertebrate paleontology, such as Robert T.

Bakker, Adrian Desmond (both of whom they cite), and others, seems to have been missed by these writers; otherwise, they would be forced to conclude that paleontology, like linguistics, alas, is itself merely a frivolous art. There is a sad historical irony in their comments, for it was Charles Darwin himself who cited comparative linguistics as a scientific paradigm for his fledgling paleontology.

Modern linguistics is a highly abstract and mathematical subject, currently in a state of rapid flux and evolution. The techniques used in my paper, however, were most elementary, harking back to the early years of the science of comparative linguistics in the late 18th and early 19th centuries. Comparative linguistics is nevertheless a science in that it postulates laws, makes predictions, and is subject to refutation. It also exhibits disagreement, and hence progress. It is not deductive, as they claim a science must be, for no science is, unless it has been subjected to an axiomatization. Such axiomatization has been occasionally attempted, for example, in that branch of physics called thermodynamics, or, as I myself have had occasion to do in the course of some of my research, in structural linguistics. Science is coherent, but it is inductive, with deduction playing a secondary, though crucial, role (for some basic reading on these issues see, for example, Imre Lakatos, 1978, *The Methodology of Scientific Research Programmes. Philosophical Papers,* Vol. 1, Cambridge University Press, Cambridge; Hilary Putnam, 1975, *Mathematics, Matter and Method. Philosophical Papers,* Vol. 1, Cambridge University Press, Cambridge; Frederick Suppe, [ed.], 1977, *The Structure of Scientific Theories,* University of Illinois Press, Urbana; Stephen Toulmin, 1972, *Human Understanding, the Collective Use and Evolution of Concepts,* Princeton University Press, Princeton).

Contrary to what Krumbiegel and Sehm assert, it is *not* an oxymoron [a self-contradicting term] "to put forth a logical, strictly scientific line of thought and still be asked for 'bodily' proof," for it is only by demanding such factual proof that science is distinguished from mathematics on the one hand or magic on the other. Such factual, bodily proof is at the heart of refutability; and refutability is the very heart of science. All these objections to linguistics are strange coming from Sehm, who apparently has had some passing acquaintance with the subject in some form.

My reasoning is accused of being intricate puzzle-work. However, my reasoning is straightforward and coherent: establish the sound laws relating the Maori dialects, establish the correct form of the name or names involved in the appropriate dialect, interpret the meaning of such names following Maori grammar and naming practices as exemplified in narrative and myth, and discuss the possible biological significance of such names. My reasoning is tedious and detailed. Most linguistics studies are not "one-sitting" readings, but one need hardly apologize for this when it is the nature of the subject at hand.

Krumbiegel and Sehm claim that paleontologists have assumed that the monotremes evolved after New Zealand separated from other land masses; that is, in the late Cretaceous, roughly 80 million years B.P. This assertion is simply incorrect. (Robert J. G. Savage and Michael R. Long, 1986, *Mammal Evolution: An Illustrated Guide,* Facts on File, New York: 180). Many paleontologists, aware of the extreme archaism of the monotremes, have assumed a very remote origin for them, connecting them with the docodonts (Middle and Upper Jurassic) and multituberculates (Upper Jurassic) (P. Murray, 1984, Furry Egg-Layers: The Monotreme Radiation. *In:* M. Archer, and G. Clayton, [eds.], 1984, *Vertebrate Zoogeography and Evolution in Australasia [Animals in Space and Time],* Hesperian Press, Carlisle, West Australia: 571–83; Carroll, 1988, above: 401–24; Alfred Sherwood Romer, 1966, *Vertebrate Paleontology,* 3rd ed., University of Chicago Press, Chicago: 197). Thus, Sehm's claim to have "predicted" a Cretaceous monotreme is otiose.

Virtually the only thing mammalian about the post-cranial skeleton of monotremes is the well-defined division between thoracic and lumbar regions, indicating the presence of a well-functioning diaphragm, and the presence of epi-pubic bones (also found in marsupials). Other features are strikingly reptilian, resembling those in early therapsids: reptilian limb girdles (the shoulder, for example, retains an interclavicle, a procoracoid, and a scapula of primitive therapsidian aspect) (Romer, 1966, above: 177, fig. 269c, 270d; Carroll, 1988, above: 421), sprawling limb posture (perhaps in some part secondarily due to specialization), and cervical ribs that remain unattached to the vertebrae (Carroll, 1988, above: 421). Since the post-cranial anatomy of monotremes is extremely archaic, it has been suggested that monotremes diverged from other mammals as early as the Late Triassic or Early Jurassic, even though cranial details, especially of the brain case, suggest some affinities with the multituberculates, which themselves originated in the Upper Jurassic. Thus, one could predict at least Jurassic and perhaps even Triassic monotreme fossils!

Krumbiegel and Sehm ridicule my term "basal monotreme." Monotremes themselves are extraordinarily primitive and extraordinarily specialized. Clearly, the original monotreme stock contained forms that had not yet undergone the specilizations seen in these two relicts. Such an unspecialized monotreme, by the norms of paleontological terminology, would be called a "stem" or "basal monotreme." Murray (1984, above: 576–77) gives a clear idea of what such a basal monotreme would have been like, even providing a reconstruction.

They seem to take strong exception to my suggestion that the Waitoreke might be an advanced therapsid, accusing me of wavering between therapsid and monotreme, and again claiming that "advanced therapsid" is an "unspecified coining." "Advanced therapsid" refers to therocephalians and cyn-

odonts, both first found in the Upper Permian (Carroll, 1988, above: 377–86). I should have used "advanced cynodonts," but feared that this might be meaningless to most readers.

I am not out of line in my reasoning to propose such a possibility, contrary to what they suggest. Since I know the difference between deductive systems and science, and have a respect for the difference between what can be proven and what can only be made plausible, I am compelled to state that the linguistic evidence for the Waitoreke points predominantly to a monotreme, but may point to something even more primitive, as Wilhelm Bölsche stressed at the very outset. Few distinctions in zoology are as arbitrary as that between mammal and mammal-like reptile. The line is arbitrarily drawn between the two groups at the jaw joint, mammals have a squamosal/dentary joint, and therapsids (and other reptiles) a quadrate/articular. Advanced cynodonts are forms such as the *Tritylodontia* and *Trithelodontia* (*Ictidosuaria*) (Upper Triassic to Middle Jurassic) (Carroll, 1988, above: 388–92), wherein a complex of mammalian traits can be observed coupled with a reptilian quadrate/articular jaw joint. To confuse matters further, in one advanced cynodont genus, *Probainognathus,* a double joint of both squamosal/dentary and quadrate/articular is found (Carroll, 1988, above: 390, fig. 17–43). Thus, there were advanced cynodonts (tritylodonts), such as *Oligokyphus* (Lower Jurassic) (Carroll, 1988, above: 391), and *Stereognathus* (Middle Jurassic) (Carroll, 1988, above: 388), that are considerably younger than the earliest true mammals of the Upper Triassic, that undoubtedly showed a host of mammalian traits, and yet were technically still reptiles. (I correct here the misconstrued framework of Krumbiegel and Sehm for therapsid and mammalian evolution, for the omission of which from our linguistics studies they criticize both me and Becker.)

The absence of intervening fossils between these last therapsids and a possible therapsid Waitoreke is in no way compelling. After all, we are talking here of New Zealand, the home of *Sphenodon punctatus,* the tuatara, the sole surviving member of the *Sphenodontidae* (*Rhynchcephalia*), whose relatives flourished in the Jurassic and trailed off in the Lower Cretaceous, a gap in the fossil record only slightly smaller than that required for a therapsid Waitoreke.

In fact, Savage and Long (1986, above: 39–40) have suggested that the tritylodont *Oligokyphus* occupied an amphibious niche, much like a water vole. Here would be another candidate for the Waitoreke, one that in many regards is more advanced than a monotreme, and yet one that might even offer a gristly pelage. Discovering another monotreme would be invaluable. Discovering a relict therapsid would be one of the major events in the history of zoology.

Leaving Krumbiegel and Sehm behind, I would like to present one final line of argument, an ecological one that is modestly deductive. New Zealand had a unique fauna dominated by lizards and birds, with only three known indigenous mammals—all bats, which, or course, are highly specialized. If there had been another mammal of any kind, and it had gained access to New Zealand at an early period, then it should have radiated and displaced most of the avian and lacertilian fauna, as is now happening, unless one of two conditions held: (1) the mammal was so highly specialized that evolution into niches occupied by birds and lizards was impossible, or (2) the mammal was so primitive in metabolic, locomotor, sensory, or reproductive capacities that it could not effectively compete with the birds in particular, and perhaps even with the lizards. Condition (1) is met by the monotremes. Condition (2) would more likely be met by an advanced cynodont. Now, if New Zealand did separate from Australia, Antarctica, and South America around 80 million years B.P., as is generally held, then there should be mammals there, mammals in profusion. The only conclusion is that the southern land mass in the Upper Cretaceous held a mammalian fauna (or sub-fauna dominated, of course, by the dinosaurian fauna) that consisted of monotremes and forms at a comparable or lower level of organization.

This conclusion is consistent with the restricted southern domain of the monotremes (Murray, 1984, above: 582), and the northern origins of the marsupials (Carroll, 1988, above: 431–32) and placentals (Carroll, 1988, above: 444). It is also consistent with the Waitoreke being *either* a monotreme *or* an advanced cynodont. Thus, whether or not there proves to be a Waitoreke, New Zealand tells us an enormous amount about the mammalian fauna throughout the entire world at the end of the Mesozoic.

Finally, I would like to propose that fossils of egg-laying mammals, monotreme or otherwise, might be found in various areas of the southern continents. It is even conceivable that, with great fortune, a living relict with a highly specialized form, such as a platypus- or echidna-like animal, might be found in the southernmost latitudes of those continents, particularly in South America.

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The Ri/Ilkai and the Irrawaddy Dolphin: More Evidence

(Comment on Thomas R. Williams, 1987, *Is* the Ri the Irrawaddy Dolphin?, *Cryptozoology,* Vol. 6: 149–51)

It might interest readers that more evidence has recently come to light that corroborates my contention that the Ri/Ilkai, a supposedly mermaid-like creature of eastern Papua New Guinea coastal waters (New Ireland), is indeed the Irrawaddy dolphin (*Orcaella brevirostris*).

Aerial survey techniques used to survey nearly 56,000 km² of coastal waters adjacent to Australia's Northern Territory revealed some populations of this dolphin species off the north-west coast (Van Diemen Gulf and adjacent areas), and substantial populations in the western Gulf of Carpentaria, which are estimated at totalling approximately 1,000 individuals—mainly concentrated in Blue Mud Bay (W. J. Freeland and P. Bayliss, 1989, The Irrawaddy River Dolphin [*Orcaella brevirostris*] in Coastal Waters of the Northern Territory, Australia: Distribution, Abundance and Seasonal Changes, *Mammalia,* Vol. 53: 49–57). The population in Blue Mud Bay is by far the largest of this species yet identified, with estimates of only 100–150 individuals given for Semayang Lake, Pela River, and Mahakam River in Kalimantan (Marsh *et al.,* 1987, Irrawaddy Dolphin, *Orcaella brevirostris* [Gray 1866]. *In:* S. H. Ridgway and R. Harrison [eds.], *Handbook of Marine Mammals,* Vol. 4: "Delphinidae," Academic Press, New York).

This population was located near a major breeding area of tiger prawns (*Penaeus esculentus* and *P. semisulcatus*) and endeavour prawns (*Metapenaeus endeavouri* and *M. ensis*). It would seem that Irrawaddy dolphins, although known to eat prawns occasionally (Marsh *et al.,* 1987, above), are attracted by the abundance of fish that feed on prawns. In the Van Diemen Gulf survey zone, 6 locations are associated with tiger prawn fisheries, 2 others with areas of banana prawns (*P. merguiensis*). From this, it becomes clear that (a) this dolphin species must no longer be called a "river" dolphin, and (b) its distribution center constitutes marine coastal waters in the relative vicinity of the area whence Ri/Ilkai reports originated. This cetacean is well known from Irian and Papua New Guinea seas; it has successfully bred in the Jaya Ancol Oceanarium of Djakarta, Indonesia.

It is the more deplorable that this dolphin had previously been omitted as a candidate for the Ri/Ilkai. Whereas it may be further discussed to what extent Ri reports from northern New Ireland may originate from confusion with the *rui* = dugong, it would now seem the more safe to conclude that Ilkai reports from central and southern New Ireland are indeed based solely on the Irrawaddy dolphin.

Gunter G. Sehm

The Ri/Ilkai: Dugong Conclusions Based on Observational Evidence

(Response to Sehm)

It is indeed heartening to hear that a not particularly common species of dolphin appears to be thriving in the coastal waters off Australia's Northern Territory. Still, I fail to see what this has to do with observations and photographs made of a quite different animal in a small bay in New Ireland, which is roughly 1,300 air miles from Blue Mud Bay. If this constitutes the "relative vicinity" of Ri/Ilkai sightings, then I'm Einstein. I might also point out that there are substantial populations of dugongs (*Dugong dugon*) in the northern coastal waters of Australia (as well as in the waters of New Ireland, as our observations attest). Does the presence of the saltwater crocodile (*Crocodylus porosus*) in these same waters also make *it* a candidate for the Ri/Ilkai?

If the Irrawaddy dolphin (*Orcaella brevirostris*) is attracted to prawns and/or the fish that eat prawns, then it would have sparse hunting in Nokon Bay, where the Ilkai has been reported. I have personally examined the bottom of Nokon Bay using SCUBA gear, and can attest that there are no prawn breeding grounds there. Indeed, there are very few fish in the bay, compared to the coral reef habitats which lie just outside it. It must be remembered that Nokon Bay is really just a small indentation in the larger Elizabeth Bay[1]. Nokon Bay has only a few minor coral heads, and the rest of the bottom is sandy with only moderate vegetation. That vegetation does attract dugongs, because several members of our expedition observed and photographed dugongs in the act of grazing along the bottom. These marine mammals were referred to as *ilkai* by the villagers of Nokon.

I am puzzled by Sehm's use of the word "evidence." He cites facts and observations, but fails to link them to the problem at hand in Nokon Bay, where Irrawaddy dolphins were *not* observed. Other species of dolphins—definitely with dorsal fins—were observed off the New Ireland coast, but we were not able to make a species identification. Sehm cites no observations or other independent facts to support his claim. I must also emphasize that the linguistic evidence was merely offered in support of our observations, and is *not* the basis of our conclusions.

I am still prepared to entertain the thought that our observations were in error, and that some other creature may be at the bottom of the Ri/Ilkai stories—even some as-yet-unknown animal. But that will require evidence based on observation. Until then, Occam's razor rules: the simpler explanation is more likely to be the correct one until refuted by new evidence. In

[1] Nokon Bay is located on the eastern shore of southern New Ireland, Papua New Guinea—just below Nokon Wood.—Editor.

the last analysis, what Sehm has presented is speculation—a method of some value in formulating scientific hypotheses, but *not* appropriate for drawing scientific conclusions.

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KLINGONS, ROMULANS, VULCANS, AND SNOWPEOPLE

(Comment on Valentin B. Sapunov, 1988, A Mathematical Analysis of "Snowman" [Wildman] Eyewitness Reports, *Cryptozoology,* Vol. 7: 61–65)

Periodic sightings of Klingons, Romulans, and Vulcans on the T.V. series *Star Trek* appear to be in accord with modern concepts of ecology, population biology, and primatology; however, as we all know, these so-called "humanoids" are merely fanciful creations. Although these visualized forms exhibit sexual dimorphism, pigmentation and hair coloration patterns, population structure, and microevolutionary change through supposed hybridization, they do not really exist as real biological entities. Neither, I suspect, does the Snowman (Wildman), whose supposed sightings are treated statistically in Sapunov's paper.

Table 1 of his report contains some obvious minor errors. The standard deviation symbol is used, although the text (p. 63) states that variances are reported in Table 1. The values given next to the standard deviation symbol (σ) are obviously incorrect given the other standard deviations for stature of 30, 24, and 15 cm presented in the Results section. Finally, the units are missing for the σ row entries in Table 1. These are, admittedly, the small points so cherished by the nitpicking reviewer. They are, however, indicative of the lack of scientific rigor exhibited elsewhere in Sapunov's paper. For instance, the conclusion that sexual dimorphism is less likely than racial dimorphism to explain the bimodal distribution of sightings (p. 63) does not necessarily follow from the observations of "a very tall female Snowman." In many human populations, females exist who are taller than all but a few males from the same deme. The great apes generally exhibit much more sexual dimorphism in weight than in height, so that occasionally female apes

are found well above the male mean stature values in a variety of pongid species.

Another issue which requires comment is the outdated and overly simplistic model for mammalian hair color presented by Sapunov, based on a 1958 publication by Montagna and Ellis. There are, in fact, two major pigments involved in mammalian hair color, but they are not melanin and eumelanin, as claimed by Sapunov. The generally accepted classification of melanin includes two major subtypes: eumelanin (black/brown) and phaeomelanin (red/yellow). A variety of other minor oxidation products exist as well, and are given their own categorical descriptions. The assertion that the sketchy model presented allows one to conclude "that the descriptions given in eyewitness reports appear to be authentic from the perspectives of population biology" lacks any kind of rigorous demonstration. Modern genetics has greatly complicated older genetic explanations for mammalian coat color. For instance, in a typical mammal such as the mouse there are more than 60 genes affecting coat color (including both structural and regulatory loci). The model on pp. 64–65 is simply inadequate for drawing any conclusions about eyewitness report authenticity.

The tendencies toward reification and *non-sequitur* also manifest themselves in the equally dubious claim that the absence of significant differences in the two data sets in Table 1 "suggests that the Snowman reports from both groups are based on objective reality." Sightings (of whatever kind) are first and foremost acts of perception, and all perception contains a subjective element. Likewise, all physical measurements contain elements of uncertainty and subjectivity due to the interaction of the measuring equipment with the object to be measured, as well as due to the interaction between the observer and the data. Sightings of supposed Snowpeople are not controlled experiments, and how much of the resulting accounts is objective and how much is subjective requires careful consideration of data from a variety of controlled situations (where eyewitness accounts are often notoriously unreliable) to provide a necessary baseline. Objective reality can be a very elusive and deceptive state for many normal as well as abnormal situations and states of mind.

The nature of natural science and the scientific method presents a conundrum for the resolution of the underlying problem of whether Snowpeople actually do exist. Negative evidence cannot lead to certainty. We live in a world subject to probabilities, uncertainty, and deterministic randomness ("chaos"). A very improbable event is not the same as an axiomatic impossibility (an event with a probability equal to zero). Although Snowpeople remain evolutionarily improbable, they are not physically impossible. Unfortunately, unless we find indisputable evidence (as we did with the coelacanth, *Latimeria*), science cannot resolve the issue of their existence to the satisfaction of a Popperian.

Lack of positive evidence does not prove their non-existence (although one may choose to believe in their non-existence without absolute certainty). Thus, *contra* to Sapunov's final conclusion, it remains unlikely that "the definitive answer to the question of the existence of the Snowman may be determined by future research."

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Snowman Based on Objective Reality

(Response to Zegura)

Zegura mentions incorrect standard deviations in Table 1 of my article. Due to a technical misprint, the values were listed as 0.24 and 0.37 instead of 24 cm and 37 cm.[1]

Detailed analysis of the data suggests that the distribution of scores has four modes attributable to two sexes and two races. However, the difference between the peaks attributable to sexual dimorphism was not significant. The existence of two races of Snowmen (Wildmen) has been noted in the literature. Analysis of the distribution appears to show an interaction between two factors: height and eyewitness variability.

Zegura is correct in stating that the main pigments determining hair color are eumelanin and phaeomelanin. The number of genes controlling hair color is indeed greater than 60. The greater the number of loci affecting a trait, the less effect any one of them has on the trait. I demonstrated this by a computer simulation of the effects of 2, 3, and 4 loci. One of the models is cited in the article. Every model is approximate because of the missing effects of natural selection. The main result is that the color pattern of Snowmen is consistent with what would be expected in mammals.

The criminalistic theory of testimonies suggests that the observers' level of education affects their testimonies only when the testimonies are fanciful creations. I did not find significant differences between groups of witnesses

[1] In the process of converting meters to centimeters in Table 1, the Editor inadvertently failed to convert the sigma units, so the expressed values became 100 times too small.—Editor.

with different levels of education. For that reason, I suggested that Snowmen reports are based on objective reality.

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Disinherit the Wind

(Comment on Paul H. LeBlond, 1988, The Tale of an Otter?, *Cryptozoology*, Vol. 7: 117–18)

I would like to comment on Paul LeBlond's response to my criticism of his wave analysis related to the 1934 Wilson photograph from Loch Ness.

LeBlond and Collins assumed that the photograph was taken at 0700 (UT?) on April 19, 1934 (BST began on April 22), but, as I pointed out previously (Steuart Campbell, 1984, The Surgeon's Monster Hoax, *The British Journal of Photography,* April 20), the original newspaper account reported Wilson as saying that the time was "about midday." This contemporary account may be more accurate than Wilson's recollection 21 years later. The weather data Leblond and Collins quoted were for 0700 (UT), and Fig. 1 shows the synoptic situation at that time. Loch Ness lies in an area where the wind

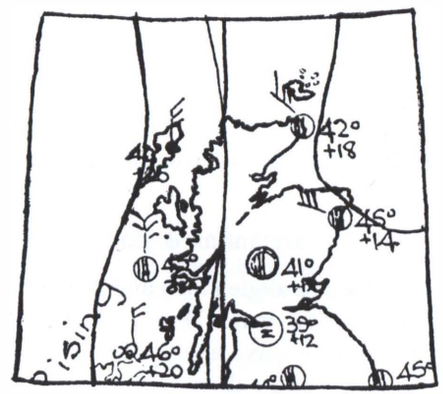

Fig. 1.—Part of the British Air Ministry synoptic chart for 0700 (UT) on April 19, 1934 (on which the axis of Loch Ness has been marked).

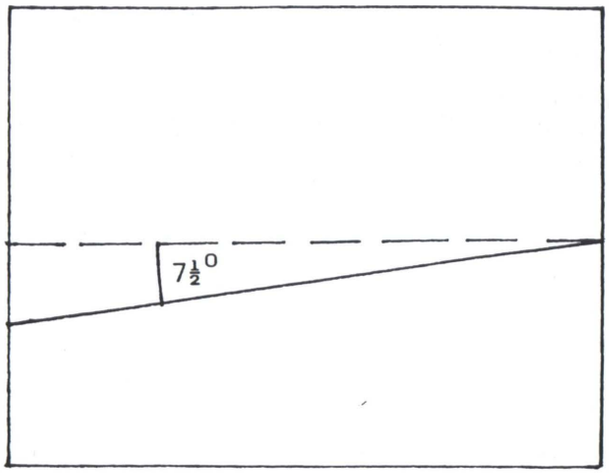

FIG. 2.—The apparent wave angle to the horizontal in the Wilson photo.

might be between N and WNW. By 1300 (UT) the wind at Dalwhinnie (lying in a valley which runs parallel to Loch Ness) had picked up and was blowing at force 2 from the N. At Aberdeen, it had increased to force 4, and was NW'N. At either 0700 or 1200, therefore, the wind might have been between N and NW. The hills that flank Lock Ness are unlikely to have steered such a wind parallel to the lake, especially if it were more NW than N. The wind at Dalwhinnie was not so steered. However, I will accept that the wind at Loch Ness might have been blowing from due N across the lake.

LeBlond states that the direction of wave propagation may not be reliably assessed from the photograph (because the exact direction of the camera is not known). However, the direction of wave propagation *relative to the direction of the camera* may be assessed and used to test LeBlond's claim that the wind was parallel to Loch Ness.

The picture clearly shows parallel waves which I assess as making an angle with the horizon of 7½° (Fig. 2). The true plane angle that this represents is given by:

$$\arctan(\tan \phi / \sin \theta)$$

where ϕ is the apparent wave angle (7½°) and θ is the angle of inclination of the camera to the plane of the water. Previous analysis has shown that $\theta = 18.8°$. Consequently, the true wave angle is about 22°, or the wind direction is at an angle of about 22° to the direction of the camera.

If we assume (with LeBlond) that the wind was parallel to the axis of the lake, then the camera must have been pointing in the direction shown as A

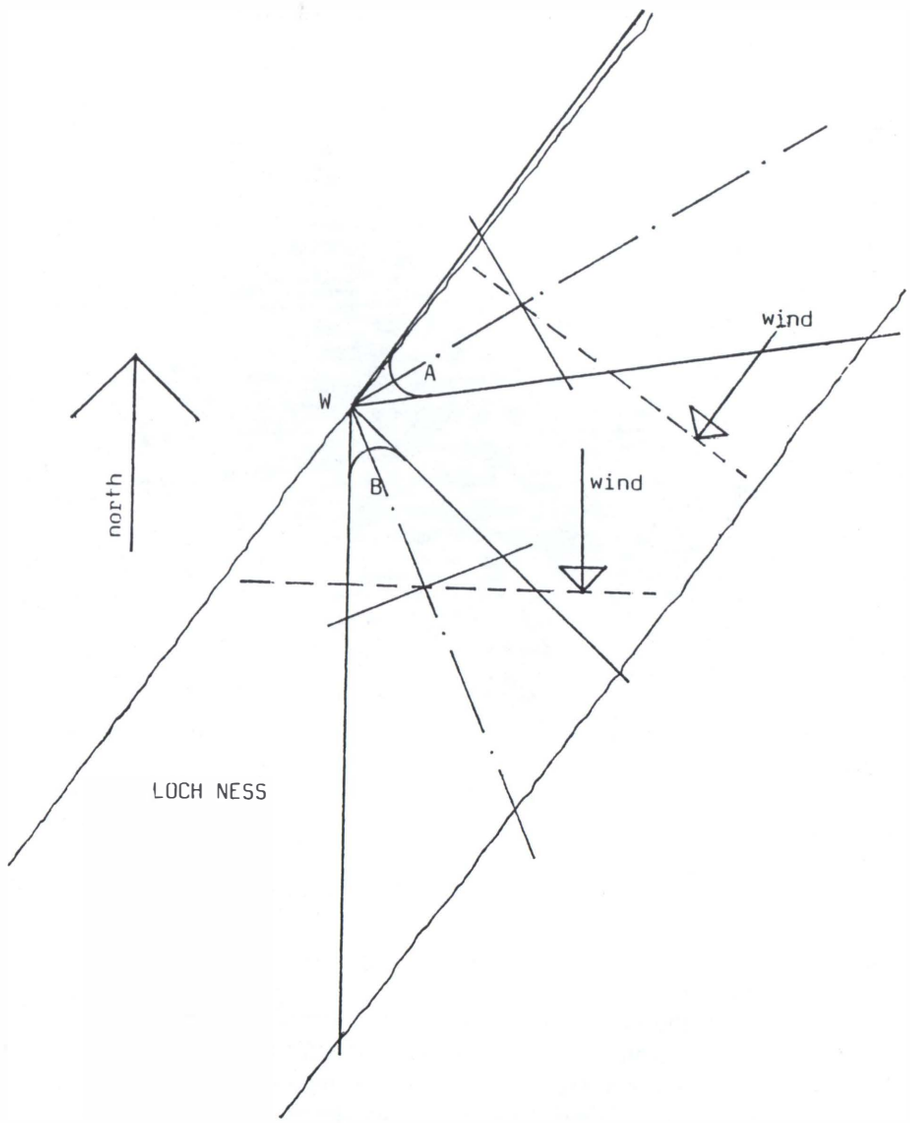

FIG. 3.—The direction of the Wilson photograph in relation to the wind direction (as revealed by the wave angle).

in Fig. 3. Clearly, this is impossible; although the picture does not necessarily show a view directly across Loch Ness, it certainly does not show a view along it! LeBlond's assumption must be incorrect; the wind cannot have been blowing along the length of Loch Ness.

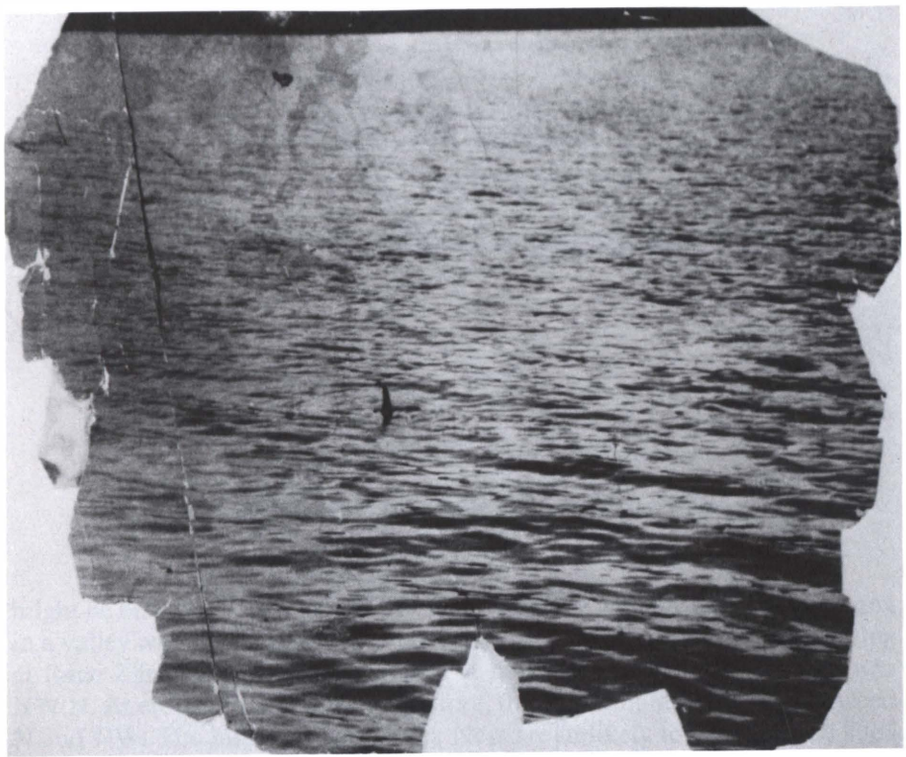

FIG. 4.—The 1934 Wilson photograph reversed.

If the angle of view is swung clockwise (to show the opposite shore, as in the photo) then the wind must turn with it, becoming either easterly or westerly. In either case, the fetch is severely reduced. However, I have accepted that the wind might have been northerly. A north wind can be reconciled with the photo, but only if it is *reversed* (see Fig. 4 and view B in Fig. 3), but even then the fetch is much shorter than 20 km. The photo may well have been reversed accidentally during one or more copying processes. Because the original plate is lost, we do not know the original orientation. The photo appears to have been taken against the sun and this would be consistent with view B at 1200. At 0700 (UT), the sun was only 15° above the horizon in the east (azimuth 93°); at 1200, it was at an altitude of 44° to the south (azimuth 174°).

I submit that a fetch of 20 km is irreconcilable with the photo, and that LeBlond is obliged to modify his calculation of the wavelength, and, consequently, the size of the object in the Wilson photograph.

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WHENCE BLEW THE WIND?

(Response to Campbell)

So, in which direction was the wind really blowing on that morning (or afternoon) of April 19, 1934? That direction is indeed crucial to an assessment of the length of the wind waves in the Wilson photo, and, by comparison, of the dimensions of the supposed Nessie appearing therein.

The analysis which I originally presented with Michael Collins (Paul H. LeBlond and Michael J. Collins, 1987, The Wilson Nessie Photo: A Size Determination Based on Physical Principles, *Cryptozoology,* Vol. 6: 55–64) was based in great part on the geometrical setting depicted by Campbell in his April 20, 1984, *British Journal of Photography* article, with his suggested date of April 19th, 1934, supporting the choice of a weather map (shown as Fig. 1 in his above Comment), and with a direction of view consistent with an early morning picture, looking eastward, with the wind blowing roughly from the left. We were careful not to infer the wind direction from that of wave crests in the foreground. Although for sufficiently long fetches wind waves run nearly with the wind, there is still some directional spread in wave propagation: it looks like waves are coming at the viewer from a range of angles on either side of the wind direction (cf. M. L. Heron, 1987, Directional Spreading of Short Wavelength Fetch-Limited Wind Waves, *Journal of Physical Oceanography,* Vol. 17: 281–85; M. A. Donelan, J. Hamilton, and W. H. Hui, 1985, Directional Spectra of Wind Generated Waves, *Philosophical Transactions of the Royal Society of London,* A, Vol. 315: 509–62, and references therein for a discussion of wind wave directionality). Another problem is that of refraction, which would bend wave crests towards the shore near the bottom of the photo. Using the orientation of wave crests in the foreground would tend to underestimate the angle between the wind direction and that of the view of the camera. So, although the view is certainly not along the Loch, it is not inconsistent with a wind along the Loch itself with wave crests appearing at an angle to the wind direction because of a combination of directional spreading and refraction.

Campbell does not agree that the Great Glenn's walls could have steered the wind along the axis of Loch Ness, and argues for a northerly wind

direction, along the isobars of his Fig. 1. Mere reiteration of our difference of opinion regarding the effectiveness of orographic steering will clearly not resolve the issue.

There are a few things that could help. Using more modern wind measurements when available, one might compare winds along Loch Ness to those at Dalwhinnie and Aberdeen for weather patterns similar to that of April 19, 1934. An examination of general statistics of wind speed and direction at available stations would also provide useful guidance. It might then be possible to infer back a reliable wind direction on the shores of Loch Ness. One might also analyze predictions of surface winds along the Loch calculated by the highest resolution models of the European Centre for Medium-Range Weather Forecasting, in Reading, England, again for appropriate isobar patterns. If those models are not detailed enough, a finer regional model could be run, using the coarser scale calculations as input. I would like to pursue these options in the future, as time permits, and would appreciate collaboration from parties in closer proximity to the scene, and more familiar with the sources of data.

The suggestion that the direction of view might be towards the south is quite interesting. It has already been suggested to me by Robert McGrath (private communication, 1989) that the length of the shadow in the Wilson photograph is more consistent with a noon-time picture, which agrees with Wilson's immediate recollection of the event. On April 19th, a month after the spring equinox, the angle of the sun at noon at 55°N would have been near 45°, giving shadows of a length comparable to the object itself, as observed in the photograph. If the wind had been blowing from the north, and thus from behind the photographer, the fetch from shore to the Nessie-object would have been only 16/cos 22° m = 17.2 m (Campbell's 1984 estimate combined with the direction estimate in his above Comment). From Campbell's analysis in his *British Journal of Photography* article, we also note that, although the nearby shoreline is not visible in the photograph, it is probably not far below it, so that the water distance between the foot of the picture and the Nessie-object would be approximately equal to the wind fetch for a northerly wind.

There are three problems with Campbell's suggestion. First, a shadow on a wavy surface is distorted, and may appear longer or shorter depending on the angle of the light source, the wave slopes, and the relative dimensions of the objects and the dominant wavelength. It is thus difficult to be definite about the angle of the sun in the photograph.

Secondly, if the wind is blowing from the north, behind the photographer, the waves are being generated in the area between the shore and the Nessie-object, and travelling away from the viewer. For a fetch (x) as short as 0.017 km, and a wind speed (U) below 3.0 m/sec, the waves are strongly fetch-limited; the period at the peak of the spectrum $T = 0.566x^{0.5}U^{0.4} = 0.25$ sec and the corresponding wavelength $L = gT^2/2 = 10$ cm (D. J. Carter, 1982, Prediction of Wave Height and Period for a Constant Wind Velocity Using the JONSWAP Results, *Ocean Engineering*, Vol. 9: 17–33).

By comparison with nearby wavelengths, we originally argued that the height of Nessie was given by $h = 0.88L \tan 19°$. With L = 10 cm, this gives h = 3 cm: a very small Nessie indeed, undoubtedly much to Campbell's pleasure! However, if that calculation is correct, other dimensions must be consistent with it. In particular, the fraction of the fetch visible in the photograph, about three wavelengths, must be near 16 m. That is certainly not the case: three wavelengths give only 30 cm. Further perusal of the photograph might suggest that the dominant wavelength could be shorter on the image than the value given by our zero crossing analysis; if there are two wave crests where we saw one, Nessie grows to 6 cm, and the visible fetch to 60 cm. That is still too short by more than an order of magnitude. Campbell's earlier calculations are not consistent with the wind direction which he proposes in his above Comment.

Thirdly, we note that, at very short fetches, wind waves have a strongly bimodal directional spectrum, with wavefronts preferentially directed at angles on each side of the wind. As noted by Campbell, who does not hesitate to define an angle between the direction of wave crests and the "horizontal," the waves seen near the bottom of the photograph are more consistent with a series of well-aligned wave crests characteristic of a rather sharply peaked directional spectrum. That is another reason for rejecting his wind direction hypothesis.

Campbell's suggestion that the wind must have been blowing from the north, and his ingenious idea that the view of Wilson's photographic plate had been reversed over all these years, are not consistent with the observations and with his earlier analyses.

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The Yahoo and the Nature of Zoological Discovery

(Comment on Colin P. Groves, 1988, On Type I and Type II Errors in Cryptozooloy; or Was Proteus a Yahoo?, *Cryptozoology,* Vol. 7: 123–28)

Groves has previously made the suggestion that, in examining reports of unknown animals, one should look through disordered information to find consistency, which he defines as two or more independent reports describing the same series of features. He believes that the best real evidence is consistency (Colin P. Groves, 1987, Noise, Signal and Evidence [Response to Bayanov and Joyner], *Cryptozoology,* Vol. 6: 128–29).

In response, I pointed out that there are indeed several characteristics which appear consistently among reports of the Yahoo (Graham C. Joyner, 1988, The Yahoo Again Misrepresented [Comment on Groves], *Cryptozoology,* Vol. 7: 121). I may, however, have misunderstood Groves' position. With this possibility in mind, I shall now try and remove any consequent misunderstanding.

Groves maintains (as already indicated) that a necessary and sufficient condition for true consistency among the various reports is that we find two or more independent reports describing the same series of features. My reconsidered reply to this proposition has two parts. First of all, I believe it to be an arbitary and unnecessarily severe condition. Correlation of independent evidence is one important factor in the evaluation of empirical data, but it is a matter of making use of what is available rather than laying down a prescription. Having said that, however, I believe Groves' condition can be met, as may be seen in Table 1. The comparison employs two of the sources referred to by Groves, as well as a report by H. J. McCooey in *The Australian Town and Country Journal,* December 9, 1882, p. 1127. This is the first item in my 1986 collection, *H. J. McCooey, the Australian Museum and the "Indigenous Ape,"* MS., Australian Museum Library. The table suggests that there are series of the same characteristics in reports originating from different sources.

Groves' own Table 1 also shows a considerable degree of correlation of another kind, i.e., among individual characteristics of the Yahoo, although, unfortunately, its merit is lessened by several omissions, by the insertion of unnecesary material, and by one considerable error of judgment. First of all, it is a bad mistake to combine items 6 and 21 (numbers refer to reports in my 1977 work *The Hairy Man of South Eastern Australia*). The first is a description from the year 1842 of the Yahoo as a mythological being. The details, e.g., backward-turning feet, are not therefore intended primarily, if at all, as a physical description, and should not appear in a table of comparative physical characteristics. The second account was written in 1912, and concerns the recollections of an Anglo settler which emphasize physical

TABLE 1.—Comparison of characteristics in Yahoo reports.

Report number	Color	Shape of foot	Nocturnal	Height	Fore legs or arms	Gait/stance	Tail
(Joyner 1977)							
1. George Osborne (1871)	black with tan-colored streak from neck to abdomen	long, with long toes	after sunset	about 5 ft	long and well developed	quadrupedal, at rest supporting body with two legs and one arm while other arm on hip	no tail
19. Charles Harper (1912)	black on shoulders and back, brownish-red on body, legs and arms	metatarsals very short, with very long toes	after dark	5 ft, 8 in. to 5 ft, 10 in.	extremely long, large and very muscular	bipedal and quadrupedal	?
(Joyner 1986)							
1. H. J. McCooey (1882)	black with dirty red or snuff color about the throat and breast	?	?	nearly 5 ft if standing perfectly upright	long	standing on hind legs partly upright	no tail

attributes. There is no identity or even similarity between the two items. Next, Groves has failed to include item 12, in which the animal observed by Johnny McWilliams had nails long enough to tear up the dust as it ran. The other notable omission is the account by H. J. McCooey, already referred to. Finally, it would have been better to omit entirely details from items of dubious worth, such as the account from Walla Walla, and even that from Rocky Bridge. The purpose of such a table is, after all, to clarify, not to obscure.

A suitably amended table would then demonstrate at least some degree of correlation among individual features ascribed to the Yahoo. It would show two instances of tree-climbing, two of dual gait (I omit Summerell's account because I am not entirely satisfied with it), two of black hair with reddish mark on the throat, three examples of long foot with five references to long toes, four references to long nails or claws, and three to a projecting face.

Unfortunately, Groves has tended to use his table not so much to promote the idea of consistency as that of inconsistency. He has accomplished this by asking not: "Do we find the same feature or series of features in more than one report?," but the apparently similar but really quite different question: "Are reports consistent with each other?" I note that he even attributes to me the view that consistency between reports is all important. What I really stated was that, in certain cases, there is correlation between characteristics which show up in two or more reports. Groves calls consistency between reports the crucial point, yet I am unable to agree with him.

In fact, the *absence* of consistency must be regarded with caution. Sometimes it indicates that a report is inaccurate or unreliable. There are many other reasons. For instance, color can vary in different light, as well as according to age and sex (as can size). A description may be merely misunderstood: Marrin's animal was *not* tan all over but "a tan color like a 'possum," which would mean tan on its underside. Some attributes, such as tree-climbing or the possession of two gaits, would not necessarily be apparent from a single brief encounter. It is absurd to suggest, as Groves appears to do, that, because a creature is observed standing upright, that it may not also move on four feet, or to claim that an animal seen on the ground may therefore not be capable of climbing a tree.

Moving on to what all this is about, Groves asserts: "I believe that we are in the presence of a remarkable intellectual and cultural phenomenon, the *bête humaine*. . . ," and claims the present case is about the existence of "an unknown hominoid or hominid in southeastern Australia." Now, this is very odd indeed because elsewhere Groves has agreed with me that "unknown species there may be, but that such a species is a hominoid (or hominid) is in no way required by the evidence" (Colin P. Groves, 1986, The Yahoo, the Yowie and Reports of Australian Hairy Bipeds, *Cryptozoology,* Vol. 5: 52). He has also indicated to me his opinion that one or other of the former giant marsupials must be the only genuine basis for such stories. The most sensible course is to accept—what was the most widely held opinion in the 19th century—that the Yahoo was some as yet undiscovered marsupial. There is simply no need to promote an untenable hominoid theory.

One wonders, too, at Groves' persistent identification of the Yahoo with the Yowie. Leaving aside the finer points, it can be stated that the Yahoo was an undiscovered marsupial of roughly bear-like conformation, which was referred to intermittently throughout most of the 19th and early 20th centuries. It is the creature we have been discussing here. The Yowie, on the other hand, is a recent fiction which came into being in 1975, and is represented as an enormous primate of a kind very similar to the North American Sasquatch. It, or course, has no history, although one has been invented for it. There is no justification for confusing the two.

Finally, I suspect that, in his demand for evidence and in his view of the nature of zoological discovery, Groves is espousing a simplistic and outdated view of the nature of science. In a recent work (Graham C. Joyner, 1989, *Scientific Discovery and the Place of the Yahoo in Australian Zoological History,* MS., Australian Museum Library), I analyze the traditional positivist response given by A. E. Montogomery to Harper's statement in 1912. I then compare the more comprehensive analysis of discovery given by Thomas Kuhn in a well-known work (Thomas S. Kuhn, 1970, *The Structure of Scientific Revolutions,* 2nd ed., University of Chicago Press, Chicago). Discovery is here revealed as an extended process culminating in the assimilation of an anomaly in which observation alone is not sufficient, but needs to be accompanied by an appropriate conceptualization. A view of scientific innovation based on evidence alone is thus shown to be inadequate.

In this context, the Yahoo is properly seen as an unresolved anomaly set against a background of zoological discovery involving such anomalies as the platypus eggs, marsupial birth, the gorilla, and the Queensland lungfish.

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The Yahoo and "Hominoid" Psychology

(Response to Joyner)

Eventually, we get to the point where we are trying to get inside the minds of our predecessors of a century or more ago. What exactly did they mean by "monkey features"? How accurate was their "five feet high"? What length did they have in mind when they wrote of "long arms"? When they gave a description of "brownish-red on body, legs and arms," were they really just seeing the reflected glow of the campfire? When Arthur Marrin wrote "tan-colored like a possum," did he actually mean that it was only its underside that was tan, and what species of possum or possum-like creature did he have in mind?

No, I do not think it too severe a condition to require two or more independent reports describing the same set of features; it is the only hope we can possibly have of judging the veracity of anecdotal evidence—of sifting sober reportage from leg-pulling, accurate observation from half-remembered glimpse, straight description from one clouded by preconception. I will return to some of these points below.

Joyner may be right to criticize my combining of items 6 and 21; neither, however, is of much assistance in deciphering the Yahoo. I did not include item 12 because it contained no indication of the six "consistent features" which Joyner had previously listed. I included everything with any kind of description of hair color, foot shape, etc., simply because one must not select one's evidence beforehand; I readily agree that much of it is of very little value.

Indeed, the absence of consistency is negative evidence; an animal may indeed have gone down on all fours as soon as the observer turned away—but we cannot assume it. If we are trying to reconstruct a mysterious beast, weeding out (metaphorical) red herrings on the way, we must first discover whether there is consistency in its description—exactly as I have outlined.

Now to the "hominoid" aspect. It is clear from reading all the reports cited by Joyner in his two compilations, even sober ones like George Osborne's, that the psychology was "hominoid." Whether stimulated by Aboriginal mythology or not, the European observers/reporters thought in terms of a gorilla-like creature, and mostly they dutifully described one. Modern commentators (Bayanov, for example) seem often to think in these terms, and perhaps Joyner at one time did so too (his first report, in 1977, was actually titled *The Hairy Man of South Eastern Australia*), gradually coming to believe that (1) there *may* be a reality to some of the tales, but (2) if so, it is a marsupial, not a hominoid. If my interpretation of Joyner's thought processes is correct, then it has to be said that we are not so far apart; I much more strongly emphasize the *may* part. So, I had turned to the *bête humaine* phenomenon as more tractable—evidently what Joyner implies in his Kuhnian analysis (which I hope he will publish, perhaps in this journal).

I also hope that Joyner will publish the McCooey report, because I agree that, with its addition to the table, for the very first time a degree of consistency has appeared. We need to know the context of the report: straight like Osborne's, or over-the-top like Harper's? Independent, or with prior knowledge of Osborne's? In short, the full data are needed.

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Giraffes All Set

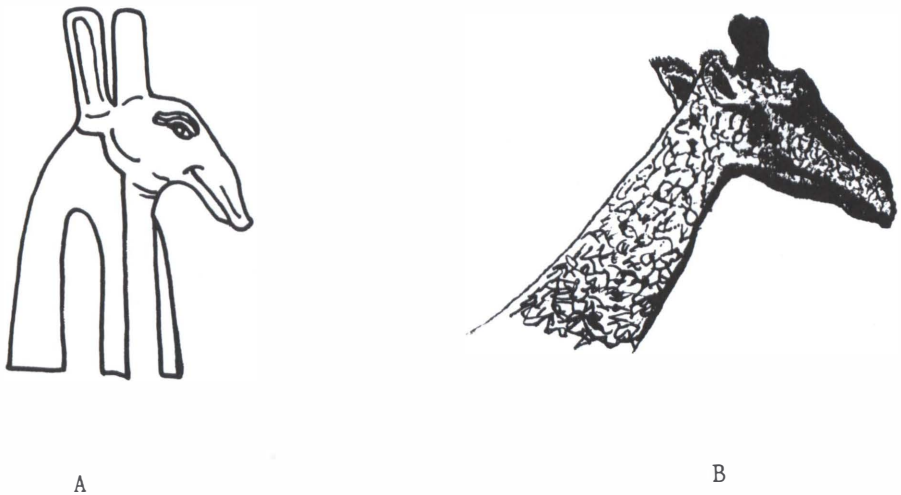
(Comment on John W. Olsen, 1986, More on the Identity of the Egyptian Animal Diety, Set, *Cryptozoology,* Vol. 5: 130–31; Michael D. Swords, 1986, On Set as an Egyptian Figment, *Cryptozoology,* Vol. 5: 131–32)

Several years ago, a review article discussed the possible zoological or mythological affinities of Set, an Egyptian god (Michael D. Swords, 1985, On the Possible Identification of the Egyptian Animal God Set, *Cryptozoology,* Vol. 4: 15–27). After dismissing a number of animal candidates, Swords essentially reduced the possibilities to three: known or crypto-canids, known or crypto-hyaenids, and an as-yet unclassified form.

Other species, he stated, "have been proposed, apparently out of desperation and without any regard to the content of the ancient texts. . . ." In a subsequent Comment on Swords, Olsen expressed his own preference, "the notion that Set may represent a combination of physical and behavioral characteristics of a number of related hyaenids and canids . . . which, if taken individually, would be identifiable as known, non-cryptozoological species" (John W. Olsen, 1986, above).

In his Response to Olsen, Swords outlined the reasoning behind his own thinking of what Set was, "a normal animal (a canid in this case)" (Michael D. Swords, 1986, above).

I have no expertise whatsoever concerning the interpretation of zoomorphic Egyptian gods. The purpose of this Comment is merely to bring to the



A B

FIG. 1.—A. Profile of Egyptian Animal God Set, as depicted by Swords (1985). B. Profile of modern African giraffe, based on random selection from giraffe photos.

readers' attention some literature which was not cited—and may have been overlooked—by the above authors.

While consulting several works on giraffe biology, I chanced upon a discussion of Set in a book by Clive A. Spinage (1968, *The Book of the Giraffe,* Houghton Mifflin, Boston) sitting right in my own library. This book was not referenced by Swords in his original article. After reviewing giraffe depictions in ancient Egyptian art, Spinage addresses the question of whether the giraffe could have been adopted as an Egyptian deity, the figures of which "were always done with great care and attention to detail and showed great conservatism in execution throughout many centuries of dynastic history."

In discussing Set, Spinage states that "the easy way out is to say that it was an imaginary animal, but this is against all the teachings of the ancient Egyptian religion." He then goes on to review—as did Swords in his own article—the Set candidates which have been proposed, but this time including the okapi—concluding that, "apart from the big ears there is no other similarity."

Spinage than addresses the giraffe idea:

> Some years after the okapi suggestion a case was put up for Set being the giraffe, the "ears" being the horns, allowing for some artistic license. This left the animal without ears but their absence was explained by the fact that the Egyptians used a headdress to shelter the neck from the sun, and this headdress was often shown covering the ears of better-known animals which only showed their horns. The story of the giraffe also accords well with the mythology surrounding Set. Like the latter, it seems that the giraffe was gradually driven out of the Nile Valley but lingered on in the western desert regions.

> When the giraffe was no longer known in the Nile Valley perhaps the representations became rather stylized, and although it was to become known again in the form of the Nubian tributes the attitude was not altered. The giraffe did undergo some change in Egyptian culture, its name being altered from *sr* to *mmi*. The Ethiopic *zarat* from which *sr* may be derived would sound rather like "set" or "sat" if pronounced quickly. In the Battlefield Palette [an Egyptian relic portraying a giraffe] the eyes and ears are remarkably like those of Set.

In view of the above, I wonder why Swords was so quick to eliminate the giraffe as a candidate for Set, lumping it with other animals which were proposed "out of desperation," such as the European boar, the camel, the fox, and the aardvark. Although my abilities do not by any stretch of the imagination include artistic talent, I have taken the liberty of including here a profile of a modern giraffe, traced from a photo selected at random from a picture book on African animals, and I have placed this next to Swords' own profile of Set (Fig. 1) so that readers can form their own opinions.

Perhaps the giraffe should be looked at again in a new light by Swords, Olsen, or other scholars interested in this topic.

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GIRAFFES ELIMINATED

(Response to Greenwell)

Although I must admit that there is a superficial facial resemblance between the giraffe and some aspects of the Set animal, there are many reasons, I believe, to eliminate the giraffe as a candidate.

First, the Egyptians seem to have known perfectly well that the giraffe was different from the Set animal, as they had two distinct forms of hieroglyphs for them (Fig. 1). Second, the Egyptians described Set as a violent force, a black-faced dismemberer which stalked the night. It was the symbol of the power of the South, which over the centuries lurked as a submerged political threat to the ruling religions of the North. The giraffe fits none of the behavioral characteristics attributed to Set. Third, giraffes really do not look that much like the representations of the Set animal. Usually, the bodyline of the latter is dramatically canid. Fourth, certain Egyptian paintings of dogs or unidentified canid forms show many features of Set (Fig. 2). The examples shown include a Set-like hound bodyline, a black face, pricked ears, and tail

FIG. 1.—Egyptian hieroglyhs of *ser* (giraffe) left, and Set.

erect while seated. It is, therefore, not inconceivable that all these characters could reside in a single canid form.

I would like to add to these "normal" reasons for rejecting not only the giraffe, but also most non-canid (or non-hyaenid) forms, a fifth, cryptozoological reason: there are hints that such a creature existed, or might still exist, in East Africa, further to the South (ex. Nandi Bear tales). To these intriguing tidbits, I would like to add one more.

In Zimbabwe, there are some odd cave paintings, some of which have caused great controversy. One of these, in Diana's Vow, Rusape, shows a cluster of figures from more than one scene painted upon another (A. R. Willcox, 1984, *The Rock Art of Africa,* Holmes and Meier, New York, especially commentary on pp. 143–45). A large dominant reclining figure stands out. It wears what could be an animal mask and an "Egyptian-like" wig (Fig. 3). Some have interpreted a number of these paintings as showing Egyptian influence, although no dating seems to be possible. What may make this something "cryptozoological" is that the animal mask is mimicked by a dog-like form elsewhere in the painting, having the same eyes and line-markings on the long snout (Fig. 4a). The animal has been described as mysterious, possibly a dog, possibly a hyaena. The dots painted on it were interpreted by researchers as later additions, making the animal perhaps more hyaena-like than originally intended. Whatever the truth of that, there is a second animal of similar form on the painting, but with no added spots (4b).

FIG. 2.—Unidentified canid forms found in Egyptian art showing features of the Set animal.

FIG. 3.—Figure wearing possible animal mask and "Egyptian-like" wig. From Diana's Vow, Rusape, Zimbabwe.

I offer the risky hypothesis that this painting *is,* in fact, a long-distance Egyptian-influenced artifact (relating perhaps to a religion-concept transfer similar to the Yoruba Obaluwan god, which was also Set, transferred to West Africa in that case). I suggest that the canid or hyaenid animal in the painting is the Set-animal.

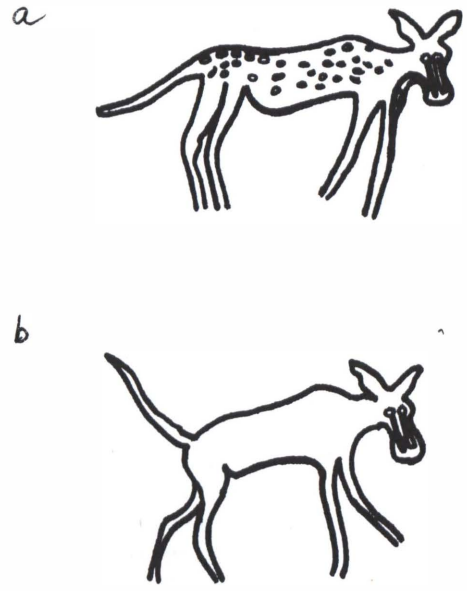

FIG. 4.—a (top) Dog-like form from Diana's Vow, Rusape, Zimbabwe, with spots. b (bottom) Dog-like form from Diana's Vow, Rusape, Zimbabwe, without spots.

If my hypothesis is correct, whatever animal this was (is?), it had (has?) a very large range. We should note the following concurrences: a proper body-form, a long curved snout, large upright ears, and a long tail which can be carried "up." If the scene is, as suggested by some, a death scene, then the Set relationship adds a further significant dimension.

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PERSEPOLIS: NILGAI—NOT OKAPI

(Comment on Michel Raynal, 1988, Persepolis: A Puzzling Case in Archaeological Cryptozoology, *Cryptozoology,* Vol. 7: 102–103; Christine Janis, 1988, Hurrah for Hyraces!, *Cryptozoology,* Vol. 7: 104–106)

Contrary to oft-repeated assertions by various authorities over much of the past half-century, the Achaemenian frieze at Takht-e-Jamshid (Persepolis) most surely does *not* represent an okapi, *Okapia johnstoni* (Sclater). Neither does it depict any other species of giraffid, living or extinct.

Eleven years ago, we positively identified the "Mystery Beast of Persepolis" (Robert G. Tuck, Jr., [writing as "Reza Gholi Takestani"], 1978, *Tehran Journal,* May 23, p. 9). The animal is a male nilgai antelope, *Boselaphus tragocamelus* (Pallas), from India. Our findings appeared in a scholarly publication of the British Institute of Persian Studies (Raul Valdez and Robert G. Tuck, Jr., 1980, On the Identification of the Animals Accompanying the "Ethiopian" Delegation in the Bas-Reliefs of the Apadana at Persepolis, *Iran,* Vol. 17: 156–57, 170).

Valdez, very familiar with introduced nilgais on ranches in his native Texas, personally examined the relief in 1971 (Fig. 1). He instantly recognized the slope-backed animal depicted on it. A wildlife biologist, as well as the Smithsonian Institution's first Peace Corps volunteer (and an advisor) to the Iran Department of the Environment at the time, Valdez was unaware of the controversy that had surrounded the carving since B. Patterson first

FIG. 1.—The "mystery beast of Persepolis." The Achaemenian frieze at Persepolis, Iran, depicting an animal offered as tribute by a foreign delegation to the King of Kings, in the Persian Empire, ca. 2500 years B.P. (Raul Valdez.)

proposed it represented an okapi (in E. F. Schmidt, 1953, *Persepolis I,* Chicago, p. 90, footnote 162).

In 1974, Valdez conveyed his conclusions to Tuck, who, like everyone but Valdez, had thought the animal must be African, since the delegation leading it was from the ancient Persian satrapy of Kushiya. However, Tuck discussed the matter with A. S Shahbazi, then Director of the Institute of Achaemenid Research, in Tehran. Shahbazi explained that foreign delegates often purchased along the way the items they brought as tribute to the King of Kings (*Shahanshah*), in the heart of the Persian Empire. Thus obtained *en route,* these gifts did not necessarily constitute livestock or artifacts originating from the lands represented. Hence, Shahbazi cautioned, one should not infer that the animal being led by human figures with Negroid features is necessarily of African origin.

In our investigation of the carving, we discussed and discarded the various identifications proposed by all previous authors. We examined the Persepolis relief, and compared it with both the okapi and the nilgai—also known as the blue bull (Fig. 2). We then showed that the Persepolis relief could not be any species of giraffid. The carved animal has dew claws. No giraffid does.

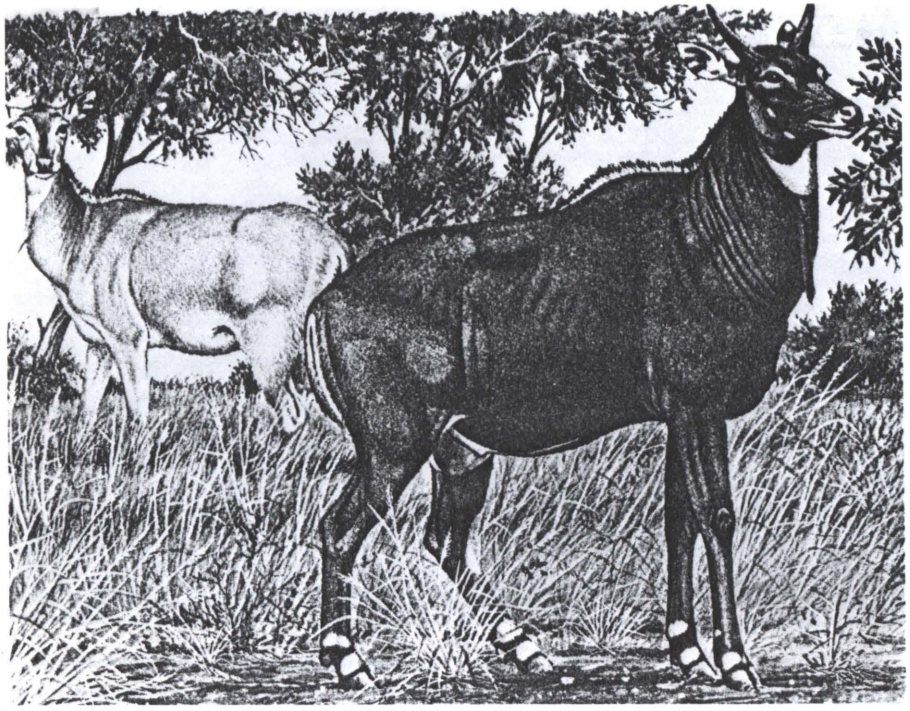

FIG. 2.—A male nilgai (blue bull), *Boselaphus tragocamelus,* from India. The authors identify this animal as that depicted at Persepolis, rather than the okapi or any other animal. (Peter Barrett, *World Guide to Mammals,* by Nicole Duplaix and Noel Simon, 1976, Crown Publishers, New York.)

The animal's other anatomical characters clearly depicted on the frieze are also unquestionably those of a male nilgai. They absolutely do not match those of an okapi. The traits visible on the carved animal include:

1) sharply pointed, hairless, upright horns atop the head, behind the eyes (unlike the blunt, hair-covered, backward-directed okapi's horns, which are above the eyes);

2) moderate, tubular ears (unlike the okapi's large, expanded, peculiar ears);

3) a short, stiff mane (absent in adult male okapis); and

4) a tail that is 29% of the head-body length, and has an extensive tuft (the okapi's tail is only 20% of the head-body length, and has only a terminal tuft).

Unfortunately, damage to the relief prevented us from confirming the existence of a throat tuft. This character is present in male nilgais, but not in okapis.

We did point out one very tenuous connection between the okapi and

Persepolis which is of historical interest (Valdez and Tuck, 1980, above, footnote 16). There is evidence that explorer Sir Henry M. Stanley saw a living okapi in Africa a decade before its discovery by Sir Harry Johnston (Willy Ley, 1968, *Dawn of Zoology,* Prentice-Hall, Englewood Cliffs, New Jersey, pp. 14, 250). Later, as correspondent for the *New York Herald,* Stanley carved his name on the southern inner wall of the Gateway of All Nations at Persepolis (Sylvia Matheson, 1976, *Persia: An Archaeological Guide,* 2nd ed., London, p. 228; also Tuck, 1963-1979, personal photographic collection). Thus, curiously, Sir Henry's graffito is the only real link that exists between the okapi and Persepolis.

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