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CONTENTS

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CRYPTOZOOLOGY

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EDITORIAL

This is the first editorial appearing in *Cryptozoology*. I have not included editorials before now because it is my opinion that readers do not necessarily want to be subjected to continual pronouncements from the editor. It is better, I think, to allow the materials published within these pages to speak for themselves.

However, this issue of *Cryptozoology*, Vol. 10, marks the journal's 10th anniversary, and I thought a few words from the editor might be appropriate. The first point one could make is that the journal has in fact survived for 10 years, and has accomplished what it was originally intended to do. When the idea of a scholarly cryptozoology journal was first being discussed, some negativists voiced their opinion that it would never get off the ground—or would not last long it if did—simply because the field of cryptozoology would not be able to produce publishable material. It was also suggested that such a journal would necessarily have to violate established methods of scholarly publishing. Such predictions have proven to be incorrect, demonstrating that, with sufficient effort and good will—and funding—scientific methodology and scholarly procedure can be applied to any reasonable field of human inquiry.

The total number of manuscripts published in *Cryptozoology* through this issue is 306. These break down as follows: 48 Articles, 9 Research Reports, 39 Field Reports, 64 Book Reviews, and 146 Comments and Responses. The total number of authors involved has been 344. The total number of text pages (not counting ad pages) through this issue has been 1,383, which gives an average of 138 pages per journal. The total number of published words comes to about 600,000.

An important component of the journal's success has been the support provided by the Editorial Board and the many other referees who have evaluated manuscripts. Generally, two referees—three in special instances—have reviewed all Articles and Research Reports. Most manuscripts, even those by the most knowledgeable authors, have required some editorial revision, however slight. Numerous manuscripts have undergone extensive author revisions based on referee recommendations prior to publication,

and, over the 10 years, 14 Article manuscripts have been rejected for publication following the review process, representing a 24 percent rejection rate.

It has not been the intent of the referees, however, to simply denigrate submitted manuscripts. Whenever a referee points out a manuscript's failings, the general intent is to assist the author in re-working it, or, if it is simply not publishable, to objectively show why this is so. As editor—assuming the manuscript is considered salvageable—I always encourage the author to re-submit a revised version.

A total of 67 individual referees have participated in this review process over the past 10 years, and I would like to acknowledge these individuals for the time and effort they have put into supporting the journal:

Aaron M. Bauer, Henry H. Bauer, Troy L. Best, Walter H. Birkby, George W. Brown, John S. Buckley, Eric Buffetaut, Peter F. Brussard, Susan Cachel, Angelo P. Capparella, John Colarusso, Edwin H. Colbert, Jean-Paul Debenat, Brent K. Dugolinsky, William Eddy, Charles Edwards, B. Roy Frieden, Joseph F. Gennaro, John Green, Colin P. Groves, Michael Heaney, John Hendrickson, David Heppell, Bernard Heuvelmans, Nicholas Hotton, Arthur Hulse, David Jacobs, Christine Janis, John Johnson, Andrew Kitchener, Piotr Klafkowski, Grover S. Krantz, James D. Lazell, Paul H. LeBlond, Phillip S. Lobel, Roy P. Mackal, James G. Mead, Mary Ellen Morbeck, John Napier, Ronald M. Nowak, John W. Olsen, John F. Pagels, Michael A. Persinger, Frank E. Poirier, Herman Reichenbach, Rosalind Ribnick, Robert H. Rines, Steve Runnells, William M. S. Russell, Wade C. Sherbrooke, C. Lavett Smith, David Soren, Nikolai Spassov, Roderick Sprague, David Steiner, Jan-Ojuind Swahn, Pascal Tassy, Charles Thomas, Leigh M. Van Valen, Kurt Von Nieda, Michael P. Walters, Ron Westrum, M. Justin Wilkinson, Donald L. Wolberg, Forest G. Wood, Bernd Wursig, and George R. Zug.

It is true that *Cryptozoology,* since the very beginning, has always appeared late, sometimes very late. But at least it has appeared. Attempts will be made in the second decade to improve our delivery schedule; this will mainly depend on the success of our continuing fundraising efforts. There is one thing that readers may always count on, however: no matter *how* late the journal may appear, it will continue to be a scholarly journal, one containing reliable information, insightful discussions, and appropriate referencing and documentation.

Most members who have given an opinion have expressed approval of the journal's style and format. We shall therefore leave well enough alone. While in the first decade we strived to present a theoretical paper as the lead Article in each issue, this will now be less of a priority, as cryptozoology's responsibilities and parameters seem to have been adequately addressed. Theoretical manuscripts will always be welcome, however, and these will be placed as lead Articles whenever they are published.

Finally, my thanks go to all authors, many of whom labored hard on manuscripts only to suffer the slings and arrows of what may have seemed at times like outrageous refereeing, as well as be subjected to my own editorial demands. And, of course, thanks go to all our readers who, by simply reading it all, have made the journal possible.

J. RICHARD GREENWELL
Editor

Cryptozoology, 10, 1991, 4–15

THE MUSK OX IN EURASIA: EXTINCT AT THE PLEISTOCENE-HOLOCENE BOUNDARY OR SURVIVOR TO HISTORICAL TIMES?

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ABSTRACT: The musk ox, *Ovibos moschatus*, now restricted to the North American Arctic, inhabited Eurasia in the Pleistocene, and was thought to have become extinct there at the Pleistocene-Holocene boundary, approximately 10,000 years B.P. However, a study of images depicted on two silver plaques recovered during excavations of 1st century B.C. northern Mongolian tombs supports the view that the musk ox persisted in some locations in Asia until as recently as 2,000 years B.P. Skulls of this species have been recovered from Siberia's Taimyr Peninsula in this century. Two such skulls have been uncertainly dated at 3,800–2,900 years B.P., but show evidence of more recent human intervention. Doubt has been expressed about these datings, and another skull exhibits a fresh appearance. It is proposed that the musk ox may have persisted in this region until much more recently than previously supposed.

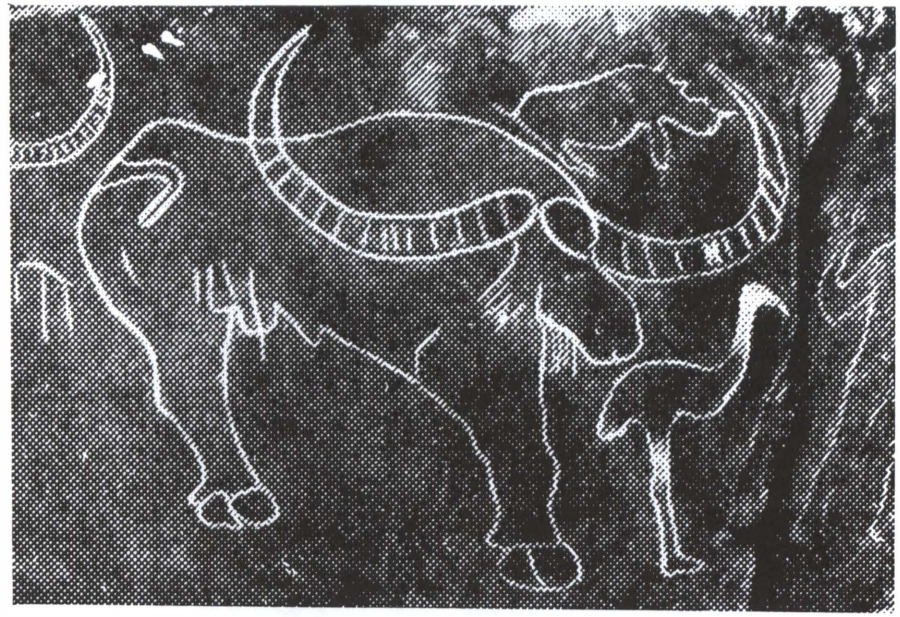
INTRODUCTION

The modern distribution of the musk ox, *Ovibos moschatus*, is restricted to the Arctic tundra of the North American mainland, numerous Arctic islands, and Greenland (Gunn 1982). In the Upper Pleistocene, it was also common in the tundra of Eurasia, as a typical representative of the mammoth steppe fauna complex. Until recently, it was considered to have become extinct in Eurasia (together with many other megafaunal species) at the end of the Pleistocene as a result of climatic changes which led to an altered landscape (Vereshchagin and Baryshnikov 1984).

Archaeological faunal depictions may sometimes represent new and valuable information on the spatial and temporal distribution of both fossil and extant animal species. For example, images of *Pelorovis antiquus*, the long-horned buffalo from the African Pleistocene, appear in north African rock drawings from the Upper Paleolithic (Epstein 1971, Bogoliubskii 1959) (Fig. 1). Several interesting examples have also been proposed recently by Janis (1987). In this paper, I will address both archaeological and osteological evidence which supports the view that the musk ox may have persisted in northern Eurasia throughout the Holocene, and perhaps even until recent centuries.

ARCHAEOLOGICAL IMAGES

In 1924, P. Kozlov, the explorer of Central Asia, discovered burial tombs in the Noin Ula Mountains of Mongolia, about 60 miles (100 km) north of



A

B

FIG. 1.—Pleistocene long-horned African buffalo, *Pelorovis antiquus*, depicted on rock drawings at Fezan, Libya. A. After Epstein (1971). B. After Bogoliubskii (1959).

A

FIG. 2.—Silver plaques recovered from tombs discovered in the Noin Ula Mountains, Mongolia, in 1924, and dated to the 1st century B.C. A. Round plaque; B. Elongated plaque. The author proposes that the animal depicted on the plaques is the musk ox, *Ovibos moschatus*.

Ulan Bator. The tombs date from the 1st century B.C. (Kozlov 1986), and were probably made by the Huns (Werner 1934). The numerous objects recovered from the tombs included two silver plaques, shaped differently but depicting almost the exact same ungulate ornamentation (Fig. 2). The images have high artistic merit, and, despite stylization, are quite detailed and realistic.

Although some subjectivity is always present in the interpretation of such archaeological images, this strong example ensures a high level of reliability, leaving little doubt, in the author's opinion, that what is being depicted is the musk ox (Fig. 3A). Indeed, this idea was not overlooked at the time.

B

FIG. 2.—Continued.

Based on the discovery of the Noin-Ula plaques, Marcus (1933) believed that the musk ox survived in Mongolia until the 1st century B.C. This supposition was later supported by Pidoplichko (1951).

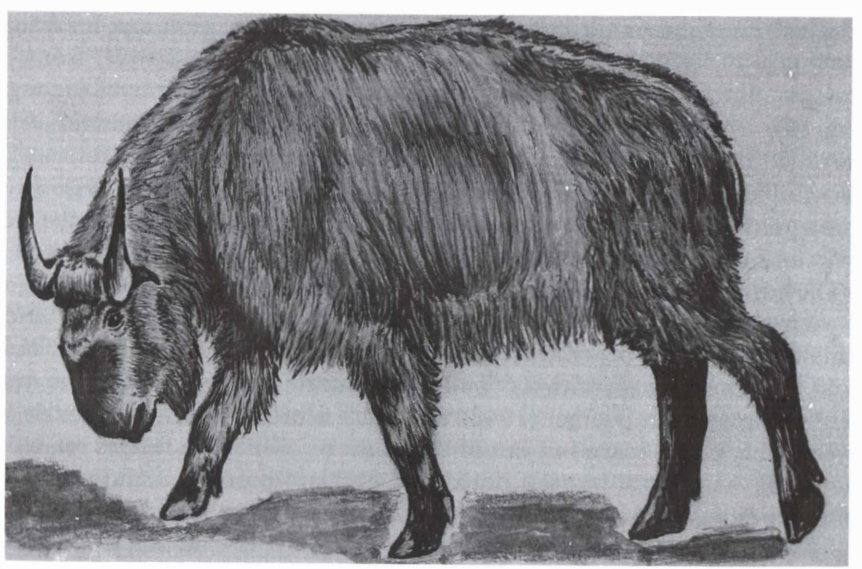
These images, which were clearly made in the local Scythian-Siberian style, have also been subject to other, more mundane interpretations. Boroffka (1926), for example, considered them to be of a "ram" (Fig. 3B), while Rostovtzeff (1929) proposed they were of a "yak" (Fig. 3C). The contemporary interpretation by Brentjes (1982) of a "yak" or a "takin" (Fig. 3D) is obviously influenced by these opinions, and, above all, by Soergel (1942), who undertook his own study of the images on the silver plaques. In his view, the animal depicted is most probably a composite: the body of a takin, *Budorcas taxicolor,* with the head of a musk ox, the latter being known to

A

B

FIG. 3.—Proposed candidates for the Noin Ula depictions: A. Musk ox, *Ovibos moschatus*; B. Argali wild sheep, *Ovis ammon*; C. Yak, *Bos mutus*; D. Takin, *Budorcas taxicolor*. (Drawings from photographs.)

C



D

FIG. 3.—Continued.

the artist from fossil skulls found in the northern permafrost zones, where well-preserved skeletons as well as the carcasses of other Pleistocene species are found to this day.

The creation of chimera-like syncretic creatures is indeed typical of the Scythian-Siberian animalist style. However, such is always done in some symbolic context, which is altogether absent in the Noin Ula plaques. The combining of different animals into one image is always emphasized in such drawings through their natural features, which is not the case with these depictions. Rather, it is evident that the idea was to present the animal in its natural setting.

The surrounding vegetation appears to be larch or Siberian cedar, and low, mountain terrain is shown at its feet. The rugged hills represented are typical of Siberia and northern Mongolia. At first, such a habitat does not seem to be adequate for the musk ox, and seems better suited to the takin, or, as proposed by Soergel, the Tibetan yak, *Bos grunniens* (*mutus*), and the argali wild sheep, *Ovis ammon*. However, the musk ox can also inhabit rocky ground, and even rugged terrain. In fact, Greenland/Alaska musk oxen introduced in 1975 on Wrangel Island, in the East Siberian Sea, usually occupy the higher ground and steeper slopes (V. F. Muzhchinkin, personal communication).

The heads and, in particular, the horns are the key identification features of the images on the plaques. The typical shape of the horns immediately rules out the yak, regardless of some similarity in the covering of body hair (Fig. 3C). A representation of yak horn shape and position can be seen in the image of "bulls" (yaks?), of the same general age and style, from the Ordos culture of south Siberia (Fig. 4). The curve of the horns of the argali (Fig. 3B) would have been depicted in an entirely different manner. Also, the argali has cross-wide rings in relief, not in longitudinal relief forms. The horns of the takin (Fig. 3D) would also be depicted with cross-wise rings, and would be bent sideways and backwards; i.e., shown in perspective sideways and upwards, not to the side and down as it is in the image.

The horns on the Noin Ula plaques (which are to a certain extent analogous in their shape and the basal concrescence with those of the recent African buffalo) could only be, considering the shape and relief, the horns of musk oxen. The broad head, with its "bull-like" muzzle is also typical of the musk ox. I disagree with Soergel (1942) that such a broad head and muzzle are typical only of the takin but not of the musk ox, which, in fact, is related to the takin. As Soergel himself noted, the eyes are protruded and point to a tube-like elongation of the orbits; this is characteristic, above all, of the musk ox. As to the base-marked forms of the muzzle, it is true that it should be less prominent because of the long hair. However, neither is it much more outlined in the takin. The drawing could have been made from memory,

FIG. 4.—Bronze plaque depicting "bulls," apparently yak from a mound in southern Siberia associated with the Scytho-Siberian animalist style, and probably dating from the last centuries of the first millennium B.C. (After Devlet 1976.)

and the artist might merely have observed some cattle to remember some of the details.

The position of the neck and head of the depicted animal is peculiar. Soergel (1942) presumed that the "unnatural" position of the head and the straightened neck were the outcome of the space limitations on the plaques. More likely, this is a case of some stylization, where the habitus and position, typical of the musk ox, are hyperbolized; namely, the hump on the back, the arc-shaped neck, and the head with the muzzle practically facing ventrally down, almost perpendicular to the longitudinal axis of the body. The rendering of the typical features of the habitus and body position of the musk ox, and their hyperbolization through stylization—the large, frontally-placed head, the head and muzzle bent down sharply, the powerful arc-like neck raising above the body—indicate that the animal was observed alive, and does not represent a drawing of the remains of a frozen Pleistocene carcass.

With regard to the emphasized hair at the lower part of the body, there is little reason to accept the views of Soergel (1942), that the hair cover was given in detail; i.e., that it contrasted with the shorter hair of the other parts of the body, which is similar to a certain extent to the takin, and, in particular, to the yak. In fact, such a stylized presentation of the long hair of the abdomen contrasting with the shorter hair of the body appears in images of yaks in the Scythian-Siberian animalist style (Fig. 4). However, in the case of the Noin Ula plaques, the stylized presentation of the long abdomen hair emphasizes more the abundance of hair in general, showing that the animal is characterized by particularly long and dense hair, as is true of the musk ox.

Attention should also be drawn to the details of abundant hair (a mane) around the neck, as well as the stylized framing of the entire body profile with long hair. This creates the general impression of the abundance of hair all over the body.

Osteological Evidence

Some interesting osteological evidence from north Siberia supports the proposition that the musk ox persisted well into the Holocene in Asia. In fact, this bone material indicates that the musk ox may have survived in Asia until recent centuries.

Some years ago, Nikolai K. Vereshchagin, the eminent specialist on Asian Quaternary mammals, reported the 1948 finding of musk ox bone material in the Taimyr Peninsula of northwest Siberia, near Cape Chelyuskin, the northernmost point in all of Asia (Vereshchagin 1959). A Soviet geologist found two skulls laying directly on the sand surface of a limestone rock niche, not far from the coast. The skulls were light-colored and had a fresh appearance. They are now in the collection of the Geological Institute, in Moscow. One of the skulls has two 5 mm holes, as if made by a drill, and the base of the skull appears to have been pierced by a bullet.

The C^{14} dating of the skulls and the horn sheaths in two different laboratories gave ages of 3,800 and 2,900 years B.P. respectively (Vereshchagin 1971). Although there is always some uncertainty as to dating precision, this suggests the persistence of musk ox in Eurasia far beyond the Pleistocene-Holocene boundary, and almost to the time of the Noin Ula silver plaques. Two possibilities exist: either the skulls are a few thousand years old and were previously handled—but in recent centuries—by humans who created the 5 mm holes and the bullet entry; or they are much more recent, and the living animals themselves were shot by hunters within recent centuries. Vereshchagin himself has doubts about the accuracy of the C^{14} datings, which were obtained using old equipment in the 1960's. He has recently expressed the view that the musk ox, adapted to conditions in the recent Asian tundra, survived until much later than assumed, and was probably known to at least the first Russian explorers in the 17th century (Vereshchagin 1988).

A more recent find is also of interest. In 1984, pinniped zoologist V. F. Muzhchinkin (personal communication) found another musk ox skull, again on the Taimyr Peninsula, some 30 miles (50 km) from the Bay of Pronchishchev. This skull, as with the 1948 specimens, was laying on a dune not far from the coast. It had clearly been placed in its position by human hands, as it was covered by some crossed sticks, perhaps to mark its location. Alternatively, the sticks may have had some ritual meaning. Unlike the bones of Upper Pleistocene animals, even those from the permafrost layers in Siberia (which are colored by mineral deposits and to a certain degree fossilized), this skull was white and, like the 1948 skulls, had a contemporary, fresh appearance, suggesting a very recent age. The sticks were fragile and rotten, and resembled, in their degree of conservation, some wooden pylons placed in the vicinity by a Soviet geological expedition which had passed through the area in the 1930's.

Could this skull have come from a Canadian musk ox population which had been introduced in the Taimyr Peninsula nine years earlier? We cannot be certain about this. The 10 musk ox were introduced in 1975 on the eastern shores of Lake Taimyr, about 120 miles (200 km) from the location of the skull find (Lever 1985, Uspenskii 1975). Vereshchagin and Baryshnikov (1984) point out that musk oxen are poorly adapted to migration in modern Siberian tundra due to dense snow cover, being unable to tolerate a snow cover deeper than 12 inches (30 cm). Furthermore, there is no evidence of musk ox migration in the region. On the other hand, the entire peninsula is quite desolate and completely uninhabited, and perhaps such migration could have occurred unseen by humans. Even local hunters do not frequent it for years. There have been no established inhabitants on the peninsula since the creation of the Soviet state, and, historically, only nomadic elk-breeders are mentioned. Perhaps significantly, however, Muzhchinkin states that, although the skull has a contemporary appearance—rather than being thousands of years old—it lacks the fresh appearance of bone which has been exposed for only a few years.

As with the two 1948 skulls, and another found near Lake Taimyr in 1952, the new 1984 skull is too fresh and well-preserved to have come from an animal that died millennia ago. At the same time, it seems not fresh enough to represent the musk ox population introduced in 1975. It thus appears to derive from an original Pleistocene population which persisted in Eurasia into the Holocene, and continued to evolve until extinction in recent centuries. In fact, Vereshchagin has noted that the measurements of the two 1948 skulls correspond more to those of the extant North American population, which likewise has evolved to a size smaller than the original Pleistocene populations.

Conclusion

Archaeological depictions apparently representing the musk ox indicate the persistence of this species in Asia—specifically in northern Mongolia and further north—beyond the Pleistocene-Holocene boundary to as recently as 2,000 years B.P. Some osteological evidence lends credibility to the supposition that this species may have persisted in the Taimyr Peninsula almost to the present time; that is, to at least the past few centuries. A more precise chronology of musk ox extinction in Asia could possibly be determined through further fieldwork in the Taimyr Peninsula, which might result in the acquisition of new osteological material for study.

I would like to thank N. Kalandadze and V. Zhegallo, of the Institute of Paleontology, Moscow, for their views concerning the subject of this paper.

I am particularly obliged to V. F. Muzhchinkin, of the Vernadsky Museum, Moscow, and N. K. Vereshchagin, of the Zoological Institute, Leningrad, for the valuable information they provided.

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GRIFFIN BONES: ANCIENT FOLKLORE AND PALEONTOLOGY

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ABSTRACT: The griffin or *gryps* was first described ca. 675 B.C. in a Greek work about Central Asia. The *gryps* was a lion-like quadruped with an eagle-like beak; it made nests on the ground and was associated with gold-mining and steppe nomads. Since the 17th century, the griffin has been interpreted as an imaginary symbol of, for example, vigilance; as a garbled description of a real but unfamiliar living creature, such as the lammergeier (bearded vulture); or as an archetypal memory of extinct prehistoric animals. A cross-disciplinary approach, drawing on Greek and Roman literature and art (700 B.C. to A.D. 300), and on modern archaeological, paleontological, and geological evidence, suggests that griffin folklore arose from ancient observations of rich dinosaur fossil beds near Central Asian gold deposits. Fossil remains—including nests and eggs—of the Late Cretaceous *Protoceratops* and other beaked dinosaurs match many of the classical griffin's attributes. Ancient caravan routes passed through the Altai-Tien Shan basins, where such fossils are continually exposed by erosion, and where alluvial gold-sand has been mined since the Bronze Age.

INTRODUCTION

Beginning in the 7th century B.C., the Greeks began to make contact with the nomadic cultures of Central Asia. Along with gold and exotic goods from the East came folktales about remote lands and their inhabitants. The first written description of the griffin, or *gryps,* appeared in Greece during this period. The griffin, already known in Near Eastern art, was said to have four legs and a strong beak. It was associated with gold-mining and nomads, and later was said to lay eggs in burrows in the ground.

In 1646, Sir Thomas Browne wrote a treatise called *Pseudodoxia Epidemica* (Vulgar Errors), in which he declared that the ancient griffin was merely a symbol, an imaginary composite created to combine the noble attributes of the eagle and the lion. In 1652, however, Andrew Ross disagreed with Browne, arguing that the *gryps* may have been an attempt to describe an unusual animal of Scythia or Cathay. According to Ross, Browne "misrepresented" the ancient writers; they had never claimed that the griffin was a crude conjunction of two animals, "a lion behind and eagle in the forepart." Instead, they said only that the four-legged creature had a beak. Ross pointed out that bats, flying fish, giraffes, marsupials, and dog-headed apes had once sounded like "mixt and dubious" fantasies, yet they later turned out to be real animals (Nigg 1982: 87–94 and refs.). But Ross's voice was drowned out in the "scientific" effort to discredit old travelers' tales. For more than three centuries, then, most scientists, classicists, and art historians have

accepted Browne's declaration that the classical griffin was purely imaginary and symbolic.

Ross's optimism was not rewarded—no living mammal with four legs and a hooked beak was ever found. But he was right to challenge Browne's reading of the ancient sources, which do not portray the griffin as a hybrid like the Centaur, Sphinx, or Minotaur. When we unbraid the strands of this Central Asian folklore, sorting out elements accumulated over a millennium of Greek and Roman retellings, paying attention to geography and chronology, and drawing on the disciplines of geology, archaeology, and paleontology, I believe that a good case can be made for a legend based on a real animal (Mayor and Heaney, in press).

The first and only attempt to explain the griffin legend in terms of prehistoric remains came in 1827, when Adolph Erman, a geologist with an interest in classics, traveled across south Russia and Siberia. He learned that Ice Age woolly mammoth bones, found in great abundance along river banks, were identified by the natives as the remains of colossal bird-monsters killed by their ancestors. Erman suggested that discoveries of such bones in antiquity might have been "the prototype of the Greek story of the Grifons." He remarked that gold-sand lay just beneath the peat that contained the mammoth remains, which seemed to account for the association of griffins and gold (Erman 1848: vol. 2: 87–89, 377–82, Bolton 1962: 84, Costello 1979: 76, Mayor and Heaney, in press, note 2).

Erman's remarks fell into obscurity until 1962, when the classicist J. D. P. Bolton wrote a book about Aristeas, the 7th century B.C. Greek traveler who first described the *gryps* of Central Asia. Bolton called Erman's theory "attractive" and "impressive," but rejected it because the griffin tale originated near the Altai Mountains, far from Erman's mammoth remains north of the Urals (Bolton 1962: 84, 93, 101, 176, Phillips 1955: 161–77). Once again, the idea that the legend might contain a kernel of zoological truth was set aside. Yet a close reading of the ancient sources (as Ross advocated in 1652), along with neglected archaeological, geological, and paleontological evidence, allows us to revise Erman's tentative fossil theory and to satisfy Bolton's geographic objections.

ANCIENT SOURCES, GEOGRAPHY, CHRONOLOGY

Griffin-type animals combining the features of birds and mammals appeared in Near Eastern art as early as 3000 B.C., and they show up in Mycenaean (Bronze Age Greek) art as well. However, we have no way of knowing what kind of folklore, if any, was attached to these creatures. The first *written* description of the griffin that we know of appeared in Greece during the 7th century B.C., when the Greeks began to make contact with the nomadic cultures of Central Asia. Little new information about the *gryps* seems to have been added after the 3rd century A.D. After that, the tradition

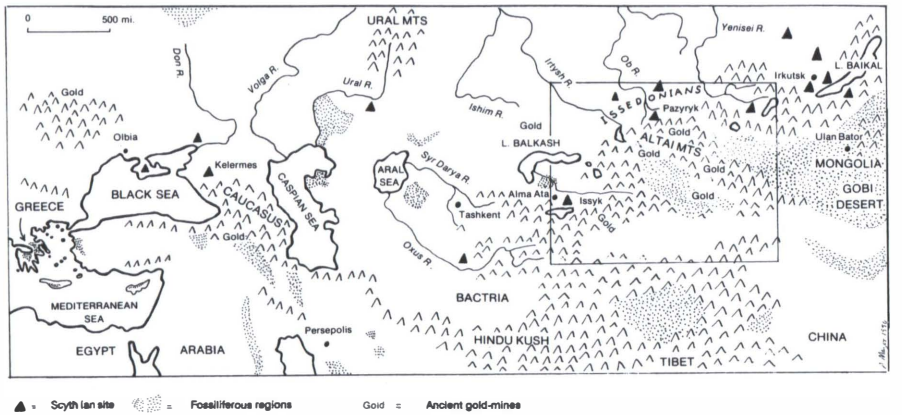

FIG. 1. Locations of Scythian sites, fossiliferous regions, and gold deposits mined in antiquity. (Adrienne Mayor.)

crystallized into what was to become the fabulous medieval griffin with its Christian symbolism. The griffin was popular in Greek and Roman art between about 700 B.C. and A.D. 300, and some of those representations appear to be related to the folk themes preserved in the coeval literature. This thousand-year period of linked, consistent, and thriving written testimonies and related art can be seen as the "natural lifespan" of the Greco-Roman version of griffin lore (Nigg 1982: 53–79, Bartscht 1987, Bliss 1987, Bisi 1965).

The word "griffin" comes from the Greek *gryps* meaning "hooked," as of a claw or beak, and may be related to the Persian *giriften,* "to grip" or "seize" (Liddell and Scott 1940: s.v. *gryps,* Costello 1979: 78). The word was apparently first used by Aristeas, a Greek from the Black Sea area, in his lost epic, the *Arimaspea,* written in about 675 B.C.—around the time of the earliest Greek contacts with Scythian nomads east of the Caucasus. According to the surviving fragments of his work (preserved in works by later authors), Aristeas said that he traveled among the Scythians, going as far as the land of the Issedonians (the easternmost Scythian tribe), where he heard about griffins and Arimaspeans. Issedonian territory formed a wedge, bounded by the Tien Shan and the Altai mountain ranges. Ptolemy, the 2nd-century A.D. geographer, and ancient Chinese sources locate the Issedonians along the old trade routes from China to the West, from the western edge of the Gobi desert to the Dzungarian Gate (Figs. 1 and 2) (Mayor and Heaney, in press; notes 5, 7, 41, Bolton 1962: chap. 4 and 5, Phillips 1955: 161–77, Minns 1913: 104ff, Smith 1856: s.v. *Issedones,* Charriére 1979: 266–67, Hudson 1931: 32–50 and chap. 2 with refs.).

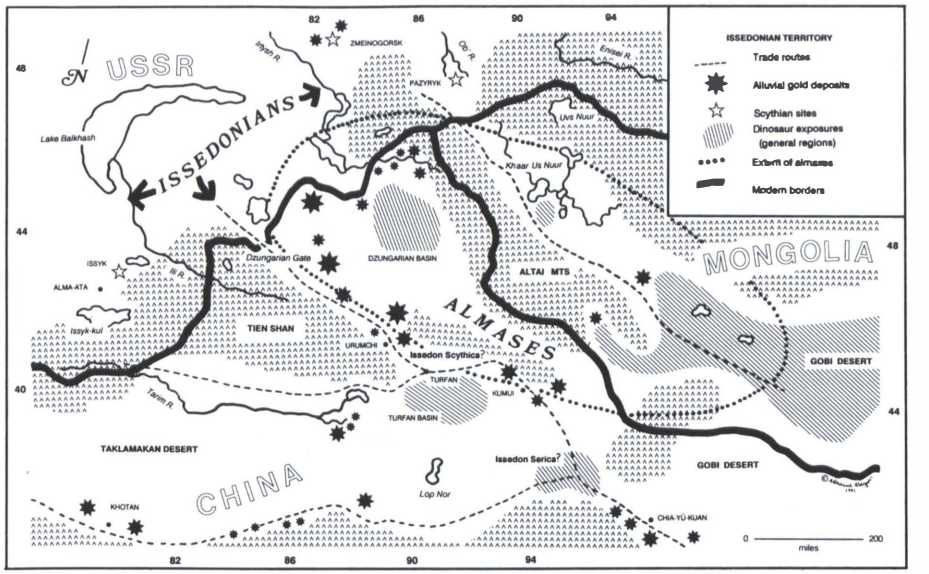

FIG. 2. "Griffin Territory": ancient trade routes; Scythian sites; locales of dinosaur fossil exposures, including *Protoceratops* skeletons, eggs, and young; extent of Almas folklore; and alluvial gold deposits. (Adrienne Mayor, based on information from Dale Russell, Boulnois 1983, Cable 1987, Hudson 1931, Thubron 1989, Weishampel, Dodson, and Omólska 1990, and Dong 1988.)

Modern archaeology confirms that Scythian nomads, as described by ancient Greek sources, flourished in this area during the time of Aristeas up to about A.D. 300, and that trade was ongoing between Scythians and the West (Mayor and Heaney, in press, Davis-Kimball 1991, Basilov 1989, Mongait 1961: 76, 170, Artamonov 1969, Metropolitan Museum of Art 1975, Rolle 1989). A recent linguistic analysis of the Scythian word *arimaspu* (recorded by Herodotus in his discussion of griffins and Arimaspeans) shows that the Arimaspeans can be identified with *almases,* elusive, shaggy, monocular creatures of Mongolian folklore (Mayor and Heaney, in press).

The first surviving ancient work to use Aristeas's *Arimaspea* was written by the Greek playwright Aeschylus, in about 460 B.C. Aeschylus relied on Aristeas for natural details about Central Asia in his tragedy *Prometheus Bound,* set far beyond the Caucasus (Bolton 1962: 45–64, Griffith 1983: 230, 266, Phillips 1955: 163). Lines 790–805 of the play describe a remote, dangerous land reached by caravan routes to the Far East, a land inhabited by nomadic horsemen who prospected for gold. The route crossed desolate plains, the home of fearsome gorgons who turned living things to stone, one-eyed beings called Arimaspeans, and finally, the *gryps* or griffin. Aeschylus

calls these animals "silent hounds with sharp beaks" (lines 803–4). The *gryps* is *not* an eagle, as many have claimed, since Aeschylus was careful to distinguish the *gryps,* which had a beak but no wings, from the winged eagle, *aetos* (line 1,022; and see Liddell and Scott 1940: s.v. *aetos*).

In Aeschylus's play, the god Hermes tells Prometheus that cosmic thunder and lightning and torrential rain will pound the mountain cliff where Prometheus is chained. An avalanche will bury him at the bottom of the ravine, and he will be "held fast by arms of stone" for eons. "You will travel through vast tracks of time, and at last come back into sunlight," a carcass for eagles to ravage (lines 1,015–25; see also Griffith 1983: 266). Using Aristeas's work, Aeschylus evoked a primeval wasteland with bizarre creatures and bodies trapped in stone for eons, a place where nomads seek gold.

The historian Herodotus was a contemporary of Aeschylus. In about 450 B.C., the well-read Herodotus traveled to Egypt, Asia Minor, and Scythia "expressly to seek information" (2.44). He asked people for explanations of unusual stories, and tried to find supporting evidence. He visited the Scythians just beyond the Black Sea, and he quoted Aristeas about lands farther east: "Aristeas says in his work that he went as far as the [land of the] Issedonians. . . . Beyond were the gold-guarding griffins" (4.13). Herodotus points out that the information about "gold-guarding griffins" and "Arimaspeans" who dwell in lands beyond the Issedonians came to the Greeks second-hand, based on what Scythians heard from Issedonians (4.25, 4.27). Of the rich gold deposits of Asia, Herodotus states: "I cannot say for sure how the gold is obtained: some say that one-eyed men called Arimaspeans steal it from griffins. But this I hold incredible (that a race of men would be born with only one eye)" (3.116). Note that while Herodotus rejects the existence of cyclopean *men,* he does not doubt the existence of the *gryps* animal.

The next item of griffin lore comes from a fragment of Ctesias, a Greek physician who lived in Persepolis around 400 B.C. (Bigwood 1989). Ctesias said that Asian gold was hard to obtain because it was "in high mountains inhabited by griffins, a race of four-footed birds, almost as large as wolves and with legs and claws like lions" (Costello 1979: 76, Photius *Bibl.* 72.46b.30).

Pliny the Elder's *Natural History* of A.D. 77 marks the next important point in the tradition. Besides noting the griffin's "terrible hooked beak" (10.70.136), Pliny was the first to mention its long ears (10.70.136) and wings (7.1.10), two features prominent in griffin art. Pliny also referred to "many authorities" besides Aristeas and Herodotus who wrote about the griffins of the gold-mining regions of far Scythia. He summarized: "Arimaspeans are . . . always fighting for gold with the griffins, winged animals whose appearance is well-known. *The griffins toss up gold when they make their burrows*" (7.2.10, my emphasis; see Bolton 1962: 64–65). This last sentence is the first mention of griffin nests.

About a hundred years after Pliny, a scholar named Philostratus wrote a biography of Apollonius of Tyana, a philosopher who was said to have traveled extensively in Asia and India during the end of the 1st century A.D. According to his biographer, Apollonius reported that the rocks in the region of the griffins are "flecked with drops of gold like sparks," which the griffins are able to quarry because of their strong beaks. The griffins "resemble lions in size and strength." As for wings, he claimed that griffins "are not winged as are real birds" but have "webbed membranes" which allow them to make short "hops" in the air during combat (Philostratus *Vita Apollonii* 3.48).

In about A.D. 170, a very knowledgeable Roman named Pausanias traveled throughout the Greek world. Pausanias (1.24.5–7) quoted Aristeas as stating that "griffins fight for gold with the Arimaspeans beyond the Issedonians. . . . Griffins are like lions but with the beak and wings of an eagle" (8.2.7). Pausanias criticized those who believed that griffins are spotted, "Those who love to hear marvelous stories cannot resist adding details, thus ruining the truth by mixing in lies" (8.2.7). He also remarked that in the land of griffins, gold "emerges near or *on the surface* of the earth," which recalls Pliny's comment that griffins scrabble up gold when digging burrows (my emphasis, 1.24.6; see Bolton 1962: 65).

Within a generation after Pausanias, the bare bones of the simple report began to be fleshed out with more vivid details. Here is the version of Aelian, a Roman compiler of natural history in about A.D. 200: "I hear that the griffin has four legs like a lion, with talons as strong as can be. . . . It is winged and the plumage on the back is reputed to be black, with red chest and white wings." Aelian then quotes Ctesias, who stated that "the neck has dark-blue feathers, it has the beak of an eagle, and a head just like artists and sculptors portray." Aelian continues: "Its eyes are fiery. It makes its nest in the mountains, and although it is impossible to take a full-growth griffin, people sometimes capture the chicks. The Bactrians say that griffins guard the gold of those parts, which they dig up and weave into their nests. . . . However, the Indians sensibly deny that the creatures intentionally guard the gold. The truth is that when the prospectors approach, the griffins take fright for their young, and so give battle to the intruders. They never fight lions or elephants, but they can easily overcome any other animal."

Aelian tells how the miners travel, in "armed groups of one or two thousand," to the wilderness of the gold deposits. "Out of respect for the bravery of these animals, they avoid hunting for gold in the day. They approach in the night when they have the most chance of avoiding discovery. Now, the place where the griffins live and the gold is found is a howling desert. The treasure-seekers, waiting for some moonless night, come with shovels and sacks and dig. If the griffins do not notice them, the men reap a double reward, for their lives are preserved and they bring home their cargo of gold . . . rich profit for the dangers they face. But if they are caught in the act it

is all over for them. . . . I am informed that the miners return home after two or three years" (4.27; see Bolton 1962: 65–72).

A thousand years after Aristeas, Aelian's account retains the basic creature: four legs, claws, and a beak, and associated with gold-mining. Aelian's version incorporates later details and adds embellishments. He gives the griffins a more natural motive for aggressive behavior: they defend nestlings, not gold. And Aelian quotes a traveler (Ctesias) who affirmed that the animal was as artists had portrayed it (see Mayor and Heaney, in press: notes 8–14).

Griffins in Art

Scythian art (700 B.C.–A.D. 300) is notable for its depictions of many unknown animals, including griffin-types (Bisi 1965, Bouras 1983, Flagge 1975, Bartscht 1987, Metropolitan Museum of Art 1975, Rudenko 1970, Bolton 1962, Rolle 1980). Scythian tombs of the time of Aeschylus and Herodotus have been excavated at Pazyryk, Issyk, and other sites near the ancient gold-mining areas of the old Issedonian territory; they have yielded hundreds of gold artifacts featuring beaked quadrupeds (Rudenko 1970, Mongait 1961, Artamonov 1969, Rolle 1989, Basilov 1989, Piotrovsky 1987, Davis-Kimball 1991). One quite remarkable discovery was the mummified body of a Scythian chief, whose skin was tattooed with griffins and other known and unknown animals (Fig. 3) (Rudenko 1970: 109–14, 260–66, Rolle 1989: 83–85).

A mirror from the Scythian site of Kelermes, east of the Black Sea, shows a griffin fighting two shaggy creatures, tentatively identified by scholars as Arimaspeans. The mirror was made in about 570 B.C. (Bolton 1962: 5–7, 89–91, Metropolitan Museum of Art 1975: plate 4, Artamonov 1969: plates 29, 33).

In the 600's B.C.—around the time of Aristeas and the first Greek contacts with Scythians—the griffin became a popular theme in Greek art. Bronze griffins in the Archaeological Museum of Olympia and the island of Samos offer typical examples (Figs. 4, 5, and 6). The earliest bronzes show naturalistic, almost reptilian creatures with open beaks, large eyes, forehead knobs, upright "ears," and scaly or crested necks. Wings were usually small and stiff, and the *gryps* was clearly earthbound. Later, the "ears" and knobs became decorative, and the neck was elongated; the wings were realistically feathered and curved (see Bolton 1962: 88–89), but the griffin was still flightless. A few exceptions have a religious context: for example, some artistic depictions show griffins drawing Apollos' chariot (Flagge 1975: 73–75, 125), or carrying a goddess aloft (Strabo 8.3.12).

A portrait of a mother griffin protecting a baby griffin can be seen in the Archaeological Museum at Olympia, Greece, on a bronze relief made in about 630 B.C. (Fig. 7) (Andronicos 1975: 16, plate 48). This early item brings to mind Aelian's later claim that griffins protected their young.

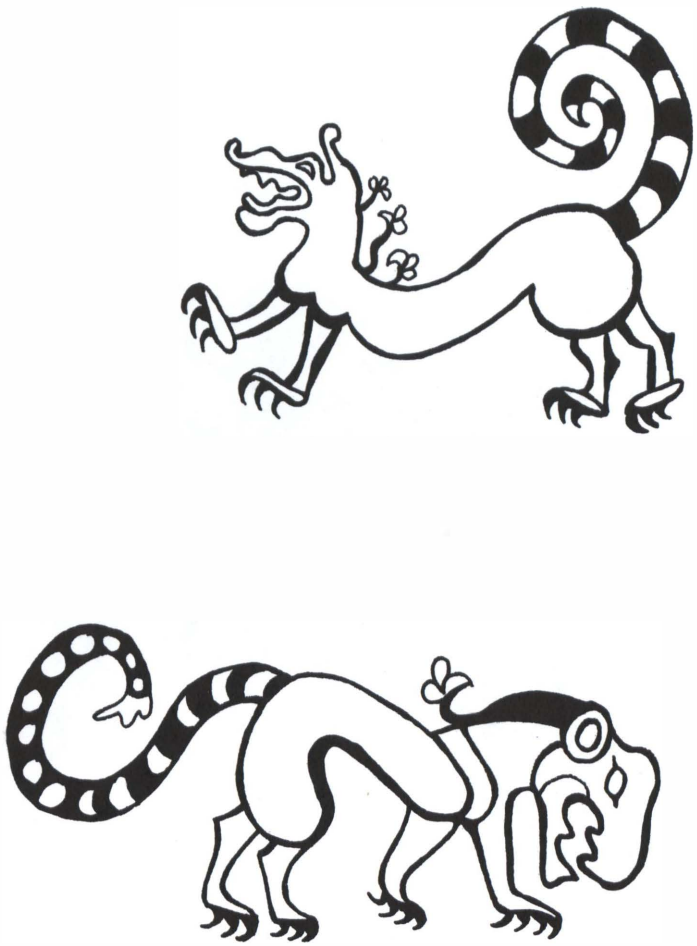

Fig. 3. Tattoos of two unknown animals from a mummified body in a Scythian tomb of mid-400's B.C., excavated by Rudenko at Pazyryk. (Adrienne Mayor, after Rudenko 1970.)

Two Greek vase paintings from the 300's B.C. show nomads on horseback battling griffins (Fig. 8). Bolton points out that "nuggets of gold" are included in the scenes, which appear to be set in a "rough" and "blasted" terrain (Bolton 1962: 37 with refs.).

In about A.D. 300, about a hundred years after Aelian wrote about Asian gold-miners and griffins, the owner of a villa in Sicily commissioned a mosaic. The "Great Hunt" mosaic, located near Piazza Armerina, is a panorama of lions, tigers, elephants, camels, ostriches, and other exotic but real

FIG. 4. Bronze griffin, Olympia, mid-600's B.C. (time of Aristeas).

animals from the far fringes of the Roman Empire. The beasts are all being captured by various methods. At the very end of the panel is a griffin, lured to a trap baited with a man (Toynbee 1982: 27–29, and Fig. 1).

These are only a few examples of thousands of griffins in Scythian, Greek, and Roman art between 700 B.C. and A.D. 300. Combining features described in the art and the literature of this period, we can draw a composite of the griffin's appearance, habitat, and behavior. The *gryps* was a four-legged animal about the size of a lion, with a tail and strong claws, but it had a powerful beak like that of a bird of prey. Despite their wings, griffins did not fly. They had large eyes, and many had upright "ears" or horns, and a forehead knob. There was often a ruff or crest on or around a leathery, plated, scaled, or feathered neck. The griffin made its nest on the ground; it fiercely protected its young. Its territory was the desolate wilderness near the gold deposits of the Altai and Tien Shan mountains and valleys. Griffins

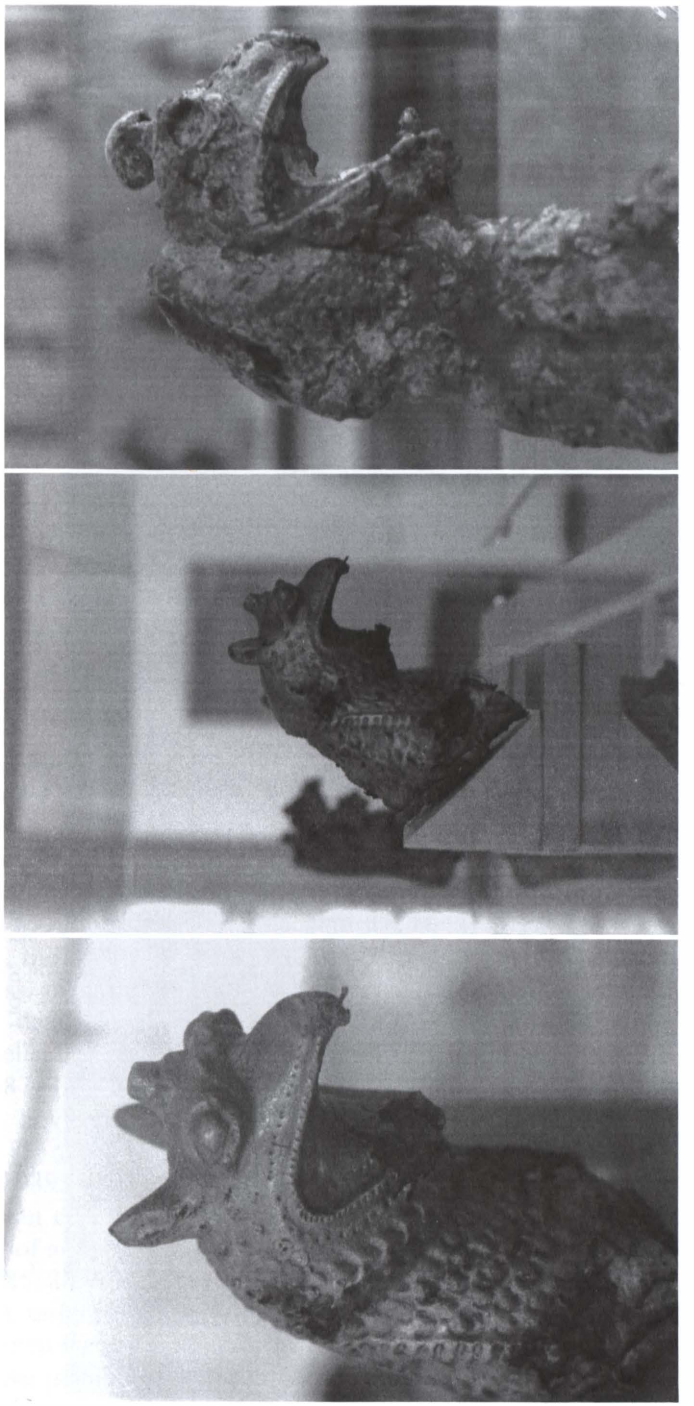

FIG. 5. Bronze griffins, Samos, 700–600 B.C. (time of Aristeas). Left and center photos show views of same head. (Adrienne Mayor.)

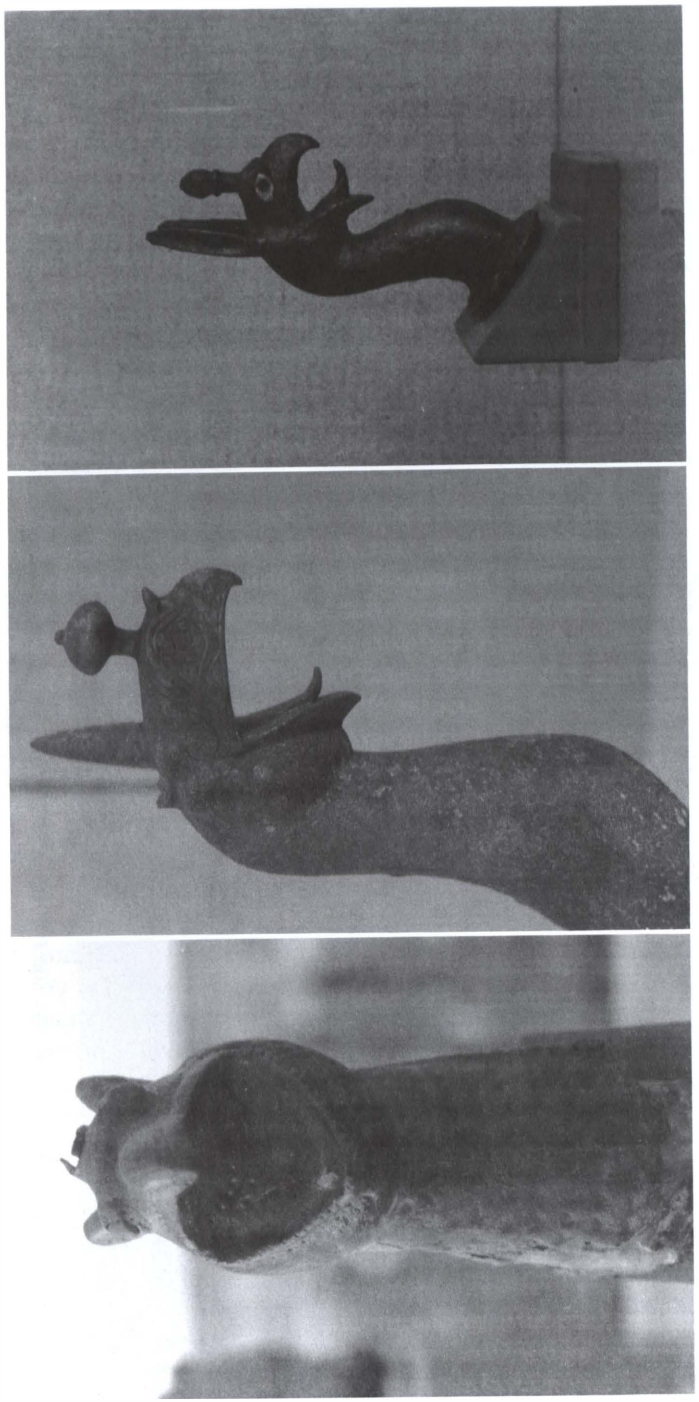

FIG. 6. Bronze griffins, Samos, 700–600 B.C. (Adrienne Mayor.)

FIG. 7. Bronze mother and baby griffin, Olympia, mid-60's B.C. (After Bouras 1983.)

were associated with nomadic horsemen, one-eyed beings, and creatures somehow turned to stone.

PREVIOUS IDENTIFICATIONS OF THE GRIFFIN

Many hypotheses have been proposed for the origin of the griffin since Ross and Erman attempted to account for the legend's zoological features. As already noted, art historians and classicists usually consider the griffin to be an imaginary composite of real animals. Their main interest is in tracing how the image spread from Asia to the Mediterranean (Charriére 1979, Piotrovsky 1987: 17–18, Phillips 1955: 172). Since Browne's 1646 treatise, the griffin has been interpreted as a symbol representing vigilance, swiftness, the sun, the sky, guardianship, generosity, Apollo, Zeus, royalty, loyalty, death, and even the difficulty of mining gold (Bartscht 1975, How and Wells 1928: Vol. 1: 307, Nigg 1982: 14–18). Art historian Charriére (1979: 87–95) speculated that the "delirium of [animal] forms" in Scythian art may have arisen from hallucinations induced by hashish, which Herodotus (4.73) says, and modern excavations prove, was used by Altai nomads (Rolle 1989: 93–94, Rudenko 1970: 197, 199, 284–85).

Bartscht calls the belief in griffins an antidote to "the intellectual desiccation" of an overly rational society, or a "mirror of our own dual nature" (1987: 85–86, 99). Others identify the griffin as a garbled description of a real but unfamiliar living animal. For example, ancient historian Minns maintained that the little *baibak* (steppe jerboa) or the *suslik* (steppe squirrel) may have inspired the legend because of their "watchfulness" and habit of

FIG. 8. Vase painting of 300's B.C., from Caria, Turkey, showing nomad on horseback fighting griffin in "wilderness" landscape; gold nugget above griffin. (Adrienne Mayor, after F. Winter, in *Mitteilungen des deutschen archaologischen Instituts. Athenische Abteilung,* 1887, plate 11.)

burrowing in gold-sand (Minns 1913: 6, 113, Bolton 1962: 84, cf. Cable 1987: 97). And Valentine Ball concluded in 1884 that the *gryps* was a "tolerably accurate description" of a large dog, the ferocious mastiff of Tibet (Costello 1979: 78, cf. Cable 1987: 29; for photograph see Andrews 1926: xix).

Some have suggested that the griffin arose from vague collective memories of flying reptiles or other long-extinct prehistoric species, an idea proposed in the 1920's by German paleontologist E. Dacqué (Carrington 1957: 76, Ley 1948: 51, Lum 1952: 24). But as Costello (1979: 81) points out, this "romantic and unscientific" notion assumes that humans coexisted with relict dinosaurs. As for Erman's mammoth bone theory, Costello follows the classicist Bolton in rejecting the idea: The mammoth "bones . . . found . . . on the banks of Siberian rivers" are "a long way from the Altai Mountains and the [nomads] that Aristeas was describing" (Costello 1979: 76).

Costello (1979: 80–81) believes that the griffin must have been based on a "common feature of [the nomads'] lives." His candidate is the Central Asian lammergeier, a bearded vulture with a ten-foot (3 m) wingspan. Others have claimed that the griffin was really an eagle or other raptor (Bartscht 1987: 91, 96, cf. Jennison 1937: 115, Griffith 1983: 231). However, as we have seen, Aeschylus, the first surviving author to describe the *gryps,* clearly distinguished it from a bird of prey (*Prometheus Bound,* lines 803–4, 1,022).

Furthermore, the Scythians, Greeks, and Romans all portrayed raptors realistically in their art, and Greek and Roman texts described these birds accurately (Pollard 1977: 76–86, 186–87, with ancient refs. and plates 3, 22, 23, 25, Artamonov 1969: plate 118, Piotrovsky 1987: plates 70, 100). It is known that eagles were trained to hunt by steppe nomads since antiquity (Basilov 1989: 149–51). It is unlikely that such familiar birds of prey would generate an image of a flightless quadruped. As Ross noted in 1652, it is the combination of the eagle-like beak with four legs that is so striking.

But Costello's insight, that the griffin must have been based on a real aspect of the nomads' lives is significant, especially when we compare the griffin to imaginary animals of Greek myth. The *gryps* is not a simple hybrid or obvious composite like the half-human, half-animal Centaur, Minotaur, and Sphinx or like Pegasus, the flying horse. In fact, the griffin plays no role in any surviving Greek myth. And unlike the other mythical monsters which are usually singular creatures with a divine genealogy and a name, griffins are generic creatures of a specific region. They do not interact with mythical heroes (Phillips 1955: 172), but are said to be encountered by real people in a distant land rich in gold. The accounts are based on hearsay: no writer claims to have actually seen a live griffin (Bartscht 1987: 90).

Contrary to what might be expected, the fragments of Aristeas's original report quoted by other authors lack sensational folkloric details. In fact, the dissimilarities between the *gryps* and Greek monsters, the consistency of the image and written narratives over centuries, and the natural details about location, nests, and gold added by later writers and travelers all suggest that the griffin legend was based on something observed and verified over time by various people in a specific landscape.

Those who study the griffin legend have tended to conflate anachronistic, fabulous medieval embellishments with the original 7th-century account of Aristeas, which remained relatively unembellished up to the time of Aelian. As we saw, the idea that fossils played a part in the legend has been rejected (Bolton 1962: 84, Carrington 1957: 76–77, Lum 1952: 48–50, Costello 1979: 76). Yet no griffin scholar seems to have considered paleontological evidence in the region where Aristeas learned about griffins from the Issedonians. Indeed, the importance and frequency of fossil discoveries in antiquity has been underestimated (Phillips 1964, Ley 1968: 191–92, Mayor 1989, Mayor and Heaney, in press: notes 25–30). Historical examples of artistic reconstructions of fossils and folklore based on prehistoric remains can serve as models in tracing the griffin legend.

FOSSIL DISCOVERIES IN ANTIQUITY

Numerous Greek and Roman texts refer to the discovery of fossil remains (Bromhead 1945, Mayor, in preparation). Some fossils were exposed by earthquakes and erosion; others were deliberately dug up. The Greeks and

Romans regarded large unfamiliar bones as either the remains of mythical heroes, monsters, and giants, *or* skeletons of extinct animals or men of the remote past (Halliday 1975: 208–11, Buffetaut 1987: chap. 1). Theories of evolution and extinction were developed in the 6th century B.C. by Anaximander and Xenophanes, who had observed marine fossils embedded in rock (Lloyd 1976: vol. 2: 36–37 and refs., Osborn 1929: 39–102, Phillips 1964). In the mid-400's B.C., Empedocles speculated about evolution based on the bones of prehistoric elephants, a common fossil in the Mediterranean (Matthews 1962: 144–45, Wendt 1968, Reese 1976).

Herodotus apparently recognized fossils as traces of extinct life-forms (Lloyd 1976, vol. 2: 36–37, 66–67, Carrington 1958: 48). For example, when he investigated the folklore of winged serpents in Arabia, he says he was shown "countless bones and backbones of serpents, many heaps of vertebrae, great and small" in the walls of a narrow mountain pass (2.75)—in an area that is fossiliferous. Elsewhere, Herodotus relates that a man digging a well at Tegea (Peloponnese) found a huge skeleton (1.67–69; see also 9.83).

Plato, Aristotle, and Theophrastus discussed extinction and changes in landforms based on observations of fossils (Plato *Critias* 110d–111e, Phillips 1964 with refs., cf. Ovid *Metamorphoses* 1 and 2). The Epicurian philosopher Lucretius (ca. 60 B.C.) wrote about the inviability of "composite monsters" and about the evolution and extinction of ancient animals (2.700–729, 4.722–76, 5.772–924). According to the Roman historian Suetonius, the Emperor Augustus built a museum for the "monstrous bones of extinct sea and land creatures," which some people called "giants' bones" (*Augustus* 72.3). The Greek biographer Plutarch (ca. A.D. 80) told of big bones exposed on the island of Skyros in about 475 B.C.; they were shipped to Athens for display (*Theseus* 36 and *Cimon* 8). Plutarch also discussed the colossal bones commonly seen on Samos, an island rich in fossils. In antiquity, some believed that Samian fossils were the remains of ancient monsters called Neïdes; others said they were elephants who died in long-ago earthquakes (Plutarch *Greek Questions* 56, Halliday 1975: 207–9; for paleontology of Samos: Solounias 1981, Brown 1927, Mayor 1989: 21).

Pliny the Elder gives the exact dimensions of huge skeletons discovered throughout the Roman Empire (e.g., a 69-foot [21-m] skeleton revealed by an earthquake in Crete, 7.16.73–75; bones from a 60-foot [18-m] skeleton used to construct houses in northern India, 9.2.7; a 40-foot [12-m] skeleton, 9.4.10–11). Pausanias describes many instances of the discovery and transport of colossal bones, said to belong to heroes, giants, sea monsters, elephants, or ancient men. Pausanias saw such relics displayed in temples and public buildings, and he tells how a boat carrying fossils was lost at sea (1.17.6, 1.28.7, 1.35.4–6, 2.10.2, 3.3.6, 3.11.10, 4.32.3, 5.12, 5.13.4–6, 8.9.3, 8.29.3–4, 8.32.5, 9.21.4).

Chinese chronicles describe the export of fossils and mammoth tusks to

Rome in A.D. 166 (Hudson 1931: 89–90); other Chinese sources of the 2nd century B.C. and the 3rd century A.D. describe the excavation of "dragon bones" (Oakley 1975: 40, Dong 1988: 9). In northern India, Apollonius of Tyana came across "a great many heads of dragons" enshrined in a holy city (Philostratus *Vita Apollonii* 3.9; see also "giant remains," 5.16). Philostratus lists a dozen instances of gigantic bones exposed by earthquakes, erosion, and ploughing around the eastern Mediterranean; they were carefully measured (*Heroicus* 8.1–14; Jeffrey Rusten, translation forthcoming). Aelian refers to the giant bones of extinct beasts displayed in Samos (17.28), and reports that a monster's bones were discovered after a great fire on the island of Chios: "From these," he says, "people were able to guess how large and how awful the brute was when still alive" (16.39).

These accounts show that, when the ancients encountered fossils, they tried to visualize the creatures in life, and they often deliberately collected and measured unusual remains. Recent archaeology reveals that fossils were stored with other valuables in treasuries, temples, and shrines. Modern evidence also shows that fossil bones, shells, claws, horns, eggs, and teeth of exotic or extinct animals were trade items in antiquity (Buffetaut 1987: chap. 1, Oakley 1965, Reese 1976: 92, 1982, 1984, Boessneck and von den Driesch 1979, 1981, Rapp and Aschenbrenner 1978: 69–70, Rothenberg 1988: 266–68, Kennedy 1976, Hudson 1931: 28–29, 89–90; for maps of trade routes see Cable 1987, Hudson 1931: 37). For example, at Egyptian shrines of the 13th century B.C., excavators found tons of fossil bones and ivory dedicated to the god Set (Oakley 1975: 42–44). A classical shipwreck discovered off Cyprus in the early 1970's may have been carrying a load of fossils (David Reese, personal communication 1989).

Fossils and Folklore

It has long been recognized that legends of dragons, giants, and monsters occur in fossil-rich areas (Thenius 1973: 34–39, Carrington 1957: 71, chap. 7, 1958: 49–54, Ley 1948: 42–54, Wendt 1968, Sutcliffe 1985: chap. 3, Buffetaut 1987, Solounias 1981: 18–19, cf. Cable 1987: 97). The Cyclops legend is perhaps the best-known model of the way fossils can generate both rational speculation and folklore. Empedocles, writing in the 400's B.C., was the first scientist to associate fossil elephant skulls (a common fossil in the Mediterranean) with the Homeric legend of the Cyclops (Reese 1976: 93). Austrian paleontologist Othenio Abel pointed out in 1914 that the elephant's large nasal cavity would naturally lead someone who looked at an elephant skull (but had never seen a live elephant) to visualize a one-eyed giant (Sutcliffe 1985: 27, Thenius 1973: 36–37, Buffetaut 1987: 5).

Another oft-cited example of the interaction of folklore and artistic reconstruction was documented in the late 1500's. In about A.D. 1300, a colossal skull was unearthed by miners near Klagenfurt, Austria (Mayer and

Heaney, in press: note 32). In 1590, sculptor U. Vogelsang made a life-size reconstruction of the head—guessing at the rest of the body—for the town square. A century later, a statue of a standard folklore giant with a club was put up near the "dragon," which was now said to have once ravaged the town. The original skull has since been identified as that of an Ice Age woolly rhinoceros (Thenius 1973: 37–38, Sutcliffe 1985: 30–31).

Paleontology and Geology in Griffin Territory

According to Herodotus, "It was among the Issedonians themselves that the strange tales" of "gold-guarding" griffins originated. To test the fossil theory, we need to locate the important gold deposits exploited by these nomadic peoples, and then determine whether ancient gold-seekers would have been likely to encounter fossil-bearing rock where prehistoric remains could have inspired the image of the *gryps*.

The extreme climate, remote and difficult terrain, long history of border wars, ongoing unrest, Cold War tensions—not to mention myriad name and spelling changes (e.g., Dzungaria, Sungaria, Zungaria, Junggar)—have all contributed to the paucity and obscurity of mineralogical and paleontological information about the Issedonians' old territory, on the border between China and the former U.S.S.R. The Altai and Tien Shan mountain ranges contain extremely abundant gold-sand which constantly erodes from the massifs down into basins below.

Geomorphologist Sheldon Judson clarifies the geological processes: auriferous sand has an igneous origin in the "granites of the high mountains; placer gold in a lighter quartz matrix is continually washed down . . . and deposited in the alluvial basins and gullies below. Besides alluvial deposits of gold nuggets, flakes, and dust, colluvial (gravity on slopes) and even aeolian (wind) transport of gold can occur in the desert" (Judson, personal communication 1991). Modern travelers report that sandstorms in the area can transport pebbles the size of U.S. silver dollars; ancient and modern writers have described the collection of precious minerals exposed after violent winds in this region (Pliny 37.17.65, Cable 1987: 24, 62–63, 79, 83, 201, Mayor and Heaney, in press: note 35).

Spectacular amounts of gold artifacts come from Scythian tombs along the slopes of the Tien Shan and Altai ranges; these mountains and their alluvial basins were the source of Scythian gold. The exact locations and volumes of gold deposits in China and the former U.S.S.R. have traditionally been closely guarded secrets, but place-names (e.g., *altai* means "gold") and the writings of travelers, archaeologists, and even Western intelligence sources indicate that gold-sand is plentiful and has been mined since antiquity in the gullies skirted by the caravan routes. Cable (1987: 23, 28–29) gives other place-names meaning "gold." Russian gold-seekers of the 1800's, guided by old place-names, exploited the detritus of ancient Scythian gold-mines. Between 1860 and 1950, Russian archaeologists discovered more than a hundred ancient Altai gold-mines worked since about 1500 B.C. (see Fig. 2). One mine even contained the skeleton of a Bronze Age miner whose leather bag still held gold (Rolle 1989: 52–53, Phillips 1955: 171, Hudson 1931: 43, Boulnois 1983, Johnston 1850: s.v. *Altai*, Stein 1981: vol. 1: 192–94).

Some of the world's richest fossil beds are located in the sedimentary cliffs and badlands along the old caravan routes across the Gobi, Turfan, and Junggar deserts between the Altai and Tien Shan ranges (see Fig. 2). Some 13th century Chinese travelers mentioned supernatural creatures, "fields of white bones," and "heaps of very hard and bright white stones like bones" in these deserts (Mayor and Heaney, in press: note 39). Ancient gold-seekers may well have been the first to encounter the eroding fossil remains, since archaeology shows that the auriferous sand of this region has been mined since the Bronze Age (cf. Mayor and Heaney, in press: note 32).

Roy Chapman Andrews was the first modern paleontologist to investigate the fossils of the Gobi desert in the 1920's, after he heard local folklore about dragon teeth and bones (Andrews 1926: 192–93, Colbert 1971: 225, cf. Sutcliffe 1985: 30–34). Andrews's Central Asiatic Expedition (sponsored by the American Museum of Natural History) traveled over ancient caravan routes to a blasted landscape where they discovered—on the surface or partially embedded—bones of Late Cretaceous dinosaurs, ranging from newly-hatched babies to adults. Many of the skulls had prominent beaks, knobs, and distinctive head frills, and were preserved in full articulation with four-limbed skeletons.

The windswept region had once been a vast nesting ground of the dinosaur *Protoceratops*. Shallow depressions still held clutches of their fossilized eggs and young. The eerie panorama of throngs of strange animals was awesome to the team. Andrews wrote that the bones were "strewn over the surface almost as thickly as stones" and that the desert was "almost paved with bones." In one afternoon, each team member found a dinosaur skull; in two weeks the group gathered more than a ton of fossils, and in two summers over one hundred *Protoceratops* were excavated (Andrews 1926: 180, 192–93, 222–23, 227–28, Colbert 1971: chap. 8).

Excavations continued sporadically in the deserts. Between 1946 and 1949, Soviet paleontologists collected 120 tons of fossils in the Gobi. In the period 1968–84, Polish-Mongolian teams located more extremely rich dinosaur exposures in the area. In 1987, a Canadian-Mongolian team began work along the Chinese-Mongolian border, also finding numerous complete protoceratopsid fossil skeletons with nests, eggs, and young, and huge isolated claws up to 28 inches (71 cm) long from other species. Since the 1950's, Chinese paleontologists working in the Junggar and Turfan basins have found very rich dinosaur exposures, yielding Psittacosaurs, "parrot-beaked" dinosaurs (Colbert 1971: 250, Kielan-Jaworowska 1968–84, Weishampel, Dod-

son, and Osmólska 1990: chap. 3, Dong 1988, Russell, personal communication 1991, Sutcliffe 1985: 30–34, Norman 1985: 13, 49, 61, 128–33, 150–51, 169, Mayor and Heaney, in press: note 34; also featured in the 1991 NOVA television program *The Hunt for China's Dinosaurs,* WGBH/PBS).

In these windswept dunes, alluvial depressions, and sandstone badlands of the Gobi, Junggar, and Turfan, fossils are constantly being revealed by the same forces of erosion that bring the gold down from the mountains. For excellent photographs of the terrain, see Thubron (1989), Dong (1988), Kielan-Jaworowska (1968–84). Dale Russell, a leading dinosaur theorist and co-leader of the Canadian-Chinese Dinosaur Project, notes that "the extreme aridity and tenuous plant cover" make it possible to see fossils on the surface: the shapes of the skulls and skeletons are "very obvious" and the rock is soft, making collection of partially embedded bones and eggs fairly easy. His colleague, Philip Currie, points out that the fossils stand out because they are "a different color than the rocks" (Russell and Currie, personal communications 1991).

Over three days in the summer of 1992, two American travelers in the Gobi found a *Protoceratops* skull and an entire skeleton. "Only the beak was protruding"; they uncovered enough of the body "to see [that] the entire animal was there . . . in a standing position" (*Bozeman Chronicle* 1992). As Russell and Currie state, "there is every reason to believe that ancient nomads would have seen dinosaur bones" in the territories known to the Issedonians (Mayor and Heaney, in press).

The most common remains are of the 7-to-8-foot (2.1–2.4-m)-long *Protoceratops* (*ceratops* means "horned face") and the smaller "parrot-beaked" Psittacosaurs, both of which combine features of a mammal and a bird of prey (Figs. 9, 10, 11). As noted, the powerfully-beaked and frilled skulls of protoceratopsids are often found attached to complete four-legged skeletons. The protoceratopsid beaks, large nostrils (perhaps mistaken for eyesockets?) and eyesockets, dorsal frills or shields (and the distinctive skulls, knobs, beaks, and giant claws of other species found in these sites) may explain the features of the archaic images of the *gryps,* as well as other unidentified Scythian animals. For *Protoceratops* features see Norman (1985: 48, 49, 53, 61, 122, 127–33, 169).

To ancient observers, the presence of adults, nests, eggs, and young may have suggested scenes of animals defending hatchlings. It is interesting that the habits of the *gryps* as imagined by the ancients anticipated some of the most recent theories about dinosaur behavior: active and agile rather than sluggish and reptilian, nurturing and protection of young (Weishampel, Dodson, and Osmólska 1990: 40–41, 578–99, 616–18).

The fossiliferous sandstone formations are prominent landmarks along the ancient caravan routes, and would have provided shelter and game for travelers and gold-seekers (Mayor and Heaney, in press: notes 34, 35). Ac-

FIG. 9. *Protoceratops* skull. (After Norman 1985.)

cording to Issedonian-Scythian folklore, griffins "guarded" gold; the proximity of the fossil exposures in the badlands and the gold deposits in the gulleys accounts for the ancient association of gold and the *gryps.* Dinosaur remains observed by prospectors may have inspired the notion of monsters "guarding" the approaches to gold, and chance finds of wind-blown gold dust near or actually in "griffin nests" would have reinforced the idea. Knowledge about real birds which collect shiny objects (Pliny 37.54.146, 37.39.149) might have been extrapolated to the *gryps*/griffin, which had a bird's beak and laid eggs in nests. Wings may have been added to enhance the creature's bird-like attributes, to indicate "divine" qualities, or to account for the fossils' odd neck frills.

The Issedonian tale about the fossil remains may have gone something like this hypothesized narration: "To reach the gold, it is a long, difficult journey through a vast, dangerous wilderness surrounded by high mountains. In the foothills and gullies, we sometimes find gold nuggets right on the surface of the sand. As we pass the red rocks of the desert, we see many strange creatures who have all perished. Only skeletons remain—all turned to stone. We find nests with eggs and babies, also turned to stone. Sometimes

FIG. 10. Partially embedded *Protoceratops* skull excavated by Ray Chapman Andrews in Mongolia, 1923. (From Chester A. Reeds, ed., *The New Conquest of Central Asia: A Narrative of the Explorations of the Central Asiatic Expeditions in Mongolia and China 1921–1930.* New York: American Museum of Natural History, 1932.)

we find huge claws as big as drinking cups. These monsters must be fierce and powerful, like lions, with horrible talons, but they are not like any four-legged beast we know. They have strong eagle-like beaks, great staring eyes, and parts like horns or pointed ears. Some seem to have wings or crests. None of us ever hopes to meet a live one!"

The Issedonian folklore about the life and fate of the remarkable animals they came across or heard about near the gold-prospecting areas must have been passed along to neighboring Scythian tribes—reaching the Black Sea Scythians through a series of seven interpreters, according to Herodotus (4.24). Once it reached Greece and Rome, such folklore was almost certainly associated with images of beaked quadrupeds already known from Near Eastern art; popular Scythian representations of griffins reinforced the tale. As trade expanded with Central Asia, the modest set of folk motifs attached to the Asian *gryps*-animal was probably kept vital by contemporary travelers who verified the striking image of the griffin by direct observation of fossils, hearsay, and information about desert gold-mining.

Prehistoric remains of extinct animals are not the universal key to the origins of all legendary creatures, and certainly imagination and symbolism play important roles in the folklore of unknown animals. However, in the case of the classical griffin, it is proposed, based on the substantial evidence

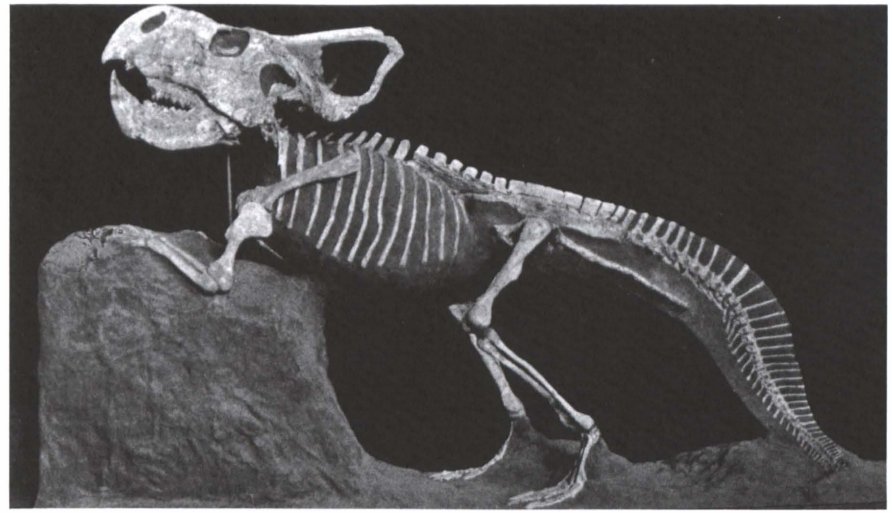

FIG. 11. Skeleton of *Protoceratops andrewsi,* Mongolia. (Source same as Fig. 10.)

reviewed above, that fossils of protoceratopsid dinosaurs were at the heart of the evolution and perpetuation of this ancient cryptid.

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SOME ECOLOGICAL NOTES ON REPORTED LARGE, UNKNOWN ANIMALS IN LAKE CHAMPLAIN

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ABSTRACT: The existence of large, unknown animals known generically as "Champ" in Lake Champlain, in the northeastern United States, is indicated by isolating a behavioral pattern, nocturnality, associated with the animals. While the animals are moving, their bodies undulate vertically. Reports of multiple humps are likely to be misidentifications of multiple one-humped individuals swimming in line. The animals possibly hibernate in the ice season, and migrate within the lake during ice-free seasons. Based on inferred patterns of behavior, appropriate methods for further investigation are discussed.

INTRODUCTION

Large, unknown animals known generically as "Champ" are allegedly inhabiting Lake Champlain in the northeastern U.S.A. (Zarzynski 1988a). There are now over 300 reported sightings of these animals. In an attempt to establish their existence, the Lake Champlain Phenomena Investigation (LCPI) has conducted long term investigations since the beginning of the 1980's (Zarzynski 1982, 1983, 1984, 1985, 1986, 1987, 1988b, 1989, 1990). Despite the efforts made by LCPI and other investigators (Smith 1984, 1985, 1986, Smith and Konrad 1987), no firm evidence has been thus far obtained.

The fact that nearly ten years of investigation has failed to prove that such animals exist could be interpreted as evidence of their non-existence. Careful analyses of existing sighting reports, however, indicate that such large unknown animals, do in fact, inhabit the lake. This paper reviews some ecological factors brought to light by such analyses. The term "ecological" is used here in a broad sense, and includes seasonal migration, behavior, locomotion, and even morphology. In terms of reported behavioral patterns, appropriate methods for future field investigations are also discussed.

NOCTURNAL BEHAVIOR

Joseph Zarzynski of LCPI has compiled and published Champ sighting reports from 1609 to 1990 (Zarzynski 1988a: 154–205, 208–16, 1988b: 74–77, 1989: 70–71, 1990: 80–81). Fig. 1A shows the distribution of sighting times in these reports. Since not every sighting report includes the exact time of the event, only cases which provided specific times have been included; i.e., sightings in "late afternoon," "dusk," "about midnight," etc. were excluded. In cases where sighting time is given as a range, e.g., "7:00 to 9:00

p.m.," or as more than one time, e.g., "7:00 or 8:00 p.m.," the midpoint of range or times was plotted.

Contrary to expectation, daytime sightings are relatively unrepresented. Sightings increase after about 4:00 p.m., and take place most frequently between 7:00 and 8:00 p.m., just before sunset at Lake Champlain in summer (most sightings take place in summer). This is an intriguing pattern in terms of the expected behavior of eyewitnesses. If the animals are active throughout the daytime, more sightings should have occurred then rather than during dusk simply because there are more people active around the lake in the daytime. The steady increase of sightings from late afternoon toward sunset indicates that the animals are nocturnal.

The decrease of sightings after sunset is probably a result of the low visibility of the lake surface in darkness, and of few observers being present on or near the lake. Similarly, some daytime sightings can be interpreted as the result of anomalous behaviour by nocturnal animals, and disturbances of resting animals by approaching boats. Nocturnal behavior would explain why these animals are so elusive, despite the probable presence of a breeding colony in the lake. It should be noted that the distribution of sighting times is quite different from that of sighting times of similar unknown animals (Nessie) reported in Loch Ness, which is shown in Fig. 1B.

LOCOMOTION AND MORPHOLOGY

There are two sighting reports indicating that the animals swam like a snake, i.e., horizontal undulation of the body. However, the majority of reported sightings that include a description of locomotion (n = 8) indicate vertical undulation of the body. Six other reports state that the body undulated but without specifying whether it was vertical or horizontal (Table 1). Bonnie Clonan, of Port Henry, New York, who is encouraging local people to report sightings of the animals, has been informed by numerous eyewitnesses that the body undulated vertically (Bonnie Clonan, personal communication 1991). Thus, vertical undulation while swimming is thought to be a genuine observation. A report describing swimming "like a snake" is likely to be a misidentification or a locomotor misdescription. Vertical undulation while swimming is a characteristic locomotor pattern of modern aquatic mammals, while horizontal undulation is that of modern reptiles and amphibians. It has been argued, however, that the bodies of plesiosaurs (Halstead 1989: 38–39) and some other Mesozoic reptiles (Buffetaut 1983) undulated vertically while they were swimming.

A number of reported sightings state that the animals had "humps" on their backs. The number of reported humps on a single animal varies from one to more than ten. For reasons given below, however, it is probable that the animals have only one hump. Reports of multiple humps might be explained by multiple individuals swimming in line. The most decisive

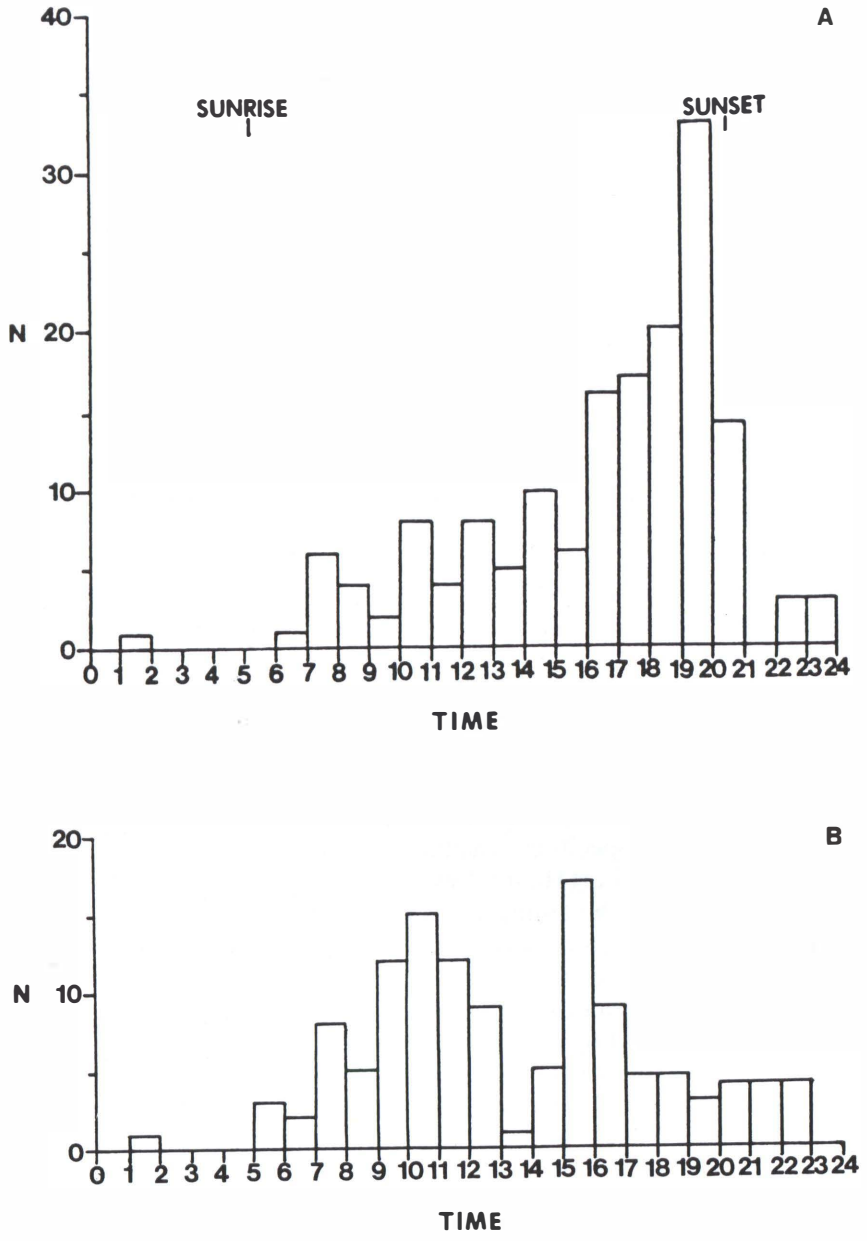

FIG. 1.—Sighting reports of large, unknown animals in Lake Champlain (A), and similar reports from Loch Ness (B), relating to time of day. (After Zarzynski 1988a, 1988b, 1989, 1990, and Mackal 1980.) Sunrise (5:11 to 5:38) and sunset (20:19 to 20:41) in July at Burlington, Vermont (United States Nautical Almanac Office 1977), are indicated.

TABLE 1.—Reported undulation during locomotion of large, unknown animals in Lake Champlain. (Based on Zarzynski 1988a, 1989.)

Sighting no.	Date	Description of locomotion
Vertical undulation		
76	October, 1961	"moving up and down"
123	1975	"moved up and down"
162	July 2, 1981	"moved up and down like a dolphin or porpoise"
170	August 15, 1981	"humps said to be rising and falling as it moved"
174	August, 1981	"swam in an up and down fashion"
263	June 29, 1985	"undulated 'similar to movement of caterpillar'"
268	August 5, 1985	"moving through the water in an up and down motion"
4	August 5, 1989	"appeared to descend in a stepping motion under the water"
Vertical or horizontal undulation		
13	August 30, 1878	"sinuous and undulating"
193	June 14, 1883	"undulating through water"
199	July 2, 1883	"humps undulated"
229	Summer, 1947	"moved in undulating fashion"
252	July 21, 1984	"undulating"
277	June 3, 1986	"3 to 4 dark humps undulating"
Horizontal undulation		
38	July, 1887	"undulations of a snake's body"
88	July, early 1970's	"swam like a snake"

evidence that indicates this possibility is the Mansi photograph, which shows only one hump present behind a head-neck rising out of the water.

In addition to this evidence, there are several reports that specifically mention sightings of two or more animals together (Nos. 186, 192, 194, 195, 201, 206, 208 and 262 in Zarzynski 1988a: 194–212; No. 4 in Zarzynski 1989: 70). One report states: "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" (Zarzynski 1989: 70). Another states: "One 15 ft. long hump, then a second hump appeared which Hall [an eyewitness] believes was another Champ" (Zarzynski 1988a: 212). Because of the misidentification of multiple humps as belonging to a single animal, the sizes of the animals are frequently overestimated in sighting reports.

SURVIVAL IN THE ICE SEASON

In the winter, but mainly in February and March, the surface of Lake Champlain freezes over, though in warm years the entire surface is not frozen. On the other hand, that the animals are air-breathing is most probable

FIG. 2.—Barrage Chambly on the Richelieu River, in the town of Saint-Jean-Sur-Richelieu, southern Quebec, Canada. Any large animal moving between Lake Champlain and the sea would have to negotiate this obstruction. Photo taken in September, 1991. (Yasuchi Kojo.)

because a number of eyewitness reports state that the animals were either seen or heard blowing through their nostrils (Nos. 52, 216, 217, and 223 in Zarzynski 1988a: 164–205; No. 9 in Zarzynski 1988b: 77; No. 3 in Zarzynski 1989: 70). Four possibilities have been proposed to account for how the animals survive the ice seasons: (1) they migrate from the lake to the Atlantic Ocean via river systems; (2) they make breathing holes through the ice; (3) they stay under natural ice openings (or in underwater caves having air circulation), from where short-distance feeding forays can be made; and (4) they hibernate in underwater caves having air circulation (Zarzynski 1988a: 105–6, 138–39).

The first possibility can be rejected outright, unless one postulates locomotion over land. There are two dams and many rapids in Saint-Jean-Sur-Richelieu, southern Quebec, along the Richelieu River that connects Lake Champlain with the St. Lawrence River and eventually with the Atlantic Ocean. One of the dams, Barrage Chambly, is an artificial fall of about 15 feet (5 m) in height (Fig. 2). The other dam, Barrage Fryers, has many watergates. The depth of the rapids in most parts appeared to be shallower than 3 feet (1 m) when the author visited there in September, 1991 (Fig. 3).

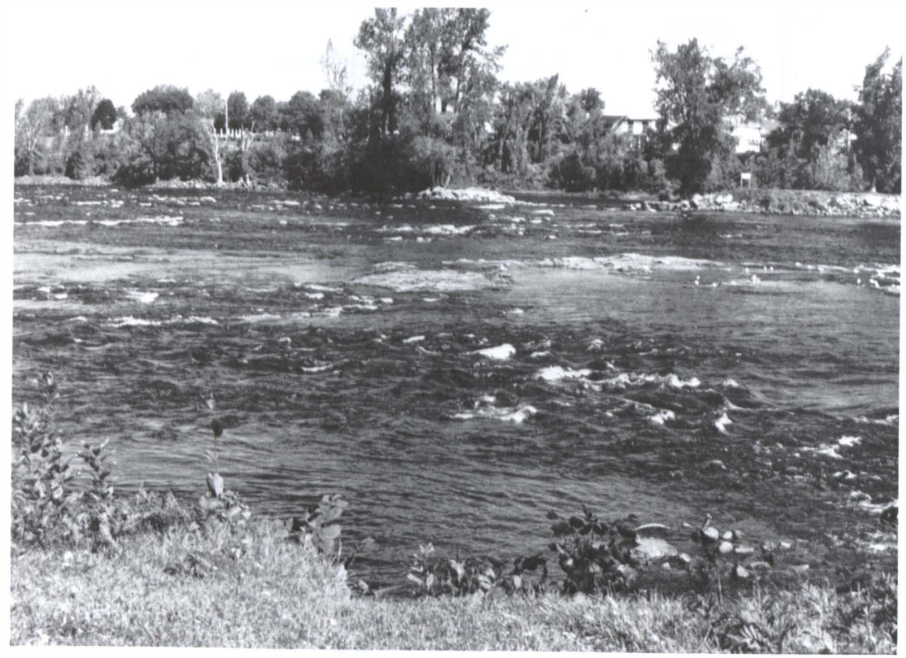

FIG. 3.—Rapids in Saint-Jean-Sur-Richelieu, southern Quebec, Canada. Photo taken in September, 1991. (Yasuchi Kojo).

In addition to these physical barriers, it should be noted that the animals in question are sighted, though not frequently, within the lake even in winter (Fig. 4).

The second possibility is also unlikely because there is only one report of one of the animals breaking ice—and no reports of an animal resting with its nostrils out of water in ice holes—although many people are active on the ice for ice fishing in several localities within the lake. The third possibility is also improbable because the animals *can* reportedly break ice for breathing air.

Because the first three possibilities seem unlikely, the fourth possibility remains as the most likely for explaining how the animals survive under the ice. It is known that caves in western New England, including the Champlain Valley, are usually located in lower Ordovician calcitic and dolomitic marbles on the slopes of the phyllitic capped hills (Quick 1979). Lower Ordovician limestone and dolostone are distributed in some localities on the side of Lake Champlain (Fig. 5). While the occurrence of phyllite in these localities is unknown, there is a possibility that openings of underwater caves are located at some of these localities.

This possibility is also suggested by the sighting report of the animal

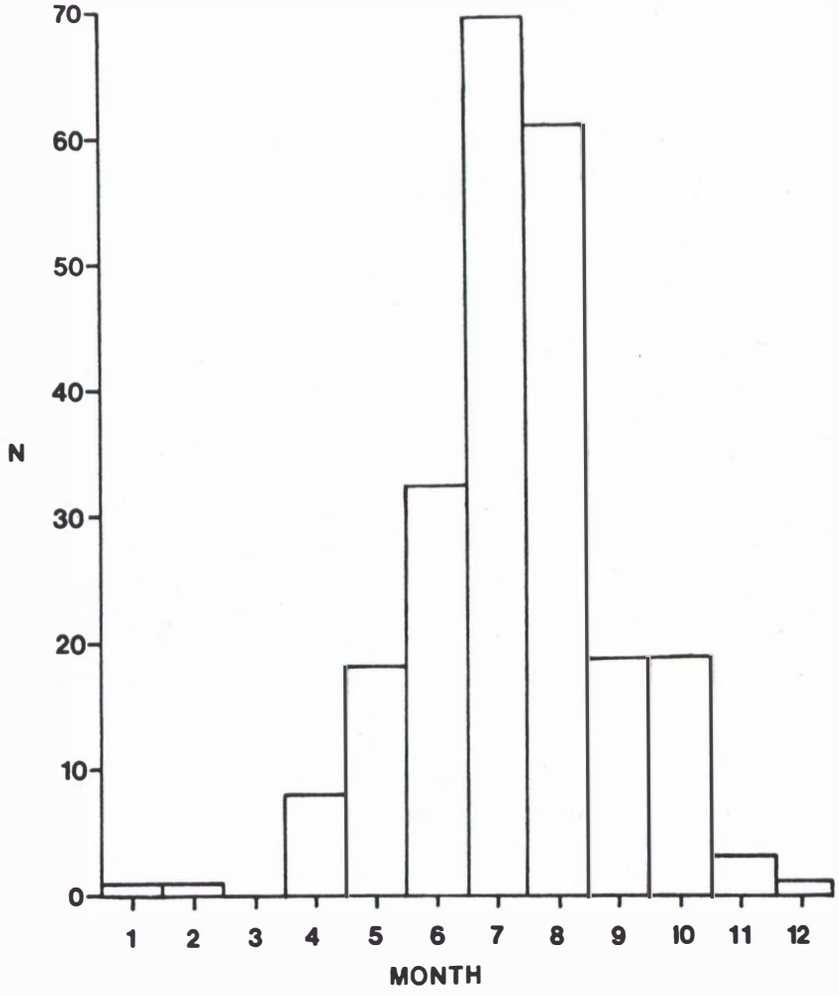

FIG. 4.—Reported sightings of large, unknown animals in Lake Champlain by month. (After Zarzynski 1988a, 1988b, 1989, 1990.)

breaking ice noted above. On February 27, 1971, "the creature broke through ice near Velez Marina (sic) and then returned under ice" (Zarzynski 1988a: 176–177). Velez Marine is located at the northern part of Port Henry. Surface geology there and in nearby Bulwagga Bay is lower Ordovician limestone and dolostone (Fig. 5). Because this is the only instance of reported ice breaking by these animals out of more than three hundred reports, it may be interpreted as anomalous behavior rather than a purposeful attempt to make air holes through the ice.

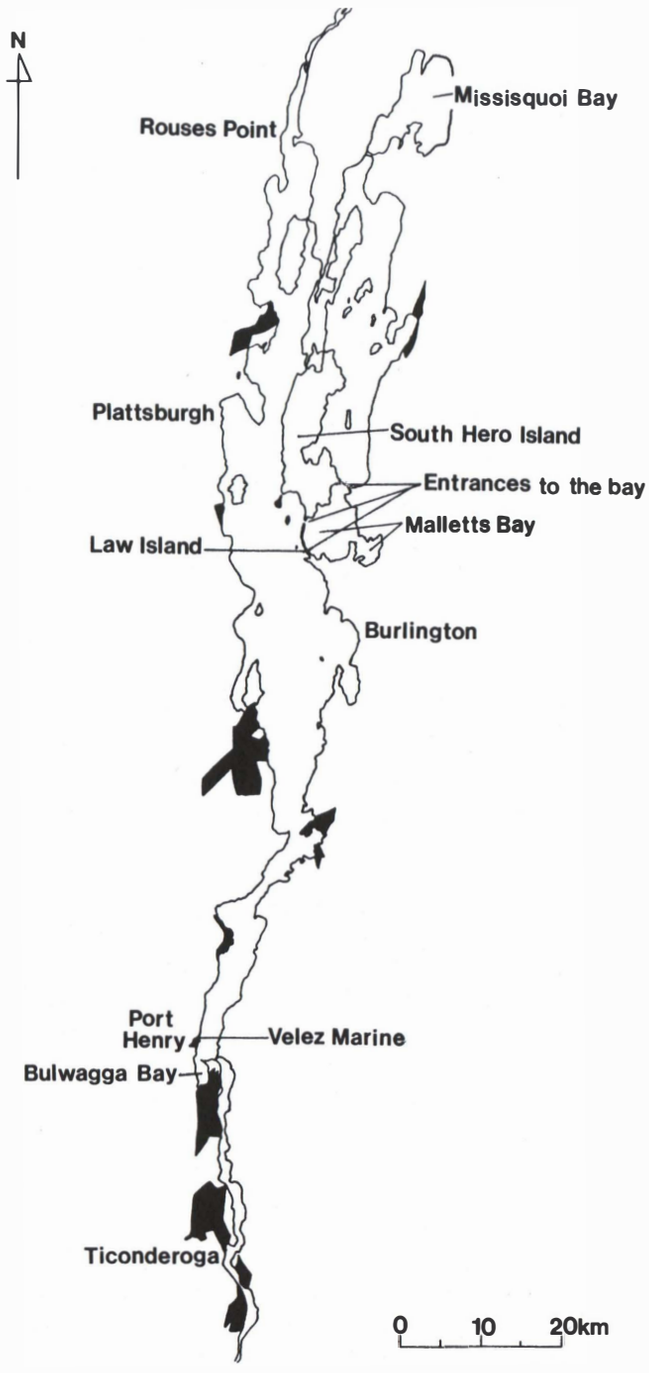

FIG. 5.—Distribution of lower Ordovician limestone and dolostone on the lakeside of Lake Champlain (blackened areas). (After Isachsen and Fischer 1970.)

Behavior in the Ice-Free Seasons

In the ice-free seasons, it is most probable that the animals subsist mainly on fish. This is supported by the number of reports stating that the animals were swimming very fast as if chasing something in the water (Zarzynski 1988a: 156–205, 208–216 *in passim*). Some other reports further strengthen this possibility: "a lot of fish were jumping out of the water surface" (Zarzynski 1990: 81); "big fish or birds were diving around the animal" (Zarzynski 1990: 81). Because several sightings have been reported from very shallow parts of the lake, such as Missisquoi Bay, which is shallower than 15 feet (5 m), and Bulwagga Bay, which is shallower than 30 feet (10 m), warm water fish may be an important food source for the animals.

There are several cases in which the animals were reported in basically the same locality over a period of a few days (Table 2). Such a pattern of sightings is most reasonably interpreted as a result of foraging behavior. The animals probably forage for fish in a specific locality for at least a few days, and then move to another locality, possibly after the fish supply in the first locality is reduced or exhausted.

Proposals for Future Investigations

Based on the preceding ecological considerations, the following suggestions for future research may be of value. In ice-free seasons, the animals are likely to be moving from one locality to another within the lake, often staying at a specific locality for at least a few days. If this is so, investigations should be conducted in a locality in which such an animal has been recently reported, hopefully within the previous few days. Because the animals are probably nocturnal and air-breathing, they may remain dormant during the daytime near the lake surface, with their nostrils protruding out of water. Such dormant animals would probably be observed more easily by an airborne observer as a large dark mass, at least in high water clarity areas. Water clarity in the lake varies substantially from one area to another largely in response to different degrees of phosphorus pollution (Fig. 6). Because noise seems to frighten the animals, and they can quietly submerge before being observed, disturbances created by motor-driven or even row boats should be avoided. Investigators would presumably have a better chance of observing the animals at night, with the minimum of noise, and with the aid of night-vision equipment.

Thus, successful investigation of these animals in the ice-free seasons is probably dependent upon 1) the speed at which sightings made by the public can be reported to investigators; and 2) the speed at which investigators can reach the sighting localities. Putting aside the second problem, the first difficulty could be overcome by the establishment of a 24-hour telephone

Table 2.—Reported sightings that occurred in the same locality within a few days. (Based on Zarzynski 1988a.) NY: New York, VT: Vermont.

Sighting no.	Date	Location
104	June, 1972	Rock Point (VT)
105	Next day	Rock Point (VT)
155	April 17, 1981	Bulwagga Bay (NY)
156	April 19, 1981	Port Henry (NY)
177	May 27, 1982	Bulwagga Bay (NY)
178	May 29, 1982	Bulwagga Bay (NY)
205	August 10, 1983	St. Albans Bay vicinity (VT)
206	August 12, 1983	St. Albans Bay vicinity (VT)
208	August 16, 1983	Fort Cassin Point (VT)
209	August 18, 1983	Fort Cassin Point (VT)
223	May 21, 1984	Off Popasquash Island, between island and shore (VT)
224	May 23, 1984	Off Popasquash Island, between island and shore (VT)
263	June 29, 1985	Near Basin Harbor (VT)
264	June 30, 1985	Mile Point (VT)
269	August 8, 1985	Mullen Bay (NY)
270	August 9, 1985	South of Mullen Bay (NY)
292	September 12, 1987	Treadwell Bay (NY)
293	September 13, 1987	Treadwell Bay (NY)

"hotline," and encouraging local residents and tourists through various media to call in their sightings as quickly as possible.

It should be noted that Lake Champlain has some advantages for observing these large aquatic animals. In some parts of the lake, the entrances to a catchment area are so few and so narrow that it is technically possible to temporarily close them for trapping purposes. This would allow photography and filming while the animal is trapped in the catchment area. For instance, Malletts Bay, located to the north of Burlington, Vermont, has only three very narrow entrances. Of these, the entrances located near Law Island and the southeastern edge of South Hero Island are possibly too shallow—4 feet (1.2 m) or shallower—for the animals to pass through. The other entrance is located near the southern edge of South Hero Island and is deep enough, about 10 feet (3 m), for the passage of such animals. Yet its width is only about 100 feet (30 m) or so (Figs. 5 and 7). Such animals have been frequently sighted inside the bay: in about 1900, 1952, 1981, 1989 and 1991 (Zarzynski 1988a: 164–65, 168–69, 210, 1989: 70, personal communication 1991). It should be noted that water visibility in Malletts Bay is still very high; Secchi clarity depth was 16 to 17 feet (5 to 5.2 m) in 1990 (Fig. 6).

During the period in which the lake surface freezes, the animals may

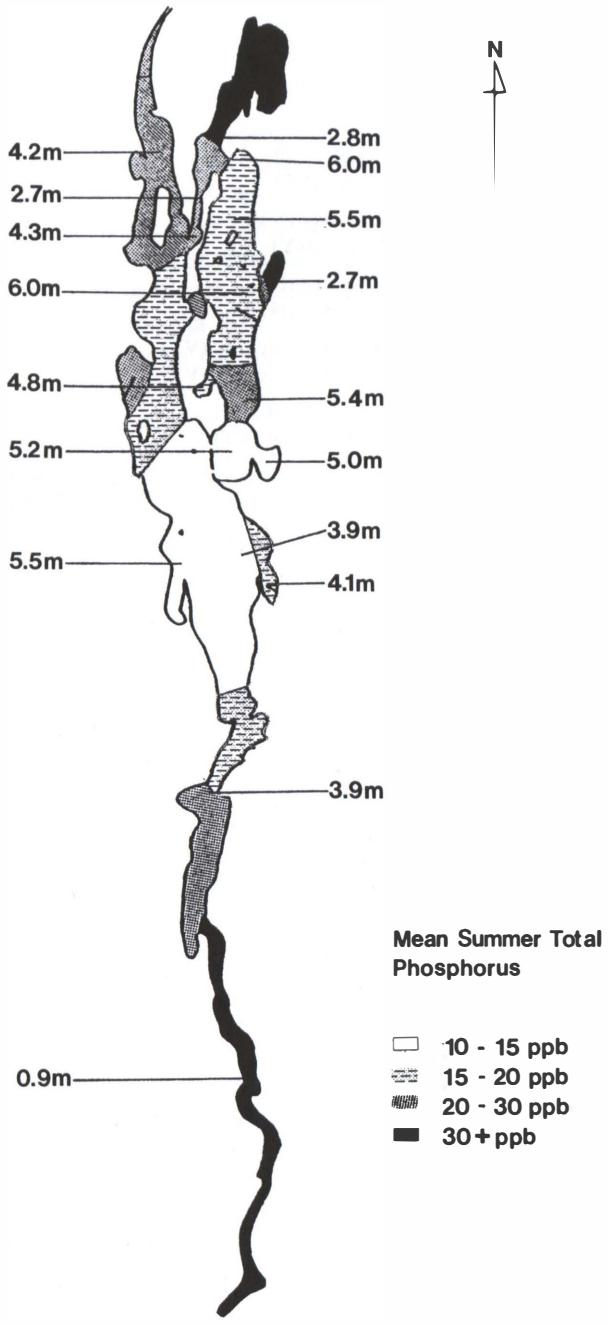

FIG. 6.—Secchi clarity depth (1990) and mean summer total phosphorus in Lake Champlain. (After Lake Champlain Committee, n.d., and Lohner and Warren 1991.)

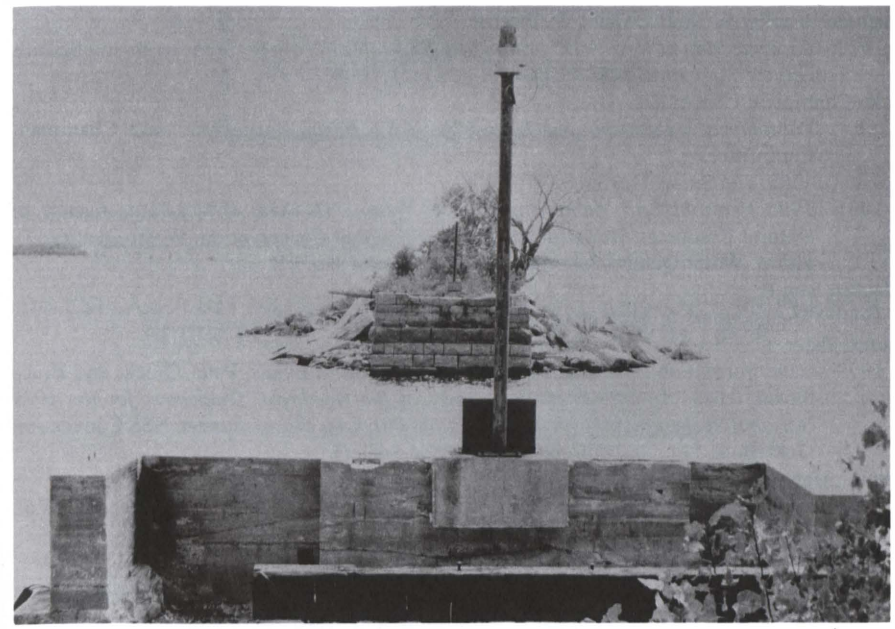

FIG. 7.—The entrance to Malletts Bay near the southern edge of South Hero Island. Photo taken in September, 1991. (Yasuchi Kojo.)

hibernate in underwater caves with air circulation. If the openings to such underwater caves could be successfully located, the animals could be observed entering or leaving the caves before and after the lake freezes over. Also, the hibernating animals in the caves could possibly be observed by an underwater robot. Localities in which underwater caves might exist can be narrowed down by a study of the surface geology of the lakeside. The search for such caves would best be conducted by an underwater robot. However, it is technically feasible to search for them in some localities by scuba diving.

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Research Reports

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PHYSICAL AND MORPHOLOGICAL ANALYSIS OF SAMPLES OF FIBER PURPORTED TO BE SASQUATCH HAIR

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ABSTRACT: Analytical tests consisting of examination by electron microscopy, melting-point measurements, and determination of solubility in seven representative solvents were conducted on samples of short, reddish, fur-like fibers purported to be samples of Sasquatch hair. The same tests were run on control samples of modacrylic fiber, a synthetic fiber commonly used for synthetic furs, wigs, and artificial hair. The results show clearly that all samples of the fiber claimed to be Sasquatch hair were in fact synthetic modacrylic fiber and, in all likelihood, were samples of the "Dynel" modacrylic fiber manufactured by the Union Carbide Corporation. The allegation that these fibers were Sasquatch hair thus clearly represents a hoax, and is described by the author as "scientific vandalism."

INTRODUCTION

There has been considerable controversy surrounding evidence for the existence of the animal known as Sasquatch. Some reported sightings are believed to be hoaxes, and certain examples of indirect evidence, such as samples of hair, blood, and fecal droppings, are also suspected of having been hoaxed.

One such case of falsification was reported in the symposium Sasquatch Evidence: Scientific and Social Implications, sponsored by the International Society of Cryptozoology and held at Washington State University, Pullman, Washington, on June 24–25, 1989 (Somer 1989). In this report, it was shown through photomicrographic analysis and observation of softening and melting at elevated temperatures that the purported hair was, in fact, a synthetic fiber.

Another presentation at the above-mentioned symposium produced physical evidence in the form of sections of twisted trunks of small trees about 3–4 inches (8–10 cm) in diameter said to have been twisted by a Sasquatch (Freeman 1989). Short strands of reddish, fur-like fiber were attached to protuberances on these twisted tree trunks—presumably left by the Sasquatch

involved. Several strands of these reddish fibers were taken surreptitiously by the author at the conclusion of the presentation, and without the knowledge of the speaker. These hair-bearing twisted trees had been found in an area of the Blue Mountains of southern Washington and northern Oregon.

Subsequently, at the end of the symposium, a larger sample of reddish-brown, hair-like fiber was supplied to the author by Grover S. Krantz, an anthropologist at Washington State University who chaired the symposium. According to Krantz, this sample had been collected a year or two previously in the same region of the Blue Mountains of Washington/Oregon.

Because of the prevalence of hoaxes in connection with claimed sightings of Sasquatches and other evidence said to be related to them, it was decided to undertake a thorough investigation of the above-mentioned fiber samples. The purpose of this investigation was to determine if the samples were in fact animal hairs, and, if not, to identify to the extent feasible their chemical composition and origin.

Method

Photomicrographs in both longitudinal and cross-sectional views were made on both of the above-described samples, the small sample collected from the Freeman evidence (Sample A), as well as the larger sample supplied by Krantz (Sample B). (Unfortunately, Sample A was too small to permit the use of any other analytical technique.)

These were compared with photomicrographs made under comparable conditions (longitudinal views and cross sections at similar degrees of magnification) of a variety of fibers, both natural and synthetic, as published in the handbook *Identification of Textile Materials* (The Textile Institute 1985). The fibers which were used as controls for this comparison are listed in Table 1.

In this comparison of photomicrographs, it was clear that the longitudinal and cross-sectional views of both purported Sasquatch hair samples, A and B, most closely resembled comparable views of modacrylic fibers. In this first step of photomicrographic examination, fibers from two sources were used: The "Teklan" modacrylic fiber produced by Courtaulds Ltd. in the United Kingdom, and the "Dynel" modacrylic fiber formerly produced by the Union Carbide Corporation in the United States. Of these two, the morphological characteristics of the "Dynel" modacrylic fiber appeared to match most closely those of the two fiber samples (A and B) purported to be Sasquatch hair, both in longitudinal and in cross-section views.

For the reason given above, the further analytical work, which consisted of comparative measurements of melting point and solubility in a number of representative solvents, was conducted only on Sample B of the purported Sasquatch hair and on a known specimen of "Dynel" modacrylic fiber.

Table 1.—Fibers used as controls in first step of identifying the samples of unknown origin and alleged to be Sasquatch hair.

Natural fibers	Synthetic fibers
Animal	Viscose
Wool	Cellulose acetate
Mohair	Cellulose triacetate
Cashmere	Acrylic
Horse hair	Modacrylic 6,6 nylon
Vegetable	
Silk	Polyester
Cotton	Polyvinyl alcohol
Flax	
Jute	
Ramie	

Results

Photomicrographs

Fig. 1a shows an electron photomicrograph of Sample A of alleged Sasquatch hair in longitudinal view at magnification of 20×. Fig. 1b shows the same sample at magnification of 200×. Fig. 2a shows an electron photomicrograph of Sample B of purported Sasquatch hair in longitudinal view at magnification of 20×, and Fig. 2b shows the same sample at magnification of 200×. Fig. 3 shows an electron photomicrograph in longitudinal view of "Dynel" modacrylic fiber at magnification 750×.

Fig. 4 shows an electron photomicrograph of Sample A in cross section at magnification 230×, and Fig. 5 shows Sample B in cross section also at magnification 230×. For comparison, Fig. 6 shows an electron photomicrograph of "Dynel" modacrylic fiber in cross section at magnification 750×. Figs. 3 and 6 have been reproduced from pp. 114–15 of the handbook *Identification of Textile Materials* (The Textile Institute 1985).

In comparing these photomicrographs, two points in particular should be noted. First, the photomicrographs in longitudinal view of Sample A, Sample B, and the "Dynel" modacrylic fiber all show clearly the deeply fluted surface which is characteristic of "Dynel" modacrylic fiber. Second, neither in Sample A nor in Sample B do the fibers show the surface scales which are characteristic of most animal fibers.

Melting Point

Melting points of the Sample B fiber and of "Dynel" modacrylic fiber were determined by the copper-block method (Preston 1949). This method uses a copper block in the form of an inverted "L," with the upper surface being slotted to hold the fiber sample and drilled for a thermometer pocket.

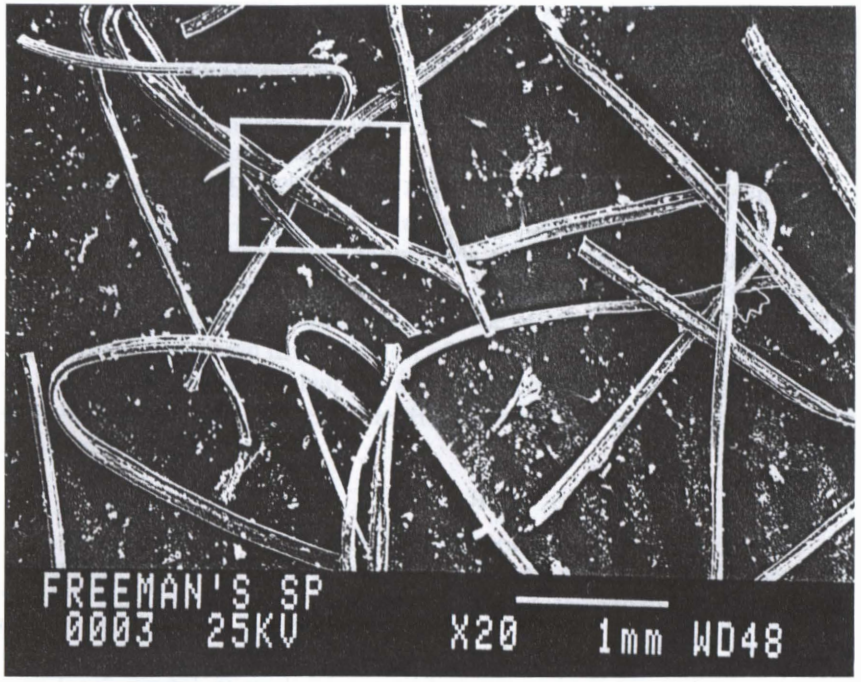

FIG. 1a.—Sample A, longitudinal view, 20×.

The shape of the copper block enables the overhanging part of the inverted "L" to be heated in a Bunsen flame without the hot gases reaching the fiber sample. The fiber sample is placed in the slot, the block is heated slowly, and the temperature at which the fibers soften can be observed to within quite narrow limits.

By this method, the following melting points were obtained:

Sample B:	188 ± 2°C
"Dynel" modacrylic fiber:	190 ± 2°C.

Both fiber samples may thus be considered to have essentially the same melting point.

It should also be noted that animal fibers in general do not exhibit well-defined melting points or even melting ranges. When heated, they char, discolor, and decompose rather than soften and melt.

The observance of a clearly defined melting point suggests quite strongly that the Sample B fiber is, in fact, composed of a synthetic polymer. Further, the only synthetic fibers known to have melting points in the region of 190°C are the modacrylics and certain chlorofibers. However, these latter may be

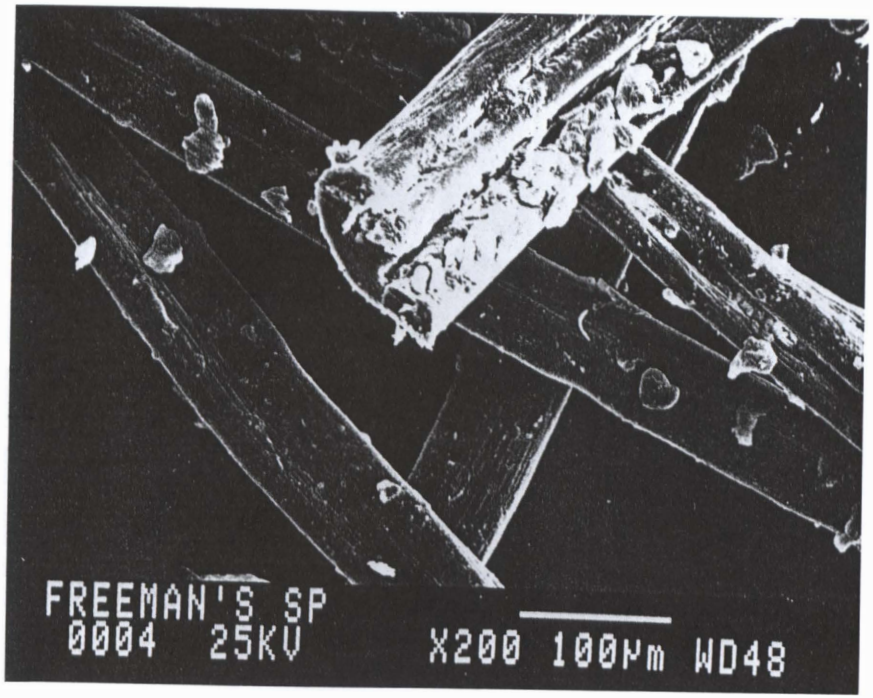

FIG. 1b.—Sample A, longitudinal view, 200×.

excluded from consideration in the comparison presented here, as their cross sections are essentially circular (The Textile Institute 1985: 111–13), and thus bear no resemblance to the fiber cross sections shown in Figs. 4, 5 and 6.

Solubility

Both the Sample B fiber and the control sample of "Dynel" modacrylic fiber were found to dissolve completely when immersed in the solvents listed in Table 2.

No animal fiber is known which will dissolve in all of the above solvents. However, "Dynel" modacrylic fiber is known to be soluble in all of these solvents. On the other hand, "Teklan" modacrylic fiber is known to dissolve in all *except* acetone at 35°C (The Textile Institute 1985).

DISCUSSION

From the electron photomicrographs and the measurements of melting point and solubility, it is clear that the Sample A and Sample B fibers are not of animal origin, but are almost certainly synthetic fibers. They do not show the surface scales characteristic of most animal hairs. Instead, they

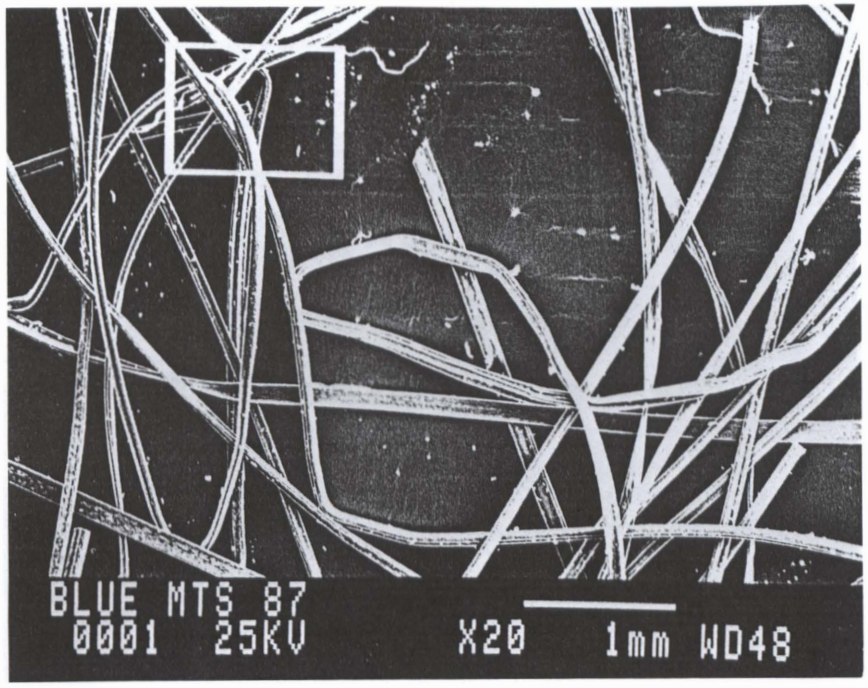

FIG. 2a.—Sample B, longitudinal view, 20×.

both show the smooth, fluted surface characteristic of modacrylic fibers. Further, the Sample B fiber exhibits the same melting point as "Dynel" modacrylic fiber, whereas animal hairs in general do not melt but, rather, char and decompose when heated. Further, the Sample B fiber proved to be soluble in seven conventional solvents in which "Dynel" modacrylic fiber is also known to dissolve. On the other hand, no known animal hair will dissolve in all of these seven solvents.

It is thus concluded that both Sample A and Sample B, both alleged to

TABLE 2.—Solvents used for testing the solubility of the Sample B fiber and of "Dynel" modacrylic fiber.

Butyrolactone at 20°C
Nitromethane at 20°C
Acetone at 35°C
Concentrated (75%) sulfuric acid at 20°C
65% nitric acid at 35°C
Cyclohexanone at 35°C
Dimethylformamide at 20°C

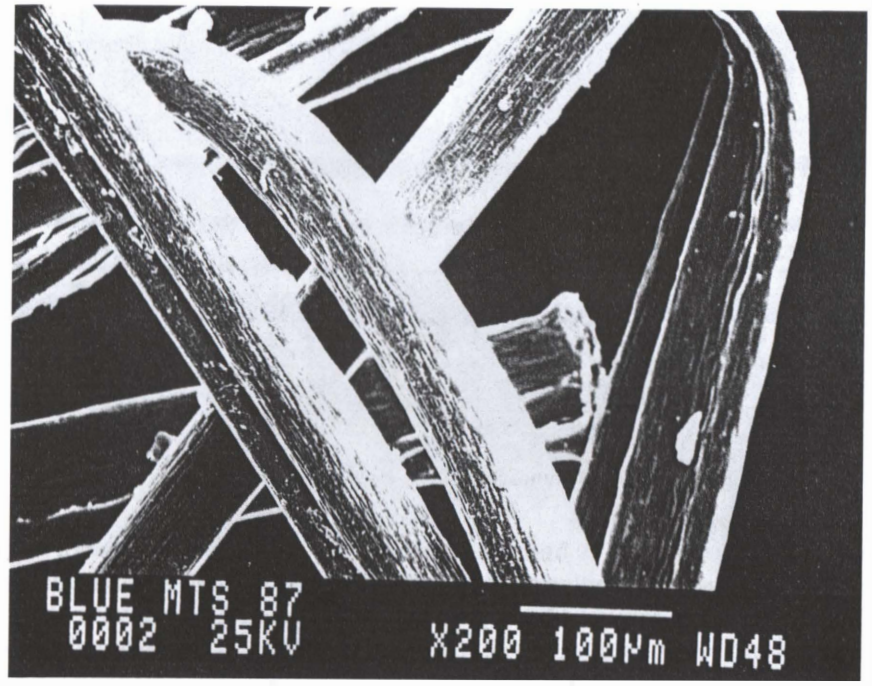

FIG. 2b.—Sample B, longitudinal view, 200×.

be Sasquatch hair, are in fact synthetic fibers. Further, both samples are almost certainly synthetic modacrylic fiber, and, very likely, are composed of the "Dynel" modacrylic fiber manufactured until recently by Union Carbide.

The "Dynel" modacrylic fiber is composed of a copolymer of acrylonitrile and vinyl chloride, containing somewhat more than 15% by weight of vinyl chloride. Its production by Union Carbide is believed to have been discontinued in recent years for economic reasons, although products containing this fiber are almost surely still available commercially. The principal uses of "Dynel" modacrylic fiber have been in synthetic furs and wigs where, when properly finished, its esthetic properties closely resemble those of natural animal fur and hair. Another important sector of use has been in fabrics for upholstery and home furnishings, where the relatively high chlorine content of the polymer imparts to the fiber a high degree of nonflammability.

Returning to the presentation by Freeman (1989), even though the fiber samples collected surreptitiously from the tree trunks were found clearly to be fraudulent, one still has to explain the condition of the tree trunks themselves, which had been fibrillated and shattered by a twisting motion. It could be argued that the force needed to accomplish such twisting is well

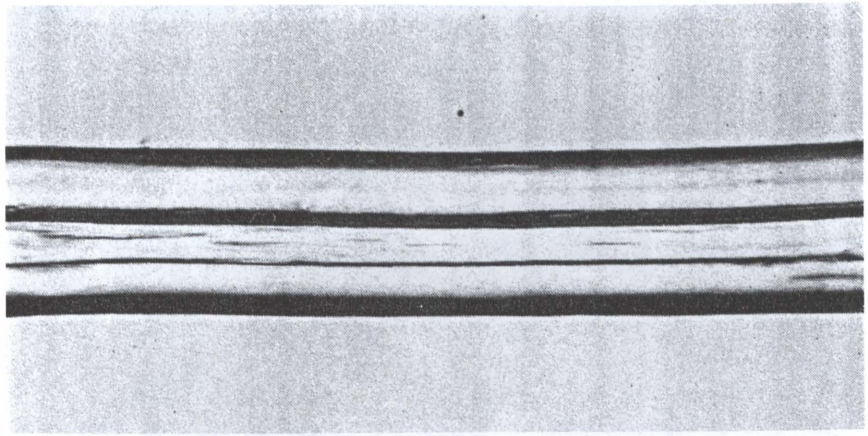

FIG. 3.—"Dynel" modacrylic fiber, longitudinal view, 750×.

beyond the strength of any human, and that the only animal capable of such strength and such manipulation would be the primate known as Sasquatch.

This conjecture can readily be disproven. In the first place, very strong gusts of wind, such as occur in severe thunderstorms, can easily twist and shatter the trunks of trees 3–4 inches (8–10 cm) in diameter, and even larger. Much more likely, however, is that the specimens of twisted tree trunks

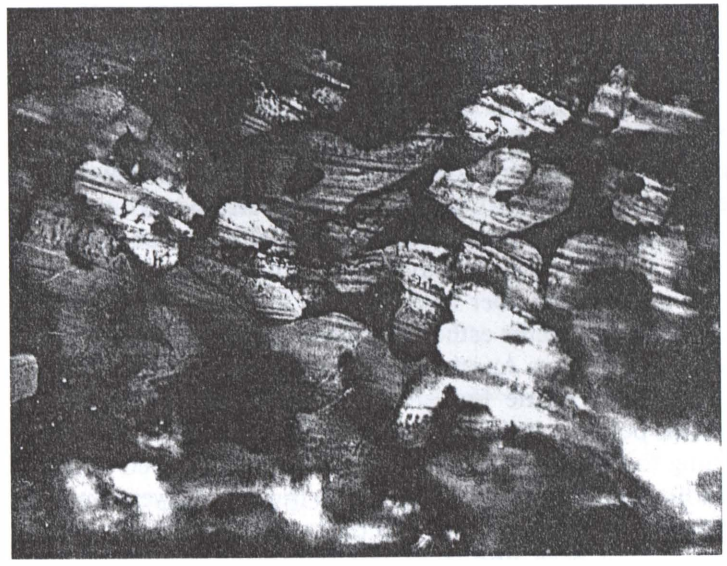

FIG. 4.—Sample A, cross section, 230×.

FIG. 5.—Sample B, cross section, 230×.

exhibited by Freeman at the symposium were shattered by human efforts. This could readily be accomplished with a chain link vise, such as those used by plumbers to hold pipes for cutting or threading. Such devices often are used on construction projects where suitable shop facilities are not readily available.

To twist and shatter a tree trunk 3–4 inches (8–10 cm) in diameter, one would attach a chain link vise to the trunk, perhaps 6 feet (2 m) or more above the ground. At the pressure point of the vise, the tree could be wrapped with a few protective layers of coarse fabric; this would avoid "bite" marks on the bark. With an extension handle of 6 feet (2 m) or more in length on the vise, one would then exert a twisting motion around the axis of the trunk. In this way, a reasonably strong human could easily exert more torque than several Sasquatches working together, and certainly enough to twist the tree to the point of shattering.

To prevent uprooting, and to focus the damage to the tree at a particular point, it would be a simple matter to apply two such vises 3–6 feet (1–2 m) apart, and then twist in opposing directions. Any telltale evidence of the use of such vises could then be eliminated simply by cutting or sawing the tree trunk between the point of fibrillation and the points where it had been held by the vises, and exhibiting only the part of the trunk showing the damage by twisting.

The samples of purported Sasquatch hair which are described here plainly

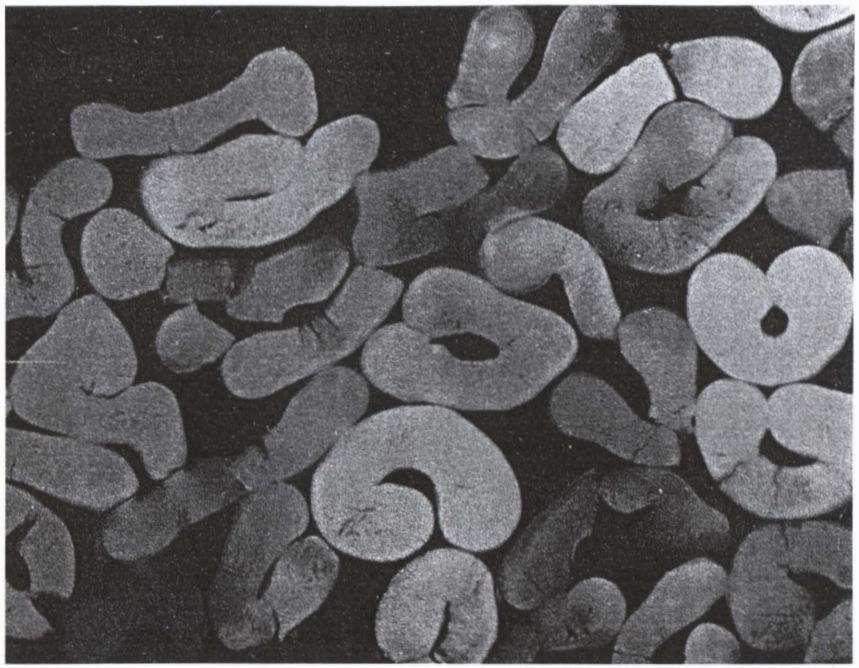

FIG. 6.—"Dynel" modacrylic fiber, cross section, 750×.

are deliberate hoaxes. This fact, combined with the proposed explanation of how one or two humans could twist small tree trunks to the point of shattering, demonstrates that the symposium presentation by Freeman (1989) is plainly a complete fabrication.

Because of the interference and damage done by such hoaxes to the reputation and standing of serious and objective scientists working in a controversial area such as Sasquatch studies, the author considers that these hoaxes may be fairly and most kindly described as "scientific vandalism."

The author wishes to express his thanks to the Physical Testing Section of the Textile Research Laboratory of Du Pont De Nemours International S.A., in Geneva, for preparing the photomicrographs of the samples of purported Sasquatch hair, and for conducting the measurements of melting point and solubility on these as well as on a control sample of "Dynel" modacrylic fiber.

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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 field work are the responsibility of the authors only, and are subject to criticism in the Comments and Responses section.*

Cryptozoology, 10, 1991, 66–71

FURTHER INVESTIGATION INTO LOYS'S "APE" IN VENEZUELA

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INTRODUCTION

In 1917, François De Loys, a Swiss geologist, set off on an expedition into the montane and lowland rain forests of Venezuela. His expedition lasted for three years, and encountered many difficulties, including sickness and hostile Indians. Only a few expedition members survived.

In 1920, in the forest along the River Tarra, Loys later reported that he and his companions encountered two tall monkeys which advanced toward them, walking upright and holding onto bushes. The monkeys appeared to be very angry, screaming, waving, and tearing off branches. They reportedly became so enraged that they defecated into their own hands and hurled their feces at the men. The group tried to shoot the male monkey, which was in front and was the most threatening of the two. However, it reportedly stepped aside and let the female charge, and it was the female which was killed by the guns of the expedition members. The male monkey fled. The dead female was carried to the riverbank, set on an oil crate, held upright by a stick placed under its chin, and photographed. The photograph (Fig. 1) shows a large monkey with a human-like facial expression. Upon Loys's examination, the monkey was found to have 32 teeth instead of the normal 36.[1] The

[1] Platyrrhine (New World) monkeys have 36 teeth, except for the marmosets, which have 32 teeth. Catarrhine (Old World) monkeys and apes have 32 teeth—Editor.

FIG. 1.—The only available photograph of Loys's "Ape," taken after the primate was shot in 1920 by Swiss geologist François de Loys and his party in the Amazon forests of Venezuela.

expedition's cook prepared and preserved the animal's skull, and put it into a salt box. It later dried and disintegrated in the heat, and little by little the pieces were lost forever; but the photograph has remained. It was reported that the monkey was almost 5 feet, 2 inches (157 cm) tall, and a new discovery for science. It appeared to be a tailless primate, with the arms longer than the legs, and it had a flattened chest. The primate became known as Loys's Ape, although, of course, apes are only found in the Old World. Only monkeys are found in the New World Neotropics.

This account was given in detail by Bernard Heuvelmans in his classic book, *On the Track of Unknown Animals* (Hill and Wang, New York, 1958). Since then, many have speculated about the possibility of Loys's monkey—as we will call it here—still existing in the Venezuelan rain forests. The most recent reports were made in the 1970's. However, stories have recently been spreading in the Venezuelan forests about encounters with this primate.

After studying the history of these reports, we decided that the Ventuari and Orinoco Rivers, in the state of Amazonas, were the most likely places for present-day encounters. In October, 1990, the authors and three others, Bill Cacciolfi, Leon Childers, and Ken Wohlers, undertook an expedition to this area.

Narrative Description

After meeting in Caracas, we proceded by bush plane to the state of Amazonas. We landed near the Ventuari River, and were met by Piaroas Indians and Lorenzo Rodriguez, a well-known jungle pilot for over 25 years.

The expedition spent a great deal of time traveling a wide variety of rivers in this area (Fig. 2), interviewing Indian villagers, and showing a variety of pictures of Loys's monkey in comparison to the African gorilla, and the American Sasquatch (Bigfoot). We embarked on a long journey up the Ventuari River and related tributaries. There we interviewed an older jaguar hunter who was a good friend of Rodriquez. When inquiring in regard to the giant monkey, he replied that, as a boy, he had captured such a monkey. It was over 3 feet (91 cm) tall, and he had placed it in a cage and sent it to Puerto Ayacucho. He did not hear any more about this animal.

Several days later, while on the river, Khryztian (Marc Miller's daughter) became ill from heat stroke, and we had to rely upon a nearby village and our own travel medicines to restore her to a level at which we could carry on with the expedition.

We then arrived at the Cabade Las Piedras Waterfalls, and spent time in a local Indian village. The inhabitants stated that, within the past year, four of them had been on the Ventuari River and had heard the cry of the giant monkey nearby. They became frightened and ran back to the village. Another of the Indians reported that he had seen giant monkey tracks, and had followed them; he said he saw a giant monkey grab a fish from the river, take its head off, and eat the fish. He tried to shoot it, but he panicked and ran. He stated that this happened a number of years ago.

Further inquiry suggested that, around 1980, two daughters of a nearby villager saw a giant monkey on the Orinoco River. This was at the village of Arrandagao.

Our stop at a village called Moro Coto also proved to be fruitful. Here the Indian hunters knew of this large monkey, and they said it runs in the mountains. They stated that they had found its tracks and heard its call many times. They call this large monkey *salvaje,* and say it cries and yells like a human, but has no language. They stated that it has not attacked people for many years.

During our evening campfire talks with Rodriguez, he told us about some

FIG. 2.—Amazonas state in Venezuela, where the authors investigated modern reports of Loys's "Ape." International boundaries are indicated by heavy lines. The boundary between Amazonas and the Venezuelan state of Bolivar is indicated by a broken line. Only the Amazonas state river system is shown.

of the many legends and stories of the jungle. The Indians talk of a giant anteater that will stand up to a jaguar, fighting with its claws. One of the Indians told us that he had seen a large animal—larger than a tapir—with large lips. Finally, Rodriguez told us that he himself had found footprints of the giant monkey on his airstrip about two years before. The prints were turned inwards and he estimated the animal weighed 80 to 100 pounds (36–45 kg). The Indians had followed the tracks into the mountains. A hunter also came forth and told us that, approximately 10 years before, he had been sleeping in his boat on the Ventauri River, close to a village, when the giant monkey came down to the boat. This was at the village of Laja Pelada. There the monkey was known to sometimes come down from the mountains. The Indians do not kill the giant monkey, as they are fearful that it may have spiritual powers.

Rodriguez told us that in 1989 he had shot an anaconda snake over 21 feet (6.4 m) long. It had tried to attack two small Indian children in a river. He emphasized how the rivers hold many unknown species, and that the forests are full of legends. The next morning, Rodriguez showed us the exact spot on the landing strip where he had found the tracks of the giant monkey. He stated it was easy to follow the tracks, as they were inbedded in the sand of the airstrip.

Our expedition then flew to Puerto Ayacucho, a small town on the Orinoco River, where we planned to investigate further reports of the giant monkey. There we met with Oswaldo Calderson, whose mother had lived on the Casicalo River. She reported to us that she had seen the giant monkey over 25 years before. She told us of another local informant named Fernando Nives. He told us that he had seen the giant monkey 10 years before while hunting 25 miles (40 km) north of Puerto Ayacucho. He described a very strong odor as he came closer to the animal. He took his boat off to the side of the river, and saw three large monkeys standing over 5 feet (152 cm) tall. He described the animals as reddish-colored.

Another incident was relayed to us by a local resident: 15 years before, while using a bulldozer to clear an area for a road, the engine had stopped and he could hear the call of a monkey. He later saw it, and also described it as having reddish hair and standing over 5 feet (152 cm) tall. Khryztian, who speaks fluent Spanish, was able to accurately interpret the information being relayed to us.

The following day, we went by boat up the Orinoco River, which serves as a border between Colombia and Venezuela. The Orinoco has over 1,000 tributaries. Huge granite mountains rose throughout the area. We stayed at an Indian village, and the chief shared many stories with us, speaking in broken Spanish. The chief told us that only a few months before, while hunting with his blow gun, he had seen a giant monkey. He described it as having reddish hair, and that it stood as tall as himself; this would be approximately 5 feet (152 cm). He told us that he had shot the giant monkey, but did not take it back to the village as he was fearful it would be a bad omen.

The area is quite remote, and we introduced the village children to ice; it was very interesting to observe their reaction. The chief of the village had lost his left leg because of a snake bite, and now used a prosthetic device that he carved from wood.

The tributary where the giant monkey had been killed is called the Baruasa River. We stayed with the Bendare Indians in the area, and offered them a handsome reward if they could find the skeletal remains of this giant monkey which the chief had told us he had killed.

Results

Zoologists will find it difficult to reach any conclusions from this expedition report. However, we believe that there is a high probability that some form of large, unknown monkey, from 3 to 5 feet (91–152 cm) tall, has been heard and seen by many Indian villagers and townspeople; and tracks have been reported by reliable sources, such as Lorenzo Rodriguez. All of the reports appear to contain the same description: a large monkey, somewhat thin in stature, with long arms, and having reddish hair.

It is our opinion that there are a wide variety of primates that have been labeled *salvaje, didi,* and a number of other names. Such reports have been made as far north as Central America, where some carvings by Mayan Indians have primate-like features.

Due to the almost impenetrable terrain and the vastness of the Venezuelan forest, further attempts to find Loys's "ape" or giant monkey would be very difficult. It is likely that there are a limited number of these large primates living in that part of South America. While rare, there are too many sighting reports from a wide variety of sources to discount this primate as myth only.

Future Plans

The authors have no plans to conduct further fieldwork at this time. However, Bill Cacciolfi indicates that he plans to return to the area, possibly in 1993. He will then try to obtain more information. We will keep in contact with Lorenzo Rodriguez in regard to further information he may be able to obtain.

Cryptozoology, 10, 1991, 72–75

BCSCC REPORT ON OKANAGAN LAKE, 1991

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INTRODUCTION

In early 1991, Nippon Television (NTV) became one of many groups to have launched a search for Ogopogo, the reputed monster said to inhabit Okanagan Lake in south-central British Columbia. Bucking the trend of usually conducting expeditions in the summer months, when sightings are more plentiful, NTV elected to conduct their search activities in the dead of a cold Canadian winter, when visibility on Okanagan Lake can be reduced to a matter of meters during snowstorms that can sometimes turn the lake into a freezing maelstrom. The British Columbia Scientific Cryptozoology Club (BCSCC) assisted NTV with its project, serving in an advisory capacity.

NARRATIVE DESCRIPTION

Heading the NTV expedition were Michihito Ogawa, producer; Hidegetsu Honda, director; and Sakuji Yoshimura, an Egyptologist from Waseda University in Tokyo. The choice of Yoshimura as an "expert" on cryptids appeared to stem from his gregarious personality and his ability to make bizarre statements—such as the one about his desire to "give Ogopogo a big kiss" if the animal were captured. Having set some of these idiosyncracies aside, the expedition began testing its equipment on February 27 and 28, 1991. Complementing their sealed underwater cameras was NTV's own remotely operated vehicle (ROM) which was to be used in conjunction with *Deep Rover,* an underwater vehicle piloted by an on-board operator (Fig. 1). *Deep Rover* was supplied by Can-Dive Services Limited of North Vancouver, and was piloted by Steve Fuzessery.

On March 1, the NTV crew set off aboard a houseboat carrying the cameramen, and a barge which was used to transport *Deep Rover.* The morning calm was disrupted when the barge was unable to pass under the Okanagan Lake bridge because the crane to be used for lifting *Deep Rover* into the water was too tall to allow passage. Attempts were made to rectify the problem, and it was finally decided to pump the barge full of water for ballast purposes. The maneuver worked, and the barge and crane were able to pass below the bridge, but not without making contact with the span above on several tension-fraught occasions. However, after having brought the barge safely through, the houseboat began spewing oil and sending out clouds of smoke. After this reversal, it was decided to abandon search attempts that day, and to return to the hotel to regroup.

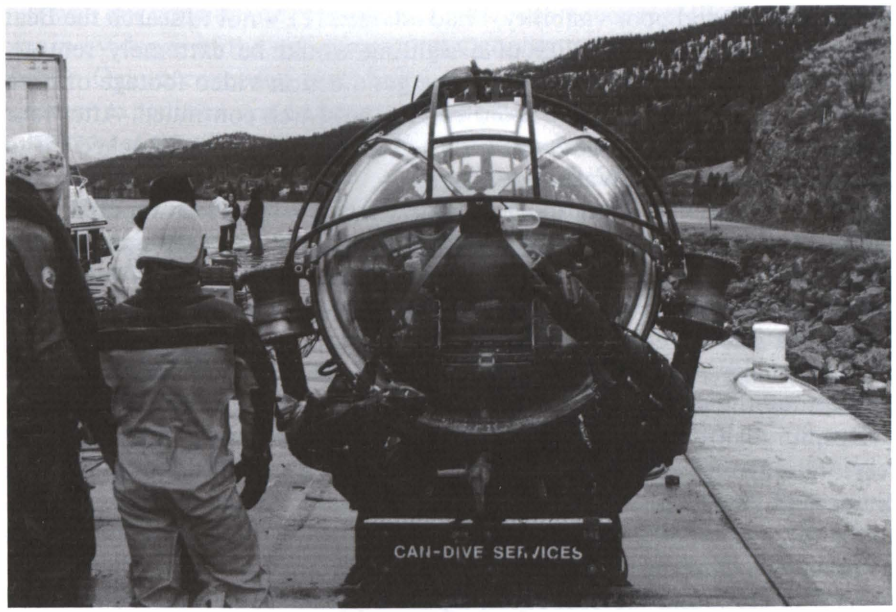

FIG. 1.—*Deep Rover,* an underwater vehicle used by the NTV team in their search for Ogopogo in Okanagan Lake. The vehicle performed flawlessly to a depth of 840 feet (255 m). (John Kirk.)

On the morning of March 2, the NTV party switched from the crippled houseboat to a massive cabin cruiser. All was smooth sailing, as both the barge and cruiser arrived safely at the deepest point of the lake, close to the Okanagan Lake Resort, 7.5 miles (12 km) north of Kelowna.

Pilot Steve Fuzessery navigated *Deep Rover* across the lake bottom. It performed flawlessly at a depth of 840 feet (255 m), in search of tell-tale signs of any large creatures. Fuzessery located a large tube-like imprint on the lake bottom, but it was impossible to conclude if this was an Ogopogo trace. No further discoveries of note were made, and operations were terminated. While NTV was in action, the author, president of BCSCC, had opted to remain on shore to conduct observations from the Kelowna lake front. During the course of my investigations, I noticed two large patches of foaming bubbles not far from my vantage point on the east side of the lake. The duration of the bubbling led me to conclude that the source of the bubbles was natural gas escaping through a fissure on the lake bottom. These bubbles have obviously been mistaken in the past for signs of Ogopogo's presence or activity by witnesses who were unaware that the Okanagan Valley is dotted with thermal vents, the vestiges of its former position as a center of volcanic activity.

Bear Creek was the location of the next intensive search. Because of the

choppy waters and poor visibility, I had advised NTV not to search the Bear Creek area, as the possibility of a sighting would be extremely remote. However, NTV was intent on obtaining good action video footage of both the ROV and *Deep Rover* in action, and so the search continued. After four hours of futility, *Deep Rover* was returned to the surface and packed off to another assignment in Washington state.

We then headed to Rattlesnake Island, which has long been reputed to be the residence of Ogopogo. Weather conditions had improved significantly, and under sunny skies and calm water divers scoured the shelf under the island for signs of caves—where Ogopogo is said to dwell. No caves of any significant size were found, and to add yet another damper to the search, one of the cameramen dropped his camera; it slid to the bottom and was lost among the rocks.

Despite slightly better weather, the next day's activity proved totally fruitless. Veteran Ogopogo researcher Arlene Gaal and myself both felt that the chances of detecting an Ogopogo during turbulent winter weather were virtually nil, and we both stood on the sidelines wondering if anything was to come of this costly exercise in futility.

As if to confound the onlookers, a partial breakthrough was obtained by March 7. Sonar equipment had not been employed to its fullest extent, and when it was fully utilized, a promising observation was made. Late that morning a 33–40-foot (10–12-m)-long object appeared on the sonar screen at a depth of between 330 and 400 feet (100–120 m). The object was animate, and appeared to be a living animal. However, procrastination by the NTV investigators allowed whatever was below the surface to slip away before anyone would adhere to a course of action.

With a seemingly endless budget at its disposal, NTV lengthened the flights of a search helicopter provided by Canadian Helicopters, an Okanagan-based firm. Despite covering vast tracts of the 79-mile (127 km) lake, no sightings or any unusual activity were recorded. With nothing to show for its efforts, NTV spent one last day combing the lake and filming eyewitness testimony before ending the venture after 8 exhausting days.

Results

It has always been the position of BCSCC that any search or investigatory activities at Okanagan Lake should be entered into during the months of June, July, and August, when most Ogopogo sightings are reported. By launching an expedition during one of the most brutal winters in recent British Columbia history, NTV erred in the extreme. Wave heights were such that any object on the surface would have been lost in the flurry of whitecaps. Subsurface activity would have been almost impossible to zero-in on as surface vessels were buffeted and moved by volatile waves. Visibility was often reduced to less than 300 feet (91 m) by blowing snow and fog.

It was our considered opinion that NTV was never in a likely position to obtain evidence of the existence of an Ogopogo-type animal. Producer Michihito Ogawa conceded that, had it not been for production deadlines, a summer expedition would have been the better option.

In view of the above, outside groups who wish to investigate the Ogopogo problem would be well-advised to first discuss their plans with individuals like Arlene Gaal, or groups like BCSCC, who by trial and error or by experience are better equipped to conduct fieldwork at Okanagan Lake.

Any expedition to Okanagan Lake should take a leaf out of the notebook of the now-defunct Loch Ness Investigation Bureau (LNIB). LNIB carried out painstaking investigations over long periods of time in the 1960's, using the services of copious amounts of volunteers and numerous still and movie cameras. Add to this the use of sonar and strategic positionings of personnel around the lake, and one has a model of what investigation at "monster lakes" should be like. Short forays by ill-trained observers are very unlikely to produce sightings or evidence. Meticulous attention to detail and planning are the only means by which an investigation will be effective.

Although the NTV participants did not adhere to the Loch Ness model of investigative technique, they are nonetheless to be commended for the enthusiasm with which they approached their task. Their willingness to expend large sums of money in search of Ogopogo should serve as a catalyst to others who have the financial means to contribute towards the solution of the mystery present at Okanagan Lake.

Future Plans

BCSCC had no firm plans for 1992 fieldwork. Due to prevailing economic conditions, sponsors have not been able to fund the Club's field activities. If any expeditions are undertaken in 1992, they will be greatly reduced and of short duration. Should sponsorship be forthcoming, BCSCC is considering a two-month-long visit to "monster lakes" across North America. These lakes would include Okanagan Lake, Lakes Pend Oreille and Tahoe, Flathead and Payette Lakes, as well as Lakes Champlain, Memphremagog, and Erie.

Cryptozoology, 10, 1991, 76–78

SASQUATCH INVESTIGATIONS IN THE PACIFIC NORTHWEST, 1991

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INTRODUCTION

This report updates my previous findings published in this journal concerning continuing attempts to obtain evidence for the Sasquatch (Bigfoot), a reported large, unknown primate in North America (James A. Hewkin, 1990, Sasquatch Investigations in the Pacific Northwest, 1990, *Cryptozoology,* Vol. 9: 82–84).

Several short field trips were undertaken in the Cascade Mountains, and one 3-day trip was taken into the Blue Mountains of northern Oregon. This report summarizes information from the more productive fieldwork.

NARRATIVE DESCRIPTION

On June 11, 1991, in the Cascades, I examined fresh evidence in the form of a torn-up log showing a possible nail mark, as well as several muddy streaks. No claw marks were evident. The site was in an older clear-cut, about 15 years old, and densely vegetated with fir tree reproduction and brush. A healthy population of mountain beaver (*Aplodontia rufa*) was present in the area, and the ground was perforated with their tunnels.

In the company of Jack Sullivan, I spent the period of July 17–19 in the Blue Mountains, from where there have been numerous Sasquatch reports in recent years. We examined three torn-up logs which indicated possible Sasquatch presence. One log revealed what appeared to be a Sasquatch nail mark. Another log had been rolled out of its half-buried position. It measured about 9 feet (2.7 m) in length, and had a diameter of about 20 inches (50 cm). The location of this log is about 1 mile (1.6 km) north of the area where we found similar evidence in 1989 (James A. Hewkin, 1989, Continuing Sasquatch Investigations in the Pacific Northwest, *Cryptozoology,* Vol. 8: 73–74).

A May 18 interview with a witness living near Estacada, Oregon, whose name is not being divulged, revealed interesting new information on possible Sasquatch behavior. The contact was initiated by a school friend of the witness's son, who informed Sullivan of a possible Sasquatch encounter. Sullivan then relayed the information to me. The reported incident occurred in 1979, while the witness was deer hunting in the Cascades, in the Molalla River drainage in western Oregon. The witness remembers the year because it was near the time his daughter was born. After he and his partner had hunted most of the day—with no success—the witness, while alone, sat on

a stump overlooking a small meadow. The meadow was surrounded by timber, and was bisected by a tiny meandering stream.

After about an hour—it was late in the day—a doe deer and fawn appeared, and began feeding close to him. He watched them for about 15 minutes. The doe was about 60 feet (18 m) from him; the fawn had moved closer to the edge of the meadow, nearer the timber. Suddenly, a large, dark animal leaped out, seized the fawn, and leaped back into the timber. The fawn was heard squealing and bleating, and then there was silence. The doe reacted immediately by snorting, blowing, and prancing around in typical alarm. Other deer, of which the witness had not previously been aware, were also heard snorting in the timber.

The attention of the doe was directed to the witness. The incident had occurred so fast that she had apparently failed to notice exactly what had happened, and had perhaps been attracted to the witness by his flinching at the moment of the incident. The witness immediately left the area and walked to a nearby logging road, about 300 feet (90 m) away, where he met his hunting partner. He told him that he had just seen a bear seize a fawn. His partner had not seen or heard anything unusual. They left for home without further investigation.

Upon getting home, the witness told his wife: "That was not a bear!" He realized that the animal had moved rapidly on two legs, and appeared to have seized the fawn with its hands. The distance from the edge of the timber to the fawn had been no more than 6–8 feet (2–2.5 m), and the witness' perception was that the animal had leaped out in one large step, made the grab, leaped back, and vanished in the timber. The incident happened so quickly, the witness stated, that he had no time to observe much detail. However, he was positive that the animal had moved bipedally, not quadrupedally, and had "hunched-over" shoulders. He described the animal's color as neither black nor brown, but "very dark." The site was less than 1 mile (1.6 km) from a logging operation, and a small pond was a short distance away.

I interviewed the witness' hunting partner on May 28, and his version confirmed what the witness had stated—although, of course, he had not been present when the supposed Sasquatch incident occurred. He remembered that the witness had appeared distraught when they had met on the logging road after the incident occurred. The two have not hunted together since. He also provided further information on the location.

The site of this reported event is about 14 air miles (22 km) south of where I reported Sasquatch tracks in 1986 (James A. Hewkin, 1987, Observations of Two Lines of Sasquatch Tracks in Oregon, *Cryptozoology,* Vol. 6: 78–84), and about 10 air miles (16 km) south of where I reported possible carrion taking by Sasquatch (James A. Hewkin, 1986, Investigating Sasquatch Evidence in the Pacific Northwest, *Cryptozoology,* Vol. 5: 27–37).

The described reaction of the alarmed doe is consistent with deer behavior when they are alarmed and confused, and I accept this as a credible report. To my knowledge, it is the only reported observation of a supposed Sasquatch actually preying on another animal. The reported speed of the animal in this incident reinforces my thinking that Sasquatch are very efficient predators, and it is possible that meat is, in fact, a major component of their diet.

On October 18–20, I undertook a field trip with John Green and John Bindernagel, a wildlife ecologist, in an area of the Cascades. We were looking for rock pits, as reported here previously (James A. Hewkin, 1986, above). We investigated an area I was familiar with, and I was surprised to find three additional, very old pits I had overlooked on previous trips.

In one area, 14 rocks had been pulled out of a trail over a distance of about 300 feet (90 m). There were no visible scratch marks on the rocks, making it unlikely that it was the work of a bear. This portion of trail was located on a heavily-vegetated north exposure, cool and shady. At one point, about 1 mile (1.6 km) further along the trail, a salamander was observed crawling under a rock. It was a rough-skinned newt (*Taricha granulosa granulosa*), which are common in boggy ponds in the area. Such small animals under rocks are a likely food source for Sasquatch.

Results

No strong evidence for Sasquatch presence was obtained in 1991. As in previous years, nail marks on logs were provocative but inconclusive. The 1979 sighting report involving the seizure of a fawn is an interesting addition to the Sasquatch literature.

Future Plans

Further fieldwork will be conducted during 1992 with the purpose of uncovering new evidence for the presence of large, unknown bipedal primates in the forests of the Pacific Northwest.

Book Reviews

Cryptozoology, 10, 1991, 79–102

Mysterious Creatures. By the Editors of Time-Life Books (part of the Mysteries of the Unknown series). Time-Life Books, Alexandria, Virginia, 1988. 144 pp. $14.99 (c.).

Mysterious Creatures, part of a Time-Life series—this one on "mysteries"—contains three main chapters: Creatures of the Sea, The Quest for Nessie, and In Pursuit of Bigfoot and Yeti, plus several separate "essays" on particular topics. The main chapters are illustrated by high-quality photographs and drawings, and the essays by both photographs and paintings.

The first essay, at the front of the book, is titled A Gallery of Fabled Beasts. This well-written but necessarily superficial text is accompanied by magnificent paintings of the Dragon, the Manticore, the Kraken, the Basilisk, the Hydra, and the Griffin. Following this comes the first chapter, on mysterious sea creatures. These include sea serpents, the coelacanth and its discovery, the giant squid (which is no longer cryptozoological), and the giant octopus. A review of several hoaxes and a discussion of cases of mistaken identity are timely and complement the chapter, as does a discussion of the research by Paul LeBlond and John Sibert on the unidentified marine animals reported off the Northwestern Pacific coast. An overview of stranded carcasses rounds out this chapter effectively.

Two essays follow, again accompanied by paintings. These are titled Confronting the Giant Squid (the famous 1873 giant squid attack on a small fishing boat off of Newfoundland), and Beasts in Human Form, which includes ancient and medieval artwork depicting what are clearly mythological composite beast-human constructions such as mermaids, dog-headed men, satyrs, and centaurs. I was left wondering why this essay was placed here rather than with the later chapter on human-like cryptids.

The second chapter, on Nessie, also discusses the Lake Champlain Monster—and shows the Mansi photograph—and Ogopogo in Lake Okanagan. It provides a fairly comprehensive and reasonably accurate overview of the Loch Ness story, covering Nessie sightings and events in the 1930's, and the work of the Loch Ness Investigation Bureau in the 1960's, the Academy of Applied Science in the 1970's, and the Loch Ness and Morar Project in the 1980's—including Operation Deepscan in 1987. The text is well-illustrated,

and included are some of the classic shots of supposed Nessies and individuals involved in the search.

For some reason, however, a short, one-page essay on the Buru mystery in Assam, northeast India, appears within the Nessie chapter. The Buru really has nothing whatever to do with Loch Ness or any animals which may exist there. This Nessie chapter is followed by a full essay, an illustrated but relatively short piece titled An Elusive Creature of the Congo, which covers my own two expeditions in search of Mokele-Mbembe, as well as that of Congolese zoologist Marcellin Agnagna.

The third and last chapter is dedicated to the Bigfoot-like creatures. It begins with a recounting of the Patterson sighting report and filming—including still shots from the film footage, which remains to this day the single most probative evidence in support of the existence of such creatures. This is followed by a review of Yeti history and lore, a sidebar story on South America's Loys's "Ape," the Russian-Mongolian Almas, another sidebar story on the Iceman, and finally the Chinese Wildman or Yeren.

This leads the narrative back to Bigfoot or Sasquatch, which, like its Chinese counterpart, is thought by many to be a surviving fossil ape known as *Gigantopithecus*. The history of Sasquatch investigations is then reviewed, including the work of John Napier, John Green, and Grover Krantz. This fairly accurate chapter is a good overview of the whole problem of such reported unknown hominoids, and will be particularly useful as an introduction for those who know little of the subject. The illustrations are again excellent, and include the famous Shipton Yeti footprint photo, and a photo of an alleged Yeti scalp (made from a serow, a goat-like animal, at least in the case of Sir Edmund Hillary's specimen).

The next and final text is the last picture essay titled Monsters at the Matinee. This is a delightful review of some classic class B monster movies, mainly from the 1950's, with accompanying posters: *Them,* on giant "atomic" ants; *The Beast From 20,000 Fathoms* (Cast of thousands! Over a year in the making!), about a giant dinosaur—revived by atomic testing—which goes on the rampage; the original version of *The Fly* starring Vincent Price; *The Mutations,* with Donald Pleasence; and the more recent *Prophecy*—our monsters are no longer "atomic," but result from industrial pollution! The book ends with acknowledgments, a useful bibliography, and a detailed index.

Although the book is clearly a worthwhile effort, it is not without its faults. A photograph of a fish purported to be a coelacanth on p. 23 is certainly not a coelacanth. Numerous minor errors and unsupported statements appear here and there, and there is a quite unnecessary discussion starting on p. 120 linking the Sasquatch question to UFO phenomena, based on some Pennsylvania reports.

What we must remember, however, is that this is not a scientific or scholarly work. In fact, strictly speaking, it is not even a book about cryptozoology. It is a book about "mysterious creatures," a topic which in this case includes both cryptozoological as well as entirely folkloric and mythical entities, and it is presented in a popular format as part of a series on many kinds of mysteries which Time-Life felt appropriate for its market. Within this context, the book will be acceptable to most ISC members, if for no other reason than to have a nice compilation of photographs and illustrations all under one cover. (While the book is difficult to obtain from Time-Life on an individual basis, some used bookstores now sell it individually at half price.)

From our perspective, of course, it is unfortunate that the editors at Time-Life did not expand the volume to cover some of the many other animals of cryptozoology. Unfortunately, and probably for the same marketing reasons mentioned above, all we get is a two-page photo spread on some major 20th century zoological discoveries: the giant panda, the mountain gorilla, the giant forest hog, the okapi, the Komodo dragon monitor lizard, and the megamouth shark.

Perhaps the main positive attribute of the book is its objective tone. The Time-Life writers and editors have generally walked a careful middle ground, and have avoided trying to convince the reader of the reality of the animals discussed within the book's pages.

Whatever its shortcomings, a final passage in the text will leave the cryptozoologist happy: "For these individuals [cryptozoologists], the planet earth remains a large place, retaining its mysteries and capable of surprises—in spite of all the efforts of civilized humans to put distance between themselves and their natural origins in the wild. There is room for giants, they say, not only in the universal myths of the monsters that came before us, but in the thickets of Oregon and the high forests of the Himalayas."

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MYSTISKE DYR: EN GUIDE TIL INFORMATION OM SOSLANGER, HAVUHYRER, AFSKYELIGE SNEMAEND OG ANDET GODT [Mysterious Animals: A Guide to Information on Lake-Monsters, Sea-Serpents, Snowmen, and Others]. By Lars Thomas (comp.). Andersen Bookservice, Klingseyvej 28, Vanlose, Denmark, 1989. 167 pp. (3 vols.), US$19/£12 (p.).

This is an unannotated bibliography which lists books, articles, and newspaper items in the following separate sections: general cryptozoology (very roughly 200 items), lake- and sea- and water-monsters (about 1,400 items), Bigfoot and other unknown hominoids (about 800 items), miscellaneous

creatures (about 400 items), and possibly extinct species (about 200 items). There is also a two-page list of periodicals and organizations of interested people; the third volume consists of alphabetized title and author indexes.

This three-volume set represents a commendable effort, and the authors are to be thanked. Any serious student of any of the matters covered by the bibliography will need to obtain a copy. In doing so, however, he or she should compare the listing with whatever other bibliographies already available, as there are some serious deficiencies.

First, one would like to know the sources that were drawn on, so that future searching of the literature could be more efficient. Related to this, one would like to know what criteria were used in deciding whether or not to include any given item. For example, has the author actually seen a copy of every item listed? One suspects that language was one criterion, since almost all of the items are in English.

Second, one would like to see better proofreading; there are innumerable errors of spelling and the like.

Third, one would like to be told at the outset how the listings were ordered. For example, there are 10 items headed "Sea serpent" or "Sea-serpent," and the first is in the *Naval Chronicle* while the third is in the *American Journal of Science*—so clearly we are not going alphabetically. The first nine items *are* in chronological order, but the tenth is not—but then one notices that the tenth is headed "Sea serpent?" and realizes that the question mark distinguishes this heading from the other nine. And beyond that, unfortunately as well as unconventionally, the articles "a" and "the" are taken as essential parts of the title, resulting in another two items—30 pages earlier—being titled "A sea-serpent," and about another 20 items—5 pages later—being titled "The sea-serpent" or "The sea serpent."

Fourth, one would like to see more information about quite a number of the items. For many of the books, the publisher or place of publication are missing (thus for Roy Mackal's *The Monsters of Loch Ness,* the British publisher Macdonald & Janes is listed, but not the original Swallow [Chicago] edition). Newspaper titles are almost always given without place of publication. Now, perhaps for the *Sunday Times* or the *National Observer* that may matter relatively little, since *most likely* they are either British or American; but what about the *Lowestoft Journal,* or *The Sun* (Vol. 1, p. 16), or the *Evening News* (Vol. 1, p. 19)?

To estimate how complete the coverage might be, I compared samples from the second section with samples from my own Nessie bibliography which appears as an appendix in *The Enigma of Loch Ness: Making Sense of a Mystery* (University of Illinois Press, Urbana and Chicago, 1986). I compared only samples because the different methods of ordering references makes comparing a very tedious process. Of the 32 books I listed, nine are not listed by Thomas, all of them books for younger readers or in languages

other than English. However, of the items I listed as "Sections or Chapters of Books," very few are given by Thomas, even where they are items in English and not for youngsters. Of the articles in periodicals that I listed, apparently *all* the book reviews and magazine items for youngsters are omitted by Thomas, and a considerable proportion of the remainder are likewise omitted.

Clearly, then, this bibliography does not supersede others, but is an adjunct to them. Certainly, I found some items here of which I had not been previously aware, and that had some interest for me. In short, here is something that most if not all cryptozoologists should have—at least those of us who belong to the armchair rather than the field variety. Much could still be done to improve this bibliography, however, and I hope the author will do so in possible future editions.

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Prodigious Birds: Moas and Moa-Hunting in Prehistoric New Zealand. By Atholl Anderson. Cambridge University Press, Cambridge, New York, Melbourne, 1989. 238 pp. US$79.50 (c.).

Much has been written about moas over the past 150 years, including several books and monographs. *Prodigious Birds* is the first comprehensive modern work that pulls it all together. Before reviewing this important volume, however, let us briefly summarize what moas were and why they are interesting.

Moas belong to a group of flightless birds known as ratites. These include the ostrich, the rhea, the cassowary, the emu, and the kiwi. All moas belong to the Order Dinornithiformes. Over a dozen species are recognized, and the largest, *Dinornis giganteus,* is probably the tallest and bulkiest bird that ever lived. It is thought to have stood 10–12 feet (3–3.6 m) tall. These giant birds were restricted to the southern Pacific islands known today as New Zealand, which, because of their geologically-caused zoogeographical isolation since breaking away from the supercontinent of Gondwanaland about 80 million years ago, possess no known native quadrupedal terrestrial mammals. This isolation, with no danger from mammalian predators, resulted in numerous New Zealand birds, including the moas, becoming flightless.

The moas might have lived happily ever after (or as long as "ever after" can mean in geological history, with its changing climate, landscape, and

resources). But then, after millions of years of quiet isolation, something happened. As recently as about 1,000 years ago, humans discovered what could almost be called "the last Eden." These Polynesian invaders, the ancestors of today's New Zealand Maori people, encountered what must have seemed at the time to be a bottomless pit of moa food availability.

Over the next centuries, moas were systematically hunted—together with other native birds and marine mammals—until about 300–400 years ago, by which time they had probably become rare. The first 70 years of European colonization following Captain Cook's arrival in 1769 did not result in the existence of moas coming to scientific attention. This did not occur until the 1830's–40's, and an intense search for moa remains then began. These specimens generally found their way back to British museums, and, eventually, to New Zealand institutions. Early on, the search for moa remains and related archaeological material was paralleled with the search for presumed live specimens. Numerous native accounts seemed to refer to living moas, or at least to persistence into the decades immediately preceding their scientific discovery. In addition, some European accounts described sightings of living moas. Unfortunately, no live moa—or absolute evidence of their recent survival—was ever produced.

Although a few such accounts have continued up to recent decades, the prospects of finding a restricted and isolated moa population alive today, even of one of the smaller species, are now considered extremely remote at best by New Zealand zoologists.

Attitudes towards the question of moa extinction/survival may be approximately divided into four categories: the conservatives, who maintain (and even maintained in the 1800's) that all moa species became extinct centuries before European arrival; the conservative moderates, who accept the possibility of some restricted moa populations persisting at least until the arrival of Captain Cook in 1769; the liberal moderates, who accept the survival of some moa species well into the 1800's; and the liberals, who accept moa survival into the early-mid-1900's—and possibly even to the present. I suspect that most persons with an interest in cryptozoology probably subscribe to the third attitudinal category—that some moas may have survived into the 1800's—with perhaps a romantic, if secret, hope of the cryptic survival of a miniscule population to the present.

Having established the background, let us now examine *Prodigious Birds.* In chapter 1 (Introduction) the author, an archaeologist at the University of Otago, in Dunedin, New Zealand, briefly reviews the first moa material discoveries, beliefs in moa survival, systematics, native moa-hunting evidence, and extinction. Part I of the book, titled Discovery and Biology of Moas, contains chapters 2 through 6. Chapter 2 covers the first discoveries, which represent a complicated and colorful history.

The first European collection of moa material occurred sometime between

1831 and 1836 when an East Coast (North Island) trader named John Harris obtained a strange bird bone from the Maoris. It was supposed to be from some kind of eagle. In early 1838, before returning to New Zealand, Harris left some things in Sydney for his uncle, John Rule, who was visiting Australia at the time. These included the bone. Returning to England in 1839, Dr. Rule was unable to identify the bone when comparing it to museum specimens. He then wrote to Richard Owen (later Sir Richard), a well-known anatomist at the Royal College of Surgeons offering to sell it to the College for 10 guineas. Upon personal inspection, Owen, preparing for a lecture, first showed little interest in the bone, implying that it had probably come from a large domestic mammal. But Rule, insisting, pointed out the avian nature of the bone's interior, and Owen then agreed to study it further.

Owen eventually concluded that it had come not from an eagle, but from a giant, flightless ratite bird. He presented the 6-inch bone shaft at a talk before the Zoological Society of London, which, despite skepticism, published his report that same year (1839) in its *Proceedings.* Ironically, despite Owen's urging, the Royal College of Surgeons declined to purchase the bone, so it was sold to a private collector who in 1873 bequeathed it to the British Museum, whose natural history division—The Natural History Museum—Owen was to become the first director of in 1880. But Owen's involvement was not entirely angelic either. In his first lecture he failed to mention that Rule had been the one to point out the important avian nature of the bone, and, writing decades later, he referred to Rule, who was also a member of the Royal College of Surgeons, as merely "the vendor."

Concurrently, while this little drama was unfolding, the first known European knowledge of moas was actually occurring back in New Zealand—specifically in 1834, when trader Joel Polack was shown large fossil bone material by native people at Tolaga Bay, also on the East Coast. The Maoris told him that the bones had come from giant birds that had long ago been exterminated by hunting. Polack sagely concluded that "a species of emu, or a bird in the genus Struthio" [ostrich] had once inhabited New Zealand, and that possibly some still survived "in parts, which, perhaps, have not yet been trodden by man." This account appeared in his 2-volume book *New Zealand, being a Narrative of Travels and Adventures,* published in London in 1838, but he had neither the bones—nor the native name *moa*—to go with the story. (The book was published just one year before Owen's talk and paper, and there has been some speculation that Owen, having seen Polack's account, may have been secretly inspired to deduce that Dr. Rule's bone had come from a ratite bird.)

But the story does not end here, by any means; 1838 was also the year when the actual word *moa* was used for the first time in the context of such flightless birds. In January of that year, missionary William Williams and printer William Colenso, when at Waiapu, were told of a giant bird by that

name that was said to live in a mountain cave, and also about large moa bones. In 1839, Williams returned to the East Coast with the Rev. Richard Taylor. According to Williams, he offered a reward for moa remains, and was given a bone which proved to be of little value. Writing 5 years later, however, Taylor claimed to have obtained a Moa toe on the same trip. By 1873, when he died, this toe had grown into part of a bone that he had readily identified as a bird bone, and which the Maoris had confirmed to him as having come from a giant bird—thus making him the "discoverer of the moa."

Colenso, meanwhile, was not to be outdone, and he retained for himself the title, as defined by Anderson, of "the first person in New Zealand to identify moa bones as the remains of struthious birds." In 1841–42, Colenso traveled on the East Coast in search in living moas—unsuccessfully. But he reportedly collected seven moa bones from natives at Waiapu, and in November, 1842, submitted å paper which was published in two separate journals in 1844 and 1846. But had Colenso benefited from Owen's 1839 paper (which he reportedly did not see until 1843), as was later implied? Colenso denied this, but Anderson states: ". . . the conclusions of it [Owen's paper] were being discussed as early as July 1842 when Williams was visited by the Rev. William Cotton, who had arrived at the Bay of Islands with Bishop Selwyn in June . . . Since others to whom Owen recalled giving copies of his paper had been in New Zealand since 1841 it is not impossible that Colenso had heard something even if he did not know its source." Anderson also expressed strong suspicion that it may have been moa bones collected from natives by Williams, as well as other information from Williams, which Colenso used without attribution in his paper.

Colenso not only did not credit Williams, but he later also attacked Polack—whose book had been published in London in 1838—when he supposedly first learned of it more than 50 years later, in 1892! This was 10 years after Polack's death. In a later letter in Walter Mantell, Colenso claimed to remember Polack well, accused him of fabrication, and defamed his Jewish ancestry.

Anderson attributes Colenso's viciousness to "the realization that he could no longer sustain a claim he had cherished for decades." Nevertheless, "his contribution was at least as important as Polack's discovery. Williams wrote very little about moas, and Taylor was careless or confused in his recollections. It fell to Colenso to describe the early results of the East Coast research, both osteological and ethnographical."

Back in London, meanwhile, Owen had received or had access to much more moa bone material, and he went on to describe 5 moa species in 1843 alone, including the largest, *Dinornis giganteus* (Fig. 1). Anderson concludes that "the discovery of moas was not, and hardly could have been, the single event which some of those involved perceived it to have been."

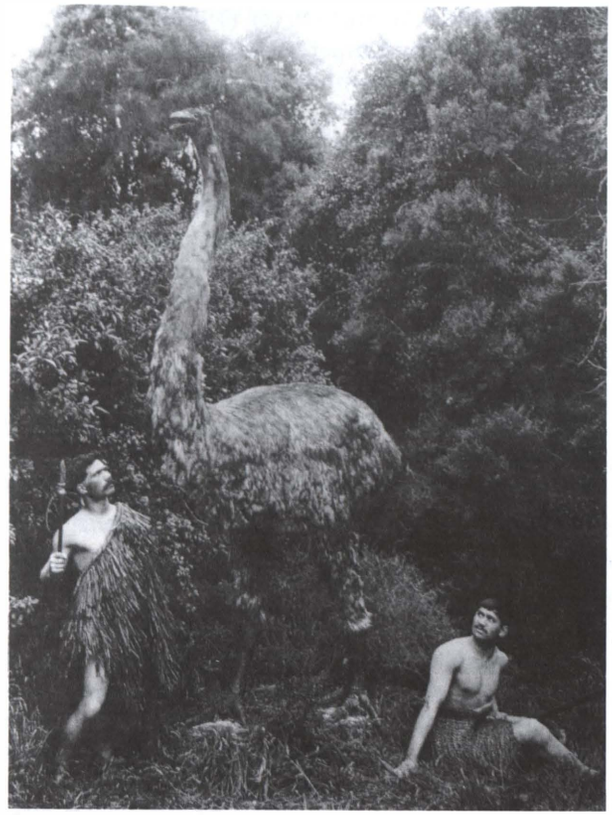

FIG. 1—Otago University Maori medical students Wi Repa and Te Rangi Hiroa (later Sir Peter Buck) posing with large restored moa (*D. giganteus*?) in Dunedin's Woodhaugh Gardens in 1903. (Photo by Augustus Hamilton. From *Prodigious Birds,* courtesy of the National Museum, New Zealand.)

Anderson's third chapter covers the systematics of moas, a topic even more convoluted than the history of their discovery. Owen was the first authority on moas, resulting in 30 papers in 40 years, and his taxonomic schemes, which often changed direction over time, are still felt to this day. Other 19th century authorities were Sir Julius von Haast, director of New Zealand's Canterbury Museum, Frederick Hutton, director of the Otago Museum, Henry Forbes, at the Canterbury Museum, Jefferey Parker, at the University of Otago, and Richard Lydekker, at the British Museum (Natural History). All these gentlemen were involved in differences of opinion based on new osteological material and differing theoretical approaches, much of which resulted in lumping or splitting.

In the 20th century, moa taxonomies were erected by Lord Rothschild

(1907), G. Archey (1941), Walter Oliver (1949), Ron Scarlett (1972), and Joel Cracraft (1976), whose lumper approach is the most accepted today. The longest (splitter) classification was by Rothschild, which included seven genera and 37 species.

In *Prodigious Birds,* Anderson uses the following taxonomy of six genera and 13 species:

Order:	Dinornithiformes;
Family:	Anomalopterygidae;
Genus:	*Anomalopteryx*; species: *didiformis*;
	Megalapteryx; species: *didinus*
	Emeus; species: *crassus*
	Euryapteryx; species: *curtus, granoides, gravis*
	Pachyornis; species: *mappini, australis, elephantopus*
Family:	Dinornithidae;
Genus:	*Dinornis*; species: *struthoides, torosus, novaezealandiae, giganteus.*

Chapter 4 reviews the origin and evolution of ratite birds, New Zealand zoogeography, and moa ancestors. Currently available fossil material unfortunately only dates to the Pliocene. Anderson goes on to describe the natural sites from which moa material has been recovered, and their geographic distribution. These are swamp sites, cave sites (including sinkholes and rock shelters), and dunes and loess sites. This gives a preliminary picture of moa ecology. Of special interest is the fact that, of the recovered material on the North Island, small species represent 80 percent, while, on the South Island, medium-to-large species represent 75 percent. Environmentally, in hill country and coastal dunes, small species represent 71 and 85 percent respectively, while in "downlands," medium species represent 59 percent and medium-large species represent 24 percent. This chapter is rounded out by further discussion of moa habitats.

In chapter 5, the author tackles the difficult questions of biology and behavior. Various methods of estimating height and weight are reviewed and then summarized in Table 5.1, although for some unexplained reason *Pachyornis australis* is omitted (and *Emeus crassus* is erroneously identified as being in the genus *Euryapteryx*). Although there is no elaboration in the text, *Dinornis giganteus* is given a "head-up stature" of 8 feet, 3 inches (252 cm or 2.5 m) in Table 5.1, substantially less than the 10–12 feet (3–3.6 m) generally attributed to it. The shortest species is *Euryapteryx curtus,* with a "head-up stature" of 3 feet (93 cm). Half of all moa species turn out to be shorter than some of the other ratites, such as the cassowary or emu; three stood at about the same height, and three more—all *Dinornis*—were significantly taller. As to weight, *D. giganteus* is estimated at 506 lb (230 kg). In fact, some of the cervical vertebrae from this species are almost as large as those from a horse. With the muscle and feathers added, its neck in life must have looked formidable, quite unlike the delicate neck of an ostrich.

Other aspects addressed in this chapter are skin, feathers, presumed feeding behavior, eggs (only 18 complete or partial eggs are known, half of them from archaeological sites), clutch size, presumed breeding behavior, and, finally, abundance. On the latter, after a series of ecological considerations and biomass calculations, including comparisons with other ratites, Anderson concludes that the North Island carried about 26,500 individuals, and the South Island about 44,000 individuals, giving a total of over 70,000 individuals of all species.

Chapter 6, titled Maori Traditions, completes Part I of the book. In this chapter, presumed 19th century Maori knowledge of moas is reviewed. This point is important: can scientific knowledge of moas be advanced by reliance on Maori traditions, either because they knew moas very recently (assuming their survival until late 1700's–early 1800's), or because authentic knowledge had been passed down from previous generations (assuming their extinction centuries earlier)? Anderson seems to discount both possibilities. Maori reports, he points out, were published by Europeans, who presumably may have colored them, and the original Maori informants are in most cases anonymous. Anderson asks why no Maori references to moas reached European ears during the almost seven decades between first colonization and the initial 1830's accounts. Although he wisely proposes the possibility that "earlier references were either not recognized or not associated with the term 'moa,' " he remains skeptical. To complicate matters further, the native term *moa* has numerous other meanings in both New Zealand and other parts of Polynesia.

Anderson then reviews ethnographic studies that include no mention of moas at all, suggesting that 19th century Maori moa information transmitted to Europeans was, at best, folkloric. That is, the folkloric moas—which in this scenario might not even have been regarded as birds, but simply as "creatures"—may not have had any connection at all with the giant ratites hunted by the earlier Maoris—which Europeans named "moa" under the assumption that they were one and the same. It seems to me that a more modern interdisciplinary evaluation of the problem, one involving the interrelationships of folklore, ethnography, linguistics, history, and even sociology, could shed light on these questions. Anderson himself concludes that at least some 19th century Maoris had some knowledge of the biological moas, but that their descriptions became increasingly colored by European influence.

Part II, chapters 7 through 14, is titled Moa Hunting, Processing, and Extinction. Chapter 7 deals with early Moa hunting and whether this began with a pre-Maori people. In the 1860s–70's there was intense debate and disagreement on alternate theoretical models. The participants this time were

mainly Sir Julius von Haast, James Hector, Walter Mantell, and Alexander McKay. Sir Julius took the position that moas had been hunted to extinction by a pre-Maori culture, and the disputes, like in earlier times, finally "descended into a clash of ethics and personalities." Sir Julius's hypothesis was to eventually be falsified, but at one point he accused McKay—who had published a paper in the belief that Sir Julius himself would suppress certain evidence—of being a simple laborer whom he had hired to do excavation work!

This chapter ends with a summary of the work of H. D. Skinner in the 1920's, and Roger Duff in the 1940's–70's. The latter resuscitated Haast's pre-Maori "moa-hunter culture" term, but this time as a generic term for an artifactual assemblage with little if any actual association with moas. Thus, when a moa was dated to Maori times, it created the ridiculous situation of the specimen, by definition, not being a moa, and the Maori not being moa hunters! So much for theoretical cultural models attempting to force reality on the progress of knowledge.

Chapters 8, 9 and 10 deal exclusively with archaeological sites containing moa material on both the North and South Islands. Space does not permit a detailed review here. There are a total of 127 sites, 40 on the North Island and 87 on the South Island. Many sites remain poorly studied, and probably many more remain undiscovered. Chapter 11 deals with hypothesized Maori moa-hunting strategies. Maoris did not have the bow and arrow, and there is no evidence supporting the use of a particular weapon. Slings and wooden spears were probably used. Driving moas into dead-ends for the kill, as was originally proposed, probably did not happen often or successfully. Moas did not flock, and it would probably only be possible to drive a few at a time and for short distances. The use of snares to facilitate killing individual moas was probable. (Dogs may also have been used. However, there is still uncertainty as to whether dog osteology recovered from sites represents domestic or feral canids.) It seems, therefore, that mass-kills did not occur.

Moas are thought to have been diurnal, and hunting may have been more or less seasonal. There is no evidence that specific species were sought, although most belonged to the "*Euryapteryx* assemblage," species which inhabited lowland open forest. On the North Island, 84 percent of moas belonged to this assemblage, and on the South Island 76 percent in coastal areas and 50 percent in the interior belonged to it. Small forest species were largely ignored, and could well have been the last to disappear (or to survive to the present if any, in fact, do persist). Chapter 12 deals with the implements used in processing moa carcasses. These were mainly stone flakes and blades.

Chapter 13, Chronology and Extinction, will be of particular interest to readers of this journal. Anderson has compiled a comprehensive listing of all radiocarbon-dated records from moa collagen and associated marine shells and charcoal. In the process, he has eliminated many records when

he suspected the dating was in error. What is left provides C^{14} dates ranging from about 1,000 to 300 years B.P. Of the 73 dated sites, 32 percent were occupied in 800–700 years B.P., 45 percent in 700–600 years B.P., 36 percent in 600–500 years B.P., 23 percent in 500–400 years B.P., 14 percent in 400–300 years B.P., and zero thereafter. Causes of extinction other than overhunting are reviewed, such as diseases, predation by Maori-introduced mammals, and habitat destruction. Anderson concludes that extinction came about gradually: ". . . the processes overlapped substantially and, together, were undoubtedly lethal." (He also discusses two overkill models, Caughly's "rolling wave" and Martin's "blitzkrieg" [see my review of *Quaternary Extinctions: A Prehistoric Revolution,* Paul S. Martin and Richard G. Klein, eds., in Vol. 6, 1987, of *Cryptozoology*], but they are discounted.)

As to the precise timing of the extinction, he discusses four kinds of evidence: sighting reports by Europeans, Maori information, surface moa remains, and archaeological evidence. In reverse order, he dismisses most of the nine datings from between 300 and 400 years B.P. because of their associations with other older datings. How much longer after 400 years B.P.—when moa-hunting essentially ended—did moas survive "can only be guessed at," he concludes. (One form of archaeological evidence which would establish moa survival into European times, without even the need for radiocarbon dating, would be moa bones found in association with European artifacts, or even with bones of animals introduced by Europeans, such as pigs. Such a discovery could still occur in the future.) His review of evidence from surface sites is similar. No (non-archaeological) surface material has definitely established, in his opinion, the late survival of moas.

Turning to the European evidence, of a total of 46 reports, 23 of them were first-hand sightings. Emphasizing again how there are no credible reports from the first 70 years of colonization, and the fact that no European scientist ever reported finding a live moa at all, and having discussed such accounts "elsewhere," Anderson ends with: ". . . they need concern us no longer." The reader may well feel frustrated at this brush-off, particularly when the sources for "elsewhere" are two incompletely-cited papers because they were still in press at the time of the book's publication. Surely a page or two of summary could have been included to satisfy readers with a particular interest in this topic? Sadly, the only other conclusion by the author on this matter appears in chapter 6, where he states, without elaboration, that the European reports do not "survive elementary tests of historiography."

The Maori evidence does not fare much better. By tabulating all published accounts of supposed Maori moa knowledge, Anderson finds that a quarter of them refer to extinction in the remote past, and another quarter place extinctions in the 1600's and 1700's. Almost half, however, place extinction (or survival) during that bothersome period between the 1770's and the

1840's. "The point of greatest difficulty . . ." he concludes, "is to accept that moas could have survived into the period 1770–1840 without much clearer [Maori] traditions about moa names, biology or associations with Maoris."

In his final, brief chapter (Conclusions, which summarizes the book in general), he emphasizes this point even further: "Much was asserted about moas, and moa-hunting and extinction as well, by those who espoused alleged traditions as historical evidence . . . But while the content of some of these stories is plausible, there are cogent arguments against accepting nearly all of them as genuine recollections." Also: "It is very unlikely that moas survived much longer than that date [400 years B.P.]. Alleged traditional evidence is thoroughly contradictory and clearly confused by naive interpretation of a complex ethno-ornithology." Thus, one of his final sentences says it all: "Dinornithiformes, known as moas, were a group of large, wingless, New Zealand ratites which became extinct prior to the arrival of Europeans."

This last chapter is followed by a 17-page bibliography containing 746 references. It is undoubtedly the most comprehensive bibliography ever published. Several appendices follow on the various taxonomies, radiocarbon dates from natural sites (nothing dates from less than 500 years B.P.), species identified from archaeological sites, radiocarbon dates from archaeological sites, and a list of Maori accounts of Moa extinction and survival. The book ends with an appropriate index.

Whatever their particular interests or orientations, readers will find *Prodigious Birds* useful. It is, in fact, unique, and will probably remain the general authoritative source on the subject for a long time to come. Few errors were detected, and the only sources of aggravation for me were the poorly designed layout in the taxonomies appendix, and the unfortunate use by the publisher of parentheses within parentheses rather than brackets, which often necessitated careful re-reading, particularly when citations were involved. Some may also feel that the high price of the book is not justified in view of its lack of any color illustrations.

I will make only two further comments. First, after all of the discussions and all of the conclusions are digested, I cannot help but feel that, somehow, there is still something missing. What the book lacks is a certain romance, a romance reflecting the magnificence of the moas themselves, both in time and place, a romance which the author, had he felt so inclined, could have shared with his readers. Coming to moas from archaeology, perhaps the author felt this unnecessary, preferring to concentrate on a solid scientific treatment—which, I should hasten to add, the volume certainly represents. I suppose, too, I would admit, if pressed, that science can indeed move forward quite well without romance. It's just less fun.

The second comment concerns the author's dismissal of the European moa sighting reports, and, consequently, moa survival until at least the early 1800's. (I won't even get into possible moa survival into this century, at least not in this review.) The author's approach is similar—but slightly more negative—to that expressed previously in 1984 (see Martin and Klein, above).

My concluding thought is this: let us picture a scenario in which, after millions of years of adaptation and evolution, all through a period when humans did not even exist, the moas suddenly become threatened, destined to soon disappear into the evolutionary night—forever—just as the first advance scouts of Science (with a capital "s") are appearing on the horizon. What a tragedy it would be if some moas *did* survive to be seen by European eyes, only to have history erroneously record otherwise—forever.

Of course, one could reverse this ending, as Anderson might propose. How tragic it would be if all moas *had* died long before Europeans arrived, only to have history erroneously record otherwise—forever. Either way, it's a peculiarly romantic notion, and I can't help but feel that this is still unfinished business.

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The Search for Nessie in the 1980's. (Proceedings of the symposium held at the Royal Museum of Scotland, Edinburgh, July 25, 1987. Published as Parts 2 & 3 of the Centennial Volume of *The Scottish Naturalist,* 1988). By J. A. Gibson and David Heppell (eds.). The Scottish Natural History Library, Foremount House, Kilbarchan, Renfreshire, Scotland, U.K., 1988. 178 pp. £20 (£11.50 or US$20 to ISC members, postage included, for 3-part set) (p.).

Casual lake monster enthusiasts may be disappointed in this collection of papers which presents, overall, a balanced treatment of a number of issues pertinent to Loch Ness and its most famous resident(s). The emphasis of the articles is on hard evidence, rather than the more colorful and entertaining speculation that has characterized many previous works in this field. However, for every amateur cryptozoologist put off by the technical nature of some of the contributions, at least one critic of cryptozoology may be converted, as the papers presented clearly reflect the fact that modern cryptozoology operates under the same rules that guide other scientific disciplines, and that cryptozoology, even in the absence of conclusive evidence for the existence of a putative "monster," can generate data useful to other fields.

The publication gathers together six papers presented at the (1987) Sixth Annual Membership Meeting of the International Society of Cryptozoology, held jointly with the Society for the History of Natural History at the Royal Museum of Scotland, in Edinburgh. The Nessie proceedings constitute parts

two and three of the centennial volume of *The Scottish Naturalist.* In addition to the full-length contributions, the proceedings include a preface by co-editor and symposium chairman David Heppell, and an obituary of Tim Dinsdale by Richard Fitter which highlights Dinsdale's contributions to cryptozoology in general and the Loch Ness problem in particular. Appropriately, Dinsdale, who died shortly after the symposium, had been publicly informed at the meeting that he had just been elected an Honorary Member of ISC. (A summary of all of the papers presented, including that of Dinsdale—who died before being able to prepare a manuscript for the proceedings—is available in a 1987 issue of *The ISC Newsletter,* Vol. 6[4]: 1–5).

Several of the papers summarize the contributions, successes, and failures of individual or group efforts to solve the "mystery of Loch Ness." The most general of these, Richard Fitter's condensed history of monster investigations, "The Loch Ness Monster: Saint Columba to the Loch Ness Investigation Bureau," is the lead article. The paper is an overview of some, but by no means all, of the significant attempts to analyze the Loch Ness phenomenon. It stresses the roles of Constance Whyte, Tim Dinsdale, and David James in this area, and follows the development of the now-defunct Loch Ness Investigation Bureau, concluding that the Bureau, despite intensive activity, made little progress towards the identification of Nessie. Aside from presenting the modern history of the problem from an insider's perspective, Fitter also highlights the role of the press in sensationalizing the Loch Ness phenomenon and detracting from serious investigation.

Robert H. Rines' contribution of eight pages is "A Review of Research Contributions at Loch Ness by the Academy of Applied Science." As the title suggests, this is a rather straightforward summary of that organization's use of sonar and underwater photography to gather conclusive evidence in support of the presence of a large animal in the loch. A series of photographs of unidentified underwater objects are reproduced in the paper, and reference is made to the well-known "flipper" photos that played a role in the scientific naming of Nessie as *Nessiteras rhombopteryx* by Sir Peter Scott and Robert Rines (1975, Naming the Loch Ness Monster, *Nature,* Vol. 258: 466–468), a name that Fitter, in his article, concludes is a junior synonym of *Megalotaria longicollis* Heuvelmans 1965.

While other names have also been proposed and may indeed have claim to priority, a cursory examination of the latter description (Bernard Heuvelmans, 1965, *Le Grand Serpent-de-Mer,* Plon, Paris) reveals that *M. longicollis* is based on a composite set of features derived from reports of many long-necked "sea serpents" from around the world. As such, the description (which, incidentally, both implicitly and explicitly identifies the organism in question as a pinniped) is not based on one or more specific individuals or types, and would therefore appear to be invalid. The more recent name proposed by Scott and Rines (1975), is, however, based on a photograph, the subject of which would serve as the name-bearing type of the species.

By far the most voluminous contribution is an 89-page treatise by Adrian J. Shine and David S. Martin titled "Loch Ness Habitats Observed by Sonar and Underwater Television." Unlike most attempts to document Nessie by means of passive surface observation, the Loch Ness and Morar Project was almost a guaranteed success because its existence could be justified on the grounds that the biology and limnology of the loch would be illuminated by the use of sonar and underwater television techniques. In this, the Project seems to have succeeded admirably. Fully 49 pages of the text are devoted to the physical and biological features of Loch Ness, with an emphasis in the remainder of the paper on the relationship of these baseline parameters to the interpretation of sonar tracings. For example, the surface winds, thermocline, thermal seiche, and walls of the loch all produce recognizable effects in sonar readings, which must be understood if they are to be distinguished from the tracings produced by biological entities. (Conveniently, the early history of the study of the physical properties of Loch Ness is treated by S.A. Thorpe in a paper in part one of the same volume of *The Scottish Naturalist.*)

The biological data presented include information on the invertebrate and fish inhabitants of the Loch, vertical migration patterns of these organisms, and biomass estimates. Clearly, the biological resources of Loch Ness are sufficient to support a small population of large consumers. Among the known fauna of Loch Ness, both fish and plankton produce complex sonar signals because they may occur in massed groups and move both horizontally and vertically through the water column. Many anomalous sonar contacts seem explicable in light of inanimate or known biotic components of the Loch, and some such interpretations have been confirmed through the use of underwater television cameras. Yet some signals, on the basis of strength and movement pattern, remain unexplained.

The paper "The Wilson Nessie Photograph: A Size Determination Based on Physical Principles" is an application of quantitative digital analysis to the problem of the independent evaluation of the magnitude of the "head and neck" in this, the most famous still photo purported to represent Nessie. A slightly different version of this paper has been published previously (Paul LeBlond and Michael Collins, 1987, The Wilson Nessie Photo: A Size Determination Based on Physical Principles, *Cryptozoology,* Vol. 6: 55–64); this proceedings version has been updated and includes an additional figure.

Henry Bauer's contribution, "Public Perception of the Loch Ness Monster," is a scholarly analysis of the sociological aspects of the Loch Ness phenomenon. It evaluates a number of questions, including the role of editorial policy in influencing the information that is ultimately presented to

the public, and the tendency among a variety of printed media to present disbelieving or believing attitudes in the reporting of Loch Ness sightings. Complementing Fitter's remarks on the role of the media in influencing the scientific process, Bauer's paper focuses on the media's role in controlling the quantity and quality of the information disseminated to the public through such factors as errors of transmission, editorial policy, and tone of presentation. The bias (believing or disbelieving) and the amount of material appearing in the press is also shown to be reflective of broader sociological phenomena that influence public receptivity to anomalous phenomena. Quite rightly, the author suggests that the globally significant feature of Nessie lies, perhaps, not in the realm of zoology per se, but in the challenge presented to the natural and social sciences by the conventionally inexplicable.

For a collection of papers relating to a cryptozoological mainstay, there is remarkably little speculation in *The Search for Nessie in the 1980's.* The contribution by Le Blond and Collins, probably to the consternation of many, is true to its word and makes no interpretative statements about the possible identity of the object in the Wilson photo, or even the genuineness of the photo itself. Shine and Martin's paper provides hard data about the physical and biotic organization of the Loch, but only hints about a few anomalous recordings perhaps attributable to Nessie. Bauer deals with the trends and implications of the media's treatment of the Loch Ness Monster, which, no matter what one thinks of Nessie, is indeed a real phenomenon in its own right. Only one contribution, by Roy P. Mackal, can truly be considered speculative, and that, by general cryptozoological standards, is still highly systematic and conservative in its approach.

Mackal's paper, "The Biology of the Loch Ness Monster," comes closest to the traditional cryptozoological approach. Seven categories of candidate Nessies are evaluated with respect to the morphological, environmental, and behavioral traits attributed to the monster, and the field of possibilities is accordingly restricted until four final candidates are presented in rank order of probability. Those familiar with Mackal's book *The Monsters of Loch Ness* will know that the author previously proposed a giant amphibian as the most likely Nessie candidate. Mackal's current prime suspect is a hypothetical, highly-derived, attenuate archeocete. Much of the evidence presented in the paper is derived from his book, although it is presented here in much abbreviated form, and with the addition of some new information. Perhaps because this contribution is most closely related to my own field, and perhaps because it is the most—or even only—controversial paper in the symposium, I feel that I should comment on a number of points.

Although the author is circumspect in those eyewitness reports he accepts as reliable (an aspect not treated in the symposium volume, but dealt with in his book), he plays fast and loose with morphology, accepting or proposing anatomical combinations which are highly unlikely or virtually impossible. For example, the probability of a long-necked archeocete is near zero because

an elongate, mobile neck is essentially incompatible with the hydrodynamic constraints on a vertebrate body using dorso-ventral flexion as its primary locomotory mode. In addition, I found a distressing number of minor factual errors, including some relevant to material that was presented without error in Mackal's earlier book. For example, *Eogyrinus,* described as a fossil salamander in this paper, is, in fact, an embolomerous anthracosaur, only very distantly related to any of the living amphibians. Along similar lines, the author states that, "by definition, amphibians give birth or lay eggs in the water" (p. 66). This is incorrect. Many amphibians with direct developing eggs breed terrestrially and lay eggs in terrestrial, not aquatic, sites. Elsewhere, cartilaginous fishes are equated with the name Selachii and bony fishes with Pisces. In fact, the former term is more restricted, and applies only to the paraphyletic grouping of elasmobranchs exclusive of skates and rays (e.g. sharks in general), and the latter term includes all fishes (Osteichthyes are the bony fishes).

Whereas these points do not really impact on the arguments made, several others may be significant. The Komodo dragon is cited as an analagous case to lake monsters with respect to low population size. In the context of the statement, the implication is that the population of this giant lizard is at or approaching the minimum of 20 individuals below which stochastic events are postulated to yield high risk of extinction. In fact, recent estimates of *Varanus komodoensis* populations place the total at 7,213 (Walter Auffenberg, 1981, *The Behavioral Ecology of the Komodo Monitor,* University of Florida Press, Gainesville).

Recent research also suggests that some of the evidence against plesiosaurs as candidate Nessies may need to be reevaluated. It now seems probable that these animals swam in a manner very similar to that of sea lions, a highly modified type of locomotion involving asymmetrical subaqueous flying and some aspects of subaqueous rowing (Stephen J. Godfrey, 1984, Plesiosaur Subaqueous Locomotion: A Reappraisal, *Neues Jahrbuch für Geologie und Paläontologie. Mh.,* Vol. 11: 661–672). More recently, evidence has been presented that plesiosaurs, contrary to previous opinion, did indeed possess a sternum (Elizabeth Nicholls and Anthony Russell, 1991, The Plesiosaur Pectoral Girdle: The Case for a Sternum, *Neues Jahrbuch für Geologie und Paläontologie Abhandlungen,* Vol. 182: 161–185), which may bear on the question of possible terrestrial forays. Despite any shortcomings, however, Mackal's paper is certainly interesting and provocative—which perhaps is appropriate for a topic as inherently controversial as cryptozoology.

The physical layout of *The Scottish Naturalist* leaves something to be desired, as it is printed on poor quality paper and the reproduction of photos and figures is inadequate. The binding is secured by heat tape, and is unlikely to withstand the test of time. I noted an average of one typographical error per paper, but these were minor. The quality and quantity of data presented varies greatly between the contributions. Shine and Martin's paper is un-

questionably data-rich, but for most readers probably too data-rich. The somewhat confusing organization of the paper masks some of the more important points, and the many tables and figures (44 pages worth), often lacking sufficient explanatory notes, are so much of an overkill as to detract from the paper as a whole. Perhaps a tighter editing job on the symposium contributions as a whole would have improved problems such as these.

Putting these problems aside, however, the symposium proceedings should be on the shelves of all those with special interests in lake monsters and/or the cryptozoology (or even general natural history) of Britain. For those whose primary interests lie elsewhere, this collection of papers is less essential, but still, at the ISC member's price, a welcome addition to the literature of Loch Ness.

As far as Nessie is concerned, I happen to be a "disbeliever," but, in the spirit of ISC, I consider it important that cryptozoological phenomena be examined in the same way that we examine traditional zoological subjects. The symposium papers will do little to convince the non-believer, shake the faith of the believer, or sway the undecided. In fact, they do little to advance our understanding of the "anomaly" at all. Nonetheless, this objective and thorough treatment of the topic from a variety of perspectives should do much to enhance the respectability of cryptozoology and demonstrate that cryptozoological research, even if unsuccessful in its attempts to solve certain problems, may generate interesting and significant data in the mainstream fields of endeavor (e.g. biology, physics, and sociology) that converge in the cryptozoological realm.

Perhaps Bauer's finding that scientific journals, while the most skeptical in their treatment of Nessie, are also the most neutral, bodes well for those bound and determined to unravel the mystery of Loch Ness. Even disbelievers like myself seem willing to entertain at least the possibility of Nessie's existence.

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Big Footnotes: A Comprehensive Bibliography Concerning Bigfoot, the Abominable Snowmen and Related Beings. By Danny Perez (comp.). Published by the author (10926 Milano Ave.), Norwalk, California, 1988. 189 pp. $14 (p.).

As the title suggests, *Big Footnotes* is a comprehensive (though not exhaustive) bibliography of thousands of worldwide references to anthropoid

cryptids in popular, scientific, and folkloric literature, spanning several centuries from the 16th century to the early 1980's. A striking cover illustration—a head-and-shoulders portrait of a Yeti by artist Donald White—introduces this compilation, and a five-page preface contains some of author Perez's observations on the Sasquatch phenomenon. This includes a sketch outline of the history and present status of Sasquatch research.

The book is divided into 13 sections according to source of reference: books, academic journals, magazines, newspapers (U.S. and international), encyclopedias, even comic books, as well as audio-visual sources such as videos, films, records and tapes, and radio and television programs. There is also a miscellaneous category in this extensive—but unannotated—bibliography for anything not included in the other categories.

However, it is the print media references that will most likely interest cryptozoologists. Books and magazine and journal articles are listed according to authors' names in alphabetical order. Newspaper articles are found in three separate sections: international (listed according to country or Canadian province), U.S. national, and U.S. regional newspapers (listed according to state). News stories are alphabetized by authors' names where known; otherwise by title. I find this arrangement quite convenient, inasmuch as research into Sasquatch and related phenomena, at least in North America, often consists of trying to locate articles in local and sometimes obscure newspapers. European research, especially prior to the establishment of the International Society of Cryptozoology (ISC), has relied significantly, though of course not entirely, on a more academic approach. This is acknowledged in the bibliography by the inclusion of European academic journals in the section on journals, dating from 1822 to 1981. In addition, the book's large format (8″ × 11″) and clear type make it generally user-friendly and easy to read.

For all this, *Big Footnotes* has two obvious and regrettable flaws. First, although the book was published in 1988, one wonders why it could not have contained more recent referential material. For instance, the last date listed for book references, according to the table of contents, is 1983; for encyclopedias and reference works, 1978; for international and U.S. newspapers and journals, 1981. This also means, of course, that ISC publications are not listed in the book, ISC having been founded in 1982. Surely it would have been possible to include more recent references, assuming, of course, that these media were not entirely barren of writings on unknown hominoids during that time.

My second criticism concerns the number of typographical errors in the book. Admittedly, any book will contain a few such errors. However, their numbers here, on certain pages, are infestations. This is particularly true of references in languages other than English. Pages 76 to 83, for instance, contain no fewer than 70 typos. Pages 25 to 31, at least 40. This presents

an unnecessary impediment to the researcher trying to decypher names, titles of articles, etc. A bit of work by the compiler in proofreading the galley proofs, if he received such from the printer, or in hiring a professional proofreader to do so, would have resulted in a more accurate reference work. The author-publisher has promised, however, that a future expanded edition will not only add more recent material, but also correct these errors.

Still and all, most of the book is relatively free of such irritating encumbrances, especially the sections dealing with U.S. and Canadian references, of which there are almost 1,800 in the newspaper section alone. It is in these sections, and in those listing Eurasian sources, that I predict *Big Footnotes* will find its greatest use. It is, in any event, a quick and ready reference for source material on the hominoid question for the cryptozoological researcher.

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Tom Slick, and the Search for the Yeti. By Loren Coleman. Faber & Faber, Boston and London, 176 pp. $11.95 (p.).

This book is a worthy addition to the sensible, serious cryptozoological literature. It is also a labor of love: the author has been interested in the subject since 1960, had wanted for 20 years to write this book, and drives an automobile whose license plates proclaim "YETIS."

We learn that Tom Slick—a wealthy man of many parts—was interested, so far as cryptozoology goes, in the lake monsters of Loch Ness and Alaska, in giant salamanders reported from California, and in "man-apes" in Sumatra; but his major searches were for the Himalayan Yeti (1956–59), and the North American Bigfoot (1959–62). (We are also told, as plain reportage, that in 1939 Slick had bought the "hoat," which was front part Poland China hog and rear part goat, and had tried to breed it with both hogs and goats. I cannot fathom whether Coleman wants his readers to take this straightfacedly.)

There is especially much of interest in this book for people whose main interest is the Yeti. On that topic, perhaps of greatest interest—for the more general reader too—is chapter 8, "Hillary's Assassination of the Yeti," a discussion of Sir Edmund Hillary's role in the debunking of the Yeti. This was done largely by prejudicial utilization of doubtful Yeti relics—stating that certain items were being claimed to be Yeti remains when in actual fact they were quite well known to be *ersatz* remains made as talismans and without any intention to mislead.

Also of wide interest is Appendix C, which lists people who had agreed

(as of September, 1959) to act as confidential consultants to Slick: in addition to such authorities as Bernard Heuvelmans, whose cryptozoological interests are well known, there were a score of people who held secure positions in mainstream scientific organizations. Of still wider interest is Appendix B, a brief description of nine large animals discovered by Western science since 1900, representing *prima facie* triumphs or justifications for cryptozoology: the okapi, the mountain nyala, the pygmy hippo, the Komodo dragon, the Andean wolf, the Congo peacock, the kouprey, the coelacanth, and the Chacoan peccary.

Of widest interest altogether is the all-too-brief Appendix A (15 pages long), in which Mark Chorvinsky, editor of *Strange* magazine, discusses "Yeti and the Cinema"—the role of film on public perception of the Yeti, and, reciprocally, the influence on films of well-publicized claims that Yetis exist. This appendix cries out to be expanded into a book on the interaction of film (or media in general) and cryptozoological pursuits. I am aware of no more than the occasional tantalizing aside comment about this in the literature. For instance, Binns (*The Loch Ness Mystery Solved,* Prometheus, Buffalo, 1983) has suggested that the chronological coincidence of the film *King Kong* and the first Loch Ness monster "flap" was not coincidental.

Aficionados of the Loch Ness enigma will be interested that World Book Encyclopedia (Field Enterprises) supported not only the search at Loch Ness for several years—through the mediating efforts of Roy Mackal—but also earlier, in 1960, a quest for the Yeti. At Loch Ness, the financial support was helpful, but the sponsor's other interests sometimes conflicted with the quest itself. In the case of the Yeti, the World Book expedition was the occasion for Hillary's debunking. Whenever cryptozoologists, or more generally other anomalists, deplore the lack of funding available to support investigations of anomalous claims, they should recall that funding often brings undesired side-effects, as he who pays the piper calls the tune (see my comments in *Zetetic Scholar,* #10 [December, 1982, pp. 109–111], and *The Enigma of Loch Ness: Making Sense of a Mystery,* University of Illinois Press, Urbana and Chicago, 1986 [especially pp. 85–89]).

Since Coleman makes so explicit his admiration of Tom Slick, it may be churlish to mention that there are a number of hints in the book that not everyone found Slick as admirable as does the author. It appears that—like almost all enthusiasts who get things done—Slick broke a few eggs in order that omelettes should result. I do take direct issue with Coleman, however, in a couple of excessive claims for Slick: that his death, "combined with the after-effects of Hillary's . . . debunking . . . changed attitudes toward cryptozoological field studies forever" (in an unfortunate direction); and that, if he were alive today, "establishment zoologists would view the new science of cryptozoology more supportively."

One should not forget the work at Loch Ness of the Academy of Applied

Science, a group of highly accomplished people—largely technologists—who received from a number of zoologists considerable moral support in their endeavors, and even for their results. There are, too, a number of individual scientists who work quite conventionally but who make few bones about their genuine interest in certain cryptids—not to speak of Constance Whyte, whose book stimulated many level-headed people to take Nessie seriously, or, after all, Bernard Heuvelmans, whose seminal works intrigued many other people in addition to Tom Slick. Surely it takes nothing from Slick to see him as a notable member of a small group instead of as a contemporary *sine qua non.*

But quibbles of this sort should not detract from the fact that Coleman's book is an unusually and admirably scholarly addition to the cryptozoological literature.

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Comments and Responses

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

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ONE MORE ATTEMPT AT DEFINITION

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

In translating into Russian some high points of Heuvelmans' article, I encountered his 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." The term "hidden animals" proved to be a stumbling block for translation because, in Russian, animals can either be "hiding"—i.e., furtive or elusive—or "hidden"—i.e., hidden by something or someone. "Hidden animals" without qualification sounds very ambiguous in Russian, and, I suspect, in English too.

The next difficulty was of a factual (not linguistic) nature, and concerns the phrase: ". . . still unknown animal forms." The supposedly-extinct Tasmanian thylacine cannot be called a "still unknown animal form," and yet it falls within the scope of cryptozoology. Lastly, do cryptozoologists really—i.e., directly—"study" any animals? Surely that is the business of zoologists once such animals have been discovered.

After some pondering, I produced my own definition of cryptozoology: "The collecting and scientific study of evidence for the existence of animals whose presence in general or in a particular time or place is not recognized by zoology."

This definition encompasses all the cases of concern to cryptozoology, and

also lends itself to adequate translation. It is the positive charge of evidence and the negative charge of non-recognition that spark the electric current of cryptozoology. As for Heuvelmans' opinion that a definition should conform to the etymology of a word, I do not think that is binding at all. Geometry is not "land measuring," and cybernetics is not the art of "steering ships."

The "cryptic" nature of cryptozoology's objects is implied in my definition by the term "evidence" and the phrase "not recognized by zoology," the latter characteristic being their only difference from a host of other elusive and rare animal species. Thus, a "cryptid" is an elusive and probably rare animal species unrecognized by zoology, which turns into an "ex-cryptid" whenever it is discovered and recognized—the okapi, for example.

The presence of *evidence* for cryptids—in fact, cryptozoology itself—invalidates such subjective and ambiguous terms as "animals of unexpected [for whom?] form or size, or unexpected occurrence in time or space."

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OTHER DEFINITIONS, OTHER HERESIES

(Response to Bayanov)

It is sad, disappointing, irritating, and, frankly, even distressing to realize that, 10 years after the founding of the International Society of Cryptozoology, some of its most senior members have still not grasped the very meaning of the term "cryptozoology," or do not seem to have a clear idea of what this new discipline of zoology concerns.

By finding himself faced with difficulties and uncertainties, my old friend Bayanov is now paying for the mistakes of others—those who have repeatedly insisted on presenting their own, sometimes fanciful definitions of cryptozoology. They have done this because of mere contrariness or a compulsive disputing spirit, perhaps with the foolish notion of somehow being immortalized by their feats.

First of all, it must be emphasized, once again, that cryptozoology aims at the discovery of *new* animal forms; otherwise, its creation would not have made any sense. Animal forms of uncertain status, because they are either on the verge of extinction or have recently become extinct, are quite irrelevant to cryptozoology, not only because they are far too numerous—as I have stressed previously (Bernard Heuvelmans, 1986, Annotated Checklist of Apparently Unknown Animals with Which Cryptozoology is Concerned, *Cryptozoology,* Vol. 5: 3)—but simply because they have already been duly described as *living* species or subspecies.

The fact that articles on the Tasmanian thylacine or the Eastern U.S. cougar have been accepted for publication in the journal *Cryptozoology*—because the search for these animals conforms to one of the cryptozoological methods of investigation—does not mean that such animals fall, as Bayanov seems to think, "within the scope of cryptozoology." They simply do not.

This does not apply, of course, to the Quaternary thylacine on mainland Australia, which belongs by definition to another geographical race or subspecies (*Thylacinus cynocephalus rostralis* Devis, 1894). Neither does it apply to the neanderthaloid surviving in Asia to the present time, which, because of its higher specialization, should also be classified at least as a distinct subspecies. That is why I described it originally as *Homo pongoides* sp. seu subsp. nov. (Bernard Heuvelmans, 1969, Note Préliminaire sur un Spécimen Conservé dans la Glace, d'une Forme Encore Inconnue d'Hominidé Vivant Homo Pongoides [sp. seu subsp. nov.], *Bulletin de l'Institut Royal des Sciences Naturelles de Belgique,* Vol. 45[4]: 1–24), but it will probably have to be named *Homo neanderthalensis pongoides* whenever we have the opportunity of making comparative studies; that is, comparing the skeleton of a specimen of the living form with the corresponding bones of a fossil Neanderthal.

Incidentally, I have never accepted Neanderthal as just a subspecies of *Homo sapiens*; that is, *Homo sapiens neanderthalensis.* This classification, in my opinion, was a mistake, as now confirmed by recent findings (Chris Singer, 1988, The Dates of Eden, *Nature,* Vol. 331: 565–66; Eric Delson, 1988, One Source Not Many, *Nature,* Vol. 332: 206; John J. Shea, 1990, A New Perspective on Neandertals from the Levantine Mousterian, *Anthroquest,* No. 41, Spring.)

Concerning the linguistic problem raised by Bayanov, it is truly providential that, in Russian, "hidden animals" sounds very ambiguous, and has, in fact, a double meaning. This is precisely how I originally intended it to be. It should be remembered that my now classic introductory work on the subject, *On the Track of Unknown Animals* (1958, Rupert Hart Davis, London; Hill and Wang, New York), had an original French title of *Sur la Piste des Bêtes Ignorées* (1955, Plon, Paris), which means, literally, "on the track of ignored beasts." This was translated differently by the publishers for editorial reasons, and, in the process, the true meaning became slightly distorted. I actually wanted to elaborate on the problem of all animals which are not only passively "unknown" in some way—they are never *completely* unknown, otherwise we would not be able to discuss them!—but also actively

"ignored" by conservative-minded zoologists. These animals thus happen to be both "hiding" *and* "hidden."

To meet Bayanov's last, very subtle criticism, I would state that cryptozoologists do indeed "study" hidden animals, just as paleontologists "study" extinct animals. That both specialists do not study animals "really" or "directly," as many kinds of zoologists do, is really not very significant. Cryptozoologists study the identikit pictures they attempt to construct of hidden animals, and paleontologists study the attempted reconstructions of the remains or traces left by animals of the past. Who would think of blaming astronomers or nuclear physicists for not *directly* studying celestial objects or elementary particles?

Admittedly, a definition need not conform necessarily to the exact etymology of a word. But it is always preferable when it really does so, which I carefully endeavored to achieve when I coined the term "cryptozoology." All the same, being a very tolerant person, even in the strict realm of science, I have never prevented anybody from creating new disciplines of zoology quite distinct from cryptozoology. How could I, in any case?

So, let people who are interested in founding a science of "unexpected animals" feel free to do so, and, if they have a smattering of Greek and are not repelled by jaw-breakers, they may call it "aprosbletozoology," or "apronoetozoology," or even "anelistozoology." Let those who would rather be searching for "bizarre animals" create a "paradoxozoology," and those who prefer to go a-hunting for "monstrous animals," or just "monsters," build up a "teratozoology," or, more simply, a "pelorology."

But, for heaven's sake, let cryptozoology be what it is, and what I meant it to be when I gave it its name over 30 years ago. That is, as I defined it in my 1988 article discussed by Bayanov—and I shall italicize here to make it more conspicuous—*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.*"

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HEUVELMANS, ELIADE, AND THE "NATIVE-KNOWN"

(Comment on Bernard Heuvelmans, 1990, The Metamorphosis of Unknown Animals into Fabulous Beasts and of Fabulous Beasts into Known Animals, *Cryptozoology,* Vol. 9: 1–12)

A cartoon in the March, 1991, issue of the British archaeological journal *Antiquity* (Vol. 65[246]: 134) portrayed a "cave man" (*Homo erectus*) discouraging the invention of the bow-and-arrow because the "new," as such, was not wanted. There may be some truth to such portrayals according to certain modern views of "natives" and the nature of their beliefs (although *Homo erectus* may have been somewhat early for the bow-and-arrow). And apparently our early human progenitors existed through lengthy eras of little or no evident change.

Heuvelmans notes the rigid and limited nature of human mental processes. Although imagination colors and adds to the gaps in our knowledge, it is ". . . strictly limited in the choice of both plots and motives" (p. 4). If these limitations lead to a certain repetitiveness and circumscribed number of mental models, will such a condition invalidate any native knowledge and make doubtful the "native-known"?

Today, many scholars would agree with the repetitive and changeless tendencies of many cultures. Freud found repetitive and even contradictory elements in the individual "unconscious mind." Marshack, who performed microscopic analysis of paleolithic art, discussed the likelihood that animal images in caves might have had a "continuous" life, to be used repetitively over time long ago (Alexander Marshack, 1991 *The Roots of Civilization: The Cognitive Beginnings of Man's First Art Symbol and Notation.* Rev. ed. Mt. Kisco: Moyer Bell: 233–34). Some smaller, portable figures seem to have been re-engraved over and over for repetitive use (e.g., Marshack, 1991, above: 255). The European scholar Burkert wrote that the psychological preconditions of fundamental layers of human life ". . . have changed only slightly from the earliest times until now" (Walter Burkert, 1983, *Homo Necans: The Anthropology of Ancient Greek Ritual and Myth.* Tr. Peter Bing. Berkeley: University of California Press, Introduction: xxiii).

Certainly, the way we view native accounts should be affected by the nature of those accounts. Consider in this connection the analyses of the well-known anthropological theorist Mircea Eliade.

Eliade considers the nature of the mental spheres of modern (recent) natives and that of ancient tribal peoples to be similar, if not the same, under a structure he terms "premodern" (Mircea Eliade, 1974, *The Myth of the Eternal Return, or Cosmos and History,* Princeton: Princeton University Press—also previously published as *Cosmos and History*). The archaic and the recent modern are continually cited in supporting references (e.g. Eliade, 1974, above: 44, 63, and especially note 2 of p. 97). Eliade's

anthropological work is approved by some, and may contain important considerations for the understanding of a native "mind-set." ("Natives" in this case include European villagers without higher education.)

According to Eliade, history does not exist for natives. To become "real" for them, people and events must be subsumed by previous ancient traditional events or characters (deities, heroes, or ancestors). The name of a man famous in his own time would be preserved by being linked to the exploits of "heroes" of the past already known, and he would be credited with adventures already part of the received tradition, as his individuality gradually sank back into that tradition:

> . . . popular memory finds difficulty in retaining individual events and real figures. The structures by means of which it functions are different: categories instead of events . . . (Eliade, 1974, above: 43).

Even the dead in their continued existence tend to lose their memory and become simply "ancestors": ghosts without memory (Eliade 1974, above: 46–47).

The result would be that events only become "real" or significant to the extent that they enter and mirror tradition, because history as such is rejected (Eliade, 1974, above: 46, 48, 85). An individual only becomes "real" or significant to himself insofar as he loses himself in a name received from the past, while dropping his own individuality:

> . . . the man of a traditional culture sees himself as real only to the extent that he ceases to be himself (for a modern observer) . . . (Eliade, 1974, above: 34).

Plato apparently gave voice to similar attitudes by seeing the "unchanging" as a valuable aspect of his carefully designed republic—artists would be excluded by him on the grounds that the creative process might bring change and something new. If you have something that is "right," why allow change? We Euro-Americans ourselves may concede that some fixed traditions passed down to people in a tribal setting might well represent survival values for that tribe.

Here we may briefly compare and contrast some thoughts of Heuvelmans with some of Eliade. For Heuvelmans, new experiences are brought into the consciousness of peoples through being subsumed into inherited emotionally tinged rigid structures (Heuvelmans, p. 2, pp. 3–4), and later modified by consciousness. The products of that process are myth or the mythic, and, though worked upon by imagination, the results are limited in both plots and motives (Heuvelmans, p. 4). Science may take the mythic animal tales produced in such ways, find and describe little-known creatures, and thus add new entries to a rational list (p. 7). For Heuvelmans, then, the finding and scientific classification of the animal at the core of imaginative mythic

descriptions amounts to a reclaiming or bringing back to reality of a misunderstood real creature.

To Eliade's tribesman, the animal will not be or become "real" until it sinks back into a received tribal or peasant tradition. It won't become "historical" because the tribesman or peasant resists history. There is both structure and rigidity here, as in Heuvelmans' views. For Eliade, natives resist history, and do not see themselves as unique and individual personalities. Heuvelmans, however, sees a common working and structure of mind worldwide:

> This distinction between "primitive" thought and "civilized" thought now strikes us as artificial (Heuvelmans, p. 3).

For Eliade, new native experiences are captured by rigid archetypes. For Heuvelmans, new experiences pass through emotionally charged rigid structures and are complicated by imagination. These things being so, some question must arise as to the basic worth of native and animal descriptions. If native societies simply carry forward traditional aspects, how may traditional tales affect the search for unexpected or hidden animals?

Eliade (with various other scholars) thinks that descriptions of "new" experiences might be lost from tribal or village memory within two or three hundred years (Eliade, 1974, above: 43). This complication actually helps us in finding whether native descriptions are worthwhile, for it does imply a memory of "the new" or exotic as existing for awhile, and the cryptozoological researcher would question natives in the present time. It also implies that the native can notice the "new" and remember it for awhile, which runs counter to Eliade's general descriptions of the nature of native or village mind-set. But a "traditional" animal may be more difficult to identify.

If unsettled, uncultivated, and desert lands represent "chaos" (Eliade, 1974, above: 9–10), how may questions be posed about animals outside a settled area, since they live in "chaos"? We may be tempted to say that his surroundings or distant lands may represent the chaotic to settled villagers, but not to hunters who often wander further from the village; but here we would be stopped by Eliade's consideration of hunting as one of the important activities which follow ancient models (Eliade, 1974, above: 35). The best we can do, if we consider Eliade's views seriously in connection with this question, is to perceive the probability that the native hunter may make observations of his environment in addition to his role as repetitious actor in an ancient procedure; he is not completely a robotic performer.

One obvious answer to the problem of the quality of native animal descriptions may be the existence of the very factor Plato wanted to limit: the artistic sense that is surely sometimes to be found in tribal individuals. The storyteller, singer, or poet may introduce elements of the new even while

dealing with traditional material. And thus new elements may enter the tribal consciousness. If such elements find concretization within song or story, then such preservative cocoons may allow a life for those elements even past the several centuries of tribal memory allowed by some anthropologists. They may survive through time without being "archetypes" of any kind at all (though perhaps attached to such, if Eliade's views are correct), after being enshrined in poetry, song, or story. If that is so, the implication is that "real" animals or other aspects of historical experience may exist in some tribal accounts. That is, the "ahistorical" tradition may have been modified in some instances by the human creative ability in such a way as to include "fossilized" historical elements as additions to received traditions composed of other matters. Eliade's system may not represent an absolute. Indeed, Eliade himself allows for some "historical" elements having entered surviving epic poetry:

> If certain epic poems preserve what is called "historical truth," this truth almost never has to do with definite persons and events, but with institutions, customs, landscapes. (Eliade, 1974, above: 43).

We should mention here that Eliade's "archetypes" are ancient models which have become part of received tradition that never varies, and are very definitely not the same as Jung's archetypes, and are not produced by the "collective unconscious" (Eliade, 1974, above, Preface: xiv). Eliade himself stresses the difference. Eliade's archetypes are in fact "celestial models" in a received social tradition (Eliade, 1974, above: 5, 10, 21, 32, 76).

Another road of penetration for non-traditional elements may lie in the fact that scholarly theories, no matter how aesthetic or logical, will often not be either perfect or complete. There was a recent scholar who contended that history had ended because his views of historical processes had seemed to fail. Clearly, history had not "ended," and, clearly, we do not yet have a complete or perfect understanding of the preservation of tribal knowledge.

We have found several possible theoretical "exceptions" to the native's fixed and unchanging world as described by Eliade which may redeem the worth of native animal descriptions. For one thing, animals may be seen by natives as part of the "landscape," which we have seen may be preserved by native cultures in poetic epics. Secondly, if natives may preserve memories of distinct things or events for up to two hundred years, and if they are questioned before such a time passes, then their animal descriptions may contain some clarity, however buried in imaginative overlay. (The "traditional" mythic animal may represent a much more difficult and complex puzzle.) Thirdly, Plato's outlawed artistry, as occurring in the individual native, may admit the entrance of the novel or exotic, though it may be complicated by constructs of the imagination. These three theoretical avenues may admit a description of some elements despite the existence of ancient and rigid models, paradigms, or archetypes.

Finally, one may well contend that Eliade's archetypes—or Jung's archetypes, for that matter—do not encompass all aspects of human mental processes, whether on a tribal or other level. Hence, we may conclude that natives are neither "locked away" from environmental observation, nor precluded by even strict theory from reporting their observations to outsiders in a valuable, though perhaps difficult or complex context. Therefore, the use of native informants should be a viable source of cryptozoological information if used with care, and always with the awareness of the problem that such informants may wish to please the questioner.

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MILL CREEK TRACKS DEFENDED

(Comment on James A. Hewkin, 1990, Sasquatch Investigations in the Pacific Northwest, 1990, *Cryptozoology,* Vol. 9: 82–84)

Along with two others, I was the one who found the January, 1991, Mill Creek Sasquatch tracks. Because Hewkin states that these tracks were fakes, I feel I should provide my own interpretation. With my brother Swede, I spent four days intensively studying the several miles of tracks. We backtracked them from the stream up into the mountains for several miles until we ran into snow country. Hewkin was here for only a few hours—four days after we found the tracks—and he only studied a few of the thousands of tracks we looked at.

Before continuing, by way of background, I would like to briefly outline some of my previous experience with Sasquatch tracks and encounters in the Walla Walla area of the Blue Mountains. My brother and I have been trackers, packers, and guides in these mountains for most of our lives. Through the 1940's, 1950's and 1960's, Swede and I spent a lot of time in the mountains, sometimes tracking down lost people. Swede raised a family of four children mostly by trapping and hunting. We are of Indian-white descent, partially raised on a reservation. We were learning to track and hunt by the time we were 3 years old, and fishing by the age of 4. During that period, we were shown and told about tracks of the Wild Man of the Mountains. He is now called Bigfoot of Sasquatch. We were also told not to pursue such tracks until we were much older. That was more than 65 years ago.

By the time I was 14 years old, I was pulling a pack train of six mules, supplying and moving four sheep camps, and hunting-down strays as part of the job—until I got married. I have been a professional big-game packer and guide for over half of my life. I have seen Sasquatch tracks off and on all these years. In 1962, we saw a Bigfoot shot three times with a high-caliber rifle. It never even broke stride as it ran past us. We have since learned to live with them, and they with us.

Now, returning to Hewkin's report, I have the weather information for a week before and two weeks after the Mill Creek tracks were made, on January 11, 1991. Hewkin failed to mention that four days prior to the tracks being made the temperature had dropped to −5°F. (−20°C.). It then began warming gradually. The tracks were made on a Friday night. The low temperature Friday night was 30°F. (−1°C.), 24°F. (−4.4°C.) on Saturday, and 25°F. (−3.8°C.) on Saturday night.

The tracks extended for about one mile down through a fall-tilled field which had a pitch of 10–12°. They had sunk 3–4 inches (7.5–10 cm) into the ground. A 350-pound (158-kg) man standing near them sank only ¾ of an inch (1.9 cm) into the ground. The Sasquatch tracks were 15 inches (38 cm) long and 7½ inches (19 cm) wide, packing no mud. The stride measured about 30 inches (76 cm), with the toes pointed slightly out for balance. In the snow, the tracks were from 5 to 16 inches (12 to 40 cm) deep, while the tracks of the man weighing 350 pounds (158 kg) sank only 1 inch (2.5 cm) or less. The stride continued in the snow at 30 inches (76 cm), and in line on flat ground, slightly pointed out, up or down hill.

Hewkin does not state that, after coming out of the field, the trackmaker took only six steps to cross a 30-foot (9-m) road. He does state that, at the wire fence, the trackmaker stood by the cable on one foot while stepping over—supposedly not like a Sasquatch would. How would Hewkin know this, never having seen a Sasquatch in action? He does not state that the cable was hanging on the edge of the bank and at a height of only about 24 inches (60 cm). From here, a rough roadway for steel tracked-tractors goes at an angle of 25–30° down from the highway to the creek. The trackmaker went down this roadway, leaving impressions in the tractor tracks!

Hewkin also states the trackmaker should have crossed Mill Creek to get into the timber on the other side. However, he does not mention that the river was running about 6 feet (1.8 m) deep, a raging torrent that would have rolled over a truck. He also fails to mention that the trackmaker's prints went through a half-acre (.2 ha) of wild blackberry bushes, and then went into timber—where they petered out. Another fact he fails to mention is that the livestock of local residents would not leave their barns for pasture, and had to be fed in the barn for several days. Hewkin did not investigate enough. He obviously had his own ideas, maybe preconceived, before he arrived here for his short inspection. He came here 4 days after we found

the tracks, and by then they were beginning to fall in; there had been two nights of freezing rain, and the snow was starting to thaw.

Besides my brother and I, several others who have been involved in tracking Sasquatch—or tracking in general—spent many days over many miles inspecting these tracks. These people include David Been, a resident on Mill Creek and an experienced tracker; Vance Orchard, an outdoors newspaper reporter in the Blue Mountains for over 40 years; Grey May, a professional tracker and outdoors survival instructor at Washington State University; Dick Bradford, a lifetime tracker and trapper in this area; and Paul Freeman, who has devoted most of his time to the Sasquatch search since he saw one near here in 1982.

For some background: In 1974–75, a buckskin-colored female Sasquatch showed up in this area. I have hair samples from her. She had a crippled right foot, and came here from the Yakima, Washington, area where she had been observed in the fall of 1973. From her actions, we figured she was young, very gentle, and curious. We were within 40 feet (12 m) of her, gazing eyeball-to-eyeball. Her track and color showed up there again in the early 1980's. Then she left, but by 1988 she was back again, and we tracked her and picked up more hair samples.

In 1977–78, a huge Bigfoot left tracks in this area. We called him the Blue Black, because of his coloring. From his size, we judged him to be a male. In the late 1980's, we came across tracks of at least four different adult Sasquatch, and what appeared to be a juvenile. By 1990, these were no longer observed in this area of the Blue Mountains. But in 1991 the Mill Creek Sasquatch turned up—which Hewkin considers to be a hoax—a much smaller and older individual, according to track and stride.

I believe these new tracks to be the same as the tracks from the 1967 Bluff Creek, California, event, when Roger Patterson filmed a Sasquatch. I have a cast copy of one of those tracks. I have mentioned this to Grover Krantz, a physical anthropologist, and he is evaluating this possibility. Swede, Dutch Jennings—an outdoorsman from Bend, Oregon—and I struck this same track on September 19, 1991, at a point an hour's horseback ride from trail head in the Wenaha-Tucannon Wilderness Area.

Now retired, I hope to renew some old friendships in the Blue Mountains, and possibly see more of the Sasquatch I once knew from close contact. I believe they are still there to be seen, and I feel certain that the tracks we found in January of 1991 were definitely not fakes.

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Mill Creek: Investigation Discrepencies

(Response to Sumerlin)

I have all due respect for Sumerlin and his considerable experience as a horse packer, hunter, and observer of wildlife in the Blue Mountains. However, there are several discrepencies in our respective investigations of the tracks on Mill Creek.

First, Sumerlin measured the tracks as 15 × 7.5 inches (38 × 19 cm). My measurements averaged about 13 × 6 inches (33 × 15 cm) when he led me to the site four days later. No doubt snow melt affected the track size. The snow was gone when I arrived, and the tracks I measured were embedded in mud which had dried out to some extent, so my measurements should be more accurate. Actually, the tracks at the upper end of the area were so completely melted out that only blobs appeared, which led me to suspect that the entire line of track was made over a 2- or 3-day period of time.

Sumerlin records a stride of about 30 inches (76 cm), which is consistant with my observations. The one serious discrepency is where the tracks crossed the highway and led beyond the fence to the creek and blackberry bushes. Where the trackmaker crossed the fence, I took the opportunity to cross also, and noted that my foot placed exactly to the track which was under the fence. Contrary to Sumerlin's statement, the top wire was tight, and I had to press down vigorously to swing my leg over the wire without snaging on it. Sumerlin's comment that a cable was hanging over the bank at a height of 24 inches (61 cm) is something I know nothing about. Perhaps he failed to look where the tracks crossed. Another discrepency is that I found no sign of tracks going into the half-acre (.2-ha) patch of blackberry bushes, only a short trail that had been made by human blackberry pickers during the summer.

Some searching revealed a few faint tracks leading back up to the road from the creek. There was no evidence of tracks up the hill again. As far as I could determine, the tracks ended at the road. I have no idea why Sumerlin ignored this, particularly when I pointed out these facts to him on site. The facts are that the tracks returned to the road—not into blackberry bushes—and then into timber.

Sumerlin stated that the creek was a raging torrent, which was true. However, in my opinion, the flow was probably not as high at the time the tracks were made, and would not have slowed a Sasquatch for even a second in crossing the creek.

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Beyond Scientific Journals

(Comment on Angelo P. Capparella, Review of *Thunderbirds! The Living Legend of Giant Birds, Cryptozoology,* Vol. 9: 94–96)

Capparella has done his best to describe a book containing serious omissions and flaws. Fortunately, he has failed to describe *Thunderbirds*!—the book that I wrote. First of all, my book is about giant birds that have been reported around the world. This happens even in North America, where at least two species of the largest birds of prey seem to have survived. They appear to be a teratorn and a giant owl.

Capparella states that my organization is haphazard; I maintain it is not. Out-takes from the Huffer film were not used for copyright reasons. The out-takes lack detail, in any case, showing only dark shapes, as noted in my text (p. 63). He would have readers believe that I do not discuss the food that the birds eat. This assertion is easily refuted by reading the book, wherein the prey of the birds is frequently cited. Also, their diet is specifically discussed on p. 53. Capparella fails to mention that Chapter 10 deals with giant birds reported around the world. More recently, in October, 1991, birds characterized as "giant eagles" were reported (in Steve Newman's syndicated environmental column Earthweek) to be carrying off reindeer in Norway. Lapp herders were complaining to their government that 1,300 reindeer had been taken in one year.

Quoting from Campbell and Tonni about *Teratornis merriami,* Capparella implies that they are discussing *Argentavis magnificens* when they are not. In addition, he fails to note that I made the same point about *T. merriami* in my book (p. 79). He takes out of context the view of a North American biologist that "all the field work has been done" in North America, and he thinks that amateur birdwatchers would be reporting these giant birds. I think birdwatchers are subject to the same sanctions as other people. As described in my book, people are discouraged from reporting giant birds. The focus of amateur birdwatchers was discussed in my book (pp. 60–61), but Capparella ignored that text.

Capperella dismisses without discussion the first three chapters of the book, where I chronicled the failure of ornithologists to explain reports in Illinois. It is not enough to write that their presence there "strains credulity"; such dismissals are typical of the worst behavior of professional scientists. They do it because they can get away with it. Capperella's charge that I do not consider misidentification of known birds can be refuted by, again, reading the book.

I have advocated that such topics as Thunderbirds and giant owls be fully aired and subject to lengthy discussion to determine their merits. To find such discussions, however, readers will have to look beyond scientific journals to Fortean publications, beyond commercial publishing houses to self-published books, and beyond even the pages of *Cryptozoology.*

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COOPERATION: A KEY TO PROGRESS IN CRYPTOZOOLOGY

(Response to Hall)

It is surprising that Hall regards my review of his book as overwhelmingly negative, specially considering my use of words such as "useful" and "considerable value" in my concluding recommendation. Hall states that his book was intended to stimulate discussion regarding the merits of his topic. That is precisely what I did in my review.

Three specific points raised by Hall need clarification. First, as stated in my review, Campbell and Tonni's 1983 discussion (*Auk,* Vol. 100: 390) covers all teratorns; my quote regarding *Argentavis magnificens* was accurate. Second, Hall's assertion that the phrase "all the field work has been done" was taken out of context puzzles me. My point was—as I thought was Hall's—that zoologists who say this do not understand how much is yet to be discovered about the existence and distribution of species, even in North America. Third, when evaluating sightings of a possible unknown animal, one must take a hard-nosed (not derisive, myopic, or uncritical) approach consistent with current zoological knowledge. For example, my level of credulity would be different for reports of teratorns from, say, the yungas of Bolivia than for reports from central Illinois.

Hall implies that I am of the breed of professional zoologists that arrogantly dismisses reports of unknown animals. However, I was trained at the Louisiana State University's Museum of Natural Science, an institution that leads in describing new species of birds from South America. While there, I was involved in the discovery of three species, the most surprising being a new parrot (Don Stap, 1990, *A Parrot Without A Name: The Search for the Last Unknown Birds on Earth,* Alfred A. Knopf, New York). In central

Illinois, I regularly investigate unusual bird sightings by the public. However, I am acutely aware of the difficulties and trickiness of sight identification, even by professional ornithologists. And I assert that zoological knowledge is sufficient to make sound evaluations regarding the likelihood that sightings represent unknown species.

This is an important consideration when judging where to target scarce resources in cryptozoological research, and such application of zoological principles is at the heart of the method by Heuvelmans to identify unknown animals from sighting reports. Of course, that does not preclude discoveries that necessitate the re-evaluation of those principles.

Finally, I reiterate the agreement in my review with Hall's recommendation that cryptozoologists establish ties with natural science institutions as well as professional zoologists. It is unproductive to have an adversarial relationship, as we have much to learn from each other. In short, cooperation may be one of the keys to progress in cryptozoology.

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LEGUAT'S WRITINGS NOT SPURIOUS

(Comment on Aaron M. Bauer, Review of *Exotic Zoology, Cryptozoology,* Vol. 9: 88–91)

Regarding Bauer's review of Willy Ley's *Exotic Zoology,* I believe certain remarks concerning the Huguenot refugee François Leguat should not remain uncommented upon. Bauer laments Ley's reliance on Leguat's book *Voyage et Avantures en Deux Isles Désertes des Indes Orientales,* London, 1708 (British edition: *A New Voyage to the East Indies,* London, 1708) for information on the extinct avifauna of Rodriguez Island, alleging that Leguat's writings are "spurious."

Bauer may not realize that it is inevitable that anyone concerning themselves with this subject should lean heavily on Leguat. There are only three sources known to me containing primary information on the extinct birds of Rodriguez. The first is the curious and rather beautiful account of Leguat himself. Leguat was the leader of a small group of Huguenot refugees supposedly marooned on the island for two years (1691–93) before escaping in

a crudely-made boat to Mauritius. The second is the anonymous document known as the *Relation de L'Ile Rodrigue* discovered in 1875 in the Ministère de la Marine, Paris, by M. Bouillard, and thought to have been written around 1730. The third source of information is that contained in skeletal material found on the island, mostly during the 19th century.

That this skeletal material—found more than 150 years after Leguat's book was published—by and large bears out Leguat's remarks should be taken as evidence enough that this writing was not entirely "spurious." Leguat wrote of a large bird he called the "solitary," and many bones of such a bird (*Pezophaps solitaria*) have since been found. He mentioned a heron, implying that it was flightless; *Nycticorax megacephalus,* a flightless or almost flightless night heron was described by Milne-Edwards in 1874 on the basis of bones found on the island. That same 19th century naturalist—Milne-Edwards—felt able to correlate bones of a large rail (*Aphanapteryx leguati*) with a bird that Leguat described as a "gelinote" or wood hen.

In addition to confirming Leguat's testimony at a general level, the skeletal remains sometimes confirm it at a very precise level. A small round mass under the feathers of the wing of the solitary was, for instance, described by Leguat as "big as a musket ball." When skeletons were found more than 150 years later, this "musket ball" could be seen as a bony knob on the metacarpal.

Even before skeletal evidence supporting Leguat came to light, H. E. Strickland and A. G. Melville (1848, *The Dodo and its Kindred: Or the History, Affinities of the Dodo and other Extinct Birds of the Island Mauritius, Rodriguez, and Bourbon,* London), most scrupulously careful researchers, went to considerable lengths to match up Leguat's account with local knowledge of Rodriguez as it then existed—that is, 150 years or so after Leguat's time—and concluded that the general accuracy of Leguat was fully established, "though some allowance must be made for that author not having been a naturalist, and from his work having probably been written from memory."

Subsequent writers who have investigated the subject of the extinct avifauna of Rodriguez and shown confidence in Leguat include, among others, Alfred and Edward Newton, Alphonse Milne-Edwards, Walter Rothschild, Storrs L. Olson, James C. Greenway, and Masauji Hachisuka.

The idea that Leguat's voyage is fanciful is an old and rather tired one, first circulated, I believe, by G. Atkinson (1922, *The Extraordinary Voyage in French Literature, 1700–1720,* Paris) and later supported by Erwin Stresemann.

Atkinson's allegations have been comprehensively refuted, however, by J. Vivielle and H. Dehérian (1926, *L'enigme du Voyage de Fr. Leguat à L'Ile Rodrigues.* Ministère de l'Instuction Publique et des Traveaux Historiques et Scientifiques, *Bulletin de la Section de Géographie,* Tome 41, Paris), and T. Mortensen (1934, On François Leguat and his Voyage. *Ardea,* Vol. 23: 67–77).

Masauji Hachisuka (1953, *The Dodo and Kindred Birds,* London) summarizes the evidence for Leguat's voyage and concludes that "few historical events have been better verified."

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STANLEY AND THE OKAPI

(Comment on Stephen F. Kredel, 1990, More on the Okapi-Persepolis Link, *Cryptozoology,* Vol. 9: 115; Robert G. Tuck and Raul Valdez, 1989, Persepolis: Nilgai—Not Okapi, *Cryptozoology,* Vol. 8: 146–49)

Kredel points out that, according to Henry M. Stanley's own account, he did not actually see an okapi in the Ituri forest, but merely collected information from Pygmy natives on what at the time he thought was a wild ass. The exact quote is: "The Wambutti knew a donkey and called it *atti.* They say that they sometimes catch them in pits. What they can find to eat is a wonder. They eat leaves" (Henry M. Stanley, 1890, *In Darkest Africa, or the Quest, Rescue and Retreat of Emin Governor of Equatoria,* Vol. 2, Appendix B, p. 490, Charles Scribner's Sons, New York).

In his Comment, Kredel was correcting a statement by Tuck and Valdez. They had stated that "there is evidence that explorer Sir Henry M. Stanley saw a living okapi in Africa a decade before its discovery by Sir Harry Johnston." Tuck and Valdez based their information on a book by Willy Ley, which they referenced (Willy Ley, 1968, *Dawn of Zoology,* Prentice-Hall, Englewood Cliffs, New Jersey, p. 250). The exact quote by Ley is: "Stanley, a year or two prior to 1890 (the year in which his book *In Darkest Africa* was published) had learned about an unknown animal which the Wambutti pygmies (sic) called *atti.* From a discussion with Stanley which took place after the animal had been scientifically described, Johnston concluded that Stanley had actually seen okapis without realizing that they were new to science."

Was Ley correct in this statement? What documented evidence is there to support it? I have reviewed the literature and can report the following. In his first exposition on the okapi's discovery, Johnston mentioned Stanley's statement in *In Darkest Africa* without elaboration, except to add that his report made him "determined to make further inquiries on the subject whenever fate should lead me in the direction of the great Kongo forest"—which, in fact, it did soon afterwards (Sir Harry H. Johnston, 1901, The Okapi; Newly Discovered Beast Living in Central Africa, *McClure's Magazine,* September, pp. 497–501).

In his autobiography, published some two decades later, Johnston merely wrote: "Re-reading all this [traveler's accounts] and discussing the matter with Stanley before going to Uganda, I came to the conclusion that there must be some large hoofed animal haunting the Congo forest and described by Stanley as a kind of donkey occasionally caught in pits" (Sir Harry H. Johnston, 1923, *The Story of My Life,* Bobbs-Merrill, Indianapolis, p. 309). He made no comment on whether he thought Stanley had actually seen any of the animals.

However, there is a monograph by Sir Edwin Ray Lankester, published in 1902 but which was "received and read on November 19, 1901" at the Zoological Society, which sheds further light on the matter. In it, Lankester—then director of the British Museum (Natural History)—stated: "From conversation with Sir Henry Stanley during the present year (1901), Sir Harry Johnston has come to the conclusion that Stanley and his companions occasionally caught sight of the Okapi when they traversed the region of the Congo forest on the western side of the Semliki River" (E. Ray Lankester, 1902, On Okapia, a New Genus of Giraffidae, from Central Africa, *Transactions of the Zoological Society,* Vol. 16, Part 6, No. 1, p. 279).

Thus, it seems that, through personal conversation with Stanley, Johnston became convinced that Stanley had actually seen okapis—presumably the first European to do so—and not just heard about them from the Pygmies. Fortunately, Lankester recorded this in his monograph, and that is probably the source of Willy Ley's statement. However, it is curious that Johnston himself was not more specific on the matter in his own writings (if he was, I am not aware of it). If others have further historical documentation on this subject, I would appreciate knowing of it.

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Persepolis Bluebuck out of the Blue

(Comment on Robert G. Tuck, Jr., and Raul Valdez, 1989, Persepolis: Nilgai—Not Okapi, *Cryptozoology,* Vol. 8: 146–149)

Shortly after 518/517 B.C., the Persian king Darius I started creating Persepolis as a royal and sacred architectural unit for non-permanent residence. After his death in 485 B.C.—5 years after the Battle of Marathon—his son Xerxes assumed the throne, and he continued the building activities, probably adding the frieze of stairway reliefs that show the procession of delegations from all parts of the empire bringing their tributes on the occasion of the New Year reception (Nourouz ceremony).

Tuck and Valdez argue that the mystery animal depicted on one of the relief panels as a gift from the Ethiopian delegation is a bovid: the nilgai or bluebuck, *Boselaphus tragocamelus,* from the Indian subcontinent. Various other suggestions had previously been proposed, with a widespread preference for, if not consensus on, the okapi, *Okapia johnstoni.* On the other hand, the accompanying men leading the animal have unequivocal Hamitic (*not* "Negroid" as stated by Tuck and Valdez) features: short-woolled, curly hair, small, straight nose, thin lips, and pointed chin (Fig. 1). They have therefore been identified as "Ethiopians" by archaeologists, and are supposed to be nobles from northeast Africa. This discrepancy of origin, India vs. Africa, is, in the opinion of Tuck and Valdez, overcome by the suggestion, as put forward by their informant A. S. Shahbazi, that delegations bringing tributes to Persepolis might have purchased particular goods along the way.

This suggestion will, I am afraid, *not* solve the mystery, as people from northeast Africa will certainly not have travelled to Persepolis via India to buy a bluebuck there. Further, it should be noted that *all* tribute scenes from the Apadana palace evidence great efforts to depict anthropologically and ethnologically *typical* delegates in *typical* dress with *typical* gifts of their country (cf. the various breeds of horses, all differently sculpted, led by their ethnically different donors, so that archaeologists have been able to attribute the breeds to different peoples and tribes, and thus, origins). It has been noted that the animals on the panels were "individually drawn with startling realism and sympathetic observation" (A. U. Pope, 1965, *Persian Architecture. The Triumph of Form and Color,* George Braziller, New York, p. 36).

If the mystery animal was purchased as a "chance bargain" *en route,* to be thrown in with other goods the delegation brought from home (e.g., the elephant tusk the leader is bearing on his shoulder), it would certainly not have figured prominently as *the typical* gift of the Ethiopians. Contrary to Shabazi's *obiter dictum,* I hold that all gifts depicted *did* originate from the countries the delegates represented. It might have been considered a sacrilege to bring a "wrong" animal to the sacred New Year ceremony. The frieze depicting the "proper" gifts, in front of which the procession of delegations

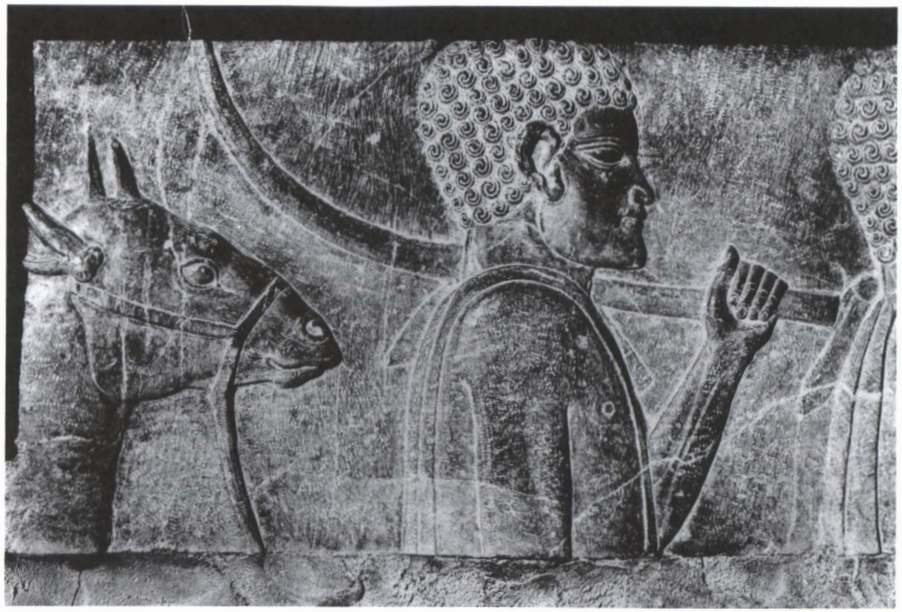

FIG. 1.—The head of the mystery animal appearing on a frieze at Persepolis, together with the head of one of the men leading the "Ethiopian" delegation submitting gifts to the King of Kings, ca. 500 B.C. (Gunter G. Sehm.)

solemnly passed, might even have been a silent admonition and constant reminder to the delegates. Therefore, people from Ethiopia would not be represented as bringing an animal from India.

For solving the evident paradox, I would like to draw attention to the fact that the bluebuck formerly inhabited not just India but also the Near East or at least parts of it. Bones of the nilgai—clearly determinable—have been found among prehistoric kitchen middens of the Upper Pleistocene period in what is today southern Jordan (Juliet Clutton-Brock, 1970, The Fossil Fauna from an Upper Pleistocene Site in Jordan, *Journal of Zoology,* Vol. 162: 19–29; Donald O. Henry, Priscilla F. Turnbull, Aline Emery-Barbier, and Arlette Leroi-Gourhan, 1985, Archaeological and Faunal Evidence From Natufian and Timnian Sites in Southern Jordan, *Bulletin of the American School of Oriental Research* Vol. 257: 45–64). Other large bovids that have been reported from western Asiatic sites, where they became extinct in historical times, include the Indian water buffalo, *Bubalus bubalis,* the aurochs, *Bos primigenius,* and the European wisent, *Bison* sp., (Juliet Clutton-Brock, 1979, The Mammalian Remains from the Jericho Tell, *Proceedings of the Prehistoric Society,* Vol. 45: 135–57; Donald O. Henry and Andrew N. Goddard, 1988, Tor Hamar: An Epipaleolithic Rockshelter in Southern Jordan. *Palestine Exploration Quarterly,* Vol. for 1988: 1–25).

As Jordan is so far the only locality where unequivocal nilgai bones have been found, it is possible that, at that period, the species was already on the brink of extinction in the Near East. Very few populations will have persisted in southern Jordan and Ethiopia (opposite coasts and hinterland of the Red Sea) into the time of the Persian Achaemenid kings, of whom Cambyses II was the first to campaign in northeast Africa (ca. 524 B.C.). According to Herodotus (Book III, 97), Cambyses, pushing southward from Lower Egypt, subdued only the northern-most Ethiopians around Nysa (near Elephantine Island, north of the Nubian Desert), who from that time figured as a satellite nation, not as a satrapy (province) of the Persian empire. There was no "satrapy of Kushiya," as Tuck and Valdez seem to think, the legendary Kingdom of Kush being situated in today's Sudan, with ancient Meroe for its capital, which, however, Cambyses did not conquer, nor even reach. Consequently, no tribute was imposed on them, but they had to deliver, every other year, "two choenixes (ca. the capacity of 2 quarts) of unrefined gold, two hundred blocks of ebony, five Ethiopian boys, and twenty great elephant tusks" (Herodotus, Book III, 97).

As, on the other hand, the "Asian Ethiopians," Hamitic nations which at that period lived on the east coast of the Red Sea (=17th satrapy), had to pay a regular tribute in silver (only), the reference to elephant tusks provides decisive evidence for my contention that the Persepolis relief represents, in fact, the Ethiopian delegation from Nysa. Thus, the animal depicted originated from this region (southernmost modern Egypt, between the Aswan Dam and Abu Simbel).

Ethiopian nilgai from this region were already extremely rare in the days of the Achaemenids, and, therefore, a precious gift worthy of a king of kings. Thus, the former distribution of the bluebuck in historical times will have to include both coasts of the Red Sea and their hinterlands, so that, with the Jordan population probably already extinct, the delegation brought, in fact, an animal "endemic" in their country, hence a *typical* animal.

Alternatively, if not yet extinct in the Levante, the delegation may have purchased the antelope along the way when travelling northward through the Jordan Valley, where I suppose the last haunts of the Levante bluebuck may have been—like those of the Near East hippopotamus. This possibility, however, I consider highly unlikely, as I would not call the "bargain-*en route*" hypothesis a viable one, less so now that another welcome option is at hand.

It is possible and likely that the Near East nilgai constituted a subspecies different from the Indian one, so that differences in morphology are not due to the sculptors' incompetence but, rather, to subspecific differences; e.g., the extremely sloping back of the Persepolis animal, its small size for an adult—cf. the horns—bull, its short "concave" muzzle, etc.

Consequently, Tuck and Valdez are correct in that a nilgai or bluebuck is

depicted on the Persepolis relief. But the live model was certainly not an Indian bluebuck, but a "genuine" Near East nilgai from Upper Egypt, between the Nile and the Red Sea north of the Nubian Desert.

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Baradiin by Michael Heaney; *Anatomy and Dermatoglyphics of Three Sasquatch Footprints* by Grover S. Krantz; *A Previously Unreported "Sea Serpent" Sighting in the South Atlantic* by Paul H. LeBlond; *Vertical Flexure in Jurassic and Cretaceous Marine Crocodilians and Its Relevance to Modern "Sea Serpent" Reports* by Eric Buffetaut; *Further Notes on the Role of Folklore in Hominology* by John Colarusso

Research Reports
Attitudes of Biological Limnologists and Oceanographers Toward Supposed Unknown Animals in Loch Ness by James E. King and J. Richard Greenwell

Field Reports
Results of the First Congolese Mokele-Mbembe Expedition by Marcellin Agnagna; *Further Investigations Into the Biological and Cultural Affinities of the Ri* by Roy Wagner, J. Richard Greenwell, Gale J. Raymond, and Kurt Von Nieda; *LCPI Work at Lake Champlain: 1983* by Joseph W. Zarzynski

Book Reviews
Les Bêtes Humaines d'Afrique [The Human Beasts of Africa] (Bernard Heuvelmans) by Pascal Tassy; *Aliens Animals* (Janet and Colin Bord) by Ronald M. Nowak; *A Geo-Bibliography of Anomalies: Primary Access to Observations of UFOs, Ghosts, and Other Mysterious Phenomena* (George M. Eberhart, comp.) by Marcello Truzzi; *Incredible Life: A Handbook of Biological Mysteries* (William R. Corliss, comp.) by J. Richard Greenwell; *Topsell's Histories of Beasts* (Malcolm South, ed.) by David Heppell; *The Magic Zoo: The Natural History of Fabulous Animals* (Peter Costello) by M. Justin Wilkinson

Comments and Responses
David Heppell, Leigh M. Van Valen, Frank E. Poirier, John R. Sibert, James G. Mead, Ron Westrum, William R. Corliss, Roy P. Mackal, Maurice Burton, Wayne Suttles

Volume 3 (1984)—160 pp.

Articles
The Birth and Early History of Cryptozoology by Bernard Heuvelmans; *The Search for Cougars in the Eastern United States* by Robert L. Downing; *"Seileag": The Unknown Animal(s) of Loch Shiel, Scotland* by Joseph W. Zarzynski; *The Orang-utan in England: An Explanation for the Use of* Yahoo *as a Name for the Australian Hairy Man* by Graham Joyner

Research Reports
Morphological Analysis of the Jiulong Mountain "Manbear" (Wildman) Hand and Foot Specimens by Zhou Guoxing

Field Reports
Activities of the Academy of Applied Science Related to Investigations at Loch Ness, 1984 by Robert H. Rines, Harold E. Edgerton and Robert Needleman; *A Field Investigation Into the Relict Hominoid Situation in Tajikistan, U.S.S.R.* by Dmitri Bayanov; *LCPI Work at Lake Champlain, 1984* by Joseph W. Zarzynski; *An Attempt to Obtain a Specimen of Sasquatch Through Prolonged Fieldwork* by Mark E. Keller; *Testing an Underwater Video System at Lake Champlain* by Richard D. Smith

Book Reviews
The Guinness Book of Animal Facts and Feats (Gerald L. Wood) by F. G. Wood; *On the Track of the Mystery Animal: The Story of the Discovery of the Okapi* (Miriam Schlein) by Wade C. Sherbrooke; *Loch Ness Monster* (Tim Dinsdale) by Roy P. Mackal; *Monstres des Lacs du Québec: Mythes et Troublantes Realites* [Monsters in Quebec Lakes: Myths and Troublesome Realities]

(Michel Meurger and Claude Gagnon) by Gerard Leduc; *Monsters Among Us* (Brad Steiger) by Daniel Cohen; *The Encyclopedia of Monsters* (Daniel Cohen) by J. Richard Greenwell; *The Bigfoot Casebook* (Janet and Colin Bord) by Charles A. Reed; *A World-Shocking Theft* [in Chinese] (Zhou Guoxing) by Paul H. LeBlond and Lichen Wang

Comments and Responses
Bernard Heuvelmans, Colin P. Groves, Peter F. Brussard and Janet Wright, Jack Lapseritis, Frank E. Poirier, Dmitri Bayanov, Michael Heaney, Michael J. Shields, Rene Dahinden, Grover S. Krantz, Ashley Montagu, Pascal Tassy, Glen J. Kuban, Marcellin Agnagna, Christine M. Janis, John R. Sibert, Richard Ellis, Kelvin M. Britton, Roy Wagner, J. Richard Greenwell, Jon-Erik Beckjord, Elizabeth C. Smith, Ron Westrum

Volume 4 (1985)—116 pp.

Articles
A Classificatory System for Cryptozoology by J. Richard Greenwell; *On the Possible Identification of the Egyptian Animal-God Set* by Michael D. Swords; *Towards an Etymology of Maori* Waitoreke by John Becker; *The Case of the Pygmy Gorilla: A Cautionary Tale for Cryptozoology* by Colin P. Groves; *Sole Pads and Dermatoglyphics of the Elk Wallow Footprints* by Susan Cachel

Research Reports
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Field Reports
Identification of the Ri Through Further Fieldwork in New Ireland, Papua New Guinea by Thomas R. Williams; *LCPI Work at Lake Champlain, 1985* by Joseph W. Zarzynski; *Investigation in the Lake Champlain Basin, 1985* by Richard D. Smith

Book Reviews
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Comments and Responses
Robert J. Meier, Roy P. Mackal, Rainer H. Brocke and Fred G. VanDyke, Robert L. Downing, Michel Raynal, John Becker, Dmitri Bayanov, Graham C. Joyner, Frank E. Poirier, Michael K. Diamond, J. Richard Greenwell

Volume 5 (1986)—152 pp.

Articles
Annotated Checklist of Animals with Which Cryptozoology is Concerned by Bernard Heuvelmans; *Investigating Sasquatch Evidence in the Pacific Northwest* by James A. Hewkin; *The Likelihood of Persistence of Small Populations of Large Animals and its Implications for Cryptozoology* by Peter F. Brussard; *The Yahoo, the Yowie, and Reports of Australian Hairy Bipeds* by Colin P. Groves

Research Reports
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Field Reports
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Book Reviews
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Comments and Responses
Zhou Guoxing, Benoit Grison, Jean-Jacques Barloy, Jean-Paul Debenat, Bernd Würsig and Greg Silber, Nikolai Spassov, J. Richard Greenwell, Ingo Krumbiegel, John W. Olsen, Michael D. Swords, Grover S. Krantz, Susan Cachel, James D. Lazell, David C. Guynn and Robert L. Downing and George R. Askew, Di Francis, Roderick Moore, Lena Bottriell, Edward B. Winn

Volume 6 (1987)—152 pp.

Articles
Why Cryptozoology? by Dmitri Bayanov; *Fossil Ungulate Mammals Depicted on Archaeological Artifacts* by Christine Janis; *A Reconstruction of the Skull of* Gigantopithecus blacki *and Its Comparison with a Living Form* by Grover S. Krantz; *The* tzuchinoko, *an Unidentified Snake from Japan* by Michel Dethier and Ayako Dethier-Sakamoto; *Cloning Extinct Genes* by Rif S. El-Mallakh

Research Reports
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Field Reports
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Book Reviews
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Comments and Responses
Thomas Tomasi, Michel Raynal, Christine Janis, Victor A. Albert, Bernard Heuvelmans, Dmitri Bayanov, Graham C. Joyner, Colin P. Groves, Charles A. Reed, Daniel Taylor-Ide, Anthony B. Wooldridge, J. Richard Greenwell, Frank E. Poirier, Michael Heaney, Jean-Paul Debenat, Gunter G. Sehm, Thomas R. Williams, Lorna E. Lloyd

Volume 7 (1988)—136 pp.

Articles
The Sources and Method of Cryptozoological Research by Bernard Heuvelmans; *Osteological Evidence for the Prior Occurrence of a Giant Gecko in Otago, New Zealand* by Aaron M. Bauer and Anthony P. Russell; *The "Monster" Episode in Adomnan's* Life *of St. Columba* by Charles Thomas; Waitoreke, *The New Zealand "Otter": A Linguistic Solution to a Cryptozoological Problem* by John Colarusso

Research Reports
A Mathematical Analysis of "Snowman" (Wildman) Eyewitness Reports by Valentin B. Sapunov

Field Reports
The First Japanese-Congolese Mokele-Mbembe Expeditions by Tokuharu Takabayashi; *LCPI Work at Lake Champlain, 1988* by Joseph W. Zarzynski

Book Reviews
Naturalized Mammals of the World (Sir Christopher Lever) by J. Richard Greenwell; *Kadimakara: Extinct Vertebrates of Australia* (Pat V. Rich and Gerard F. Van Tets, eds.) by Aaron M. Bauer; *Thylacine: The Tragedy of the Tasmanian Tiger* (Eric R. Guiler) by Malcolm Smith; *The Beast of Exmoor: Fact or Legend?* (Trevor Beer) by Karl P. N. Shuker; *The Enigma of Loch Ness: Making Sense of a Mystery* (Henry H. Bauer) by Robert H. Rines; *The Loch Ness Monster: The Evidence* (Steuart Campbell) by Richard Fitter; *Sticking My Neck Out! By Nessie* (Edward H. Armstrong) by Jack A. Gibson

Comments and Responses
Pascal Tassy, Nikolai Spassov, Michel Raynal, Christine Janis, John S. Buckley and Sally Y. Shelton, Grover S. Krantz, Aaron M. Bauer and Anthony P. Russell, Michel Dethier and Ayako Dethier-Sakamoto, Roy P. Mackal, Steuart Campbell, Paul H. LeBlond, Hugh H. Trotti, Eric Buffetaut, Graham Joyner, J. Richard Greenwell, Colin P. Groves, Dmitri Bayanov, James A. Hewkin, Richard D. Smith

Volume 8 (1989)—160 pp.

Articles
Reminiscences of the Discovery of the Coelacanth, Latimeria chalumnae *Smith* by Marjorie Courtenay-Latimer; *Paleocryptozoology: A Call for Collaboration Between Classicists and Cryptozoologists* by Adrienne Mayor; *Analysis of the Australian "Hairy Man" Data* by Malcolm Smith; *The Case for a Legal Inquiry into Sasquatch Evidence* by John Green; *Sasquatch Footprints: A Proposed Method of Fabrication* by Donald Baird

Field Reports
Further Investigations into the Reported Yeren—*The Wildman of China* by J. Richard Greenwell and Frank E. Poirier; *AAS Underwater Elapsed Time Camera Silhouette Photography Experiments at Loch Ness, 1989* by Harold E. Edgerton, Charles W. Wyckoff, Robert H. Rines, Robert Needleman, and Justice C. Rines; *Results of Chimpanzee Pheromone Use in Snowman (Wildman) Field Investigations* by Valentin B. Sapunov; *LCPI Work at Lake Champlain, 1989* by Joseph W. Zarzynski; *Continuing Sasquatch Investigations in the Pacific Northwest* by James A. Hewkin; *BCCC Report on Okanagan Lake, 1989* by John Kirk

Book Reviews
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Comments and Responses
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Volume 9 (1990)—140 pp.

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Dmitri Bayanov, Bernard Heuvelmans, Hugh H. Trotti, Jr., Wes Sumerlin, James A. Hewkin, Mark A. Hall, Angelo P. Capparella, Errol Fuller, J. Richard Greenwell, Gunter G. Sehm

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Walla Walla Casts Show Dermal Ridges; *Champ Photo Analysis Supports Animal Hypothesis*; *Board of Directors Meets in Vancouver*; Cryptozoology *Editorial Board Established*; *Message from the Editor*; *Honorary Members Elected*; *Cryptoletters, Cryptoquote*

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Interview (Paul H. LeBlond and Forrest G. Wood); *Nessie's 50th Birthday*; *Message from the Editor*; *Champ Passes New York Assembly*; *Chessie Videotape Analysis Inconclusive*; *Cryptoletters, Cryptoquote*

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Evidence for New Bear Species in Nepal; *Lloyds of London to Insure Ogopogo*; *Message from the Editor*; Cryptozoology *in* Nature; *Second Volume of* Cryptozoology *Published*; *ISC Membership Meeting*; *Lake Champlain Update: 1983*; *Cryptoletters, Cryptoquote, Wood's Animal Facts*

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Volume 3, no. 3 (Autumn, 1984)
Interview(Bernard Heuvelmans); *Controversial Sasquatch Hunt Unsuccessful*; *1985 Membership Meeting Scheduled*; *ISC Secretariat for Europe Established*; *Message from the Editor*; *ISC Policy Statement*; *Cryptozoology Symposium to be Held in Brighton*; *Cryptoletters, Cryptoquote, Wood's Animal Facts*

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Cryptozoology Books 1980–1987; *Message from the Editor*; *News & Notes*; *Cryptoletters, Wood's Animal Facts*

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Folklore and Cryptozoology Subject of Joint Conference; *Yemen Monitor Described*; *Message from the Editor*; *Fieldwork Fails to Ferret Out Mysterious Mustelid*; *1991 Meeting Program*; *Cryptoletters*; *Wood's Animal Facts*

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Search for Giant Gecko Intensifies; *Message from the Editor*; *Cryptids on Canadian Stamps*; *Nessie Bibliography Available*; *News & Notes*; *Membership Survey*; *Sustaining Members, 1990*; *European Secretariat Closed*; *Cryptoletters*; *Wood's Animal Facts*

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