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THE SOURCES AND METHOD OF CRYPTOZOOLOGICAL RESEARCH

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ABSTRACT: "Hidden" animals, with which cryptozoology is concerned, are by definition very incompletely known. To gain more credence, they have to be documented as carefully and exhaustively as possible by a search throughout the most diverse fields of knowledge. Cryptozoological research thus requires not only a thorough grasp of most of the zoological sciences, including, of course, physical anthropology, but also a certain training in such extraneous branches of knowledge as mythology, linguistics, archaeology, and history. It will consequently be conducted more extensively in libraries, newspaper morgues, regional archives, museums, art galleries, laboratories, and zoological parks, rather than in the field.

If proper cryptozoological work entails an exceptionally wide scope of multidisciplinary competence, the actual multifariousness of the tasks it involves implies that, according to his or her personal educational background, specialization, skill, or inclination, *anybody* interested in cryptozoology may contribute to its progress.

The purpose of the present paper is to assist all those concerned, not only by briefly outlining the right procedures to follow and suggesting the best sources of information, but also by delineating some promising areas to prospect, as well as matters deserving special consideration. To emphasize the extreme versatility of cryptozoology, this paper is deliberately expressed in a forensic perspective, rather than in a strictly zoological one.

INTRODUCTION

As various definitions of cryptozoology have been proposed which are either incomplete or overelaborate, fancy or quite erroneous, it would not be superfluous to first reassert what this discipline of science really is. It should, incidentally, be recalled that he or she who coins a new term has the privilege of choosing its meaning, provided it conforms to the etymology of the word.

As its name implies, being coined from the Greek roots *kryptos* (hidden), *zoon* (animal), and *logos* (discourse), cryptozoology is the science which deals with hidden animals. "Hidden" animals here refers to animal forms (species or subspecies) still unknown to, or undescribed by, traditional zoology (ac-

tually, unrecognized by a consensus of its representatives) because their asserted or alleged existence is supported by testimonial and circumstantial evidence only, or even by autoptical (i.e., material) evidence considered insufficient by some. Or, briefly, as it would appear in a dictionary: "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."

Neither more nor less. This strict definition encompasses all possible objects of cryptozoological research, and these objects only to the exclusion of all others (Heuvelmans 1987a).

The animal forms within which some of these "hidden" animals can be classified may admittedly be well known to zoologists or paleontologists. In these instances, it is their presence in a certain geographical area, or at a certain period of history (at the present time, for instance), which is not generally accepted. Representatives of a known species reported persistently in an unexpected region, belong perforce to at least a new geographical race; that is, a new subspecies. And representatives of a known species supposed to have become distinct during a past geological epoch have continued to evolve since the time of their alleged disappearance, and thus belong to another form: usually a new species, but sometimes a new genus. Both of these apparently alien or achronistic forms of known animals are to be considered unrecorded, unknown, or, better still, *well-hidden* species or subspecies, and thus fall within the scope of cryptozoology as defined above.

Outline of the Cryptozoological Method

What cryptozoology aims at is essentially the scientific description and naming of the aforesaid "hidden animals" as recognizable species or subspecies, which could eventually receive legal protection if threatened with extinction. It aims accessorily at a systematized search for specimens of these animal forms, which, if successful, would make them pass from a cryptozoological status to a zoological status.

To achieve these purposes, as far as any particular "hidden" animal is concerned, the cryptozoologist has first of all to patiently and doggedly collect the available evidence from all possible sources, then analyze these heterogeneous data, compare them, sift them (eliminating patent hoaxes, misidentifications of known animals or phenomena, and confusions with other unknown animals, by reducing exaggerations, and by deleting fantastic traits borrowed from mythological models), make them complementary to each other, and, finally, produce a coherent, consistent synthesis of them. Thus, by resorting to all and sundry resources of the zoological sciences, but also of extraneous branches of knowledge such as mythology and folklore, linguistics and semantics, archaeology and history, the cryptozoologist can endeavor to build up a sort of identikit-picture of the relevant animal,

including not only the anatomical traits which characterize its morphology, but also information on its physiology, behavior, ecology, and geographical distribution.

Thus, he or she will finally be able to identify the animal with some precision; that is, to classify it with some accuracy within the categories of the natural system, and perhaps even give it a legitimate scientific name. (For a detailed treatment of the whole procedure, see Heuvelmans 1965, 1968.)

This is where proper cryptozoological work ends, because, from that time forth, *any* laymen well informed about the cryptozoologist's conclusions—but preferably a skilled and well-trained tracker, animal catcher, gamekeeper, or hunter—will have a clear idea of where, and in which surroundings, to search for the animal in question, and what kind of tracks or other traces it is likely to leave of its presence; how to recognize it once sighted, even dimly; knowing the best time of day and which season for approaching it; knowing with which specific bait it can most plausibly be lured into a trap, a snare, a pitfall, or a net, and thus be captured—even if only on film—or just temporarily immobilized; knowing the time required for proper scientific inspection (the taking of photographs and measurements, the collection of samples, the performing of tests); and, ideally, determining if it is possible to establish a friendly—or at least pacific—relationship with it, and hence study it at leisure.

The whole process of cryptozoological work can best be compared with legal proceedings, with apparently unknown animals put on trial, a traditional, conservative zoologist acting as the public prosecutor, and the cryptozoologist as the counsel for the defense. To plead his case—for the enlightenment of his fellow zoologists, concerned conservationists, possible workers in the field, or potential sponsors—the defending cryptozoologist has to adduce evidence in support of his claims. Just as in a court of law, he should produce witnesses and documents, invoke circumstantial evidence, exhibit material proof if some is at hand, successfully argue all objections which are liable to be raised, and cite precedents. Unless the case has to be dismissed—at least provisionally—for lack of sufficient or unequivocal evidence, the cryptozoologist should be able to assign reasons for all relevant phenomena, introduce suggestions as to the identity of his "client," and offer pleas for its hearty reception within the community of known animals.

Let us review seriatim which tasks he must perform to achieve these goals, and hopefully win his case.

The Search for Testimonial Evidence

To find testimonial evidence supporting his case, the cryptozoologist has initially to consult the collections of newspapers, magazines and/or scientific

journals from all those countries in which articles may have been published reporting local rumors about the existence of the relevant animal; actual sightings of it, dead or alive; possibly an actual fight or physical encounter with it; the discovery of its supposed tracks or other material traces; or the result of its alleged predatory incursions or other depredations.

If such reports are recent enough, and if the names of the eyewitnesses have been duly recorded, the investigating cryptozoologist should endeavor to contact them directly, request a first-hand and detailed account (with relevant sketches), and then ask them specific questions—without, however, trying to influence them, and run the risk of distorting their testimony.

The perusal of collections of newspapers year after year is, of course, a most time-consuming, thankless, and often boring task, and the journalistic information it can produce is, moreover, of a less reliable nature. It is, nevertheless, the most rewarding search for testimonial evidence one can wish for, since—unless one has been informed personally—it is practically the only way of collecting the initial clues which enable further and more thorough investigation; namely, in the field.

From the classic *The Great Sea Serpent* by Oudemans (1892) to the recent volume *The Enigma of Loch Ness* by Henry H. Bauer (1986), the best documented works on cryptozoology always mention as many relevant newspaper articles as possible. For the most recent ones, this is often the result of the unobtrusive ants' work achieved by dedicated researchers who have preceded or helped their authors. So, one could never praise highly enough such individuals as the late George F. Haas (1906–78), editor of the *Bigfoot Bulletin* (1969–71), and his successor Warren Thompson, for their obstinate collecting of data on Sasquatch and other unknown hominoids, which have been regularly published since 1975 in their *Bigfoot Bibliography*.

Appreciation should also be extended to Gary S. Mangiacopra for scanning the collections in old newspaper morgues in the eastern U.S.A. in search of information about marine "monsters" (and incidentally some lake "monsters"), which resulted in a series of well-documented articles published from 1975 on in *Of Sea and Shore* magazine. Similar systematic quests, which will almost certainly prove as fruitful, have more recently been undertaken for the aquatic "monsters" of the West Coast of Canada by James Clark of Coquitlam, British Columbia, and for all unknown or fabulous animals reported in the state of Maryland by Mark Opsasnick of Greenbelt. It is to be hoped, for the sake of cryptozoology, that such enterprises will become increasingly frequent in the future, as they provide us with the basic building materials of our discipline.

Fortunately for cryptozoology, the more faithful disciples of the great Charles Hoyt Fort (1874–1932), tongue-in-cheek philosopher of science and collector of anomalous events extraordinaire, followed the path opened by him, and, at an early date, began to publish the latest reports on anomalous

animals. In this respect, the richest veins of cryptozoological news have been essential Fortean publications, to wit: *Doubt* (originally *Fortean Society Magazine*) (1937–59), *INFO Journal* (1967–present), *Pursuit* (1967–present), and the new *Strange Magazine* (1987–present), in the United States. It is actually *Fate* (1948–present) which has published by far the greatest number of articles of cryptozoological interest in the United States, but, unfortunately, the original sources are seldom cited. In the United Kingdom, it has been the *Fortean Times* (originally *The News*) (1973–present), and, in Canada, the (alas) defunct *Res Bureaux Bulletin* (1976–80).

Two one-man newsletters on unknown animals are now also disseminated from Continental Europe: Jean-Jacques Barloy's *Enquête sur le Serpent-de-Mer et les Animaux Mystérieux* (1978–present) in France, and Ulrich Magin's *BILK Magazine* (1985–present) in West Germany, which deals mainly with aquatic "monsters." Both these publications list much data irrelevant to cryptozoology, are in the habit of quoting one another, and report much third-hand information borrowed from other cryptozoological and Fortean periodicals. The former sometimes mentions first-hand information, but it is so vague as to be almost impossible to trace or check. Both publications are, however, worth consulting, as they occasionally give an interesting lead; a good tip should never be neglected.

The More the Heavier

Needless to say, the more witnesses who can be produced for each particular sighting—and, in the long run, for each kind of hidden animal—the stronger the case. Essentially, this is because an abundance and unanimity of testimonies has perforce greater weight. Sir Arthur Conan Doyle (1859–1930) once expressed this, much to the point (more so when one kindly replaces "shot" by "seen" in his statement): "If, for example, an okapi had only once been shot in Africa, its existence on the evidence of a single sportsman might reasonably be doubted. If 10 men agreed that they had shot such a creature, the evidence would be strong. If 50 had done so, it would become convincing. This is common sense" (Doyle 1919). It should be stressed here, incidentally, that scientific "truth" is, as a matter of fact, *always* statistical.

Accordingly, the inquiring cryptozoologist should endeavor, as his first important task in the field, to collect as many eyewitness testimonies as possible, but also to attempt to trace in libraries similar reports from former times, and even from the remotest past—not to mention traditions about the relevant animals which may have been current throughout history.

Information of this kind can be found in such varied documents as the Chaldean *Epic of Gilgamesh,* the Old Testament, and the commentaries of the Fathers of the Church; the writings of the first naturalists and encyclopedists of both classical and oriental antiquity; the Medieval bestiaries,

generated by the original *Physiologus*; the works of the three Dominican friars of the 13th century, who attempted to summarize all that was then known about zoology: Vincent of Beauvais, Albertus Magnus, and Thomas of Cantimpré; the accounts of the Moslem geographers and naturalists of the Middle Ages; the subsequent encyclopedias of the erudite compilers of the Renaissance, from Edward Wotton and Conrad Gesner to Jan Jonston and Father Juan Eusebio Nieremberg; the ancient chronicles of the countries where the relevant "hidden" animals are now said to dwell, be it on land, in lakes and rivers, or in the contiguous sea; the accounts of the early explorers, missionaries and merchants concerning all these regions; the later narratives of travelers, and, more especially, of wandering naturalists; the zoological treatises of the 18th and 19th centuries, both scholarly and popular, from Buffon, Cuvier, and Linnaeus to Jardine, Chenu, and Lydekker; and, finally, the more recent reports of zoological expeditions, the memoirs of colonial administrators and soldiers, game wardens and scouts, hunters and fishermen, farmers and stockbreeders—essentially, of *all* people interested even casually in wild animals.

All this requires, of course, intensive bibliographical research, for which the best guide is the fundamental opus *An Introduction to the Literature of Vertebrate Zoology* by Casey A. Wood (1931). This volume is even useful for the study of the few unknown invertebrates involved in cryptozoology, as it mentions all general treatises on the world's fauna. Moreover, it lists several prominent works of the travel and exploration literature, which has, of course, to be thoroughly reviewed. To achieve this herculean task, the following works should prove extremely helpful for the English-speaking researcher: *A Reference Guide to the Literature of Travel,* by Edward Godfrey Cox (1935–38), and Volume I (*Voyages and Travel*) of *National Maritime Museum: Catalogue of the Library* (Anonymous 1968).

The greatest part of this systematic bibliographical search has already been accomplished by the author himself during 40 years of constant, uninterrupted work. The references I have found have been duly cited in the relevant bibliographies of my published works. However, as the latter are far from covering all and sundry cryptozoological problems, which I tentatively reviewed in a checklist (Heuvelmans 1986), others will have to patiently wait for the author's further writings to obtain knowledge of the numerous remaining references. In the meantime, it will be necessary for others to find these for themselves by following the general lines which I have set out here. The diligent researcher may even uncover texts which have escaped my attention during a long and lonely quest. It certainly makes such bibliographic discoveries the more exciting when one knows that they are becoming rarer and rarer as work progresses.

Mercifully, the gathering of data, whether recent or ancient, is getting better organized and easier every year. So, to those interested in cryptozoology who feel ready to undertake the indispensable documenting task its research implies, I highly recommend a remarkably concise summary, by Scott Parker, of the correct procedures to be followed (Parker 1988).

The Search for Circumstantial Evidence

The hitherto mentioned sources are not only liable to yield more or less detailed accounts of actual sightings of apparently unknown animals, they may also contain a wealth of matter-of-fact data which can sometimes be used as circumstantial evidence.

This kind of evidence can assume the most diverse forms. Henry Thoreau (1817–62) wrote in his *Journal,* in November, 1850: "Some circumstantial evidence is very strong, as when you find a trout in the milk." Now, to find in the African rain forest a pygmy hunter describing casually a saber-toothed predator, or what sounds very much like a sauropod dinosaur, or to discover in a Spanish fishermen's church a silver ex-voto representing a coelacanth, forged long before this Devonian fish was discovered alive in 1938, this is, by all means, strong circumstantial evidence.

Of course, the less ancient the information one obtains on a "hidden" animal, the more convincing it is. When it is rather remote, the possibility always remains that the form in question has, in the meantime, died out. All the same, the alleged existence nowadays of any particular species of animal is assuredly supported in an indirect way by its acknowledged presence in the same area in former times, especially when this has been unequivocally established by the fossil record.

The further persistence of the relevant form up to more recent times can possibly be gathered from the evidence of prehistoric representations of it (drawings, paintings, engravings, carvings and sculptures), which would demonstrate, furthermore, that it was also contemporaneous with man in the past. This is particularly important when the depicted animal is thought by paleontologists to have died out during bygone geological periods, long before the coming of *Homo sapiens.*

Finally, the survival into historical times of such "prehistoric" animals, or even of more recent forms fallen into oblivion during the Pleistocene, can similarly be inferred from later works of art, starting with the treasures of antiquity through the most modern productions. In this perspective one should consult the classic opus *Die Antike Tierwelt* by Otto Keller (1909), *Animals in Archaeology* by Houghton Broderick (ed.) (1972) and *Die Zoologische Buchillustration* by Claus Nissen (1972). As K'ung Fu-tze, better known as Confucius (who lived around 551–479 B.C.), once put it: "An image is worth more than a thousand words."

A thorough investigation of the paleontological, prehistorical, and archaeological records is thus required of the conscientious cryptozoologist, in addition to his mastery of written sources. (It should be noted that, in

certain countries, namely French-speaking ones, one often distinguishes between prehistorical archaeology, labeled simply "prehistory," and historical or classical archaeology—history starting with the written record. This is at least partly due to the fact that two kinds of cultural relics and remains are studied by people with quite different backgrounds, the former having been trained as anthropologists, and the latter as historians.) A careful scrutiny of old maps, on which animals reported by travelers are sometimes drawn, can also be enlightening. In this connection, one could read *Animals and Maps* by Wilma George (1969).

In a comparative study of depictions of cetaceans in both cave art and ancient Scandinavian sketches, Lucie Arvy (1978) summarized the situation thusly: "The extinct faunas escape us. To imagine them we have only three kinds of data at our disposal: archaeological, rupestral [from rock art], and descriptive." This is also true, of course, for animals *supposedly* extinct, but which actually survived beyond their alleged date of disappearance, possibly until the present time.

Most animals, even the rarest and most elusive ones, are more or less familiar to the native people who share their habitat or live in or near their range. They are, to put it briefly, native-known, or, if one wishes to use scientific jargon, ethnognostic. Therefore, more circumstantial evidence can be extracted by the cryptozoologist from ethnographical sources. Needless to say, the asserted existence of an animal form by aboriginal or indigenous people can but strongly support the reports of travelers who claim to have had chance encounters with one of its representatives.

When, in Central Asia, for instance, a local hunter casually lists among the game a wild man, as well as a wild horse and a wild camel, one should seriously start wondering whether the extant rumors about creatures looking like reconstructions of prehistoric men are not founded, after all, upon reality.

As in the case of "hidden" animals, of which ancient depictions vouch for their past presence in a certain region, modern works of native art can also betray the present existence of some. The alleged survival of dinosaurs in Central Africa is not only supported indirectly by a Babylonian bas-relief of the Ishtar Gate depicting the Mesopotamian dragon or *sirrush*—a rather distorted version of an ornithopod dinosaur—it is supported much more directly by the recent use, by the Ashanti of West Africa, of small gold-weights representing totemistic animals, one of them being a long-necked quadruped with a reptile's head—which can hardly be anything else but a sauropod dinosaur (Heuvelmans 1978).

The Importance of Animalists

The prime importance of a systematic survey of both ancient and recent items from the iconographic record has been emphasized as early as 1955 by the author: "All too often zoologists make the mistake of underestimating

ancient or primitive art as a source of investigation" (the original French version was slightly different, as it spoke of ancient and *exotic* art) (Heuvelmans 1955, 1958).

This was particularly true of paleontologists, and some of them soon realized it. In 1968, for instance, F. Prat claimed that, in studying prehistoric depictions of European horses, he was able, based on anatomical criteria, to identify two distinct morphological types corresponding to subspecies—*Equus caballus germanicus* and *E. c. gallicus*—and possibly a third, ill-defined, rarer, and more doubtful one. He went so far as to suggest: "It is finally not impossible that the prehistoric artist has seen and depicted forms still unknown to paleontology" (Prat 1968).

Ten years later, in a survey of the paleolithic engravings on pebbles and bones from La Colombière (Ain, France), Martine Faure (1978) commented on this daring suggestion by stating that it "would certainly not have been denied by zoologist B. Heuvelmans." To support the importance of Quaternary art in the reconstruction of past faunas, she then reviewed an impressive array of prehistoric works, which not only corroborated the findings of paleontology but often completed and preceded them. After all, was it not the discovery of pictures of "ante-diluvian" animals that first proved their contemporaneity with man, and thus started the great 19th-century controversy over "fossil man," then considered unacceptable by the scientific establishment? It all happened, Faure recalled, just as the author first showed in 1955 in his book *Sur la Piste des Bêtes Ignorées* [On the Track of Unknown Animals], "how works of art have been able to confirm the existence of certain animal species until then unknown." She illustrated this by the examples I had given of the earliest known representatives of the gerenuk or giraffe-necked gazelle, the okapi, and Grévy's zebra, which preceded by centuries and even millenia their scientific descriptions. Finally, she drew my own attention to several intriguing but suggestive animal depictions of prehistoric art, including apparently achronistic saber-toothed cats and woolly rhinoceroses.

Incidentally, ancient representations of animals discovered far from their known range today do not necessarily imply a broader range in the past. They sometimes betray unsuspected relations, either commercial or cultural, between peoples living in lands rather distant from one another: as, for instance, a bas-relief of an indisputable cassowary (*Casuarius casuarius*) from a temple near Wadjak, in eastern Java, and another one of a man-sized moa (*Euryapteryx*?), squeezed between a rhinoceros and a buffalo, from the main temple of Angkor, built around 1200 A.D. in Cambodia (Koenigswald and Steinbacher 1986). The latter, of course, throws some light on the early migrations of the Maoris, as well as on the time of survival of some of the gigantic birds of New Zealand.

More recently, our thanks go to Christine Janis for having started scanning

the archaeological record in search of representations of fossil ungulate mammals generally thought extinct before the coming of modern man. Her first, brilliant contribution to cryptozoology (Janis 1987) opens the road to a most promising outlook.

The benefits science can reap from cryptozoological research can best be appraised by recalling the story of the discovery of the mammoth. Its existence was only disclosed, at the end of the 18th century, by the unearthing of fossil bones, but it was not before the last century that the examination in Siberia of whole carcasses preserved in permafrost informed us of its outer appearance. All the same, excellent depictions of this huge animal could already have been beheld at least as early as the 16th century, in the cave of Rouffignac (Dordogne, France), for instance, now famed as "la grotte aux cents mammouths" [the grotto with the hundred mammoths]. That this cave was visited at least as early as the Middle Ages is vouched for by François de Belleforest (1530–83), when he states in his *Cosmographie Universelle* that, in the "Cro du Cluzeau" (its former name), which he describes at length, ". . . on voit quelques autels, et des paintures en plusieurs endroits" [one can see a few altars, and in several places some paintings] (Belleforest 1575).

Now, by gazing at the latter, a naturalist with a cryptozoological turn of mind would have been led to conclude that, in former times, "hairy elephants" roamed about the countryside. No doubt this would have made him the laughing-stock of the zoological community, but time would have rendered him justice, and he would now be considered a pioneer of genius, one of the forerunners of paleontology.

Resorting to Linguistics

Since every conspicuous animal, down to the smallest one, is generally native-known, it will accordingly have been given a vernacular name, stressing as usual its more striking characteristics, or even several such names, particularly where different cultures meet or overlap within the geographical range of the animal. When one wants to collect more circumstantial evidence, therefore, resorting to linguistics and semantics can, at times, be extremely rewarding.

Take, for instance, the names given in many different countries of Eurasia to puzzling hairy hominoids, which Western scientists and their followers would be tempted, in a Darwinian perspective, to call "ape-men," "pre-men," "sub-men," or—why not?—"missing links." Since, on the one hand, there are no apes in these regions, and as the small, local monkeys look more like little cats than like men (whence such popular names as the Italian *marcati* and the German *Meerkatzen,* meaning "cats of [beyond] the seas"), and since, on the other hand, bears are common in these regions, and resemble big furry men when they occasionally stand up, it is not amazing that the said hominoids are almost everywhere known as "bear-men": namely *mi-dred* or *mi-teh* in Tibetan, *iu-wun* in Burmese, *ren-xiong* in Chinese, and *chelovek-medvied,* or rather, more familiarly, *chelovek-mishka* in Russian.

This shows very clearly that these vernacular names, given spontaneously, are quite original, meaningful, and probably very ancient: they can obviously not have been influenced by either modern scientific theories or the latest fashions of journalism. Thus, the creatures they are unanimously and perennially applied to, do not stand open to question or doubt, to hoaxes or occasional cases of mistaken identity.

There is, of course, much more to extract from linguistics and semantics in support of some cryptozoological cases. In this regard, it is to be hoped that we will soon see in print the well-documented and enlightening paper presented by Polish philologist Piotr Klafkowski (now living in Norway) at the symposium "Cryptozoology: The Search for Unknown or Supposedly Extinct Animals," III International Congress of Systematic and Evolutionary Biology, held in Brighton, England, in July of 1985 (Klafkowski 1985).

The Pervasion of Knowledge by Myth

The last source of ethnological information where circumstantial evidence can be gleaned by the cryptozoologist is, of all places, mythology. The main reference works to be consulted in this connection are those of Edward B. Tylor (1871), James G. Frazer (1890), James Hastings (1908–22), Antti Aarne (1928), John Arnott MacCulloch, L. H. Gray, and G. F. Moore (1916–31), Stith Thompson (1932–36), Maria Leach (1949–50), Ad. De Vries (1974), and Joseph Campbell (1984).

It cannot be too often repeated that "mythic" should never be used as synonymous with "fictitious"; on the contrary, it generally refers to what is thought to be fundamentally true (Heuvelmans 1982). It would seem that any information from the outside world received through our senses, when passing that part of the central nervous system sometimes called the "emotional brain," is systematically distorted and completed according to deeply embedded mental stereotypes in such a way that it loses the frightening nature of the unknown, and, having thus become more familiar, is accepted without the risk of harming the mind's balance and comfort. This is what one calls the process of mythification: the depth and scope of its action varies in direct proportion to the extent of our ignorance.

Unknown animals are actually *very incompletely known* animals. As they have to be largely mythicized to compensate for the disquieting gaps in our knowledge, their shape and behavior are subtly distorted according to the mythical "monster" they best fit, and some of their attributes are borrowed from it.

The archetype in question is, in accordance with the circumstances, the old devilish sea serpent; the tentacular island-beast or Kraken; the beguiling

mermaid (much less often her bearded companion); the evil Western dragon; the benevolent Eastern dragon; the crowing, crested Basilisk; the ravenous beast or werewolf; the ithyphallic-intended unicorn; the hedonistic wild hairy man or satyr; the men-enslaving Amazon; the oversexed hermaphrodite; the child-eating ogre or bogey-man; the goblin or cooperative denizen of the little people's underworld; the kidnapping Roc-Bird or sternly watching Thunder-Bird; the endlessly resuscitating phoenix; the multiweaponed, almost invulnerable griffin; the bloodsucking and eternity-seeking vampire; or the lake monster, custodian of sunken treasures, but also of unspeakable lusts. Each of these features is specifically linked with one or another of our psychological problems.

This explains why "hidden" animals are generally adorned with fantastic and even supernatural traits, which make them less credible or, frankly, unacceptable to sober-minded scientists. It is, however, by virtue of a universal mental process that these flesh-and-blood animals are fatefully transmogrified into fabulous beasts (Heuvelmans 1983a, 1987b).

One of the main tasks of cryptozoology is the attempt to strip off of the resulting "monsters" those attributes which have been borrowed from their mythological models, and which are often easily recognizable because of their stereotyped nature. For example, in former days the orang-utan and the gorilla were slanderously accused of being bloodthirsty brutes eager to abduct and rape women: this supposed behavior was simply to conform to the archetypal image of the "wild man of the woods," or satyr. The Yeti, or Abominable Snowman, is presently surrounded—obviously for the same reason—by an aura of violence and lubricity: the rumor has it that this creature, which is said to walk erect like man, can knock down yaks with a blow from one of its fists, can rip them open with its claws, and will devour their steaming bowels. It gulps down the eyeballs—or the testicles—of the men it kills, and never fails to abuse young girls.

Some natives also report that the mysterious Himalayan creature, which runs on all fours when hurried, feeds mainly on small mammals, such as marmots and pikas (or "mouse-hares"), and on large insects, besides consuming "clay-like earth, perhaps for bulk or some mineral value" (Izzard 1955: 106). It has also been reported that it is partial to some saline moss, which it scrapes from the rocks on the moraine fields. It would be while searching for this delicacy (probably rich in vitamins) that it leaves its tracks on the snow fields before returning to the rhododendron forests, in which it must normally hide.

In front of this heap of information on the mores of the alleged "snowman," it does not take much shrewdness to separate the wheat from the chaff, to discard what is ascribable to an inescapable mythicizing process, and to make the animal in question appear like it really is.

So, when a Scots crofter says he has seen an *each uisge* (water-horse) or a *kelpie* in a loch; when a Sherpa herdsman asserts that the *yeteh*—the gluttonous bogey-man he conjures up when his children refuse to eat their panada—has been roaming around his hut; when a Scandinavian sailor says his boat passed several times the *soe-orm* (sea-worm) or the immense *kraken*; when an Amazonian Indian alleges he once encountered the *curupira,* the boy-like bush spirit whose feet are turned backwards; when a Nandi stock-breeder in Kenya reports that the dreadful *chemosit,* a one-legged devil, half-man, half-bird, has been raiding his cattle-pen; when a female zootechnician working in Kabarda, U.S.S.R., wonders whether Satan himself did not pay her a nocturnal visit; whenever people, either simple-minded, frightened, terribly upset, or just inclined to sensationalize, recall their encounters with monsters, dragons, devils, demons, trolls, goblins, or fairies, it is just a manner of speech.

Their stories should not be dismissed off-hand as tall tales or figments of the imagination, as some folklorists wearing blinders do (Kirtley 1964, Meurger 1988). Their aprioristic debunking attitude before mythicized phenomena is perfectly absurd, as it should also logically lead them to conclude that anthropoid apes (the "wild men of the woods"), meteorites (curses from heaven), and television (the magic mirror) do not exist but in our imagination.

A Supplement of Circumstantial Proofs

When the detailed identikit-picture of any particular "hidden" animal has at last been completed, further circumstantial evidence supporting its existence can be extracted from the comparison of diverse—but in a way similar—cryptozoological cases.

So, it was by a comparative study of the obviously different types of so-called "sea-serpents" that the author was able to determine that some of them have a worldwide distribution, while others are restricted to certain rather well-defined areas (Heuvelmans 1965: Ch. 13, 1968: Ch. 14). This is, of course, consistent with what we know of other marine animals, and it can shed light on specific physiological characteristics of individual species. It also entails some comforting conclusions. If "sea-serpents" were merely a creation of human imagination, one would expect that they would all be described identically along the lines of just one stereotyped myth, or, alternatively, that they would be described very differently in each country or cultural region, according to local traditions and beliefs.

The case is even more remarkable as far as freshwater "monsters" are concerned. In the 1960's, while collecting additional material for Peter Costello, who was at the time preparing his book *In Search of Lake Monsters* (Costello 1974), it struck me that all such animals were described exactly as my long-necked type of "sea-serpent"—in my opinion, a large, giraffe-necked seal, undoubtedly a huge mammal—when reported from cold, temperate

fjord-like lakes or steep-sided rivers in both the northern and southern hemispheres. Lake "monsters" were described quite differently, and, moreover, heterogeneously, when reported from tropical lakes, rivers, or swamps; to wit, as normal-sized pinnipeds or sirenians, but more often as enormous catfishes or gigantic crocodiles, monitor lizards, and snakes, or even as amphibious dinosaurs of both the Saurischian and Ornithischian orders.

When I plotted on a map the various freshwater bodies involved, it became apparent that *all* the lakes where long-necked mammals with humps had been reported were located around isothermic line 10°C (between 0°C and 20°C) in both the southern and northern hemispheres. This consistency, revealing that the same form of animal had been reported independently in the same ecological niche from different regions of the world, is, of course, the best evidence one could wish for its existence. (I expressed this view for the first time during a British natural history television program introduced by [later Sir] Peter Scott, and transmitted by BBC1 on December 9, 1966.) It is, incidentally, in better conformity with the rules of animal physiology that the obvious reptilian types should have been reported from the warmer regions.

Quite similarly, the present existence, alleged by some cryptozoologists, of at least one form of large carnivorous marsupial on the Australian continent is consistent with ecological principles: such a powerful predator fills a niche which would better maintain a natural equilibrium. And it is for zoogeographical, geological, and paleontological reasons that the cryptic *waitoreke,* the only native mammal which would be dwelling in New Zealand, has a better chance of being an unknown marsupial "otter," or, even better, a new species of monotreme allied to the Australian platypus, than a placental otter (Heuvelmans 1955, 1958).

The Valuation of Material Evidence

Directly contrary to what many laymen and journalists state, it is not true that cryptozoologists cannot produce the slightest material evidence of the existence of the animals with which they are concerned. And I am not referring only to tracks, which some consider just molds—that is, "negatives"—of truly concrete flesh-and-blood animals, and are thus inclined to ignore. This is forgetting that much paleontological material is made up of prints or casts, and that palichnology, i.e., the study of ancient tracks, is now becoming the most promising trend of research for the reconstruction of fossil behavior (Seilacher 1967, Mossman and Sarjeant 1983, Vidal 1987). A leading naturalist, Ernest Thompson Seton, warned us some time ago: "Never forget the trail. . . . It is the priceless, unimpeachable record of the creature's life" (Seton 1958).

There is actually a wealth of physical evidence of various sorts supporting

the reality of some "hidden" animals, namely footprints (*sasquatch, yeti,* Central Asian giant, pygmy *orang pendek* from Sumatra and *batūtūt* from Borneo, Nandi-bear, African "dragons"), outsized sucker-marks on the skins of cetaceans (super-giant squids), photos and motion-picture film footage (Loch Ness and Lake Champlain "monsters," gigantic African snakes, South American "ape," Australia's mainland thylacine, Bigfoot), feces containing unknown parasites (*yeti*), tufts of hair or incomplete skins (unusual British cats, the spotted lion of Kenya, a spotted antelope from Liberia, the North African bear *Ursus crowtheri*), masses of preserved tissue (*Octopus* [or *Otoctopus*] *giganteus*), skulls (the Canadian bear *Vetularctos inopinatus,* the Andean wolf *Dasycyon hagenbecki*), a whole specimen (Mexican *onza,* the pygmy gorilla *Pseudogorilla ellioti,* the surviving Neanderthal from Vietnam *Homo pongoides*), and even, exceptionally, live specimens kept in a zoo (white bear from Central China) (Heuvelmans 1986). However, as mentioned in my definition of cryptozoology, all this material evidence is still controversial, as it is considered insufficient by some.

Material proofs are admittedly not conclusive, as they can be faked, or simply misinterpreted. One does not have to evoke the ominous hoax of Piltdown Man to be convinced of this. The history of zoology, and more especially of paleontology, teems with colossal blunders made by the most distinguished scientists on the strength of objects which had *not* been faked. The bones of the zeuglodont, an archaic whale, where first attributed by Harlan to a huge reptile, which was named *Basilosaurus*; the dagger-like thumb of the iguanodon was, amusingly, placed on its nose in the scientific reconstruction of Gideon Mantell; the monstrous claws of *Chalicotherium,* a peculiar ungulate mammal, were taken by Cuvier, the "father of paleontology," for those of a gigantic pangolin; a pig's molar was thought by Osborn to belong to an American ancestor of man, which he christened *Hesperopithecus,* the "dawn-monkey"; and, not so long ago—the incident has modestly been kept secret—a fragment of a human skull, found in a Mousterian stratum from the Charente, France, was identified as a piece of skull from an extinct tortoise by—*horresco referens*!—a prominent French physical anthropologist, who, significantly, is an avowed enemy of cryptozoological claims.

The sundry items listed above are of very unequal value. Footprints in mud or clay, particularly when they are so distinct as to show dermatoglyphs (Krantz 1983), are, of course, more reliable than footprints in snow. The film by the late Tim Dinsdale, revealing the surface movements of one of the Loch Ness animals, and which was carefully analyzed by experts at the Joint Air Reconnaissance Intelligence Center (JARIC) of the Royal Air Force, is certainly more convincing than the Patterson film showing an alleged Bigfoot with some grievous anatomical inconsistencies. A single skin, a skull,

or even a whole specimen runs the risk of being dismissed as representing an individual variation or a freak, which again stresses the fact that scientific truth is always statistical.

The greatest naivete of beginners in cryptozoology is to imagine that they will definitely solve a case by producing a solid piece of evidence: a very distinct movie, perfect tracks, or, preferably, an acquired specimen. This is simply not realistic; only a large amount of such material proof would actually achieve this. I do not mean to imply that cryptozoologists should not be searching for evidence in the field. What they should be aware of, however, is that it is only when added to both testimonial and circumstantial evidence that autoptical evidence will help them win a case.

The Citation of Precedents

The more convincing precedents a cryptozoologist can cite in support of any case is the long list of major zoological discoveries made during the last centuries and up to the present time.

He or she should first show, by calculating the past rate of discovery of new species in all groups of animals, that this rate, which had been continually increasing for the majority of phyla since the birth of systematic zoology, and reached its peak around the first quarter of the present century, is now decreasing—but rather slowly. At present, more than 5,000 species are still discovered every year. Of these, about 4,000 are new insects, but the number of new vertebrates, which are of greater interest for cryptozoology, is far from being negligible: "Quite recently, in the mid-1970's there were discovered, each year, around 112 new species of fishes, 18 new species of reptiles, about 10 new species of amphibians, the same number of mammals, and 3 or 4 new species of birds" (Heuvelmans 1983b).

This mathematical demonstration of the high hopes which cryptozoologists may entertain for the future, can be supported in an even more striking way by a historical survey listing the most spectacular or astonishing zoological discoveries made in this century. The author has already sketched the broader outlines of this story in a chapter of *On the Track of Unknown Animals* (Heuvelmans 1955, 1958), but he more recently refined this demonstration by providing a rigorous and exhaustive list, although restricted to mammals only. This list does not include mammals weighing less than 80 pounds, new subspecies, or still controverted forms (Heuvelmans 1984).

Similar chronological lists of the most striking new species of birds, reptiles, amphibians, and fishes discovered since the turn of the century would be very welcome, as these precedents would demonstrate that the probability of discovering medium- to large-sized vertebrates of most classes is still high.

Additional precedents, which should be cited whenever incredulity is exhibited concerning the claimed survival nowadays of animals considered long extinct, can easily be gleaned in the rich harvest of so-called "living fossils."

This paradoxical term was coined by Darwin, when, in *The Origin of Species,* he expressed amazement at the existence of such freshwater animals as ganoid fishes (*Polypterus,* sturgeons, gar-pikes, and bowfins), the platypuses, and the South American lungfish *Lepidosiren,* ". . . which, like fossils, connect to a certain extent orders at present widely sundered in the natural scale. These anomalous forms may be called living fossils; they have endured to the present day, from having inhabited a confined area, and from having been exposed to less varied, and therefore less severe, competition" (Darwin 1859).

In a chapter of his first major cryptozoological work (Heuvelmans 1955, 1958), the author attempted to demonstrate that the notion of "living fossils" is a very loose one, and could actually be applied to a tremendous array of life forms. That many supposedly extinct animal species—be they prehistoric men, archaeocetes, or dinosaurs—can be surviving today should not be surprising. This can only surprise people who continue to subscribe to an ancient and obsolete view of the mechanics of evolution, one based upon the naive and anthropocentric concept of the *scala naturae,* which is older than the idea of evolution itself; that is, the concept of "the great chain of beings," so well analyzed in a 1942 book bearing the same title (Lovejoy 1942).

The idea of such a natural ladder, or staircase, leading from the simplest and least organized forms to the most sophisticated ones, was originally linked with the religious belief that man stands not only at the pinnacle of life's creation on earth, but was indeed the very end of it. When the theory of biological evolution gained acceptance, man again became the summit and final outcome of it. There is, however, nothing scientific in such a conclusion, as the premises of the syllogism are purely dogmatic and based upon faith alone. When evolution is represented graphically, the result does not look at all like a ladder, a flight of stairs, or a simple chain, but rather like a thick bush. New forms do not evolve from ancient ones which then automatically disappear, but branch off from them, while the older forms usually persist. This is why there are still representatives today of *all* known zoological phyla, even those thought to be the most "primitive," "archaic," or "inferior."

Animal species, genera, families, and even whole orders rightly considered "living fossils" are so numerous (Delamare-Deboutteville and Botošanéanu 1970) that they can, in a way, be taken as the rule rather than the exception. Only very few classes of animals—or groups of almost equal importance—seem to have entirely disappeared in the past. To wit, the archaeocytes, of dubious affinities but distantly related to the Foraminiferida, the sponges and the cnidarians, the bellerophontids of the phylum Mollusca, the graptolithes, thought until recently to be related to the Pterobranchia and thus belonging to the phylum Hemichordata, and, last but not least, the trilobites, of the phylum Crustacea.

Why these rare—one is justified to state *exceptional*—classes are appar-

ently extinct today, nobody knows for certain. It is even more difficult to understand why numerous orders of animals seem to have vanished entirely, while other, closely related ones are still flourishing. One has only to review the plethora of "scientific" theories, usually of the most extravagant nature, which have been put forth to account for the supposed—at least generally accepted—demise of dinosaurs, the latest craze being an epidemic of comets. It seems that none of the scientists responsible for such lucubrations thought of first asking this very simple question: "How can *any* catastrophic impact wipe out selectively a whole group of animals and leave unscathed all other ones sharing the same habitat or landscape, even their closest relatives?"

The truth is that, just as with individuals, larger or smaller groups of animals eventually die of "old age," from fair wear and tear; that is, from the progressive accumulation of mostly harmless ailments for individuals, and of unfavorable factors of all sorts for species or more comprehensive categories. All the same, when particularly well-adjusted to their respective ecological niches, groups of any class of animals manage to survive much longer than others, as Darwin judiciously surmised. This is what makes "living fossils" so common.

To summarize, what should surprise us is *not* that numerous, supposedly extinct animal species may survive today, but, rather, that so many species—sometimes whole zoological groups—*have* actually disappeared for reasons nobody has ever been able to unequivocally explain.

This should help to rule out the last objection—but also the most common one—which can be raised against some cryptozoological claims.

In concluding, I would like to provide prospective cryptozoologists with some sound advice. First of all, one should always brief one's case very carefully, collecting as much data as possible, and taking one's time (as Molière once put it: "Time does not spare what has been achieved without its help"). Secondly, one should stick to facts, not to faith; to strict logic, not to conventional wisdom. That people, be it scientists, journalists, or laymen, believe or not in the existence of a "hidden" animal is of no importance whatsoever from a zoological perspective, which is the only one of consequence.

Finally, one should not let anybody (debunkers or authorities) nor anything (weariness or despair) interfere with one's determination. Cryptozoological research should be actuated by two major forces: patience and passion.

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OSTEOLOGICAL EVIDENCE FOR THE PRIOR OCCURRENCE OF A GIANT GECKO IN OTAGO, NEW ZEALAND

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ABSTRACT: Reevaluation of a mandibular ramus and "rib" discovered at Earnscleugh, Otago, New Zealand suggests that the bones are assignable to a gekkonid lizard of gigantic size. The "rib" is consistent in form with a cloacal bone, a feature lacking in other New Zealand lepidosaurian reptiles. A comparison with many gekkonid cloacal bones, including that of *Hoplodactylus delcourti*, the largest gecko known to have ever lived, indicates that the Earnscleugh lizard was between 275 and 325 mm in snout-vent length. The applicability of the Maori names *kawekaweau, ngarara,* and *kumi* to the Otago remains is discussed.

INTRODUCTION

Hoplodactylus delcourti is the largest gekkonid lizard known to have ever lived (Russell and Bauer 1986). This species has been described from a single skin and partial skeleton preserved in the Marseille Natural History Museum since the 19th century (Fig. 1), the biological significance of which has only recently been recognized (Bauer and Russell 1986). The specimen measures 370 mm in snout-vent length [SVL] (total length is 622 mm), and therefore exceeds the next largest species, the New Caledonian giant forest gecko, *Rhacodactylus leachianus,* by 54%. The magnitude of this size in gekkonid terms may be appreciated by noting that fewer than 30 of the 800+ gecko species attain a body length of even one-third (and hence a body weight approximately $\frac{1}{27}$) that of *H. delcourti.*

Unfortunately, no locality data accompany the Marseille holotype of *Hoplodactylus delcourti.* Nonetheless, Bauer and Russell (1986, 1987) have argued for a New Zealand origin on the basis of both morphological and ethnographic evidence. Although the skeleton is incomplete, the skull and limb bones are present, and have been examined by means of radiography. Osteology, along with certain features of toe structure and scalation, demonstrates the affinity of the species to the paraphyletic grouping of endemic New Zealand "brown geckos" currently placed in the genus *Hoplodactylus* (Bauer 1986).

Additional evidence drawn from early European reports (Mair 1873, Buller 1895, Walsh 1905), as well as Maori folklore (Best 1909, 1923), has led us to postulate that *H. delcourti* is identifiable with the Maori name *kawe-*

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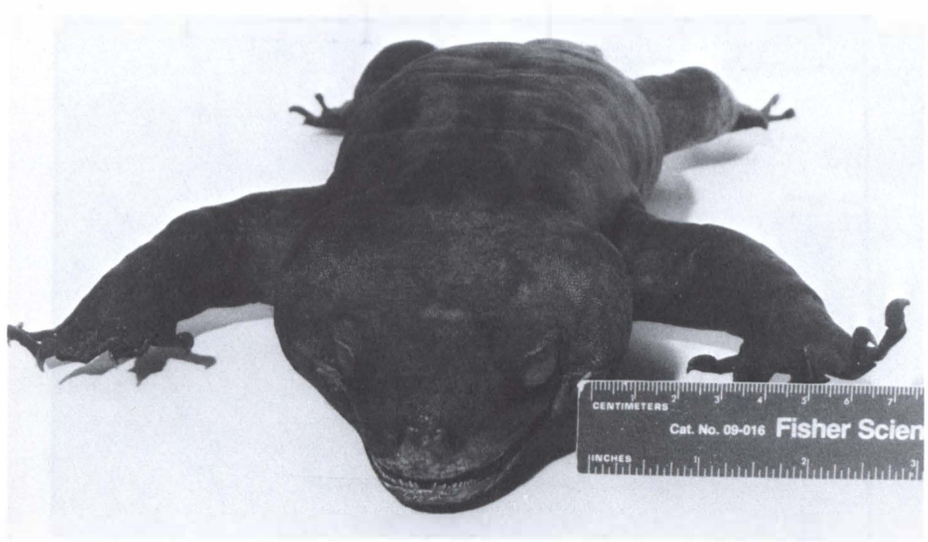

FIG. 1.—Head-on view of the holotype of *Hoplodactylus delcourti* (MNHM 1985-35), the largest known gecko.

kaweau, used in reference to a large, lizard-like creature (mistakenly identified by Williams [1975] as the tuatara, *Sphenodon punctatus*), known until about 1875 from various localities on North Island, New Zealand (Fig. 2). Details supporting our identification are provided elsewhere (Bauer and Russell 1987). Confirmation of both the New Zealand origin of *Hoplodactylus delcourti* and its association with the *kawekaweau,* and perhaps other Maori reptile names, must await the location of additional specimens (living or subfossil). We here report on osteological evidence first presented by Hutton (1875, 1899) that suggests that *Hoplodactylus delcourti,* or an unknown gecko of similar size, once occurred in Otago, on South Island, New Zealand.

The evidence derives from two bones discovered in the Earnscleugh Cave in Central Otago, near the present town of Clyde (Fig. 2). The age of the cave deposits is quite recent (Hutton 1875), as indicated by the co-occurrence of rats (introduced by man ca. 1000 A.D.) with moas, tuataras, and other small native animals. Hutton (1875) believed that the remains dated only from the beginning of the 19th century. The lizard bones in question are a lower jaw ramus with pleurodont dentition (teeth attached to the inner side of the jaw) (Fig. 3), "about the size of that of a tuatara" (Hutton 1875, 1899: 485), and a small bone (Fig. 4A) described by Hutton (1899) as "what appears to be a small vertebral rib, belonging to the left side, and which may possibly

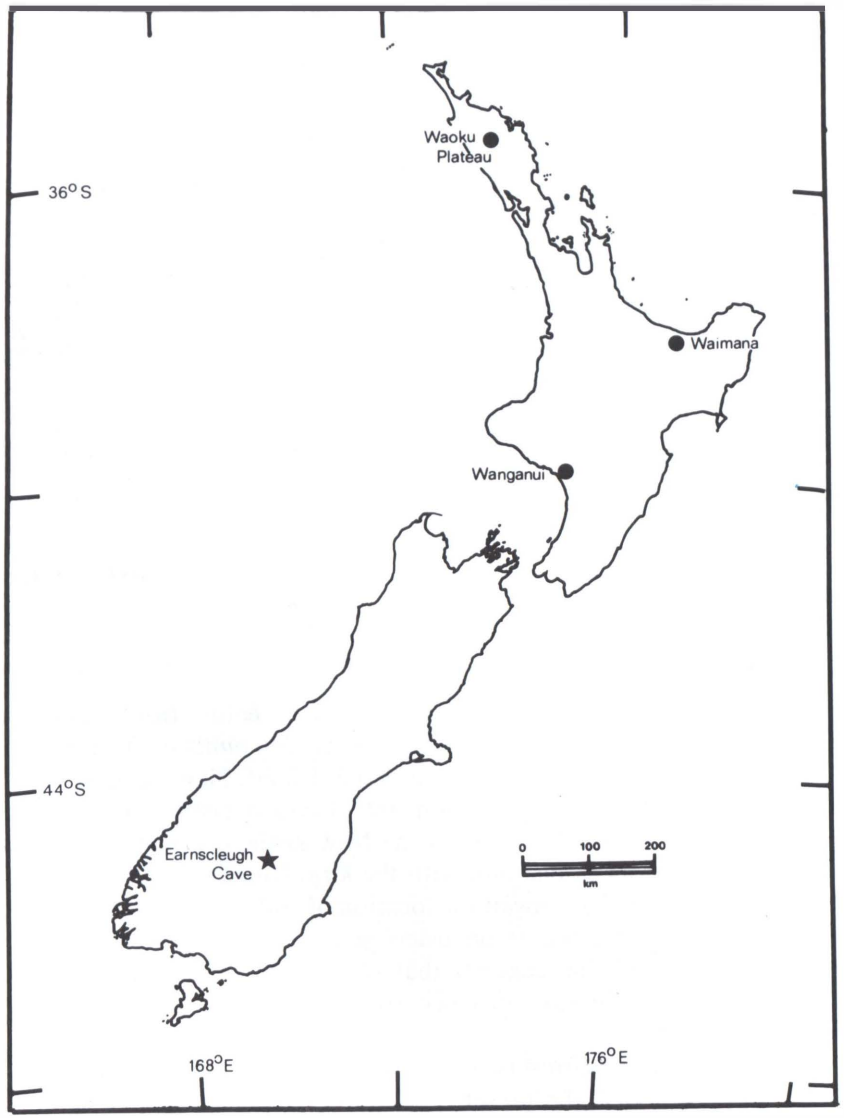

FIG. 2.—Map of New Zealand indicating localities associated with the *kawekaweau* (*H. delcourti*)—circles; and the cave deposits discussed by Hutton (1875, 1899)—star.

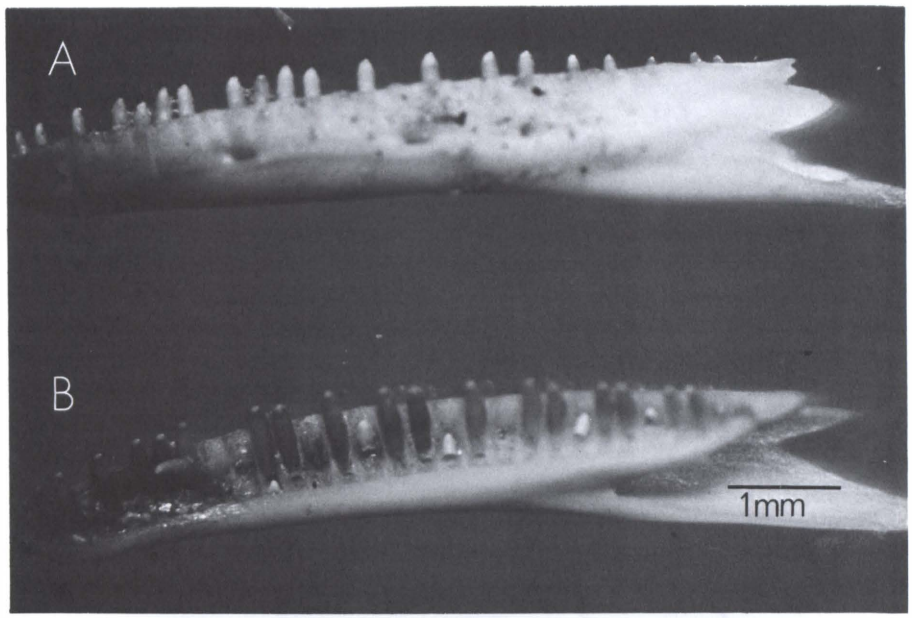

FIG. 3.—A. Lateral (labial) and B. medial (lingual) views of the mandible of *Hoplodactylus maculatus* showing pleurodont dentition.

have belonged to the same animals [as the jaw]. It may be the last cervical of a reptile."

METHOD

The Hutton (1875, 1899) literature descriptions and figure were compared with radiographs of *Hoplodactylus delcourti* and with radiographs, dry skeletal preparations, and cleared and stained specimens (see Wassersug 1976 and Hanken and Wassersug 1981 for technique protocol) of representative species of the following gekkonid genera: Subfamily Eublepharinae—*Aeleuroscalabotes, Coleonyx, Eublepharis, Hemitheconyx, Holodactylus*; Subfamily Diplodactylinae—*Bavayia, Carphodactylus, Crenadactylus, Diplodactylus, Eurydactylodes, Hoplodactylus, Naultinus, Nephrurus, Oedura, Phyllurus, Pseudothecadactylus, Rhacodactylus, Rhynchoedura*; Subfamily Gekkoninae—*Afroedura, Agamura, Ailuronyx, Alsophylax, Bogertia, Briba, Bunopus, Calodactylodes, Chondrodactylus, Cnemaspis, Colopus, Cosymbotus, Crossobamon, Cyrtodactylus, Geckonia, Gehyra, Gekko, Hemidactylus, Hemiphyllodactylus, Heteronotia, Homonota, Homopholis, Lepidodactylus, Luperosaurus, Nactus, Pachydactylus, Palmatogecko, Perochirus, Phelsuma, Phyllodactylus, Phyllopezus, Ptenopus, Ptychozoon, Ptyodactylus, Rhoptropus, Stenodactylus, Tarentola, Teratoscincus, Teratolepis, Thecadactylus,*

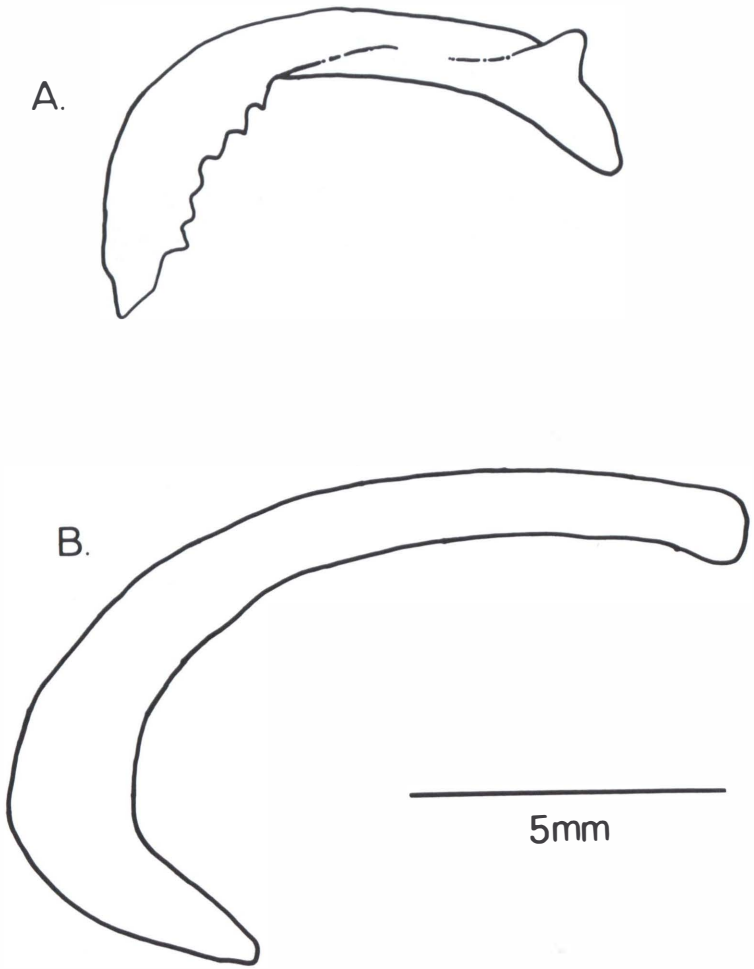

FIG. 4.—A. Hutton's (1899) illustration of the Earnscleugh "rib" (redrawn in horizontal orientation). B. The right cloacal bone of *Hoplodactylus delcourti* (MNHM 1985-35) drawn to same scale as Fig. 3A. Details of the morphology of this element cannot be seen in the radiographs from which it was drawn, hence only an outline is presented.

Tropicolotes, Uroplatus. Within *Hoplodactylus,* seven of the eight congeners of *H. delcourti* (*H. chrysosireticus, H. duvaucelii, H. granulatus, H. maculatus, H. pacificus, H. rakiurae* and *H. stephensi*) were examined. Only the recently described *H. kahutarae* (Whitaker 1985) was unavailable for comparison. A small number of skink and *Sphenodon* skeletons were also examined.

FIG. 5.—A tuatara, *Sphenodon punctatus,* biting a stick and revealing its acrodont dentition. Photo taken on Lady Alice Island, New Zealand.

EVALUATION OF THE EARNSCLEUGH LIZARD REMAINS

Lower jaw.—Unfortunately, Hutton does not provide an illustration of the lower jaw ramus from Earnscleugh. He states, however, that the dentition was pleurodont (Fig. 3), and that the size was about that of a tuatara (*Sphenodon punctatus*) jaw. The dentition alone is sufficient to rule out *Sphenodon* as the animal involved, as it is typified by acrodont dentition (teeth attached to dorsal surface of dentary) (Fig 5), rather than pleurodont dentition.

The pleurodont condition is found in both of the families of lizards known from New Zealand, the Gekkonidae and Scincidae. The size of the jaw ramus from Earnscleugh would appear to have been approximately 75 mm total length. This value was determined by averaging the mandibular lengths of a sample of tuataras (n = 5) within the adult body size range. This length is far greater than that found in any of the New Zealand lizard taxa known to be extant. Indeed, it is approximately half of the total head plus body size of the largest representatives of both families—160 mm SVL for *Hoplodactylus duvaucelii* (Russell and Bauer 1986) in the Gekkonidae, and 143 mm for *Leiolopisma homalonotum* (Gill 1985) in the Scincidae. Both of these lizards have maximal mandibular lengths of well below 40 mm. The size of

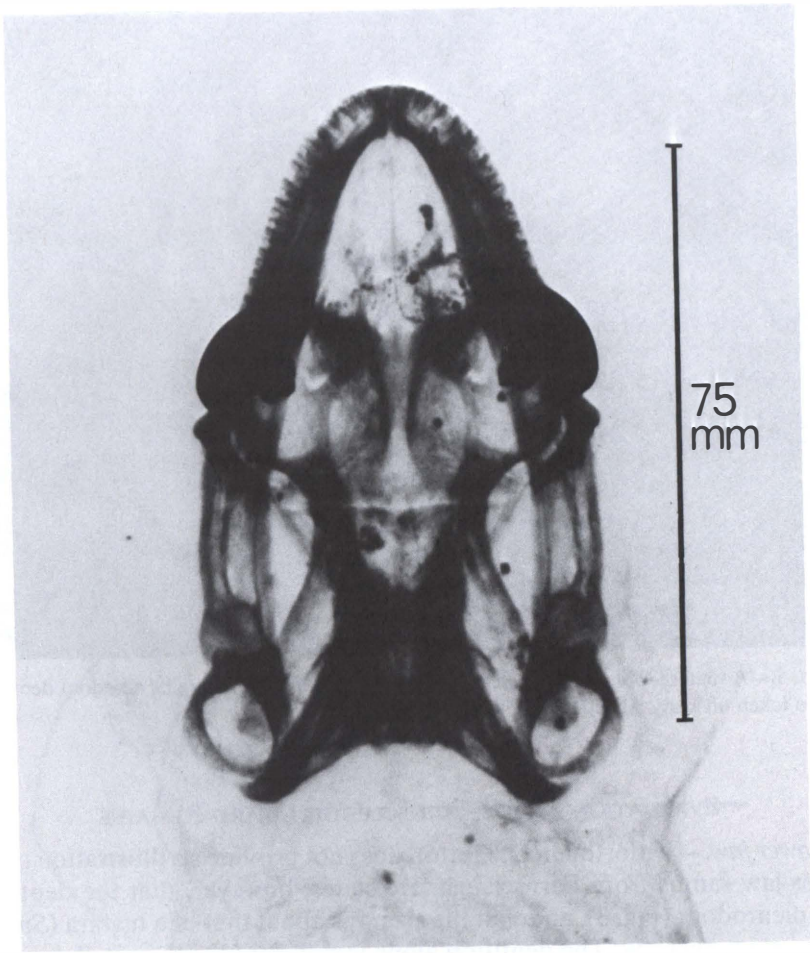

FIG. 6.—Radiograph of the skull of *Hoplodactylus delcourti*. The 75 mm bar represents the approximate size of an adult tuatara, *Sphenodon punctatus*, skull.

the dentary of the largest subfossil New Zealand skink yet reported, estimated SVL at 155 mm (Gill 1985), suggests a total mandibular length of only about 32 mm. By contrast, the jaw ramus of *H. delcourti* is approximately 82 mm in length (extent of the retroacticular process is partially obscured on the radiographs) (Fig. 6).

Rib.—The bone believed by Hutton (1899) to be a posterior cervical rib is problematic. Hutton described the bone (Fig. 4A) as being 14 mm in length (measured along the curve), and possessing a distal row of denticulations on the flattened portion of the element. Its overall aspect was described as

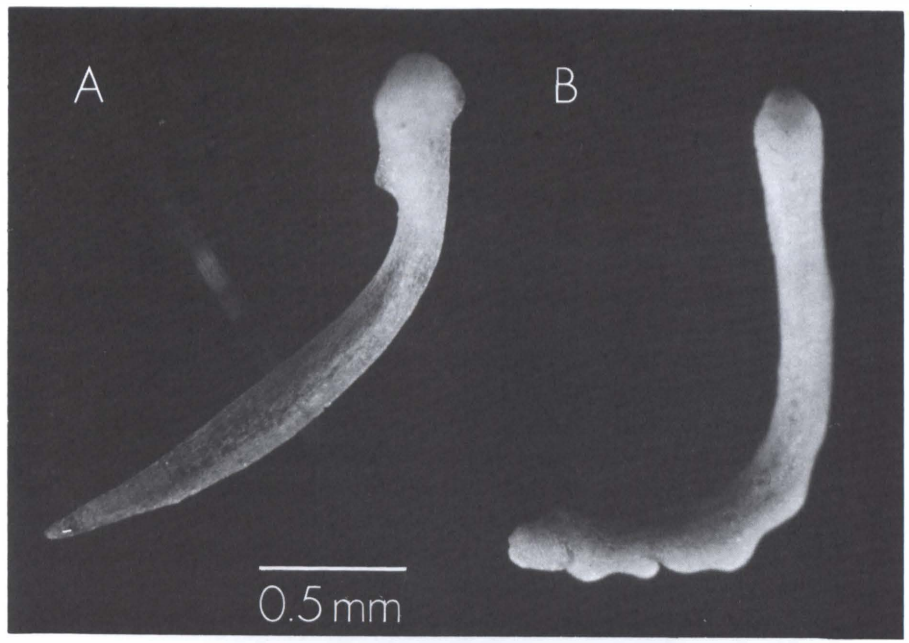

FIG. 7.—A. Anterior cervical rib from the right side of *Hoplodactylus maculatus* (Aaron M. Bauer pers. coll. 88). B. Right medial cloacal bone from the same specimen.

"robust." A comparison with the skeletons of living species of geckos, skinks, and *Sphenodon* has revealed that Hutton's conclusion as to the identity of the bone was incorrect. Hutton (1899) himself stated of the bone: "It is, indeed, unlike anything known to me."

Hutton's identification of it as a rib may have been based on the apparent bicondylar "head" of the element, and its curvature. However, this arrangement, typical of mammals, does not generally occur in lepidosaurian reptiles, which possess single rib heads (Stephenson and Stephenson 1956). The posterior cervical ribs of both geckos and skinks have a broad head and taper into a narrow, somewhat flattened shaft which again expands distally, yielding a barbell shape. The distal portion of the ribs is often partially cartilaginous. Typically, there is little curvature along the length of the ribs. The more posterior ribs—sternal, mesosternal, and thoracic—are thin and elongate, entirely unlike the Earnscleugh element.

Only the anterior cervical ribs (especially the second) bear a resemblance to the bone from Otago (Fig. 7A). Both bear a small proximal flange, and a flattened distal portion. However, the curvature is different, and there are never denticulations, and, again, there is Hutton's statement that the bone was unlike any known to him. Likewise, there is no element in the appen-

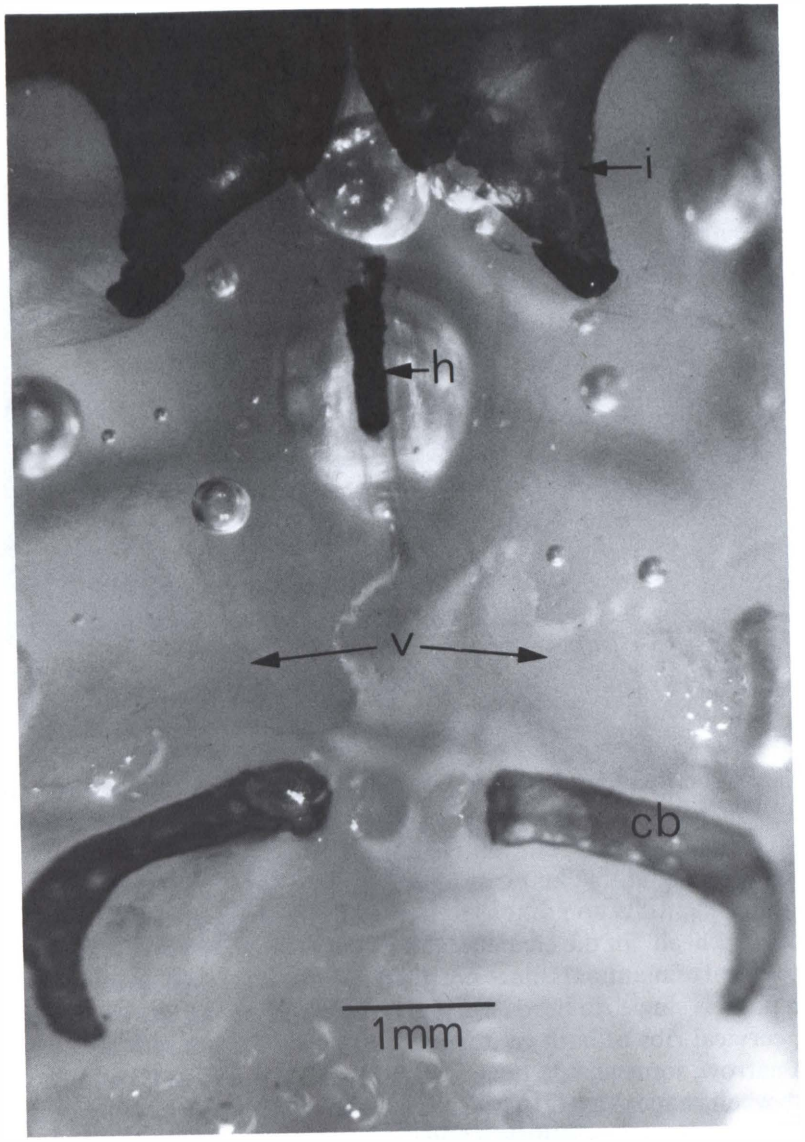

FIG. 8.—Cloacal bones *in situ* in a cleared and stained specimen of the gekkonine gecko *Ptychozoon lionatum* (Anthony P. Russell pers. coll. 54). cb = cloacal bone, h = os hypoischium, i = ischium, v = position of vent.

dicular or axial skeletons of geckos or skinks that approximates the element from the cave deposits.

The closest match to the element is the medial cloacal bone (or "postcloacal" bone). Cloacal bones are small, paired elements typically present in association with the hemipenes of geckos (Fig. 8). These structures were first noted in *Gekko japonicus* by Schlegel (1838), and have since been reported in males of most taxa of geckos, excluding those of the New World sphaerodactyline radiation (Wiedersheim 1876, Kluge 1967, 1982, Russell 1977, Bastinck 1986). They are also present in the highly derived geckos commonly accorded separate familial rank, such as the Pygodopidae (Kluge 1967, 1974, 1987). Similar but apparently non-homologous elements are found in the North American night lizards, Xantusiidae (Savage 1957, Rieppel 1976), and in certain fossil "prolacertilians." There may be two sets of cloacal bones in geckos, a larger medial pair and a smaller pair of often irregular lateral bones. Although many taxa possess only medial bones, lateral elements never occur alone.

The function of cloacal bones is unclear, but they are intimately related to cloacal sacs (Smith 1933), and perhaps to hemipenial stability (Wiedersheim 1876). The bones are usually associated with external cloacal spurs, which may act in the physical enlargement of the aperture of the female's vent prior to intromission (Noble and Bradley 1933, Greenberg 1943, Russell 1977).

Among the taxa examined, the shape and size of the medial cloacal bones varies greatly, even intraspecifically. In all cases, the bone is either C-shaped or J-shaped (see Figs. 4B, 7B, 8). The curvature may be abrupt, as in *Nephrurus,* or more gently bowed, as in *Hoplodactylus* and the majority of the other genera sampled. The "head" or proximal tip of the bone may be enlarged or tapered, or may bear a flange. The shaft of cloacal bones may be rounded in cross-section or compressed. The distal portion may be smooth, but often bears a series of rugosities or denticulations. This is especially true of (but not limited to) taxa that possess lateral cloacal bones as well.

The strongest similarity of the Earnscleugh bone is shown by members of the Tasmantis (New Zealand and New Caledonia) radiation of the tribe Carphodactylini in the subfamily Diplodactylinae. This is the monophyletic group to which all of the living geckos of New Zealand belong (Bauer 1986). The greatest resemblance to the subfossil bone was seen in the species *Hoplodactylus granulatus, Hoplodactylus maculatus* (Fig. 7B), *Naultinus elegans, Naultinus grayii, Naultinus gemmeus* (all from New Zealand), and *Rhacodactylus trachyrhynchus* (from New Caledonia). If this identification is correct, the bone described by Hutton would appear to be a left cloacal bone, with Hutton's illustration (Fig. 4A) representing a dorsal view.

Although there are no diagnostic features to permit the identification of

the Earnscleugh lizard on the basis of its cloacal bone, the size of the lizard can be estimated by comparison of closely related taxa. Table 1 records the SVL and left cloacal bone size (right in the case of *Hoplodactylus delcourti*) in a number of Tasmantis carphodactyline gecko species. Intraspecific measurements vary, especially among juveniles (e.g., *Hoplodactylus duvaucelii*), but clear size trends are seen. The cloacal bones of *H. delcourti* (at 370 mm SVL) are nearly twice as large as those of *Rhacodactylus leachianus* (220–240 mm SVL).

On the basis of the patterns suggested by these measurements, the possessor of a 14 mm cloacal bone would have been approximately 275–325 mm SVL. This is consistent with the size derived from a jaw ramus of 75 mm (a slightly larger size would be implied if the Earnscleugh element were, in fact, an anterior cervical rib). If both bones are indeed from the same animal, they give evidence that a gecko almost as large as the single known specimen of *Hoplodactylus delcourti* was living in Otago until at least the early 1800's.

The Identity of the Earnscleugh Lizard

The large pleurodont jaw reported by Hutton (1875) alone suffices as evidence for the recent occurrence of a lizard larger than any presently known from South Island, New Zealand. If our interpretation of the smaller element from Earnscleugh as a cloacal bone is correct, the animal must have been a gekkonid. The total size of the lizard, as suggested by these elements, would have been slightly less than that of the known specimen of the Maori *kawekaweau,* described by us as *Hoplodactylus delcourti*; but was the Earnscleugh animal conspecific with the *kawekaweau*?

As previously reported (Bauer and Russell 1987), all references to the *kaweau* or *kawekaweau* appear to be derived from North Island, New Zealand, and we have earlier hypothesized that Northland, the region including the Waoku Plateau (Fig. 2), might be the origin of the type of *H. delcourti,* and perhaps the last haunt of the species if still extant. The distance between these areas and Earnscleugh cannot be taken as evidence against the former occurrence of *H. delcourti* in Otago. Indeed, until early European times, both tuataras (Crook 1975, Worthy 1984) and lizard species now restricted to offshore islands (Trevor Worthy, personal communication) were distributed widely across both North and South Island mainlands. The absence of local folklore or reports of the reptile might simply be the outcome of the much less dense Maori settlement of the region and late arrival of European explorers and pioneers.

Hutton (1899) believed that the remains from Earnscleugh Cave might be referrable to the *kumi* or the *ngarara.* Unfortunately, the literature relating to these mythical creatures is vague and confused. Williams (1975) defines the terms respectively as "a huge, fabulous, reptile" and "reptile, monster,"

Table 1.—Cloacal bone size (measured along curve) relative to snout-vent length (SVL) in New Zealand and New Caledonian carphodactyline geckos (as measured from radiographs).

Taxon	SVL (mm)	Cloacal bone (mm)
Hoplodactylus delcourti	370	19.8
H. duvaucelii	74	4.0
	115	7.9
	117	5.2
H. granulatus	72	4.3
H. maculatus	54	2.5
H. rakiurae	58	5.0
H. stephensi	77	5.9
Naultinus elegans	53	4.3
N. gemmeus	67	4.0
N. grayii	90	6.3
Bavayia sauvagii	63	2.7
Eurydactylodes vieillardi	51	2.9
	56	4.0
Rhacodactylus auriculatus	121	6.3
R. chahoua	136	6.5
R. ciliatus	117	7.6
R. leachianus	220	11.5
	240	10.4
R. sarasinorum	125	8.5
R. trachyrhynchus	170	9.7
	175	9.2
Earnscleugh Cave Lizard	275–325 (estimated)	14.0

hardly definitive in a biological sense. The *kumi* was described by Hector (1899) as 5–6 feet in length and he considered it to be related to Australian "iguanas" (= goannas or monitor lizards, family Varanidae). This large size, combined with the improbable attribute of six-leggedness (Hector 1899) makes identification with any known (or even suspected) New Zealand vertebrates highly unlikely.

The *ngarara,* or *unu ngarara* was referred to by Stack (1875) as an animal inhabiting the region of Eyreton (vic. Christchurch), with burrowing habits, a serrated dorsal crest, and large teeth which cause the upper lip to project. Although distinguished from the "ruatara" [= tuatara] by its darker color, the description of the *unu ngarara* is clearly that of *Sphenodon punctatus,* down to the details of the characteristic enlarged premaxillary teeth. A second type of *ngarara* described by Stack (1875) was about 45 cm long, and was said to inhabit streams. This, again, might be a reference to the tuatara, or perhaps to a large scincid lizard such as *Leiolopisma otagense* (although neither this species nor other likely candidates presently inhabit the North Canterbury region).

In combination form, *ngarara-papa* has been suggested as a term for the small brown geckos of New Zealand, *Hoplodactylus maculatus* and *H. pacificus* (Best 1909). Aside from these specific creatures implied by *ngarara,* the term was widely used as a general name for all lizards by many Maori groups (Best 1909, 1923), and in other contexts more closely approximated in its attributes the mythical *taniwha* (Downes 1937, Skinner 1964).

A review of other Maori names associated with lepidosaurian reptiles suggests no obvious alternatives for the Earnscleugh gecko. There may be some association with the *waitoreke,* or New Zealand "otter" (Watson 1960), as these reports derive chiefly from southern South Island, but this is purely speculation. We therefore conclude that either the animal was unknown to local residents or that no record of such knowledge has come to light. On the basis of the lower jaw ramus and cloacal bone, we are confident in our identification of the central Otago giant lizard as a gecko, and tentatively accept that the material represents *Hoplodactylus delcourti,* formerly believed to have been restricted in distribution to North Island, New Zealand.

We do not discount the possibility, however, that the remains may prove to belong to yet another probably extinct giant gecko of the genus *Hoplodactylus* (or less likely, *Naultinus*). Regardless, the Earnscleugh gecko material, with *H. delcourti* and other recent finds of larger lizard remains (Gill 1985), adds to our knowledge of a recent lepidosaurian reptile fauna characterized by incidences of gigantism which persisted until historical times and may still be represented today by *Hoplodactylus delcourti.*

Acknowledgments

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THE "MONSTER" EPISODE IN ADOMNAN'S *LIFE* OF ST. COLUMBA

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ABSTRACT: The story of St. Columba's supposed encounter with an aquatic beast, contained in Adomnan's *Life* of the saint, is critically examined and placed in historical context. Its cryptozoological relevance should be discounted. An actual event of ca. 580 A.D., reported from oral tradition 110 years later, can be postulated, but it would have been no more than the disturbance of some large, stray marine mammal in the River Ness, apparently at a ferry crossing near the Moray Firth river-mouth.

INTRODUCTION

Statements by almost every writer on the Loch Ness Monster suggest that the late 6th century sighting associated with St. Columba has now become almost an article of faith. No cryptozoologist has discussed this topic fully and critically, and only a few (Whyte 1957, Binns 1983) have bothered to look at (secondary) documentation instead of simply repeating assertions already published by others.

It is rare to find an author (though cf. Witchell 1975) who realizes that this is not the only allusion to unidentified aquatic creatures in early Irish and British secular or hagiographical literature. It would be unfair to expect zoologists to be familiar with what is admittedly a specialized area of historical and linguistic scholarship. This paper, from an amateur cryptozoologist who does happen to work professionally in the appropriate field of insular Celtic scholarship, hopes to offer a corrective view.

SUBJECT AND AUTHOR

In the mid-first millennium A.D., Scotland north of the Clyde-Forth (Glasgow to Edinburgh) narrowing was inhabited by peoples of Iron Age background identified in Latin sources as *Picti,* the Picts. Starting in the later 5th century, an area of western Scotland north of the Clyde was colonized from parts of Ireland, the settlers being known as *Scoti* or *Scotti*—whence, ultimately, the name "Scotland"—and their new realm as *Dalriada.* Initially pagan, they were converted to Christianity during the period 475 to 600 A.D. An Irish aristocrat from Donegal whom we know as Columba in Latin and Columcille in Irish (names meaning "dove" and "dove of the Church") was born about 521 A.D. He founded a major monastery at Derry in 545, and in 563 sailed with some companions to Dalriada. His monastic foundation at *Hy,* Iona, an islet west of the larger Isle of Mull, by the Argyll

coast, was not the only such church center provided for the *Scotti,* but it rapidly became the most influential and best known. Columba was its abbot, or absolute spiritual and legal ruler, until his death in 597.

During his long sojourn, he made visits to identifiable localities in the Isles and in western and northern Scotland, including at least one trip up the Great Glen to Inverness. Here he encountered the citadel of a Pictish king, Brude, probably sited in the hill-fort of Craig Phadraig, just west of Inverness. On one such journey, either arriving or returning, Columba visited a Pict named Emchat, whose home was *Airchartdan*; this is now equated with a living-site stratified below the present Urquhart Castle ruin overlooking Loch Ness, where excavations have dated levels by the C^{14} method of determination to the late 6th and late 8th centuries (Foster, Driscoll, and Alcock 1985). Craig Phadraig, also excavated, has produced finds of the same general phase (Small and Cottam 1972).

Just as the localities involved confirm, upon archaeological examination, activity at the dates that history would assign to them, so we can be certain that Columba was a historical person. The date-framework of his career, and of persons said to have been associated with him, can be established independently from non-Columban sources. (For a discussion of sources, see Anderson and Anderson 1961.) Adomnan (note the correct spelling) was another Irish cleric from the same extended family as Columba's, four generations on; he was born about 628 A.D. In the decade 660–70, he moved to Iona to become the community's ninth abbot. He returned to Ireland in 692, but later re-visited Iona, where he died in 704. The *Life of Columba,* his principal surviving writing, was composed between 688 and 692.

Adomnan, like Columba and most of the Iona brethren, spoke archaic Old Irish, an extremely complex Celtic language that in his own time was not written down and had no fixed spelling. He wrote in Latin, the conventional medium for the Church. As far as we can tell, the Picts had (or, in earlier centuries, had had) two languages, the main one being an archaic dialect of British, the ancestor of Welsh and Cornish. Between the Dalriadic Scots and the Picts, interpreters were necessary. The oral traditions on which Adomnan drew were, as the *Life* shows, mainly Iona-based—anecdotes of the founder's doings at two, or three, removes—but one should not rule out the possibility that traditional or factual accounts of events on the Scottish mainland, in the lands of the Picts, reached Iona and Adomnan from other sources, including Pictish ones.

THE *LIFE*

Adomnan's composition was not, and was not meant to be, a factual chronological narrative on the life of St. Columba. He was himself quite capable of "relatively high critical standards, good scriptural scholarship, and painstaking accuracy in the manipulation of his material" (Meehan 1958:

6), and the library at Iona held enough classical and biblical literature to enable him to formulate a respectable Latin prose style. Literary qualities were displayed in his earlier book *De Locis Sanctis* ("About the Holy Places"), a travelogue-guide to the Holy Land taken down from Arculf, a Gaulish bishop who somehow visited Iona. The sole purpose of the *Life* was, however, to confirm and to advertise the sanctity of Columba, offering proof of this under three heads—prophetic revelations, divine miracles effected through the saint, and appearances of angels with manifestations of heavenly brightness. The author assures us that, while omitting items for the sake of brevity, he has included nothing false, doubtful, or unsure. He would have been able to talk to people who, in youth, had known Columba's own friends, and, just possibly, to meet a few old men who had lived before 597.

The "monster" episode occurs in the second book of the *Life*. Forty-six miracles are related, arranged not in chronological order but in a fairly loose thematic grouping. After no. 25, we read that *Nunc de bestiis aliqua narrabimus pauca* ("Now we will relate a few things concerning animals"), and the next three miracles refer to a wild boar and some hunting dogs (26), an *aquatilis bestia* or "water beast" (27), and the absence of vipers on Iona (28). The context of the much-cited extract on the "monster" thus emerges, not as part of any constructed historical account, but as one minor literary trope within a deliberate and overt piece of religious propaganda. It remains to be seen what historicity it could possibly display.

The "Monster" Episode

I give a fresh and completely literal translation of this from the best (Schaffhausen) text of the *Life* (Anderson and Anderson 1961), with only the key words from the Latin italicized in parentheses.

About the repelling, by the power of the prayer of the blessed man (= Columba), *of a certain water beast.*

And again at another time, while the blessed man was tarrying for a number of days in a province of the Picts, he needed to cross the river Ness (*fluvium Nesam*). When he reached its bank, he saw some other inhabitants burying a wretched fellow whom, the buriers themselves said, a water beast (*aquatilis bestia*) a short time before had bitten with a most savage biting while it seized him. Certain men going to his aid in a little wooden boat, albeit too late, had caught hold of the unfortunate corpse with outstretched boathooks.

On hearing this, the blessed man nevertheless ordered that one of his own companions should, by swimming across, fetch a boat moored on the far bank by sailing it back. Listening to this command of the holy and outstanding man, Lugneus mocu-Min—obeying him without delay, and having taken off all his clothes except his tunic—threw himself into the water.

But the beast (*belua*), not so much satiated by what had gone before as whetted for prey, was lurking at the bottom of the river (*fluminis*). Feeling the water above it disturbed by the swimming, and suddenly coming up to the surface, it rushed with a great roaring and with a wide-open mouth at the man swimming in the middle of the stream-bed (*alveus*). On seeing this, the blessed man, together with all who were there—the barbarians as much as the brethren being struck with terror—drew the sign of the saving cross in the empty air with his upraised holy hand. Having invoked the name of God, he commanded the ferocious beast (*bestia*) saying "Go no further! Nor shall you touch the man. Turn back at once." Then indeed the beast, hearing this command of the holy man, fled terrified in pretty swift retreat, as if it were being hauled back by ropes; though just before this it had approached the swimming Lugneus so closely that, between man and beast, there had not been more than the length of a single small boat-pole.

Then the brethren, seeing that the beast had retreated and that their fellow-soldier of God, Lugneus, was returning to them unharmed and safe and sound in the boat, glorified God in the person of the blessed man with great wonderment. And the pagan barbarians who were there, too, impelled by the greatness of the miracle that they themselves had witnessed, magnified the God of the Christians.

Discussion

This tale, which we cannot pretend was included solely for the benefit of latter-day cryptozoologists, typifies Adomnan's style and the general character of lives of saints at this period. Its whole structure is entirely familiar. A savage creature—its dangerous nature advertised by the fact that it has previously attacked a human—is thwarted and repelled by the sign of the Cross and the name of the Almighty, the inherent and miraculous sanctity of Columba is again demonstrated, and by way of bonus some terrified pagans are led to acknowledge the greatness of God. The previous miracle in the *Life,* no. 26, is shorter but similar—a wild boar, a potential if not actual killer, is stopped in its tracks by Columba's uplifted hand and spoken invocation, and then falls dead at his command. Other Lives of the early British and Irish saints provide numerous instances, much on the same model.

These comments do not affect Adomnan's sincerity, nor his standards as an author. Insofar as this must represent a hard core of tradition gathered at Iona, certain details are clear. Lugneus, or Lugne, was a monk of Iona, a historical person and a younger associate of Columba, and Adomnan mentions him again as becoming prior of some other monastery in later life. If Lugne experienced an incident during his travels with the saint, he will have passed this on to Adomnan through (probably) not more than one intermediary.

The locality is, however, *not* Loch Ness. We must accept that, whether or not Adomnan ever visited Pictland himself, at Iona he would certainly have had access to the detailed geography of the whole Great Glen. Elsewhere he distinguishes Loch Ness as *lacus Nisae fluminis longus* ("the long lake of the river Ness"), and he knew that *Airchartdan* (Urquhart) was alongside *Nisae fluminis lacus* ("the lake of the river Ness"). By contrast, Lugne's adventure occurred on the River Ness proper. *Fluvius, flumen,* and *alveus* all mean "river," and the last word is a classical Latin term that implies a narrower stream between its banks.

The river in the story was apparently narrow enough for anyone to see a boat moored on the opposite bank, and to contemplate a quick swim across. This was at some spot where the local Picts normally congregated; where they had at least one other boat, *alnus,* literally a small craft made out of alder (perhaps a fisherman's little wicker-and-hide coracle); and where there were boat-hooks, if not ropes. In fact, the place was obviously some well-known traditional river crossing, where local fishermen used the narrows, and a punt or raft was kept for use as a ferry. The circumstances of Columba's visit tell us that he was on one or other of his trips to Brude's citadel. Craig Phadraig lies some two miles west of Inverness, and about two miles north of the River Ness. The spot in question is thus more likely to have been near the mouth of the River Ness—where it flows into the Moray Firth and the North Sea—rather than by its outflow at Lochend from the loch itself.

The monster is described throughout as *bestia,* and only once as *belua*—another classical word implying a creature marked by size, ferocity, or strangeness, and one used by several Roman writers as an appropriate way to describe an elephant. These words possess, as such, no particular reptilian, mammalian, or even marine significance. Adomnan is obliged to add that the beast was *aquatilis,* "aquatic" or "marine." This beast could swim, could dive or ascend and surface, move quickly, and respond to noise; it could make noises, bite, and, if too closely approached, it could attack or threaten a swimming person.

We hardly need recourse to cryptozoology to suspect that Adomnan was including a record of something that actually happened—if this is relevant, probably around 580 A.D.—and that Lugne had good cause to remember the occasion. The Iona monks were familiar with seals (*vituli*), and elsewhere in the *Life* there is reference to customary seal hunting, presumably for *Phoca vitulina,* at Staffa or the Treshnish Isles.

The *bestia* was probably a larger animal of the same general configuration, temporarily astray in a ferry-narrows near the Moray Firth mouth of the river. An isolated bearded seal, *Erignathus barbatus,* or even a walrus, *Odobenus rosmarus,* may be indicated. The huge destruction of walruses in the 19th century obscures the realization that, in earlier centuries, occasional individuals may be been carried far south (Ritchie 1920: 171), and that walrus remains are known from excavated sites in Orkney and Shetland (Clark 1952: 84). Whether or not one such specimen had actually caused the death of the hapless Pict, this postulated intruder probably threatened Lugne when he swam too close to it, and then vanished underwater when the saint shouted and gesticulated.

Other Accounts

The importance of Adomnan's *Life* lies partly in the very wide and influential circulation it enjoyed from the moment of its composition, and partly

because of its relatively early date (most similar "Lives" are later productions). Behind it lies, or lay, a now-lost compilation issued before 669 A.D. by the seventh abbot of Iona, Cuimine Ailbe. Adomnan drew on Cuimine's work for his third book, though this is less certain where the second book (the miracles) is concerned.

Another "Life" in Irish (as opposed to Latin), cast in the shape of a homily for Columba's feastday, may have been put together in the 10th or even later 9th century (Kenney 1929: 434). It was an Irish presentation of material to which Adomnan had given his earlier, Iona-centered, slant. Here the "monster" tale occurs in a much truncated guise. Columba is on a preaching tour through Scotland. One day, when he is addressing a host, a certain person tries to cross a river; before he can hear the word of God, he is bitten and killed while still in the water. The boy is brought before the saint, who makes the sign of the Cross with his staff over his breast, whereupon the seemingly dead lad arises.

The relevant sentence, in Irish, is: *Nos-benann in naithir he isin nusqui ocus nos-marbhunn focetoir.* A literal translation would be: "The *naithir* does bite him in the water and he does die at once." Here, the word *naithir,* in Old Irish more usually *nathir, nathair,* is the standard term for "snake, venomous serpent," and is cognate with Latin *natrix,* and for that matter with English "adder" (*an adder* was earlier *a nadder*).

By the 10th century, there were many "Lives" of saints in circulation in which snakes, or serpents, or dragons—terrestrial or aquatic, with or without wings, silent or bellowing—figured as stock properties in every variety of resuscitation or repulsion miracle (Cross 1954). No zoological nor cryptozoological weight can be attached to any of them. Their currency and popularity were enough to cause the insertion of a *naithir* into this late and muddled version of Lugne's swim for the boat.

Finally, there is a second, but also much later, Latin "Life" of Columba, contained in the 11th century collection known as the *Codex Salmanticensis* (Heist 1965). It is of some interest because its first nineteen chapters have been modified from a much older source; either Adomnan's *Life* (at one remove), or an early form of the existing Old Irish *Life.* The "monster" miracle occurs in chapter 8. Columba and his monks are in Pictland; they want to cross the river, but find that the necessary boat is tied to the far bank. A monk is told to swim over and get it. The creature duly appears. It can swim rapidly, has a fearsome appearance, opens its gaping maw and makes towards the monk, but it too is immediately dispelled by the sign of the Cross and the Almighty's name. Again, it is simply *atrocissima bestia,* "a very horrible beast."

The compiler, who omits any mention of native Picts standing around, very clearly pictures both a river and a traditional ferry crossing. The boat is neither *ratis,* "a raft," or *cimba,* an obscure word that ought to mean

"bell-like in shape" (early Irish bells in bronze or iron were trapezoidal in outline and rectangular in section).

Conclusion

There is no reason to doubt that, around 580 A.D., St. Columba and a party of monks including Lugne mocu-Min were on their way to visit Brude, travelled up the southern shore of Loch Ness and then intended to cross the River Ness by a ferry somewhere near its outflow into the North Sea; nor that Lugne, swimming over to fetch the raft or punt, probably encountered an alarmed and aggressive marine mammal of a species that the Iona monks did not recognize, doubtless a stray more at home in the Arctic or the waters of Spitzbergen. What this tale—included by Adomnan in perfectly good faith but with a strong religious slant—does *not* involve is any part of Loch Ness, or any reference to the unidentified animal or animals currently being investigated in that loch.

It would probably be too much to hope that future writers on the topic of the Loch Ness Monster will abandon this reference as irrelevant and misleading, but at least the readers of this journal will be in possession of an accurate critique of this interesting 7th century story.

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WAITOREKE, THE NEW ZEALAND "OTTER": A LINGUISTIC SOLUTION TO A CRYPTOZOOLOGICAL PROBLEM

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ABSTRACT: A variety of Maori words have come to be associated with the putative New Zealand "otter," often termed *waitoreke, kaureke,* or *kaurehe.* The present study examines all the terms that have been associated with this supposed animal, particularly those in Becker (1985) and Watson (1960). Where possible, two types of data are examined for each term. First, an etymology is given based upon the phonological (sound) form together with the morphological (semantic sense) structure, these being determined as accurately as possible given the strictures of Maori grammar. Second, historical citations are documented in order to assess the status of the word: whether or not it is attested; if attested, whether or not it is old or relatively new. Two words survive this analysis as being Maori forms that have a high probability of having been used to denote a native mammal of amphibious habits: *waitoreke* and *kaureke.*

Both names prove to be of rare attestation, but in these cases such poor attestation reflects the antiquity of the word. These names both involve "quill, spur, knob" in their meanings, *kaureke* possibly meaning "very spiney." This unexpected semantic parallel between these names suggests that these survive from a period when direct knowledge of this animal was widespread among the Maori. The well attested name, *kaurehe,* however, reveals very little about the animal to which it refers. This suggests that *kaurehe* is a newer word based upon a diminished acquaintance with the animal itself. Further, the meaning of *kaurehe* is such that it might also be applied to the tuatara. *Kaurehe,* therefore, is rejected as a later distortion of *kaureke.*

There is an excellent possibility that a rare mammal with a foot spur, of restricted habitat and distribution, survived in New Zealand until recent times—if, indeed, not up to the present time. A spur would be diagnostic of a member of the order Monotremata, and an amphibious habit would suggest a monotreme of the genus *Ornithorhynchus,* a New Zealand variant of the Australian platypus. A spiney pelage, however, suggests a member of the genera *Tachyglossus* or *Zaglossus,* though an even more primitive basal monotreme or advanced therapsid (mammal-like reptile) cannot be excluded from consideration.

INTRODUCTION

New Zealand is a land singularly bereft of native terrestrial mammals, only two bats being indigenous. This paucity has been explained as due to an early separation of New Zealand's land mass from the rest of the continents (Stevens 1980), so that later groups of mammals were not able to reach it by migration. Any indigenous quadrupedal mammal would therefore be highly likely to be extremely archaic. Thus, reports extending back at least to the first half of the 19th century (Watson 1960) of a native quadrupedal mammal of amphibious habits have always been of interest to naturalists,

particularly cryptozoologists. Such reports offer a chance, however slim, of finding another mammal at the monotreme level of organization, or perhaps even an advanced therapsid (mammal-like reptile) (see Romer 1966: 173–86, for an account of the subclass Synapsida, the mammal-like reptiles).

Recently, the putative New Zealand "otter" has received renewed attention (Becker 1985, Heuvelmans 1986: 22, 1987: 122–23, Janis 1987: 117). Heuvelmans endorses the old notion that a relict monotreme lies behind the sketchy accounts of this creature (see Watson 1960, for quotes taken from some of the older accounts), while Janis tends to favor either the water rat, *Hydromys chyrogaster,* or some other rodent, most likely brought in with the Maoris. Becker, on the other hand, has assigned the beast to the realm of Maori mythology on the basis of a linguistic scrutiny of the names that have been associated in one way or another with reports of this creature.

Becker's chief point is that *waitoreke* and associated terms have as much to do with the realm of mythology as with that of "water" (*wai-* in Maori), and that, therefore, the words under examination lend little probative force to any arguments favoring the existence of a quadrupedal mammal indigenous to New Zealand. Becker's linguistic approach is a very useful one, insofar as, in this cryptozoological case, the linguistic data associated with this putative animal are richer than the sketchy accounts of its appearance or habits. In most cases, however, I have found other interpretations for the terms involved, and as a result have reached very different conclusions.

I have reassessed all the forms associated with this putative mammal. Only *waitoreke* and *kaureke* survive as likely forms, both with several possible meanings. *Waitoreke* resembles two mythological names that appear to belong to a giant lizard, but phonological details set it apart from these. *Kaureke* does not exhibit mythological parallels, and is not an error for *karara ke,* as has been suggested by Duff (1956: 289, fn. 1).

Also, I have examined the attestation of each term. In other words, not all forms cited for a language have the same significance. Some words may appear to be Maori, but may prove upon closer scrutiny to be errors, never having appeared in the language. Other words may be part of Maori, but be of greater antiquity than others, and as a result convey information from an earlier cultural stage.

METHODOLOGY

First, I have viewed *waitoreke* and its constellation of kindred names as primarily a linguistic problem. The forms put forth by Becker vary in their linguistic status: some are attested and some are not; some are plausible and others not, some are clearly related to one another, while others are not. Further, some confusion regarding dialect similarities and links is evident in Becker's paper, and these can now be untangled, discarding certain forms in the process as being irrelevant or nonexistent. To some extent, the lin-

guistic status of the terms is a function of whether or not they have plausible etymologies; but only in part. I have therefore treated the question of meaning as logically a separate issue, albeit in the act of linguistic analysis itself the meaning of a form is inextricably intertwined with considerations of linguistic structure, "morphology."

In short, I have set about to determine which forms are actually words in Maori, and what these may mean. I shall close by discussing Maori folklore, revising the history of the attestation of this animal, and presenting my conclusions. I might add that the Maori material is often very frustrating to work with, not least because Maori roots exhibit extreme polysemy (many meanings). Becker can hardly be faulted in his efforts to analyze these terms.

I have treated the Maori terms one by one. To arrive at my conclusions, I have set about analyzing the relevant forms in a two-fold fashion: (1) morphology, and (2) plausible meanings. Citations of the forms that are typical in the literature (Becker 1985, Watson 1960) I have placed in italics. My efforts at morphological analysis I have placed in standard "phonemic" notation (representing psychologically distinctive sounds) between slashes, thus "/. . . /." For glosses (rough English equivalents) to the forms, I have used Williams (1971). Forms that are unattested or hypothetical are often marked by an asterisk, thus "*." A form has been glossed morpheme (minimal unit of meaning) by morpheme with each gloss in brackets and set off within single quotes.

Where ambiguity is involved, I have set distinct alternates within the same brackets and separated them by "or." For example: /wai-rua/ ['one' or 'water']-['bone pin' or 'knob, butt']. Thus, both /wai-/ and /-rua/ have been given two glosses each. In many cases, I have ignored glosses that were obviously irrelevant to any conceivable animal name. For grammar, I have used a book by the same Williams (1940), and one by Biggs (1969).

Finally, I have tried to make the study accessible to the non-linguist while at the same time maintaining scientific standards of linguistic rigor.

Criticisms of Becker

I shall begin by demonstrating that Becker's links between Maori /wai-/ and the realm of spirits and mythology are unfounded.

First, Maori /wai-/ has no connection with any Proto-Austronesian word for "spirit." Proto-Austronesian */qanitu/ ['demon'] (Becker 1985: 32) (which in more familiar transcription would be */ʔanitu/, with /ʔ/ a glottal stop, as occurs at the onset of each syllable in the casual English negative "uh-uh"), yields /(w)aitu/ in Maori, and this can only mean "demon," not "ghost," as Becker has it. It is in no way connected with /wai/ or /wairua/, despite the fact that the Maori /r/, which is flapped, is phonetically identical to the flapped intervocalic /t/ or /d/ of some English dialects, and might therefore suggest some link with the /t/ in /(w)aitu/. /Wairua/ is Maori for "spirit,"

and does not appear to be analyzable into smaller morphemes, despite the resemblance of its first syllable to /wai/ ['water'].

Were one to try, one would obtain /wai-rua/ ['water' or 'one (indefinite pronoun)']-['two' or 'pit, hole' or 'store for provisions' or 'grave' or 'a fish' or 'left weaving peg of a loom frame' or 'a special house post' or 'a net'], of which the best reading is "water-fish." "Water-fish," though redundant, is nevertheless coherent. It is quite unrelated, however, to the sense of "spirit."

Second, *waituhi* ['birthrites'], the word for which Becker makes the strongest case of a mythological or spiritual connection, appears to contain the word for "water," from /wai-tuhi/ ['water']-['invoke'], and may well reflect a ritual in which water and cleansing are essential. However, other words put forth by him as containing a morpheme /wai-/ need not have the /wai/ of "water." For example, *whaka-wai(a)* "to entice, beguile, tempt" (with /hʷ/ a rounded *h* as in English "what") (Becker 1985: 32), is from /hʷaka-wai-a/ ['causative']-['entice']-['passive'], "to cause someone to be beguiled, entice," and has /wai-/ the verb, meaning "to entice, beguile." /Wai/ the verb is based upon the noun, /wai/, ['memory for words'], which itself has a kinetic nuance of "ability to follow the gist of a talk."

This notion of leading someone along carries over into the causative-passive /hʷaka-wai-a/. No word for "water" is involved. In a language where words can be homonyms in multi-fold ways, one must take great care to insure that two or more words are not confused merely because they have the same phonological "shapes."

I emphasize that these distinctive /wai-/'s and /wairua/ are linguistic facts, quite independent of certain mythic tendencies to associate water or rivers with the spirit world and death, as some of the etymologies in Becker (1985: 32) do. Any mythical link of *waitoreke* and related terms need not rest with the /wai-/ portion, but is more likely to rest in the following parts of the words. Becker has failed to establish an irrefutable linguistic link between /wai-/ and words containing it, on the one hand, and the realm of mythology, including /waitoreke/ and its variants, on the other.

As to general linguistic patterns for words beginning in /wai-/, I have shown that only one etymology involving "water" is possible, and that the most plausible gloss for this morpheme is "(the) one (who)," based upon the indefinite pronoun /wai/. In modern Maori, /wai/ is used primarily as a separate word, and the grammatical sources do not indicate that it can appear bound within a larger word. One form, however, /wai-raŋi/ ['(the) one'-'(of) heaven'], (discussed in the text), does exhibit /wai-/ in this function. Thus, all the etymologies involving /wai-/ that I have put forth are presumed to be based upon an archaic pattern no longer productive (capable of creating new forms) in Maori, namely /wai-X/ ['(the) one (who is) (of)]-['X'] and clearly preserved only in one other name, which has a largely mythological provenance.

Such archaism is not unusual in names, animal or otherwise, and is probably why one native speaker, Sir Peter Buck, found *waitoreke* "quite ungrammatical" (Krumbiegel 1950). (As an example of non-productive linguistic features being preserved in names, note the English surname "Eaton" from /ea-/ ['water' or 'river'], a word no longer used, thus /ea-ton/ ['water' or 'river']-['town'].) Further, this archaism does not violate current Maori word order restrictions, and is thus not a fanciful departure from Maori grammar. I shall begin with *waitoreke* itself.

The Forms; Their Etymologies and Status

Waitoreke: This is the usual citation. The form cannot be found in the dictionary by Williams (1971). In fact, its attestation is relatively poor. It appears first in 1848 in an early work by R. Taylor, where it is glossed as "otter (uncertain, perhaps 'seal')" (Watson 1960: 180). Its only other attestation is in a work on the southern Maoris (Beattie 1949). Watson (1960: 181) discusses this later citation, and suggests that Beattie was perhaps following an early newspaper account. The evidence suggests that the term is a genuine Maori one, but an old one that was virtually dead by the mid-19th century.

Further, as this term stands it is ambiguous within Maori sound patterns. Maori makes phonemic ("distinctive") contrasts based upon vowel length, which I shall denote by a macron over the vowel. Thus, /o/ is contrasted with long/ō/, as in /poua/ ['grandfather' or 'rain squall'] vs. /pōua/ [a species of bivalve mollusc (*Longimarcata elongata*)], and similarly for the other vowels. I shall consider both /waitōreke/ and /waitoreke/.

/Wai-tō-reke/ = ['water' or 'one']-['dive' or 'set (of the sun)']-['bone pin' or 'knob, butt'], taking only the likely glosses for the morphemes. The form is a compound noun, "water-dive(r)" (Maori can have compound nouns of the form noun-verb), modified by an adjective, "bone pin" or "knob, butt," intended to denote a quill or spur (adjectives follow their nouns in Maori). Thus, one can extract "water-diver (with a) quill or knob," perhaps with a poisonous spur like that of the male platypus.

As evidence against this interpretation, it must be pointed out that Maori can have recently-constructed nouns that preserve sentential word order within the confines of word morpheme order: verb-noun (see Williams 1940: 42, §61). Thus, the verb /-tō-/ ['dive'] could be expected to precede /wai-/, giving an unattested */tō-wai-reke/. This newer pattern of compounding serves to suggest that /wai-tō-reke/ is an old compound name, and serves to explain why, as I have already mentioned, Sir Peter Buck, himself a native Maori, may have found /wai-tō-reke/ ungrammatical.

This etymology immediately explains why this name might be applied to a monotreme of amphibious habits. This monotreme interpretation receives

further support not only from the next etymology, but also from an entirely separate name, /kau-reke/, to be discussed below.

The morpheme /-tō-/ can have a source from /-te-o-/ ['the (singular)']-['of (partitive)'], "(a part) of the. . . ." Thus /wai-te-o-reke/ is a possible source of /waitōreke/, giving a likely sense "the one of the quill, knob." Such a name might describe an animal with a prominent spine or knob somewhere on its anatomy, quite apart from any amphibious habit. A spiny or knobby creature with a multitude of such protuberances, however, appears to be ruled out as an interpretation of this morphemic analysis, despite the temptation to envision a monotreme with quills like the echidna (but see below under *kaureke*). The possessive phrase /-te-o-reke/ is clearly a singular. The plural would have been /-ŋa-o-reke/, with /ŋ/ (called "angma") a velar nasal, as the *ng* in English *sing*. Thus, this reading of the form also suggests a possible monotreme, but one with only a spur and not a set of quills.

/Wai-toreke/ = ['water' or 'one']-['left behind, forsaken']: Here "the one forsaken" is the only plausible reading, and this makes little sense unless one has in mind something like a forlorn ghost. I know of no mythological material that would support this reading.

/Wai-tō-rehe/ = /wai-te-o-rehe/ ['water' or 'one']-['the']-['of']-['wrinkled' or 'tattooed above the eyes' or 'wizened, puny']: Again, this form is not in the dictionary. Here the best reading is "the wrinkled one, the one with folds in his skin," a term that one might imagine applying to a creature like the tuatara (*Sphenodon punctatus*) with its heavy lateral skin folds. One might still maintain, however, that this reading applied to a monotreme with some prominent wrinkles in its hide, much as with the spiny echidna, which has deep folds of skin around the joints of its stubby limbs.

While *waitoreke* is absent from the Williams (1971) dictionary, it at least is attested in the work by Taylor (1848). *Waitorehe,* however, is introduced by Becker (1985: 33) solely on the basis of phonological similarity to *waitoreke,* and is unattested elsewhere. /K/ and /h/ are distinct phonemes in Maori, and any confusion between them must rest entirely upon misreadings of handwriting or typographical errors. Thus, */waitōrehe/ should be rejected as nonexistent.

The morpheme /-rehe/ ['wrinkled' or 'tattooed above the eyes' or 'wizened, puny'] will appear again in /kau-rehe/ (see below).

/Wai-toreŋi/ = ['water' or 'one']-['disappear' or 'set (as the sun)']: Once again, this form is not in the dictionary. Here, two readings are possible: (1) "the one who disappears," (2) "water-disappear(er)." Reading (2) exhibits the same word order archaism as was noted for /wai-tō-reke/ ['water-diver with a quill or knob']. Reading (1) might as well be construed as referring to a ghost as to an amphibious creature. This form is introduced because Becker (1985: 32) has taken note of a sound correspondence between North-

ern Maori (NM) /ŋ/ and Southern Maori (SM) /k/. This correspondence is due to a shift in SM from an earlier */ŋ/ to an attested /k/ (with "*" denoting here an earlier, reconstructed sound). Thus, /wai-toreŋi/ would be NM, and Becker cites Costello (1974), who cites Pollack, who takes *waitoreke* to be its SM equivalent.

This equivalence is based, however, upon two errors: (1) one expects an unattested SM */waitoreki/ with a final /i/ as the equivalent to NM /wai-toreŋi/, and (2) all SM /k/'s need not represent earlier */ŋ/'s, and so need not correspond to NM /ŋ/'s. SM /k/ can correspond to NM /k/ in precisely those words where original */k/ is involved. For these two reasons, *waitoreke* has nothing to do with a hypothetical */wai-toreŋi/. Strictly speaking, even a hypothetical */waitoreki/ need not have anything to do with */wai-toreŋi/ either, unless strong semantic parallels force an equivalence upon the investigator, so that he can make the same analysis of the term as */wai-toreki/. */Waitoreŋi/, therefore, should be rejected as a nonexistent form.

Waitoteke = /wai-tō-teke/ from /wai-te-o-teke/ ['water' or 'one']-['the']-['of']-['female genitals']: This form appears to be an error committed by Wallace (1880) in quoting Hochstetter (1867). Watson (1960: 181) suggests that this is the form of the name used by the tribe, the Ngatimamoe, among whom knowledge of the creature may have originated, since Beattie (1954) put this form in the mouth of one Tuture te Kene, a survivor of this decimated group.

If this attribution is correct, then we would seem to have here one of the oldest forms of the name. Unfortunately, this form seems to mean literally, "water from the female genitals," i.e., "vaginal secretions" or, less likely, "menstrual flow," suggesting that it is, in fact, an error for /wai-tō-reke/. */Wai-tō-teke/, therefore, seems to be spurious. At best, I would tentatively take it to be another attestation of /wai-tō-reke/.

I introduce the next term, /wairaŋi/ solely because it exhibits /wai-/ in its sense of "(the) one (of)," and thus lends support to my previous etymologies. It was for this reason that I first introduced the term to Becker (1985: 33) in my capacity as one of the referees of his paper. The term has no attestation in connection with a putative New Zealand "otter," but, rather, is restricted to a mythological provenance.

/Wai-raŋi/ = ['water' or 'one']-['sky' or 'heaven' or 'head" or 'chief']: Becker (1985: 33) assumes also the unattested SM */wai-raki/. Here /raŋi/ has nine separate meanings. I have given only the appropriate alternates. The best reading would seem to be "celestial one," though "sky water" (that is to say, "rain") is plausible, but merely irrelevant as an animal name. This term as a whole is glossed in the dictionary with three meanings: (1) "beside oneself, excited, infatuated," (2) "foolish," and (3) "a monster." Meaning (3) is the primary sense. The others arise from the monster's conduct in a myth. This monster is a cosmic being with a gigantic stomach. This is evident

from the Maori passage cited in the dictionary by Williams (1971: 476) (I have restored vowel length):

> *Kātahi ka kotia taua ika; kei roto . . . ŋa tāne me ŋa wāhine, kei roto i te puku o taua wairaŋi e pukei (= pūkai) ana.*
>
> Now then, that pile was divided in two; inside, in the stomach of that monster (= /wairaŋi/), the husbands were lying in one heap and the wives in another.

This gargantuan stomach will tie the /wairaŋi/ in with another monster, /ŋārara/ ['reptile' or 'insect'], which will be discussed next. That /wai-raŋi/ would have a celestial connotation, therefore, is plausible in terms of the word's components. That it means "foolish" or "infatuated" arises from another myth, which shall be discussed shortly.

Ngarara is mentioned twice by Becker (1985: 30, 34). The form is actually /ŋārara/ ['insect' or 'reptile'], and is unanalyzable. The word occurs in the name for the brown gecko, *Hoplodactylus pacificus*), /ŋārara pāpā/, also known as /moko-pāpā/ ['lizard']-['gecko'], as /tere-tere/ ['fast']-['fast'] = "very fast," or simply as /pā/ ['gecko']. Becker (1985: 34) suggests that a newly uncovered giant gecko (Bauer and Russell 1986, 1987) will prove to be the /ŋārara/, but Bauer and Russell themselves believe this specimen (370 mm snout-vent length, 622 mm total length) to be the *kawekaweau* (Bauer and Russell 1986: 147, 1987: 86–87). Bauer and Russell (1987: 88) point out that the term /ŋārara/ was first used in 1875 for what was likely to have been a still larger (450 mm snout-vent length) lizard from the Waimakariri River Valley.

For my own purposes, the term /ŋārara/ is important only because it throws light upon the meaning of /wai-raŋi/, and thereby indirectly upon that of /wai-tō-reke/. The relevant meanings emerge in several myths.

The mythological provenance of this term is attested in the following short myth passage (Williams 1971: 229) (I have restored vowel length):

> *I pēnei te ahua o taua ŋārara nei me te kurī e mau nei i te potete. . . .*
>
> The spectre of that /ŋārara/ treated the dog that it had captured on a leash [actually, "a stick for holding a dog"] thus. . . .

The two myths translated by Margaret Orbell (1968: 38–45) link this term with two other names, and it is these names that are linked to /wai-raŋi/. In both myths, a woman is abducted by a /ŋārara/ when she strays near his lair at the base of a tree. The monster keeps her as his wife until she escapes by a simple ruse, showing the foolishness of the monster in the process. She returns to her people, but they persuade her to return to the monster and invite him to visit his new in-laws. This she does, and the monster is so infatuated with her that he foolishly agrees to visit her family. Upon his arrival, he shows the utmost gullibility, and is eventually slain.

This is the source of the senses of /wai-raŋi/ as "foolish" and "infatuated."

In the first myth, he is said to have a huge belly (Orbell 1968: 39). In the second, he is said to be nearly 60 feet long (Orbell 1968: 42, fn. 1), and to show a tremendous capacity for eating (i.e., he has a huge stomach). Here, too, the huge stomach of the /wai-raŋi/ comes to mind.

The link with /wai-raŋi/ becomes compelling, however, when the two names of the monster are analyzed. In the first myth, he is /te kakau-o-te-raŋi/ ['the'] ['swimmer' or 'wader']-['of']-['the']-['sky' or 'heaven'], with the preposed partitive /o/ ['of'], rather than the suffixed form seen above in /-te-o-/ ['the']-['of']. In the second myth, he is called /te mata-o-te-raŋi/ ['the'] ['a small salt water fish somewhat like a minnow']-['of']-['the']-['sky' or 'heaven']. The allusion to a tiny fish is not merely ironic and insulting (the monster grows angry when he is addressed by this name), but also shows the aquatic character of the monster, as well as the Maori belief that, as scaly creatures, fish and lizards are remotely related (Orbell 1968: 44, fn. 1, Becker 1985: 30).

The modern Maori use of /wai/ is to replace names that the speaker does not wish to bother stipulating (Williams 1971: 474, *Wai*(i), sense 3). Given these names, /wai-raŋi/ can now be seen to be a "short-hand" for both of them, replacing /kakau/ and /mata/.

Thus, the /ŋārara/ and the /wai-raŋi/ are linked mythologically, and can be considered to be one and the same monster. Further, names in which /wai-/ plays the role of an indefinite pronoun, "(the) one (of)," can be considered well established on the basis of these myths and /wai-raŋi/.

Karara ke: This form, while not in the dictionary, is clearly SM /kārara kē/, corresponding to NM /ŋārara kē/ ['ŋārara']-['different' or 'extraordinary' or 'apart'] (Duff 1956: 289, fn. 1). The big belly of the /ŋārara/ might indeed be a reference to the large abdomen of the typical gecko. This term would then mean "different, extraordinary, distinct kind of gecko" and might well denote the tuatara, or, perhaps even more likely, the new-found giant gecko, *Hoplodactylus delcourti.* However, as mentioned above, Bauer and Russell (1986, 1987) take /ŋārara/ (= *karara ke*) as referring to yet another unknown lizard.

/Kārara kē/ enters the literature on the *waitoreke* only because Duff (1956: 289, fn. 1) takes the form *kaureke* (see below) to be a typographical error for *karara ke.* There is nothing wrong with /kau-reke/, and so, like Becker (1985: 34), I too reject Duff's argument on this point. One does not have to explain away *kaureke.*

/Kau-rehe/ = ['very' or 'swim (verb)' or 'ancestor']-['fold in the skin, wrinkle' or 'puny']: I have given only the appropriate senses of /kau/. /Kaurehe/ has three meanings: (1) a monster, (2) the tuatara, *Sphenodon punctatus,* (3) a term of derision. The sense of derision most likely arises from the reading "very puny." The sense referring to the tuatara is most likely that of "wrinkled ancestor" or "ancestor with folds in his skin," the

tuatara having prominent folds in its hide, but "wrinkled swimmer" is also possible, since the tuatara has been reported as having some amphibious tendencies (Newman 1878). This term first appears in Mantell (1851: 105), where it replaces the earlier *kaureke* (Mantell 1850).

If the gloss of *Sphenodon punctatus* (the tuatara) is correct for this term, then there are three other names for this animal: /tua-tara/ ['back']-['spine'], /tua-tete/ ['back']-['curly' or 'spear' or 'scrub, brushwood'], and /tua-keke/ ['back']-['obstinate'(?)]. The first, of course, is the name by which the creature is known to us. The last may be a corruption of /tua-kea/ ['back']-['wedge'] (Aaron Bauer, personal communication). All these names suggest an effort to describe the fleshy protuberances that run down the animal's back. Only the last is a bit odd, but might refer to the slight resilience of this rill of cutaneous protuberances. These names are important because they are straightforward descriptive names and should serve as standards, along with /kaurehe/ itself, by which all the preceding names should be judged.

It is quite possible that /kau-rehe/ denotes the tuatara in the wider folk taxonomy of the Maoris, and that it has nothing to do with a putative native mammal. Nevertheless, the term that it replaced, *kaureke,* also makes sense and suggests that /kaur-rehe/ is an effort to accommodate an older term of restricted provenance to a wider realm of Maori culture. Supporting this contention is the fact that /kau-rehe/ tends to mean "monster" (Watson 1960: 180–81, Beattie 1930) more often than it does "tuatara." Therefore, I would assign /kau-rehe/, even though it has come to be frequently used in accounts of the "otter," to the realm of Maori folklore, and see its association with the tuatara as a late gesture toward European standards of realism in biology.

/Kau-reke/ ['very' or 'swim (verb)' or 'ancestor']-['bone pin' or 'knob, butt']: This could mean "very knobby" or "covered in quills," or, less likely, "quill, knob ancestor" or "quill, knob swimmer." This term is not in the dictionary, and cannot, by Maori sound laws, be related to /kaurehe/. Watson (1960: 180) gives the impression that this term is first attested in 1850 in a work by Mantell (1850). This would make its attestation only two years later than that of /wai-tō-reke/ (1848). In fact, this term is the oldest one we have, being attested in 1838 in a letter to Mantell from his son, Walter (Duff 1956: 288–89). This short report is worth citing:

> G. A. Mantell (1850) records that near Temuka in 1838 his son was told about the "only indigenous terrestrial quadruped, except a species of rat, which there are reasonable grounds for concluding New Zealand ever possessed." The Maoris informed him that about ten miles inland there was a "quadruped which they called Kaureke, and that it was formerly abundant, and often kept by their ancestors in a domestic state as a pet animal." It was described as about two feet long "and covered with coarse gristly hair."

Duff (1956: 289, Watson 1960: 190, fn.) claims that Walter Mantell mistook a Maori term meaning "gristly hair" for "grizzly hair," and concludes

that the Maori were trying to describe to the younger Mantell the cutaneous dorsal protuberances of the tuatara. Duff's suggestion regarding confusions over hair may have merit. The Maori may have been trying to describe the fleshy protuberances on the tuatara's back by using a term for "hair" modified by one for "gristle." Most Polynesian languages have one term referring to any growth that protrudes—even slightly but in some profusion—above a well-defined surface. This term would include, therefore, hair, feather, scales, chitinous parts, as well as shrubs, brushwood, undergrowth, leaves, fern fronds, and grass.

The Maori noun /huru/ can refer to "hair" or "feather" or "brushwood, undergrowth" or "white dogskin mat," and the reduplicated intensive /huru-huru/ ['hair']-['hair'] can mean "coarse hair, bristles," or, again, "feather" (Williams 1971: 72). The intensive can also occur in the names of species of fern, such as /huru-huru hʷenua/ ['hair']-['hair'] ['land' or 'ground' or 'placenta'], *Asplenium lucidum,* or in that of a type of flax, *Phormium tenax,* /huru-huru-hika/ ['hair']-['hair']-['plant'].

I reject Duff's argument, however, because the account clearly states that the animal was covered by this "gristly hair," and that it was also coarse. Given semantic divergences between Maori and English, one could still reasonably expect a coat of hair of whatever sort to be clearly distinguished from a dorsal rill of protuberances characteristic of the tuatara. In fact, this feature of the animal is most interesting, for it suggests that it has habits like the platypus, but a pelage more like that of the echidna. The usual hypothesis regarding the origin of mammalian hair asserts that it evolved from fiber-like sensory organs, the proto-trichia, associated with reptile scales (Smith 1960: 114–17). This sensory function is still preserved in the vibrissae, the whiskers so common on mammalian snouts. The ancestral monotreme might have lacked such vibrissae on its rostrum (Murray 1984: 576–77, Fig. 10), but perhaps it exhibited them elsewhere on its body.

It is possible that a very primitive monotreme, therefore, might be expected to have exactly the sort of coarse gristly hair, in effect a sort of proto-hair, attributed to the /kaureke/. Further, the spines of the echidna might, in the light of my suggestion, be seen as secondary modifications of these vibrissae, rather than as modifications of true hair. The split within the monotremes between a soft fur, as in the platypus, and spines or a mixture of spines and hair, as in the echidnas, may be seen as an early differentiation of such coarse proto-hair as reported for the /kaureke/.

In fact, the two terms, /kau-reke/ and /wai-tō-reke/, support one another in that both refer to a quill, spur, or knob, Maori /reke/. I would suggest that both terms are old, are of restricted provenance, and encode the vital fact regarding this animal that it had both a spur, a feature certainly likely if the beast was (or is) a monotreme, as well as a spiny pelage, a feature I would suggest diagnostic of a very primitive level of mammalian organization. The presence of the spur, if it were poisonous as is the case in monotremes, might account for why one Maori said: "We are afraid of them" (Watson 1960: 177).

Mythology

Both /wai-tō-reke/ and /kau-reke/ lack any significant mythological correlates. Maori folklore is rich in monsters of enormous size and appetite: /wai-raŋi/ (SM /wai-raki/), /ŋārara kē/ (SM /kārara kē/), with the epithets /kakau-o-te-raŋi/ and /mata-o-te-raŋi/, /kau-rehe/, /tuatara/, and the /kawe-kawe-au/. The last means ['tendrils' or 'strands' or 'fringe' or 'tentacles']-[(repeated)]-['string' or 'cord' or 'mat pin' or 'small oblong bead made of whale's tooth'] (Williams 1971: 111). The latter dictionary citation contains a short mythological text associating /kawe-kawe-au/ with the tuatara:

> *A ka mau a Kahuŋunu i te kaweau i roto i te ipu, he hʷakawehi i ŋā iwi tūkino mai: he nui te wehi o te iwi Maori i tena mea i te tuatara.*
>
> When the home of Kahungunu was established, in the time of the kaweau in the lake in the cloud, a safeguard for troubled peoples [was] the many spines of the Maori people, of that one, of the tuatara.

Most of these mythic figures have correlates in the folk taxonomy. The /ŋārara kē/ may be a large, as yet unknown, lizard (Bauer and Russell 1987: 87–88). The terms /wai-raŋi/, /kaka-o-te-raŋi/, and /mata-o-te-raŋi/ may refer to the same lizard. /Kau-rehe/ may refer to the tuatara, and /kawe-kawe-au/ may refer to the recently recognized giant gecko, *Haplodactylus delcourti* (Bauer and Russell 1986, 1987). Both /wai-tō-reke/ and /kau-reke/ are conspicuous both for their absence from mythology and for their very modest size, a feature incompatible with the usual lineaments of mythic beasts in Maori lore.

The Maori criterion for inclusion of an animal into the realm of mythology appears to be the inverse of that of the European. Whereas the European typically has assigned to myth creatures that are rare, poorly known, or utterly fanciful, the Maori appears to have assigned to myth animals that are somewhat familiar, animals whose existence in the physical world requires a corollary existence in the parallel world of myth, and whose incorporation into that world lends to myth a sense of coherence and familiarity.

The utter absence of any mythical correlates for either /wai-tō-reke/ or /kau-reke/ does not directly argue for the reality of the animal referred to, as Watson (1960: 181) erroneously suggests, but, rather, supports the hypothesis that both terms are old and refer to a very rare animal known only to a limited circle of Maori; an animal whose incorporation into mythic lore would not have served to illuminate any particular trait or lend familiarity to a myth, simply because it and its habits were so poorly known.

Earliest Accounts

It is worth mentioning that the earliest accounts, those prior to 1838, do not use any native name to denote this putative mammal. The accounts by Captain Cook's men, the first reported (Cook 1777, Vol. 1: 98), are vague, with one man suggesting that the beast most closely resembled a jackal. As Wall (1926) first suggested (as cited in Watson 1960: 181), this first set of sightings might well have been of the Maori dog. If the animal sighted was a monotreme, then it was highly unlikely to have resembled a jackal or a blend of cat and dog, as reported.

Existing monotremes are extremely archaic (Murray 1984), not merely in details of cranial and cervical structure, but also in the extremely reptilian aspects of their shoulder girdles (Murray 1984: 573–74, Romer 1966: 197–98). Some of the peculiarities of their limbs are due to their specializations for digging, but much of their anatomy is simply extraordinarily archaic. It is highly unlikely, therefore, that any monotreme that ever existed would have given—or would still give—a strong impression of being a blend of cat and dog. The animal reported by Cook's men seems to have had nothing to do with the New Zealand "otter."

Accounts beginning in 1844 (Watson 1960: 176–80) clearly refer to an animal of amphibious habits, with one hint that a more terrestrial species might also have existed (Watson 1960: 177), and with one account (pp. 179–80) of an actual skin having been obtained. The animal appears to have been small, with a thick flat tail, white spots, coarse gristly hair (spine-like hair), and, by one account, to have laid eggs the size of a duck's (Watson 1960: 177, quoting Mantell's letter of 1848). Given the meanings of /wai-tō-reke/ and /kau-reke/ posited in the preceding analysis, an animal of aquatic or amphibious characteristics appears most likely. Therefore, I would maintain that the accounts presented in Cook's writings have nothing to do with the later reports, and that the history of the /wai-tō-reke/ rightly begins with young Mantell in 1838 (Duff 1956: 288–89).

Conclusions

The proper terms referring to the putative New Zealand "otter" are /wai-tō-reke/ and /kau-reke/, all others being spurious or later alterations of the earlier terms. Both terms are old and are of limited provenance, reflecting the restricted nature of Maori knowledge of this animal. The first term means either "spurred one who sinks into the water," or, more likely, "the one of the quill/spur." The second term means "very spiney." The first meaning accords perfectly with the native reports of the amphibious habits of the creature, and with the likely habits of an ornithorhynchid monotreme. The second accords well with a monotreme with echidna-like spines. Further, all possible meanings point to an animal with a quill or spur as a part of its

anatomy, a feature important enough to be incorporated into its name. This fact also accords perfectly with the peculiar spur characteristic of male monotremes.

Finally, the strong linguistic evidence at hand pointing to a New Zealand monotreme accords very well with the fact that New Zealand separated from all other land masses at an early period, so that any quadrupedal mammal indigenous to it would likely be at a monotreme level of organization, if not an actual member of the order Monotremata.

After careful consideration of the linguistic material, I conclude that the South Island of New Zealand harbored until the mid-19th century, and perhaps still harbors, a species of monotreme of amphibious habits as yet unknown to science, and most likely a member of the genus *Ornithorhynchus*. The animal was, or is, spiney, showing an extremely primitive form of mammalian hair, a pelage transitional between vibrissae (enlarged prototrichia associated with scales) and true hair. This pelage suggests that the animal may represent an amphibious form of basal monotreme.

One must still recognize the possibility, however, that the animal might be even more primitive than the monotremes, and that egg-laying may be merely a shared and not a diagnostic feature. If ever found alive—or even found in sub-fossil or fossil form—in New Zealand, the animal could well prove to be an advanced therapsid.

The linguistic evidence is good; the conclusions are straightforward and plausible; the significance of such an animal is self-evident. The stakes are high enough, and the likelihood of the persistence of this animal into historical times good enough, that searches for either recent remains or living specimens are warranted.

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A MATHEMATICAL ANALYSIS OF "SNOWMAN" (WILDMAN) EYEWITNESS REPORTS

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ABSTRACT: Eyewitness reports from people who claimed to have observed a Snowman (wildman), collected by a commission of the Academy of Sciences of the U.S.S.R. in 1957–58 and from expeditions in 1984–86, were analyzed by applied mathematical techniques. The methods of biological statistics, phenetics, and criminalistics (theory of testimonies) were used. The following traits are apparent: tall stature mode (2.04 m), and short stature mode (1.56 m); four variants of color: yellow-brown, brown, grey, and black. As a whole, the reports are consistent, and no hoaxing is detectable. The data concur with modern ideas concerning the population genetics of rare mammals on the verge of extinction. Verifying the reality of the Snowman will require additional research.

INTRODUCTION

Our main source of information on the Snowman[1] is in the form of eyewitness reports. Let us consider whether such information is authentic. Snowman reports were summarized by a commission of the Academy of Sciences of the U.S.S.R. (1957, 1958). Subsequently, the commission ceased to function. Some modern scientific techniques were not known at the time, and much progress in the field of statistics has taken place in recent decades.

For example, criminalistics has developed a mathematical theory of testimonies. Another new technique is phenetics, genetic analysis based on external discrete traits, initiated by Yablokov (1980).

After extracting maximal reliable information from all the available reports on chance Snowman observations, two alternative hypotheses present themselves:

[1] The term "Snowman" is used in the Soviet Union to refer to the reported Soviet hominid or hominoid, not necessarily the Himalayan Yeti.—Editor.

1. The Snowman represents a real, biological species.
2. All Snowman information is based on misidentifications and hoaxes.

If the first hypothesis is correct, the reported descriptions should concur with concepts of modern ecology and genetics concerning species whose population is becoming extinct. If the second hypothesis is correct, Snowman reports should be removed from the realm of modern biology. In arriving at the correct hypothesis, we should note the improbability of there being a large number of independent, mendacious (or misguided) people having competence in modern biology.

Method

The following information was used:

1. Reports compiled by the Snowman Commission of the Academy of Sciences of the U.S.S.R. in 1957-58.
2. Information collected during 1984–85 in the Tien Shan mountains by an expedition of the Vertical Alpine Club, Volgograd, and specialists from Leningrad, Moscow, and the Aksu-Dzabagly Reserve, near the city of Chimkent. The author participated in this expedition. Materials other than eyewitness accounts—i.e., photography and footprint tracks—are not considered in this paper.

Only the most credible reports, involving 200 Snowman sightings, were analyzed. The following characteristics were recorded: color, height, and sex. The following statistics were used: the range of distribution of quantitative traits, mean (x), standard deviation (σ), and the coefficient of intrapopulation variability (Zhivotovsky 1980). The significance of differences between means were checked by methods of Student (t) and of Fisher (F).

The mathematical theory of testimonies was developed mainly on data from traffic incidents (Rossinsky 1984). According to the theory, the distribution of quantitative characters of observed items within a group of witnesses must be normal or Gaussian. Subjective biases on the part of witnesses tend to displace the mode of distribution. The qualifications or educational backgrounds of witnesses influence the variance of distribution: the higher the qualifications or education, the less is the variance of distribution.

Results

Let us examine the data on the height of the reported Snowman. The mean is 191 $\pm$ 3 cm, σ = 30 cm. For comparison, the average height of modern *Homo sapiens* is 166 $\pm$ 0.4 cm, σ = 5.7 cm (students at Leningrad State University were used without taking sexual dimorphism into account). Hence, the reported Snowman is taller and more variable in stature than is usual for humans. The great variability could be due to: (a) an unreliable method of measurement (human perception); or (b) greater variability in

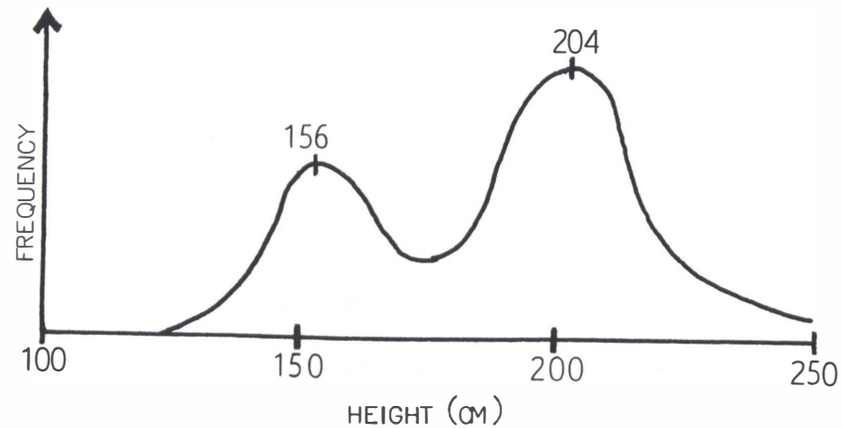

FIG. 1.—Distribution of Snowman stature in eyewitness reports.

actual size, which is typical for species under conditions of stress or population decline (Sapunov 1984).

The height distribution appears to be Gauss bimodal (Fig. 1). The first mode is 204 cm, σ = 24 cm, error = 3 cm. The second mode is 156 cm, σ = 15 cm, error = 3 cm. The difference between the two modes is significant. There are two possible sources of such a difference:

1. Sexual dimorphism, males in most primate species being larger than females.
2. Racial dimorphism.

The second is more likely because some witnesses reported seeing a very tall female Snowman.

What about the validity of such reports? The possibility of hoaxing for personal notoriety, or to call attention to the problem of the Snowman, certainly exists. However, different people have different *a priori* information on the problem. Highly educated people obtain much of their information from the scientific literature, while persons with lower levels of education use other, more popular sources of information. Hence, levels of education can influence the character of such reports. The existence of specific differences in descriptions by persons of different educational levels may indicate whether such reports are hoaxed or authentic.

To test this, I divided all the witnesses into two groups. The first group consisted of people with advanced formal education; the second group consisted of people with little formal education. The distributions of the heights of the reported Snowman in both groups were analyzed separately. In both groups, the distribution was bimodal. The means and variances are listed in Table 1. Significant differences between the two sets of data are absent.

TABLE 1—Dependence of Snowman height reports on educational level of witnesses.

Trait	Witnesses	
	Highly educated	Not highly educated
Height	196 cm	190 cm
Error	5 cm	3 cm
σ	0.24	0.37

This suggests that the Snowman reports from both groups are based on objective reality.

According to the theory of testimonies, the extremes of the quantitative traits reported by a group of independent witnesses should be distributed in the tail or tails of a normal or Gaussian distribution if the data are authentic (Rossinsky 1984). False reports would be distributed with many peaks, and without tails. The existence of one or two modes suggests a single direction of hoaxing—which is unlikely—or the objective reality of the reports.

Let us consider the data on the reported colors of the Snowman. The qualitative characters within populations is the field of phenetics (Yablokov 1980). I attempted to follow the main phenes of color. Age polymorphism (e.g., white hairs) was not considered.

There are four colors (phenes) with the following frequencies: brown = 0.63, gray = 0.23, yellow-brown = 0.09, and black = 0.05. The intrapopulation coefficient of variability (Zhivotovsky 1980) is $M = (\Sigma \sqrt{pi})^2 = 3.2 \pm 0.06$. Such a mean is usual for zoological species, and it also suggests that the population is suffering destabilization. The same pattern of colors may be seen in other primates.

The genetics of color and pigments of mammals is well known (Montagna and Ellis 1958). Data in the literature may be used to interpret possible Snowman genetics. Mammal hair color depends on two pigments. They are melanin (black) and eumelanin (yellow-brown). The resulting color depends on the ratio between the two pigments, on their pattern, and on the production of the pigment cells (melanocytes).

We will now consider some genes with their usual nomenclature. Gene B influences the structure of the pigment matrix of melanosomes within melanocytes. The dominant allele B produces black, the recessive one, b, produces brown. Gene D influences the strength of pigment. The dominant allele insures strong, intensive color (e.g., black or brown), the recessive one, d, leads to dilution of color to gray or yellow. Gene E influences the number of eumelanin granules in the hairs. Dominant allele E leads to production of black, recessive e to yellow. These genes are not linked. The gene combinations may produce a large number of color variants, including the four variants listed above in the Snowman phenotypes.

Let us assume the frequency of alleles D and d, as well as E and e, are equal, while the frequency of allele b is more than that of allele B. Under these conditions, the expected pattern of colors may be close to what is reported. Because of limited data, it is of little value to consider the correct computing model of the genetic structure of the population. The main conclusion here is that the descriptions given in eyewitness reports appear to be authentic from the perspective of population biology.

The next question deals with sex ratio. Witnesses sometimes identified Snowman sex by secondary sexual traits. Analysis of the data indicates that the percent of reported males is 56 ± 3. This is close to the usual relation of 1 to 1. A little oversupply of males is typical for a population in extreme conditions. Such situations are well known in human populations (Beckerman 1976).

DISCUSSION

The question of the existence of the Snowman is still unresolved at the present time. However, based on the analysis of existing eyewitness reports, we may conclude the following: (a) the numerous reported Snowman observations are not contradictory; the reports are consistent, and hoaxing is not statistically detectable; (b) the descriptions given appear to be in accord with modern concepts of ecology, population biology, and primatology; and (c) the definitive answer to the question of the existence of the Snowman may be determined by future research.

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Field Reports

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

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THE FIRST JAPANESE-CONGOLESE MOKELE-MBEMBE EXPEDITIONS

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INTRODUCTION

Interest has been generated by reports of a large, unknown animal supposedly inhabiting the swamps of the Congo Basin. Called Mokele-Mbembe locally, descriptions from sightings have suggests a morphology similar to that of the sauropod dinosaurs of the Mesozoic (Roy P. Mackal, J. Richard Greenwell, and M. Justin Wilkinson, 1982, The Search for Evidence of Mokele-Mbembe in the People's Republic of the Congo, *Cryptozoology,* Vol. 1: 62–72; Herman Regusters, 1982, *Final Report of the Regusters African Congo Expedition*).

On March 10, 1988, the author and 10 others arrived in Brazzaville, People's Republic of the Congo, with the purpose of investigating the possible existence of this animal. Tomio Nonoyama, Kohji Minaguchi, and the author had visited Impfondo in 1986, and had obtained useful information from missionary Eugene Thomas. A Japanese expedition was then conducted in September, 1987, and included Hideyuki Takano and Tohru Mukai, from the Waseda University Expedition Club. That expedition visited Epena, Boha, Djeke, Minganga, Mokengui, and the Mondongouma River, and included Marcellin Agnagna and Jose Bourge Djoni from the Congolese Ministry of Forest Economy.

The new expedition, under the leadership of Takano, left Brazzaville for the northern Likouala region on March 22, 1988.

NARRATIVE DESCRIPTION

The expedition consisted of the following persons: the author, Tokuharu Takabayashi; Tomio Nonoyama, a naturalist; student members of the Waseda University Expedition Club: Hideyuki Takano, Yohsuke Takahashi, Toshiki Murakami, Takuya Kaneko, Hajime Tobe, Tohru Mukai, Michiaki Funakoshi, Kenichi Moriyama, and Osamu Tamura. Marcellin Agnagna, Dieudonné (DeDe) Moubiala, and Jean Claude Ngamokouba, all of the Ministry of Forest Economy, represented the Congolese Government.

We departed from Impfondo to Epena, located on the Likouala-aux-Herbes River. Our first leg took us down the Likouala-aux-Herbes River by motorized dugout to Boha, which took about 6 hours. A meeting was conducted with the Boha village elders, who demanded a substantial fee for our expedition to gain access to Lake Telle. After reaching an agreement, our team was ready to leave with 35 village porters on March 28. Before we embarked, a traditional ceremony was held in front of the chief's home.

It took us 3 days to hike the 50+ kilometers through the swamp forest to Lake Telle. The last 3 kilometers took 5 hours of strenuous effort, as it involved slogging through mud. We arrived at Lake Telle on March 30. A base camp was established at the northern end of the lake, near an old pygmy village, and a subcamp was set up at Bogongo, at the lake's east-southeast edge. Unlike the surrounding swamp-forest, Lake Telle provided an unobstructed view of several kilometers (Fig. 1).

Fieldwork consisted of daylight surface surveillance, using 35mm cameras with telephoto lenses, all-night surface surveillance using a Starlight scope, and sonar scans from a rubber boat. Unfortunately, our sonar unit broke down after only 2 days of operation.

Although our stay at Lake Telle was during the dry season, very rainy days occurred. The average base camp temperature was about 25°C, with a maximum of 32°C (at 2 p.m.), and a minimum of 19°C (midnight to 3 a.m.).

Tsetse flies were encountered on both the edges of the lake and its surface. There were few mosquitos at the camp. Many animals were observed. Our main diet consisted of crocodile, python, turtle, wild pig, and monkey.

During the early morning of April 5, two Boa villagers, Isaac and Double, reportedly observed an unidentified object in the lake. They had taken a dugout and gone to the western side of the lake to hunt. It had been raining, but the rain had stopped, and mist remained on some parts of the lake's surface. After some time, they returned to base camp shouting for Marcellin Agnagna. They said a large black object was floating at the center of the lake. However, heavy mist prevented us from seeing anything, even with the aid of binoculars.

On April 28, expedition members Nonoyama, DeDe, and the author set out in the rubber boat to explore the tributaries or *molibos* of the lake. On April 29, we arrived at the Oumé *molibo,* which local natives say is connected

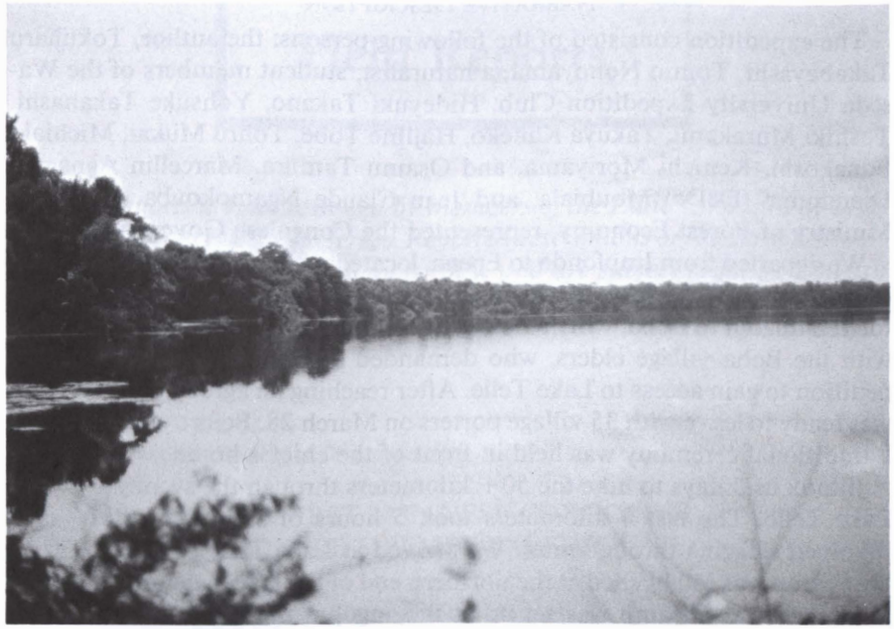

FIG. 1.—A panoramic view of Lake Telle taken from the expedition's base camp.

to the Bai River (Fig. 2). The water was very murky, and only about 1 meter deep. Floating grass covered much of the surface, clogging our passage. We paddled downstream toward the Bai River, observing as we went along. Eventually, DeDe warned that it would be dangerous to go further, so we returned to base camp.

On May 4, after a 35-day stay at the lake, the team returned to Boha along the same route it had followed to the lake. Discussions with villagers at Boha and Djeke confirmed previous reports of Mokele-Mbembe sightings.

RESULTS

We obtained much information on Mokele-Mbembe from villagers. Although there was no evidence of the molombo plant at Lake Telle, Mokele-Mbembe is said to also eat the leaves of the mabondji tree, which never goes out of season. One of the Boha porters, who hunts at Lake Telle, claimed to have seen a Mokele-Mbembe enter the lake in February of 1988.

A Boha elder, between 70 and 80 years of age, said that he had never seen Mokele-Mbembe, but that, earlier in the century, there had been a reported killing of such a creature by pygmies in the Oumé *molibo*.

An elephant hunter at Djeke named Immanuel Mongoumela, who is known to Roy P. Mackal, said he believes Mokele-Mbembe lives in the Likouala-

FIG. 2.—The Oumé *molibo* connecting Lake Telle to the Bai River. A supposed haunt of Mokele-Mbembe.

aux-Herbes, the Bai, and the Sanga rivers. He believes it was improbable that Mokele-Mbembe could ever have lived permanently in Lake Telle. One of the Boha porters said that, when forest elephants ford rivers, they raise their trunks above the surface. This could be one explanation for some of the sightings.

Our expedition found no evidence of the existence of Mokele-Mbembe in the area. No tracks of the creature were observed. We saw many mammals (lowland gorilla, chimpanzee) and reptiles (crocodile, Nile monitor) in the lake area. We searched for Mokele-Mbembe in the tributaries (*molibos*) of the lake. No sightings nor other evidence were collected, although this did not dissuade expedition members from acknowledging the possibility that there may well be an unidentified animal living in the swamps of the Likouala region.

FUTURE PLANS

Although there are no current plans for future expeditions to the Likouala region, the author hopes to return in the future, preferably during the rainy season, particularly to the Sanga and Bai rivers, and the Oumé tributary between the Bai River and Lake Telle. Each expedition may increase the probability of the discovery of such an unknown animal.

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LCPI WORK AT LAKE CHAMPLAIN, 1988

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INTRODUCTION

The year 1988 was highlighted by the continuation of the use of high technology equipment in both underwater exploration and surveying, and automated lake surface video surveillance by the Lake Champlain Phenomena Investigation (LCPI) in its ongoing search for evidence of "Champ," Lake Champlain's version of the Loch Ness Monster. The year 1987 had ushered in this new reliance upon more sophisticated equipment—a ROV (remotely-operated-vehicle), side-scan sonar, Loran-C, and a video digitizer computer monitoring system. More conventional forms of searching—surface surveillance with cameras and binoculars/telescope, scuba searches, and night-scope surveillance—were also conducted. This report serves as an update on the LCPI's ongoing fieldwork, and also records the reported 1988 Champ sightings in the 109-mile-long lake, located in Vermont, New York, and Quebec.

LCPI fieldwork during 1988 was primarily directed at: (1) locating a carcass of a Champ animal using side-scan sonar surveys and ROV searches; (2) continuing field tests of an experimental video digitizer computer monitoring system (VDCMS), which underwent initial testing at Lake Champlain in 1987; (3) daylight surface surveillance with cameras and binoculars/telescope from boat and shore-based sites; (4) underwater searching using scuba divers; (5) providing Champ and search procedure-related educational lessons to the crew of an Air-Sea-Rescue vessel; (6) further documentation, analysis, and publication of reported Champ sightings; (7) encouraging residents and visitors at Lake Champlain to carry cameras to possibly get photographic documentation of Champ; and (8) providing assistance to serious individuals and groups involved in Champ-related research and fieldwork.

NARRATIVE DESCRIPTION

LCPI conducted 41 days of fieldwork at Lake Champlain during 1988. As in 1987, donated gear and personnel helped to make the fieldwork a highly equipped expedition. Donated equipment and personnel came from: Klein Associates, Inc., Salem, New Hampshire; Kaselaan & D'Angelo Associates, Inc., Haddon Heights, New Jersey; Sea Scouts, Kearney, New Jersey; John Becker, Brooktondale, New York; Susan Schmidt, New York City; and George Zug, Smithsonian Institution, Washington, D.C. Klein Associates, Inc. provided a Klein 500-kHz side-scan sonar. Martin Klein, President, and Garry Kozak, Manager, Field Operations, acted as our primary

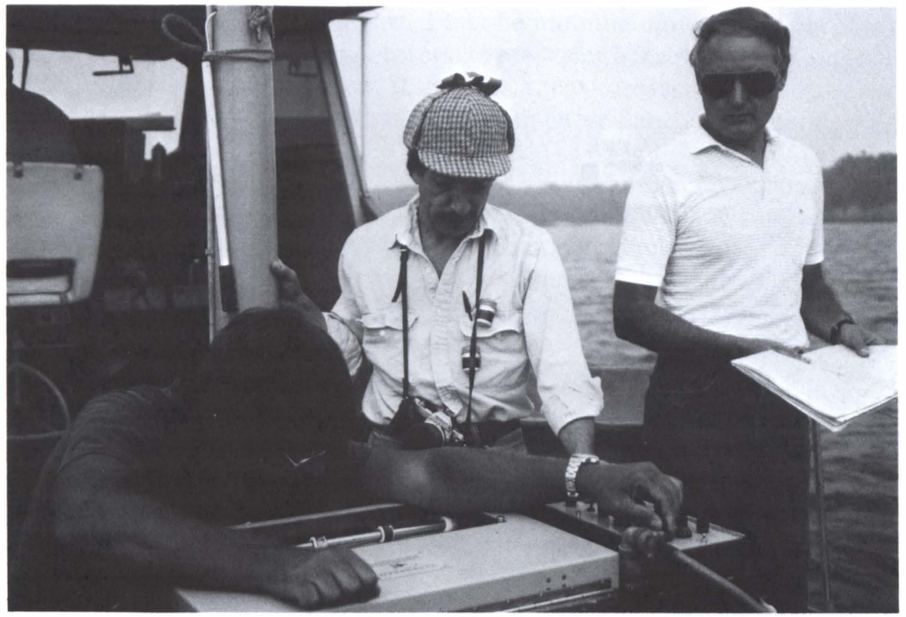

FIG. 1.—Garry Kozak (left), Martin Klein (center), and Russell Bellico observing side-scan sonar returns.

side-scan sonar operators (Fig. 1). Kaselaan & D'Angelo Associates, Inc. provided a MiniRover MK II ROV, equipped with a VHS video camera, a manipulator arm for object retrieval, and a newly fitted 35mm camera for underwater still photography. Vincent Capone, Division Manager, Marine & Aquatic Sciences, Kaselaan & D'Angelo Associates, Inc., was our ROV operator, and he also ran several shifts as side-scan sonar operator. He and Bob Gallagher, also of Kaselaan & D'Angelo Associates, Inc., were also members of our scuba team.

Captain Charles Welte donated the use of his Air-Sea-Rescue 1357 vessel, *ASR 1357*, and his Sea Scout crew. The 63-foot-long vessel was built in 1954 for the U.S. military establishment, and now functions as a training vessel for Sea Scouts based during the summer months at Plattsburgh, New York (Fig. 2). George Zug, Smithsonian Institution, acquired the use of a liquid nitrogen container to be used to hold, preserve, and transfer a small specimen sample, should a carcass of Champ be discovered. John Becker donated an ImageWise video digitizer, an Atari 1040ST computer, a Zenith Universal Infrared VCR/TV/CATV Controler, a Panasonic TV camera, a Sakata Composite monitor, and a 24 Bit I/O Board. Susan Schmidt donated night-scope gear for low visibility surveillance. M. Patram Meaney and

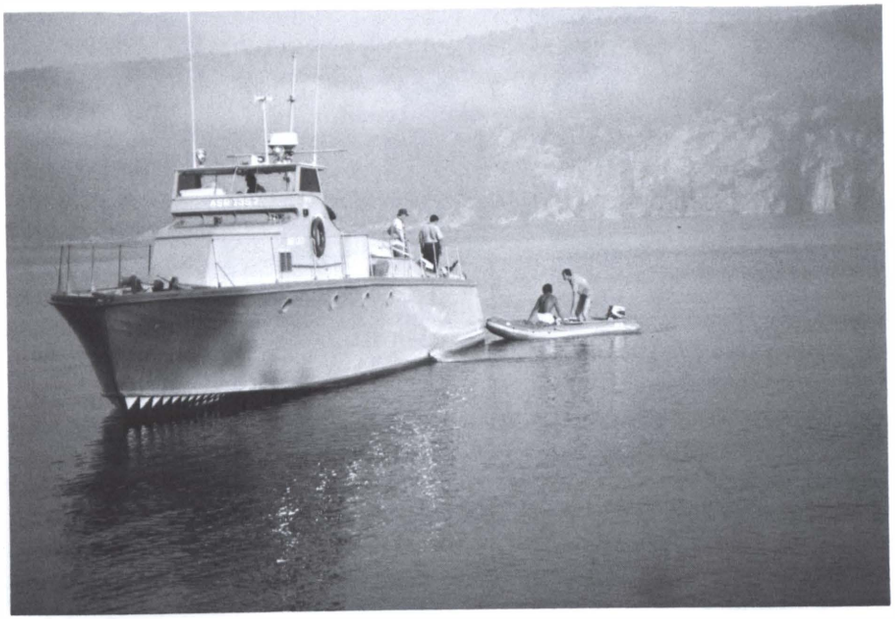

FIG. 2.—The 63-foot-long *ASR 1357* vessel and a 13-foot-long Avon inflatable used during LCPI's 1988 fieldwork at Lake Champlain.

Joseph W. Zarzynski donated various camera, video, boat, and scuba gear, plus providing lakeside housing to LCPI members.

Zarzynski participated in all 41 days of LCPI fieldwork. Others participated on the following date(s): M. Patram Meaney (April 20, May 29, June 25–30, July 1, 31, August 1–3, 5–8, 10, 16–25); Vincent Capone (June 25–26, August 6–14); Russell Bellico (July 6, 9, August 5, 7, 10, 23, 30); Garry Kozak and Martin Klein (August 10–11); Bob and Mike Gallagher (August 10–14); Bob Benway (June 11, August 2–3, September 10); Jack Sullivan (June 11, July 6, 9, August 25–26, September 10); Jerry Pepper (June 11, July 9); John Farrell (June 11, July 9, August 18–19, 30); Johnny Farrell (August 18–19); Barbara and Terry Crandall (August 8–14); Captain Dan Couture (June 11, July 9, August 30, September 10); Bill Currie (August 7–14); Judy Currie (August 8–10); Liz Meaney (August 23–25); Ted Straiton (August 19–22, September 17); John Becker (August 19–22, September 17); Dave Pitkin, Joe Citro, George Earley, and Ken Bartowski (September 17); and Captain Charles Welte, Brian Heinzmann, Michael Golan, Jeff Gregory, Robert Del Freo, Anthony Siano, and Edmund Phelan (August 7–14).

Special thanks go to Richard Smith and Richard Deuel for bringing to my attention some previously unrecorded Champ sightings. Richard Smith, an

active Champ fieldworker, reported that he mounted no major effort at the lake in 1988, instead devoting his time to preparing a research boat for 1989. Richard Deuel, an independent researcher, spent considerable time at Lake Champlain in 1988 doing shore-based lake surveillance and interviewing Champ eyewitnesses.

The 1988 LCPI fieldwork consisted of: daytime surface surveillance from boat and shore using 35mm cameras, a Super 8mm camera, a VHS video camera, telephoto lenses, camera tripods, binoculars/telescope, surveillance camera/monitor, video recorder, and computer with digitizer; nocturnal surface surveillance using a hand-held night-scope; scuba monitoring; scuba diver underwater photography; side-scan sonar; and operation of the ROV.

Lake surface surveillance was conducted primarily from Kimball Dock, Vermont. Numerous surface surveillance sessions were conducted from boats. Side-scan sonar sweeps and underwater videography/photography using the ROV were made from the *ASR 1357* vessel.

The dates of the fieldwork were: April 20, May 29, June 11, 25–30, July 1, 6, 9, 31, August 1–14, 16–26, 30, September 10, 17. Daytime surface surveillance from shore sites or boats using cameras and binoculars/telescope was conducted during all 41 days; nocturnal surface surveillance from a shore site using a night-scope was done during two nights; boat-deployed side-scan sonar was used during two days; the ROV deployed from a boat was used during three days; use of the video digitizer computer monitoring system was used during three days; and 32 scuba dives were conducted, 12 with diver(s) using a Nikonos underwater camera, and one with divers working with the ROV.

The 32 scuba dives were made as part of an underwater reconnaissance survey, to inspect and untangle the ROV cable, or to examine side-scan sonar targets. The dates of these scuba dives, the divers involved, and the type of dive were: June 11 (Benway, Pepper, Sullivan, Zarzynski, two reconnaissance dives); July 6 (Bellico, Sullivan, Zarzynski, three reconnaissance and sonar dives); July 9 (Bellico, Farrell, Pepper, Sullivan, Zarzynski, two reconnaissance dives); August 2 (Benway, Zarzynski, one reconnaissance dive); August 3 (Benway, Zarzynski, three reconnaissance dives); August 5 (Bellico, Zarzynski, one reconnaissance dive); August 7 (Capone, Zarzynski, one ROV inspection/rescue); August 9 (T. Crandall, B. Currie, Zarzynski, one reconnaissance dive); August 10 (Capone, Zarzynski, one reconnaissance dive); August 12 (T. Crandall, B. Gallagher, three Sea Scouts, Zarzynski, three reconnaissance dives); August 13 (Capone, Zarzynski, one reconnaissance dive); August 17 (Bellico, Zarzynski, one reconnaissance dive); August 19 (John Farrell, Zarzynski, three reconnaissance dives); August 23 (Bellico, Zarzynski, one reconnaissance dive); August 25 (Sullivan, Zarzynski, two reconnaissance dives); August 26 (Sullivan, Zarzynski, two reconnaissance dives); August 30 (Bellico, Couture, John Farrell, Zarzynski, two recon-

naissance dives); and September 10 (Benway, Couture, Sullivan, Zarzynski, two reconnaissance dives).

Results

One visual sighting of a Champ-like animal was made by LCPI personnel. The sighting was made on August 10 by Patram Meaney, Martin Klein, Joseph Zarzynski, and several others aboard the *ASR 1357* vessel while towing a Klein 500-kHz side-scan sonar in approximately 150 feet of water. This marks the first time that LCPI fieldworkers have observed an unidentified animate object—possibly a Champ animal—swimming on the surface of Lake Champlain. However, due to the vessel towing electronic gear at a slow speed, and the distance from the object, no good photographic record of the sighting was recorded. Sonar records from that time are currently being examined to see if the unidentified animal was recorded by sonar. We suspect, however, that, due to the prohibitive range, no sonar contact was made.

Nonetheless, "seeing is believing," and this sighting helped to personally buoy the author's belief in the existence of a large, unknown animal or animals—dubbed Champ—in Lake Champlain. Finally, one wonders whether, had we not been restricted by technology (towing a side-scan sonar towfish transducer/cable), we might have been able to give chase and possibly obtain a better view at the unidentified animal.

LCPI divers unexpectedly discovered two previously unknown shipwrecks lying on the New York State side of Lake Champlain. The shipwreck sites have been reported to the Office of the State Archaeologist, in Albany, and a Vermont-based underwater archaeologist will attempt to identify them.

With help from Richard Deuel, LCPI is able to record one more 1987 Champ sighting not included in last year's report (Joseph W. Zarzynski, 1987, LCPI Work at Lake Champlain, 1987, *Cryptozoology,* Vol. 6: 71–77). This brings the 1987 sightings to eight. Details of the sighting follow:—about October 15, 1987: Mason Jackman and John Douville; while duck hunting at Panton, Vermont; at Kent's Bay, by Rock and Mud Islands; around 9:00 a.m.; range 200 yards; "nice, partly cloudy"; observed a 30-foot-long animal with 3–4 feet of head out of water, and 6–8 humps; sighting lasted for 1½ minutes.

The nine 1988 Champ sightings uncovered to date are listed below in chronological order (Fig. 3):

(1) June 19, 1988: Mary Jane Neale and Paul D. Damkot; near Colchester, Vermont, while sailing from Shelburne to Mallett's Bay; clear, sunny, very light wind; Neale reported seeing a head or neck of animal about two feet out of water; Damkot reported seeing 5–6 humps; sighting lasted about 30 seconds.

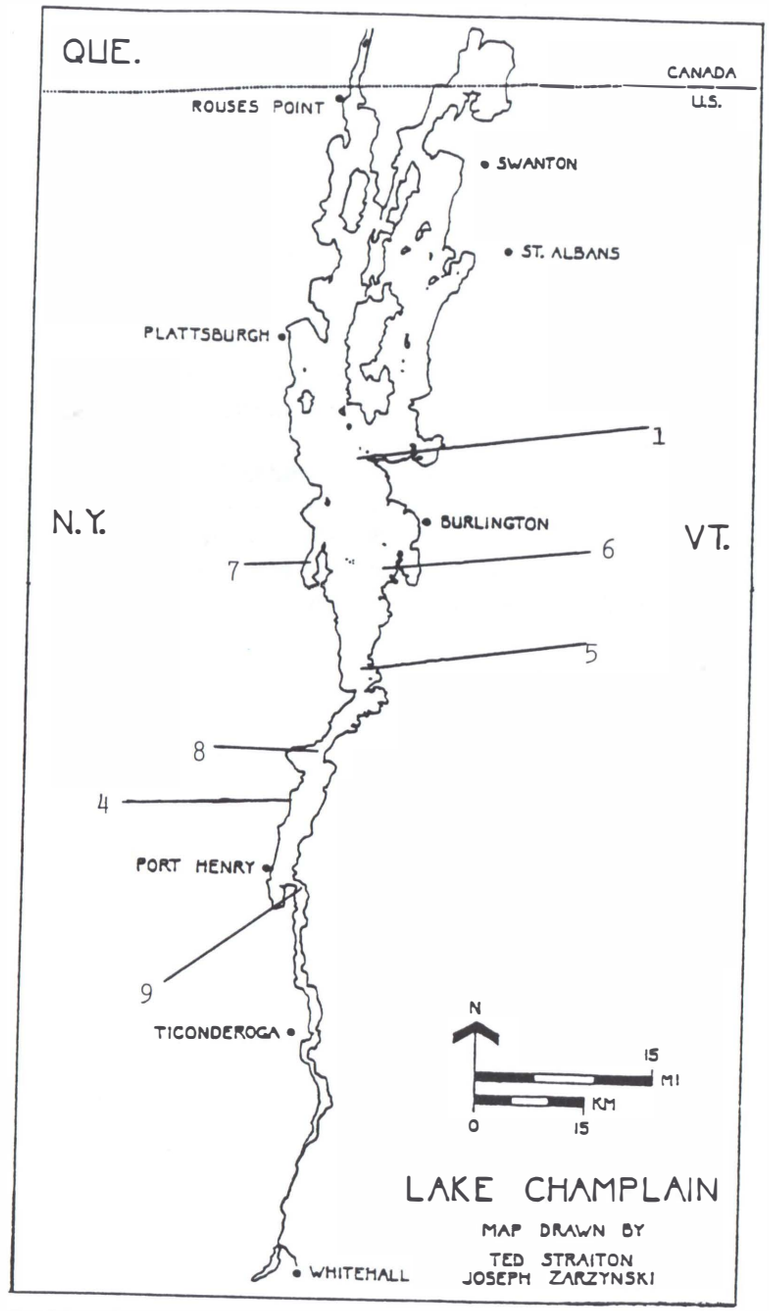

FIG. 3.—Map of Lake Champlain, with numbers indicating the locations of the eyewitness sightings logged by LCPI during 1988. Locations of sighting #2 and #3 have not yet been revealed by the witnesses.

(2) July 5, 1988: Walter, Sandra, and Heidi Tappan; location of sighting not yet provided; 7:30–8:45 p.m.; range from 600 feet to 12 feet, with an average of 100 feet; calm lake surface and clear weather; they both observed (and videotaped using a VHS camcorder) a black-colored animal or animals; "head and neck were smooth; the humps were alligator-like"; Sandra said she saw animal's head and neck.

(3) July 6, 1988: Walter, Sandra, and Heidi Tappan; location/time/conditions reported to be similar to July 5, 1988, sighting.

(4) July 17, 1988: Theresa May McCutcheon, Mrs. M. O'Neill, Mrs. J. May, and Mrs. R. Cousins; Westport, New York, south on Route 9N, approximately 2 miles from Camp Dudley; 7:30 p.m.; range ½ mile; fog/mist; flat lake surface; warm; McCutcheon saw "a large blackish-green hump . . . followed by 4 other humps"; reportedly three photographs taken using a Canon 35mm camera.

(5) August 1, 1988: Elizabeth K. Fox, Abby and Colin D. Rehkugler, Nick P. Karthaus, Bill Lockwood, and Allison Connors; at Charlotte, Vermont, town beach; around 7:00 to 8:00 p.m.; range varied according to eyewitnesses' estimate from 30 yards up to as far as ¾ of a mile; no wind; hot, clear and calm; accounts varied, but most agree two humps observed for several minutes.

(6) August 2, 1988: Judith, Harold, Dawn, and Francine Heisler; about 10 miles north of Converse Bay, Vermont; 7:30 p.m.; range 1,000 feet; very calm lake surface; clear and calm weather; while out in a boat fishing, the Heislers saw a dark-colored animal some 20–30 feet long with a head and three humps; photograph taken using a Kodak disc camera; according to Judith Heisler, "the photo doesn't show much."

(7) August 2, 1988: Jack H., Mabel, and daughter (whose name is not given) Wearstler, and George and Jackie Marshall; Willsboro Bay, New York; 8:35 p.m.; range between 100 feet and 100 yards; while boating, they "observed a very large object rise up out of the water"; 25–30 feet long, two feet in diameter; three or four segments, with one hump having "something like a fin or flipper which appeared to be lighter in color than the main part of the body"; the boat's depth-finder registered a depth of 135 feet at sighting location; sighting lasted 5 minutes.

(8) August 10, 1988: M. Patram Meaney, Martin Klein, Joseph W. Zarzynski, and several others aboard *ASR 1357* vessel; between Westport, New York, and Basin Harbor, Vermont; 6:50 p.m.; range, several hundred yards up to ½ mile; flat, calm lake surface; Meaney and Klein were the first to observe the animate object on lake surface moving away from boat, which was motoring about 3–4 knots towing a side-scan sonar towfish transducer; object black in color; no head/neck observed; object "churned" and "thrashed" on surface as it propelled itself through the water; observed with naked eye and binoculars; Meaney stated: "It moved slowly, would go down, and then surface again a little further away. It was a movement independent of the boat wakes in the area."

(9) August 31, 1988: Lee and Clayton Smith; Crown Point, New York; 12:30 p.m.; range ¼ mile; smooth lake surface; clear and sunny; Lee Smith videotaped object using a VHS recorder; animal dark grey to black in color; length estimated at 50 to 60 feet; height above lake surface 3 to 5 feet; head observed approximately 15 feet ahead of main body (one hump); it appeared "to blow water from its nostrils when surfacing."

Future Plans

LCPI intends to continue its fieldwork at Lake Champlain in 1989, again emphasizing a high technology approach to the search—underwater surveying and video digitizer computer monitoring. We hope to be able to test John Becker's VDCMS in an underwater surveillance mode to ascertain its feasibility in that environment. Vincent Capone hopes to complete his computer study, begun in 1988, of the over 300 recorded Champ sightings.

As previously proposed, the high technology approach—especially the search for a Champ carcass using side-scan sonar, a ROV, and teams of scuba divers—may best bring about the definitive evidence for Champ—a carcass. Nevertheless, LCPI does not advocate the killing of a Champ animal to secure such a specimen for scientific study. Rather, it advocates a more passive strategy, such as lake bottom searches, and encourages protection for this possible new species.

Book Reviews

Cryptozoology, 7, 1988, 78–99

Naturalized Mammals of the World. By Christopher Lever. Longman, London, 1985. 487 pp. £40 (John Wiley, New York, $87.95) (c.).

While much has been written about introduced or naturalized animals, the literature has been dispersed and of varying quality. The only two volumes on the subject in my own library are *The Alien Animals,* by George Laycock (Natural History Press, New York, 1966), and *Animal Invaders,* by Clive Roots (Universe Books, New York, 1976).

While these popular works covering various vertebrate forms are of some use, the new book by British naturalist Sir Christopher Lever—which includes a foreword by H.R.H. The Duke of Edinburgh (Prince Philip)—has brought together for the first time, and under one cover, almost all the information available on the worldwide distribution of naturalized mammals. It is a work of enormous magnitude, and it must have required many years of painstaking bibliographic research, not to mention personal investigation and correspondence.

The new volume contains sections on each of the 61 mammals considered naturalized by the author, and each mammal has a fine-line drawing by artist Norma Hansell. These 61 mammals include the domestic cat and dog (the stoat, weasel, polecat, and ferret are treated jointly in one section). Almost all the sections contain two world maps: the first map shows the natural distribution of the mammal in question, while the second shows the naturalized distribution. These are very helpful, as one can see at a glance "what has happened" with a particular species. The texts, of course, provide much more information, including the often convoluted histories—and even failures—of introductions.

There are often, after the main sections, small "capsule" texts on related species—and sometimes not so related—on which there is less information and/or the introductions were not that significant, either geographically or numerically. However, and this is my only real criticism of the book, it was not always clear to me why particular introduced species deserved only capsule entries (thus not appearing in the book's table of contents) rather than full sections. No doubt the author himself struggled with this problem, and arrived at what he thought were suitable compromises.

While not a book on cryptozoology, this volume can be of much interest to the cryptozoologist for a number of reasons. First, there are instances in which animal forms from other parts of the world are seen by observers who are quite unable to recognize them or to reconcile them with the known animals of their area, often "adding" features to the animals seen. Knowing *what* transplanted animals exist *where* can help the cryptozoological investigator solve a puzzling observation or series of observations.

A second reason is that perhaps some species have become naturalized surreptitiously, or are in the process of doing so, and this is of cryptozoological interest if zoological and official authorities are unaware or incredulous of such a possibility, dismissing the reports of such animals as nonsense. One may recall, for example, the continuing sightings of kangaroos or wallabies in the U.S. Midwest, and more recently in Nova Scotia, Canada. I shall return to this topic below.

A third relevance to cryptozoology is that animals know to be naturalized may sometimes legitimately—if I may be allowed to use that term—become cryptozoological by assuming a cryptic existence in their new habitat. How many readers are aware, for example, that North American moose (elk in Europe) were released in a remote area of South Island, New Zealand, in 1900 and 1910? I certainly didn't until I read Sir Christopher's book. The population is now thought to be very low, or possibly extinct. Even when extinction is presumed, however, small populations may sometimes hang on, unnoticed and unsought by man, except perhaps by curious naturalists.

Sir Christopher, an ISC member, is very clear in his preface on the criteria for the inclusion of a species in the volume: "It should have been imported from its natural range to a new country or region either deliberately or accidentally by human agency, and that it should currently be established in the wild in self-maintaining and self-perpetuating populations unsupported by, and independent of, man. . . . Natural immigrants from one country to another have normally been included only if they have been augmented by deliberate or accidental introductions by man. . . . Introductions of domestic species from one country to another are only mentioned if feral populations have resulted."

In his introduction, Sir Christopher lists the various motives behind deliberate animal transplantations: for sport/game; for nostalgic or sentimental reasons; for aesthetic reasons; as a source of food; for fur harvesting; for control of local pest species; and, finally, out of simple—but irresponsible—curiosity to see how an "alien" species would adapt to a new environment. While some, like the camel, were deliberately introduced for use as a means of transportation, others, like rats, have used man as a means of transportation in the accidental introduction process.

Many 19th century introductions were made by "acclimatization" societies in the British colonies, particularly in Australasia, although, as Sir

Christopher points out, they were more commonly made for "sporting" purposes rather than for purely nostalgic or aesthetic reasons. Fortunately, many such introductions did not succeed.

Factors which determine whether an introduction will ultimately succeed are: suitable climate; suitable habitat; adequate food supply; freedom from predators; absence of competition from more aggressive native species; ability to reproduce rapidly; absence (or abandonment) of the instinct to migrate; a large enough initial (transplanted) stock; and a degree of adaptability—the more generalized a species is, the better its chances of becoming established. Another factor, which for some reason Sir Christopher does not mention, is tolerance of local parasites and viruses.

Once a species has become successfully established, the repercussions begin. When all is said and done, rarely is an introduced mammal of ecological benefit, although it may be of economic benefit, such as producing income from hunting licenses or fur exportation. The list of dangers include: parasite/disease transmission; damage to human food/resources; disturbance of ecosystems; interspecific competition with native species; predation of native species; and genetic changes in native populations. Thus, for the zoologist and conservationist, such introductions generally herald bad news. In fact, Sir Christopher quotes an alarming statistic: since 1600, "animals (mainly mammals) introduced around the world have been responsible for some 23 percent of mammalian and 19 percent of avian extinctions."

There are some interesting naturalizations worth noting: the European hedgehog, *Erinaceus eoropaeus,* "is the only large insectivore to have become widely established outside of its natural range"; "feral cats have had a more devastating effect on the wildlife of New Zealand and its offshore islands than on the whole of the rest of the world"; "no other mammal deliberately spread by man has subsequently expanded its range as widely and as rapidly as the [North American] muskrat (*Ondatra zibethicus*): its Eurasian distribution is now greater than that of its [original] North American homeland."

The muskrat holds another distinction: it is "the only naturalized alien vertebrate to have been successfully eliminated (by the mid-1930's) in Britain, and, indeed, one of the few in the world." And, as often happens with island introductions (of which more below), ". . . the mongoose in the Caribbean has devastated more indigenous animals than any other species deliberately introduced by man anywhere in the world."

Of particular interest to this reviewer is the recent British naturalization of the North American mink, *Mustela vision,* which was imported to Britain for fur farming before the war. Postwar escapes resulted in established populations, which were officially acknowledged in 1957. The mammal guidebooks of the period—which, as a youth, I revered as holy scriptures—had no mention of the mink. I suppose it was enough that we had the American grey squirrel to contend with as a "native" mammal!

Now, however, the mink has established itself to the point of overcoming

Anglo-American animosities, and is to be found in most 1980's British mammal guides. In fact, the mink is "the only alien non-domesticated carnivore to have become firmly established in Britain in historic times." Even here, however, the damage has been great to native species, poultry, avicultural collections, and even fish farms.

Another British naturalization, which may have a different ending, is that of *Myocastor coypus,* a giant and aquatic South American rodent known as the coypu or nutria (not to be confused with *nutria,* the Spanish name for otter). It was imported into southern England in 1929 for fur farming, resulting in the inevitable escapes. By the 1940's, the species had become established in the wild, reaching a peak of 200,000 by 1962, when, because of its affect on agriculture, it was listed under the Destructive Imported Animals Act. Following intensive eradication campaigns, it is now down to a few thousand individuals. Due to interest in nutria-farming, it was also imported into several other countries in Europe, Africa, and even Japan, resulting in escapes and subsequent wild populations.

In looking at the "big picture" presented by Lever's book, I found some interesting patterns related to how many native species from one zoogeographical region ended up as introduced species in other zoogeographical regions. I have omitted species transplanted from one country or area to another if they are within the same zoogeographical region. I have also omitted five problematical species which Lever includes, domestic cats and dogs, the brown rat, the black rat, and the house mouse—these now being universally distributed—and I have come up with the following results.

Palearctic (Eurasian and North African) mammal species are now established wild in the Ethiopian (tropical African) region (5), the Oriental (tropical Asian) region (3), the Australasian/Oceanic regions (17), the Neotropical (Central and South American) region (11), and the Nearctic (North American) region (12). This surely reflects intensive European colonization of North and South America, and Australia and New Zealand—the latter island-nation having been clobbered more than any other isolated ecology on the planet.

Oriental mammal species are now established in the Australasian/Oceanic regions (11), the Neotropical (5), the Nearctic (7), the Palearctic (6), and the Ethiopian (4). Next comes the Nearctic, which has wild species in the Palearctic (6), the Ethiopian (1), the Australasian/Oceanic (3), and the Neotropical (3). The Neotropical only has one species established wild in three regions, the Nearctic (1), the Palearctic (1), and the Ethiopian (1), and all three are the above-mentioned coypu.

But the real surprise to me was the absence of significant transplantations of African and Australian mammals into other regions. Only one Ethiopian species, the green monkey, is found elsewhere, and that is in the Neotropics, on the Caribbean islands of St. Kitts, Nevis, and Barbados. And, excluding some Australian macropids in New Zealand and one on Oceanic Hawaii,

only one marsupial, the red-necked wallaby, is to be found elsewhere, and that is in the Palearctic, specifically England. I leave for others the further interpretation of these worldwide patterns of transplantation.

It is also interesting to examine what kinds of animals are involved (no longer omitting any species or forms). These have been:

5 marsupials
1 edentate
1 insectivore
4 primates
9 carnivores (with stoat, weasel, polecat, and ferret combined)
2 perissodactyls
28 artiodactyls (half of them deer)
9 rodents
2 lagomorphs

The book has three concluding sections, all of which are of great value. The first is Table 26, which lists, country by country, all the principal mammal introductions. This is extremely useful, as one can then study the worldwide pattern from a different perspective. The second is an enormous 34-page bibliography on the literature of naturalized mammals worldwide—surely the most comprehensive ever compiled—which Sir Christopher modestly states contains only "the more important references." And the third is a 13-page comprehensive index of vertebrate species mentioned in the text (i.e., not only naturalized mammals).

Returning to Table 26, we find that Australasia is host to the largest number of mammals transplanted from elsewhere (note: not just from other zoogeographical regions), with 31 in New Zealand and 25 in Australia. New Zealand has had forced upon it wallabies from Australia, mustelids from Europe, rodents from Asia, rabbits and hares from Europe, and deer from practically everywhere. The country had no native quadrupedal terrestrial mammals—except perhaps the still undiscovered Waitoreke—so these mammals encountered practically no competition. However, the effects on many reptile and bird species were, in many cases, nothing but terminal. In Oceania, Hawaii comes in first, with 20 transplants, which—as in New Zealand—have caused havoc on the native wildlife.

The United States excluding Hawaii is next, with 27 transplanted species, Canada having 11. Britain is the largest Palearctic recipient, with 18, followed by the Soviet Union with 13. In the Neotropical region, the West Indies is first with 17 transplants, which, as mentioned above, have often led to devastating consequences. Japan is the highest in the Oriental region, with 4 transplants, and, in the Ethiopian, South Africa leads with 10, followed by Kenya and Tanzania with one each (the coypu again). According to these data, Africa south of the Sahara—excluding the Republic of South Africa—

stands as the region which has exchanged naturalized mammals the least with other world regions.

Before concluding this review, I would like to again touch upon what may be an important cryptozoological component; and that is what the book does *not* contain. There is no mention of American kangaroo reports, the many "puma" reports in Britain, or of similar reports of placental big cats in southern and western Australia, now numbering over 1,000. The reason they are not included, of course, is because there is no verifiable evidence that such mammals have, in fact, become independently established in the wild in those countries.

Also, many such reports, it could be argued, may be the result of escapees, which might survive wild for some time—or be recaptured—but without establishing a viable, breeding population. While it is difficult to propose the scenario of so many escaped kangaroos in North Aemrica, it is possible that the British and Australian cat sightings are, in fact, at least partly due to escaped or released exotic felids. (Di Francis, a British investigator, believes the British cats belong to a yet-undiscovered native species, but that hypothesis does not concern us here.) If this is so, then the next question is: are such felids, as Sir Christopher would have it, "in self-maintaining and self-perpetuating populations unsupported by, and independent of, man"?

In looking at the table of contents of Sir Christopher's book, one is struck by the lack of felid representation. Canids are moderately represented—domestic dog, dingo, red fox, and raccoon-dog—but the only felid is the domestic cat. Is it possible that no wild member of the Felidae has ever become naturalized anywhere? In weighing this question, we should take into account at least two factors: first, cats are much more disposed than dogs towards living independently of man, a fact which is clearly visible in looking at the habits of the domestic versions of these animals.

The other factor is that cats are very elusive—perhaps one could say "cryptic"—more so than any of the other mammals appearing in Sir Christopher's book. Is it possible that large escaped or released felids are, in fact, naturalized in Britain, and perhaps also in Australia, but have not been recognized as such because of their independent and elusive nature? If this is ever found to be the case, an important addition will be necessary to a future edition of Sir Christopher's book—an addition which I am sure he would be delighted to make.

In the meantime, we can enjoy—and use—the valuable information made available to us in this most comprehensive volume, as well as that in a recent companion volume, *Naturalized Birds of the World,* published by Longman in 1987.

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Kadimakara: Extinct Vertebrates of Australia. By Pat V. Rich and Gerard F. Van Tets (eds.). Illustrated by Frank Knight. Pioneer Design Studios, Melbourne, 1985. 284 pp. A$49 (c.).

The *kadimakara,* or animals of the Dreamtime, should be familiar to many readers of *Cryptozoology.* Heuvelmans, in his 1958 book *On the Track of Unknown Animals* and subsequently (1986, Annotated Checklist of Apparently Unknown Animals with which Cryptozoology is Concerned, *Cryptozoology,* Vol. 5: 1–26) has discussed the large, Pleistocene marsupials frequently implied by this Aboriginal term. In the broader sense, however, the *kadimakara* include, in addition to *Diprotodon* and other great pouched mammals, all of the extinct (or presumed extinct) vertebrates of the Australian continent.

In this handsome and eminently readable volume, Rich and Van Tets have drawn together portraits of selected vertebrate species (or groups of species) that once roamed Australia or swam in its waters. The book opens with two chapters on the history of vertebrate paleontology in Australia, one by Rich the other by Dick Tedford—both paleontologists with international reputations. This 39-page overview is generously illustrated with numerous black and white photographs of both noted personalities and fossil sites that have played a role in the rise of Australian paleontology. The historical outlook of these chapters permeates the entire book, as the species accounts are written by experts who have played a role in the discovery and/or interpretation of the fossils.

These authors successfully impart the excitement—and frustration—of paleontological research, and thereby bring new meaning to the taxa they discuss. Active workers in the field of cryptozoology will surely recognize in these passages reflections of their own emotions experienced in the course of "the hunt."

These historical chapters are followed by 33 pages devoted to the earth sciences, authored by E. M. Truswell and G. E. Wilford. This discourse on geology, paleogeography, and paleoclimatology sets the stage for the calling of the role of the *kadimakara,* and provides the environmental context into which the animals may be placed.

Thirty-two accounts (4 on fish, 1 on amphibians, 11 on reptiles, 6 on birds, and 10 on mammals), authored by 25 experts, deal with the *kadimakara* themselves. These accounts are only four to seven pages in length, but each succinctly treats the discovery, reconstruction, biology, and phylogeny of the taxa discussed. Although the emphasis of the accounts varies, all are enhanced by full-page color illustrations by Frank Knight depicting the living *kadimakara* and their habitats. Line drawings of skeletal elements, osteological reconstructions, and related taxa further highlight the text. Each

account is prefaced by a point locality map of fossil sites, a classificatory summary, and a statement of known geological age.

It is beyond the scope of this review to discuss each of the *kadimakara,* but some of the animals are of particular cryptozoological interest, and thus deserve note here. Among the reptiles discussed are four Pleistocene forms which may have survived until the arrival of man in Australia, some 40–50,000 years B.P.; two of these are particularly noteworthy. One is the giant horned turtle *Meiolania.* Members of this genus have been found throughout the Southern hemisphere land masses, and in New Caledonia it may have been exterminated by Melanesians as recently as a few thousand years ago.

The other is the giant goanna or monitor lizard, *Megalania prisca,* the largest Australian predator of its time. Its story is told by Tom Rich. *Megalania,* which may have grown to seven meters in length and 620 kg in weight, has been involved in a number of cryptozoological considerations. Heuvelmans (1986, above) lists it as a likely candidate for the identity of the Aboriginal *mungoon galli,* and information about *Megalania* has been used as evidence for the varanid identity of the *buru* (Roy P. Mackal, 1980, *Searching for Hidden Animals: An Inquiry into Zoological Mysteries,* Doubleday, New York) and other large reptilian forms. The comparatively recent (1912) discovery and description of the largest known living monitor, *Varanus komodoensis,* and the very recent discovery of new, large monitors in rugged and inaccessible regions of the Old World tropics, lend credence to the possibility that *Megalania*—or a related form—may yet persist in Australia.

Among the birds, editors Rich and Van Tets respectively present informative chapters on *Genyornis newtoni,* a giant ratite, and *Progura gallinacea,* a giant megapode. Both birds co-existed with early Aboriginal peoples. The former, known as the *mihirung paringmal,* was a dromornithid, a member of the group including the heaviest bird that ever lived. These birds are represented in cave paintings, and the memory of their existence, as recently as 6,000 years B.P., has been preserved in Aboriginal oral tradition. The giant megapode is but one of many related species that have been exterminated by invading humans throughout the Pacific. Occasional reports of sightings of some of these "extinct" species suggest that cryptozoological research may yet unearth relict megapode populations.

The mammalian fauna of Australia has long provided surprises for science, with such oddities as the platypus and a host of marsupial equivalents of the placentals populating the rest of the world. Modern marsupials are relatively small, but many larger mammals were typical of the Australian marsupial megafauna that began to decline during the last glacial period (about 40,000 years B.P.), at about the same time as the human species began its spread across the continent.

The largest known marsupial was *Diprotodon,* a Pleistocene herbivore first described in 1838. Tom Rich, who authored the section on this beast, indicates that *Diprotodon* co-occurred with humans for about 10–15,000 years, but only recently have rock paintings, perhaps referable to this taxon, been located in Australia. Heuvelmans (1986, above) has suggested that *Diprotodon* could be the model for frequent sightings of giant "rabbits" in Australia. Pat Rich and Van Tets, on the other hand, suggest that the memory of these large marsh-dwelling animals may have fueled the sightings of *bunyips* (large aquatic animals) that have been known to the Aborigines for thousands of years.

Alternatively, *Palorchestes* sp., a Plio-Pleistocene diprotodontid which is reconstructed in *Kadimakara* as a ground sloth-like animal with an elongate snout, may have been the model for *bunyips.* This is the suggestion of Timothy Flannery and Michael Archer, the authors of the *Palorchestes* account. These animals had earlier been conceived of as kangaroo-like, and Heuvelmans (1986, above) has proposed them as another potential model for the giant Australian "rabbits." Christine Janis (1987, A Reevaluation of Some Cryptozoological Animals [Comment on Heuvelmans], *Cryptozoology,* Vol. 6: 115–18) has more recently proposed that *Sthenurus,* a giant monodactyl kangaroo (also treated in this book), may be the basis of the "rabbit" sightings.

Finally, *Thylacoleo carnifex,* the marsupial "lion," should be mentioned. This Pleistocene animal is described by Rod Wells, and was probably known to earlier Aborigines as it lived until at least 18,000 years before the present. *Thylacoleo* is believed to have died out during a period of great aridity. Much speculation has surrounded the habits and diet of this animal, which Wells describes as carnivorous and leopard-like. This marsupial lion, or a relative, has been considered as a possible basis for reports of the famous Queensland Tiger (Heuvelmans, 1986, above; Victor A. Albert, 1987, A Bungle in the Jungle, or Why Specialization is Important in Cryptozoology [Comment on Heuvelmans], *Cryptozoology,* Vol. 6: 119–20; Bernard Heuvelmans, 1987, Checklist Corrected and Completed [Response to Tomasi, Raynal, Janis, and Albert], *Cryptozoology,* Vol. 6: 121–24), although most cryptozoological reports of large carnivorous marsupials appear to be referable to the thylacine or Tasmanian Tiger, *Thylacinus cynocephalus.*

Although the thylacine supposedly became extinct in 1936, evidence for its persistence in Tasmania—or of a similar form on mainland Australia—is among the strongest for any large mammal of cryptozoological note (1985, Thylacine Reports Persist After 50 Years, *The ISC Newsletter,* Vol. 4[4]: 1–5). Indeed, the possible survival of the thylacine gives hope that some of the animals of the Dreamtime may yet be found alive.

Kadimakara concludes with a short review of "what to do" if you discover a fossil, and two invaluable resources—a list of the names and addresses of Australian paleontologists, and a selected bibliography of over 375 titles. This introduction to both literature and personal resources will prove a boon to serious researchers, paleontological or cryptozoological.

Heuvelmans (1982, What is Cryptozoology?, *Cryptozoology,* Vol. 1: 1–12) considers cryptozoology and paleontology as sister sciences, both founded on *exceptional* data—rare remains, rare observations—their objects of study being animals lost in time and lost in space, respectively. *Kadimakara*'s historical perspective serves to emphasize the similarities of the two sciences, and, though most of the contributors to *Kadimakara* appear to be unfamiliar with the cryptozoological significance of the animals they discuss, their clear and professional descriptions will be of interest to cryptozoologists concerned with Australian fauna.

Indeed, a view from outside one's own discipline can sometimes provide new insights and a more balanced picture of the available data. Several other books dealing with Australian paleontology have recently appeared. However, *Kadimakara*'s fine illustrations, historical perspective, and detailed, yet concise text make this volume particularly appealing.

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Thylacine: The Tragedy of the Tasmanian Tiger. By Eric R. Guiler. Oxford University Press, Oxford and Melbourne, 1985. 207 pp. £19.50 (c.).

The thylacine, the world's largest Recent marsupial predator, occupies a special position within cryptozoology. On the one hand, it is known to have been extant in Tasmania until the 1930's, and few would be really surprised if it were rediscovered alive. It is thus in a class separate from the Yeti and the Loch Ness Monster. On the other hand, the many searches made for it, and the innumerable reported sightings, put it on a par with other "mystery" animals.

When I wrote a review on the species myself (Malcolm Smith, 1982, Review of the Thylacine [Marsupialia, Thylacinidae] *In* Michael Archer (ed.), *Carnivorous Marsupials,* Royal Zoological Society of New South Wales, Sydney), I naively supposed that I had covered nearly all the relevant literature. While this may have been true for the major scientific journals, I was unprepared for the enormous mass of information collated by zoologist Eric Guiler in the book being here reviewed—enough to fill a 14-page bibliography with 245 references. Many of these are not readily available outside Tasmania, let alone Australia. Guiler, for 35 years a lecturer at the University

of Tasmania, has produced a work for both the professional and the layman. His data are made all the more accessible by the extensive use of tabulation, an appendix, and an 8-page index.

Chapter 1 is an 8-page "Introduction." Of particular value to non-Australians is a description of the environment of Tasmania, a large island off southeast Australia. A map shows all the places mentioned in the text.

Chapter 2, "History of the Thylacine," begins with the fossil evidence, and speculation on the cause of its extinction on the mainland (probably due to competition with the dingo). It does not, however, describe the fossils in detail. From there, we are taken on a *tour de force* of early settler contact with the animal, its conflict with sheep farmers, the setting of a bounty on its head, the sudden collapse of its populations for reasons that are far from straightforward, and official protection after the last known individual had died in the Hobart Zoo in 1936. The chapter is enriched by numerous references to early colonial writings and parliamentary debates.

Chapter 3, "The Animal," does not claim to provide a detailed anatomical description, but all the characteristic features are covered, and references are provided for those seeking further details. The author does not enter the specialized field of dental morphology, and he skirts the minefield of taxonomic relationships. Probably, like 99 percent of us, he would be out of his depth there.

The most valuable section for the local cryptozoologist is a full description of the thylacine's spoor, and those of animals easily mistaken for it. A diagram and a tabulated guide to identification are included. Guiler himself claims to have seen unmistakable thylacine footprints on a few occasions, and, significantly, the chapter is written in the present tense.

Chapter 4, "Thylacines in Zoos," exemplifies the author's ability to dig out facts from private contacts and official—but very local—organizations. It is by far the most thorough review of the subject the reader is ever likely to find. I can state that I myself was only able to scratch the surface when researching my own contribution for *Carnivorous Marsupials.*

In Chapter 5, "Some Facts and Some Deductions," the author attempts to collate the data from all published and unpublished sources in order to speculate on the animal's ecology and behavior. Since the data are meagre, his interpretations are certainly open to challenge, but the critics will still need to rely on the information he has collected. It is highly unlikely that anyone else will be able to provide any alternative corpus of facts in the future.

Most significantly, Guiler describes how he himself has come across macropodid remains bearing signs of the thylacine's unique method of killing and eating, the latest in 1976. Not surprisingly, this chapter is also written largely in the present tense.

In Chapter 6, "The Van Diemen's Land Company and the Thylacine,"

Guiler again draws on material unpublished and unavailable elsewhere; i.e., the full records of two large sheep stations, reaching back more than a hundred years. Again, there are excellent tables providing much data.

Chapter 7, "Some Tiger Tales," consists of anecdotes culled from private letters, old newspaper reports, and personal interviews with "old timers." It is a mine of information that will never be found elsewhere.

It is the next two chapters that will be of most interest to cryptozoologists, however. Chapter 8, "Expeditions and Searches," covers, in 39 pages, the quest for the thylacine since it disappeared from public view. He provides a table of 64 sightings, complete with a reliability rating, and another table of 18 investigations, searches, and expeditions, many of which Guiler himself participated in. They are therefore described in some detail. The account given of the Broadmarsh sheep killings is more detailed than the original report (Eric R. Guiler and G. K. Meldrum, 1958, Suspected Sheep Killing by the Thylacine, *Thylacinus cynocephalus* (Harris), *Australian Journal of Science,* Vol. 20: 214–15), and the complete story is also provided of the thylacine allegedly killed in 1961. It differs somewhat from other published accounts. He also repeats rumours of several illegal captures of live thylacines. One was supposed to have been brought into Launceston, but then released before the authorities heard about it.

However, the overwhelming effect of reading about these expeditions is the realization of the tremendous amount of work (126,000 snare-nights in one expedition) undertaken for the most meagre results (a few clear footprints, and a few sightings by people who weren't even looking). It is enough to make a cryptozoologist throw up his hands in despair.

It is with Chapter 9, "Mainland 'Tigers'," that we move deep into cryptozoological territory. Here, Guiler faces the quandary that all serious investigators must face. It is extremely unlikely that the species could have survived on mainland Australia for so long without having been recorded by the early settlers—or known to the Aborigines. On the other hand, as he accepts 10 percent of the Tasmanian reports as valid, he cannot reject equally good reports from the mainland. He is therefore forced to admit that "there may be an animal, particularly in Western Australia, South Australia, and Gippsland, which does not fit the general description of animals known in the vicinity." In fact, the casts he examined of some South Australian footprints caused him to state: "I doubt if it was the footprint of any animal we know today."

Kevin Cameron's photos (Athol M. Douglas, 1986, Tigers in Western Australia?, *New Scientist,* April 24) are dismissed without actually being specified. Instead, the author provides more than a dozen reports of sightings, some very suggestive, from all over Australia, but mostly Victoria, which is closest to Tasmania. Many references are provided, but nearly all turn out to be undated newspaper articles or private correspondence received by

the author. It would have been better if they had been printed in full, in order to allow readers to judge for themselves. However, because of space limitations, we cannot blame Guiler on that score; it is to be hoped that full details will one day be published elsewhere.

Although nearly all the sightings were new to me, I was surprised to find that some already documented were not included. The reports compiled by Samela Harris in 1968 (Hold That Tiger! *Walkabout,* Vol. 34[6]: 28–31) were sketchy, but still worth repeating. Also, the creature sighted in southeast New South Wales on August 15, 1984, and reported at the time in the press, was an almost archetypal thylacine.

In the final chapter, "Retrospect and Prospect," Guiler states how, although the thylacine might be "the rarest of the world's mammals," he is constantly surprised by a common propensity to write it off as extinct. Instead, he comes up with an unexpected scenario. The thylacine, he points out, was rare in the early colonial period, but underwent a population explosion in the period 1875–86, followed by a crash about 1908. If this was a cyclic change, he suggests that, assuming the population hasn't fallen below recovery level, the thylacine should make a comeback in the same way the Tasmanian devil did.

It is traditional for the reviewer of a book to point out a few minor errors, if for nothing else than to demonstrate that he has been thorough. I noticed that one of the references in the text, Smith and Rounsvell (1980), has the authors' names reversed in the bibliography, while another, Mould (1984), has been omitted altogether. From the context, it was probably a personal letter. The Tasmanian coat of arms is said to have a thylacine as a supporter. In fact, it has two. Also, I would take issue with the statement that the first description of a thylacine, made by Lieut. Governor Paterson, was so vague that it could just as easily be applied to a Tasmanian devil. Paterson's beast had 23 stripes, and a head-body length of 53 inches, which puts its identity beyond doubt.

Slips like these, however, are of minor significance in a work of such excellence. Indeed, this volume is virtually unique in that the author has thoroughly researched a subject on which very little future research, if any, will be possible. It will therefore remain *the* definitive work on the species for a very long time—but not, we may hope, forever.

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The Beast of Exmoor: Fact or Legend? By Trevor Beer. Countryside Productions, Barnstaple, Devon, England, 1984. 48 pp. £2.00 (p.).

Since 1983, the Loch Ness Monster's hitherto undisputed position as Britain's premier mystery beast has been seriously threatened by an equally evanescent challenger, a challenger whose undiscovered identity and infamous reputation for farmstock killing has frequently provided dramatic headline material for newspapers not only throughout this country but also overseas.

I refer, of course, to the Beast of Exmoor—popularly conceived as a ferocious pantheresque creature stealthily prowling the rugged, secluded expanse of North Devon moorland and forest, and slaughtering the farming community's sheep on a mass scale. As revealed by the investigations of naturalist, artist, and acknowledged Beast authority Trevor Beer, however, this latter portrayal is a vast simplification of the multi-layered, myth-enshrouded phenomenon which truly constitutes the Beast of Exmoor.

Beer's booklet begins with a brief introduction to the Beast saga, and to his own Beast investigations—together with those of fellow Exmoor naturalist Nigel Brierley. A scene-setting description of Exmoor is also included, and serves well in underlining the author's intimate native knowledge of the region's wildlife and landscape. Following this is the main body of the booklet, divided into two sections of roughly equal length, and accompanied by 4 pages of black and white photographs.

The first section provides a personal account of Beast history from its public debut in the Spring of 1983—marked by a spate of sheep killings near a farm in the Drewstone area of South Molton—to the booklet's completion in November, 1984. Even today, this period still epitomizes the Beast phenomenon, thereby bestowing upon Beer's publication a unique historical significance. In this first section, the author describes his own investigations of some of the incidents (including Drewstone) which claimed such massive media attention, in particular his own close-range sighting of a leopard-sized cat with jet-black pelage, long body and tail, broad head, and powerful limbs.

The second section deals with the elusive identity of Exmoor's sheep killer, and, in a series of concise analyses, Beer covers a number of those put forward during the Beast saga. Such identities range from the strictly zoological, such as exotic escapee felids, rogue dogs, a strain of mutant feral, domestic cat which has attained abnormally large proportions, to the distinctly bizarre, such as eagles, phantom carnivores, and werewolves. Also included here are details concerning comparable mystery animals sighted elsewhere in Britain, but especially upon neighboring Dartmoor, together with evidence obtained from spoor and from the mode of killing exhibited by observed sheep carcases.

As I write this review, in 1988, the mystery of the Beast is still unsolved. Consequently, the author's booklet does not contain the answer. What it

does contain, however, is a body of material which succinctly and successfully does justice to his booklet's subtitle. It does this by separating fact from legend, and offering the reader a new and totally fascinating perspective of the Beast phenomenon. For example, his vast collection of reputable eyewitness reports of mystery creatures sighted on Exmoor, many at close range and in good viewing conditions, has enabled him to redefine the Beast's status from a numerical, taxonomic, and behavioral standpoint.

To begin with, as a result of having received reliable reports of large-sized mystery cats sighted at the same time but at totally separate localities on Exmoor, Beer can provide evidence for believing that more than one such animal inhabits Exmoor. Furthermore, although approximately 80 percent of the reports describe the all-black panther-like cat receiving heavy newspaper coverage, the remaining 20 percent or so of reports refer to a tawny-colored, puma-like form not previously publicized by the media. In short, not only does more than one *individual* mystery cat exist, but also more than one *type* of mystery cat. The author believes that these will most likely prove to be *bona fide* leopard and puma escapees from captivity. In view of the numerous reports of identical creatures from world areas as disparate as mainland Europe, Australia, and eastern North America, for example, this would certainly seem to be the most satisfactory explanation.

Most remarkable and stimulating of all, however, are the author's discoveries and conclusions drawn in relation to the identity of the Beast. For he demonstrates that there is increasing evidence to suggest that the mystery cats being seen are *not* the ones responsible for the killings. Amazing as it may seem, although leopard-like and puma-like cats have been observed chasing deer and killing rabbits on Exmoor, during the period covered by Beer's booklet (and indeed, to my knowledge, right up to the present time) no eyewitness account describing a sighting of any such animal actually killing a sheep—or even devouring the remains of an already dead carcass—has ever been brought to public attention.

Conversely, reports from the moors of such behavior as displayed by large dogs are on record. When the Drewstone episode took place, the author considered a rogue dog to be the most likely culprit; consequently, he was very surprised to spy the panther-like cat in 1984. Later, however, he saw various sheep killings known to be the work of dogs which appeared to be identical in killing mode to those examined by him at South Molton. And so, by the end of 1984, he was becoming ever more convinced that, although sizeable cat forms did exist on Exmoor, the animals actually responsible for the killings were dogs.

In more recent times, the problem of large dogs roaming the moors and slaughtering sheep has become increasingly recognized by the farming community there as a very real and serious threat, and many dogs have been shot while in the act of savaging livestock—all of which lends support to Beer's conviction. Beer also alludes to the apparent sighting by various locals

of mysterious vans on the moors at night, and accompanying rumors of very large lurcher-like dogs being deliberately released there by poachers.

So, is it possible that the mystery cats are being used as scapegoats, receiving the blame for kills actually perpetrated by pet dogs? Certainly, there is more than enough space and wild prey on Exmoor to satisfy a quite sizeable population of naturalized felids without the need arising for such animals to venture near human settlements and prey upon domestic livestock. This is not true of the mystery cat phenomenon throughout Britain; indeed, many reports from elsewhere in this country concern farmstock killed in an unequivocally feline manner. But, according to Beer's findings, this does not seem to be the prevailing state of affairs on Exmoor.

Since this publication appeared, new developments have occurred there. Most notable are the frequent reports of lynx-like beasts on Exmoor, supported in at least one instance by the discovery of fur later identified as lynx hair; the capture by Beer on film of a panther-like animal; the shooting on Dartmoor of an Asian leopard cat *Felis bengalensis*; and the discovery on Dartmoor of a leopard-like skull. All of which serves to demonstrate that the concept of exotic cat species being kept (presumably as pets) in the area and subsequently escaping (or being deliberately released?) onto the moors is by no means as unlikely as it may otherwise seem. Also, we should not forget to allocate in our scenario an adequate space for the inevitable hoax factor. Whether or not such animals are responsible for the killings, conversely, is clearly a very different matter indeed.

An essential addition to any cryptozoologist's library, in a mere 48 pages and via a relaxed, almost conversational style of writing, Beer's booklet serves admirably not only in distinguishing fact from fiction, but also in exposing a hitherto unrealized dimension to the Beast story—one in which the real Beast may well be very different from its popular portrayal. Indeed, were it not for the fact that this is a real saga, one may be forgiven for wondering whether it has been lifted directly from the pages of Sir Arthur Conan Doyle's acclaimed Sherlock Holmes novel *The Hound of the Baskervilles*—itself set on nearby Dartmoor. In that tale, the animal in question was merely the quadrupedal tool of a much more cunning, bipedal beast. An instance of fiction preceding fact?

Whatever the answer, one thing is certain. It is quite ridiculous and irresponsible to adopt the attitude of many media reporters and zoologists in the past, and derisively dismiss the sheep-killing Beast of Exmoor as a figment of lurid imagination. For, as Beer so judiciously points out: "*The sheep did not commit suicide.*"

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The Enigma of Loch Ness: Making Sense of a Mystery. By Henry H. Bauer. The University of Illinois Press, Urbana & Chicago, 1986. 243 pp. $22.95 (c.); 1988, $9.95 (p.).

From his vantage point as a former Dean of the College of Arts and Sciences at Virginia Polytechnic Institute and State University and professor of chemistry and science studies, Henry Bauer has approached his chronicle of the evidentiary pros and cons as to the presence of large underwater animals in Scotland's Loch Ness much as, I presume, he would introduce his students to the realm of scientific areas not yet fully discovered or documented.

Supplementing as complete a literature survey and bibliography as has yet been compiled on this subject, Bauer takes the reader on a journey exploring myth and reality; hard, soft, questionable, and unreliable evidence; multiple possibilities of interpretation and illusion in so-called evidence; hoaxes and frauds; and bad and good reasons for belief and disbelief.

Bauer points out that persons of no one background at Loch Ness can claim this quest as their individual monopoly. Those few who are professional or accomplished physical and applied scientists have delved at the risk of ridicule, and but briefly; and, in their own view, inadequately. There is also a vast army of interested observers from all other walks of life, all of whom have opinions based on their respective educational and experiential backgrounds, and all too many of whom, unfortunately, have crowned themselves with a pseudo-scientific aura, proclaiming themselves high lord executioners and official authorities.

And it is into this apple and orange mix that Bauer takes the reader, effectively pointing out—not apologetically, but factually—that professional scientific study has been mainly in an avocational setting, with unanticipated surprises and generally inadequate planning and expectations. And, until recent years, with lack of interest (and, indeed, sometimes hostility) on the part of many fellow professionals, some of whom have denied the cryptozoological approach, and some of whom have even denied a scientific publication forum.

The Bauer task, however—so well achieved in this important volume—is neither to prove nor disprove Nessie's existence. His concern, to the contrary, is much more to do with an examination of the sociological, psychological, and philosophical aspects of the controversy. How do we acquire our beliefs? How are they affected by our biases and illusions? How should science ideally approach this problem? What would be required for such an enterprise? The latter questions are clearly not comfortable ones for those unwilling even to imagine cryptozoological possibilities.

As expressed by the author, his hope is "to illuminate why the matter has been controversial, and why the controversy has persisted, and why science has remained disengaged and, at least implicity, what must happen if science is to become engaged."

The reader is left by this balanced presentation of the history of the subject to make up his or her own mind in the light of the views of the proponents who accept the possibility of large unknown animals in this very deep, 25-mile-long lake, and the counter views of the debunkers, who find everything that has been reported to be otherwise explainable—and the existence of Nessie to be only in the realm of human fantasy.

The book's dust-jacket ably characterizes its contribution: "While most authors have focused upon the search for Nessie, Bauer is the first to present a detailed and balanced look at the history of the controversy surrounding the search." Of interest too is the dust-jacket's quote on the book by ISC's own George Zug, chief curator of herpetology at the Smithsonian Institution: "Personally, Bauer has made me better aware of why I react the way I do when confronted by Nessie-type topics."

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The Loch Ness Monster: The Evidence. By Steuart Campbell. The Aquarian Press, Wellingborough, England, 1986. 128 pp. £3.99 (Sterling Publishing, New York, n/p) (p.).

Steuart Campbell has certainly done an efficient hatchet job on the idea that the Nessie phenomenon could represent a relict population of plesiosaurs, archaic whales, or whatever—let alone giant invertebrates, which he does not even mention. He is less convincing on proving that Nessie does not exist at all.

The book is substantially a critique of Roy Mackal's *The Monsters of Loch Ness* (Swallow Press, 1976), with its extensive analysis of the data, and it is in demolishing many of Mackal's conclusions that he is most successful. But to prove the null hypothesis, that Nessie does not exist at all, calls for more than the demolition of Roy Mackal. To start with, Campbell selects an arbitrary definition of Nessie: that it is an unknown aquatic animal. The thesis that Nessie observations result from gigantism in *known* animals, eels for instance, must also be rebutted. Of course, there is no more evidence that gigantic eels live in Loch Ness than there is that freshwater plesiosaurs live there. But at least gigantism in known animals does not necessarily assume the existence over thousands of years of a small breeding population in the loch, as eels are well known to migrate in and out of freshwater lochs.

However, we should be grateful to Campbell for what he has demonstrated; that is, the extreme unlikelihood—he would say impossibility—of a breeding population of large, unknown animals existing in the loch. His strongest arguments are in the disposal of the photographic, film, and sonar evidence. Most of this is clearly inconclusive, to say the least, but a question mark still remains over the famous Dinsdale film of 1960.

I would be happier about his critique of the assessment by the RAF's Joint Air Reconnaissance Intelligence Center (JARIC) of the Dinsdale film if I could know what JARIC's response might be. How important is it, for instance, that the surface of the loch is 16 meters above sea-level, and not *at* sea-level, as JARIC apparently assumed? He draws attention to the important negative evidence that at least one complete sonar sweep of the loch from end to end found nothing corresponding to Nessie. And one must accept that the burden of proof that the Academy of Applied Science underwater photographs represent any kind of animal now rests firmly on the Academy.

Campbell's weakest point is his wholesale rejection of all visual sightings. I fully agree that sightings by people who are not trained observers usually represent very dubious evidence, and probably all but a dozen or so of the 251 reports listed by Mackal, and certainly *all* the 18 land reports, are almost certainly due to mistaken observations by those unfamiliar with the fauna and other phenomena of the Highlands. But there remain a few sightings, mainly by local people—and I have myself spoken to some of these eyewitnesses—which cannot be so explained. Many, if not most, sightings are, I am sure, due to either wave effects or flotsam of one kind or another. I am less sure that many are due to otters, seals, or deer.

Visitors from the south or from urban areas might well mistake even a labrador dog swimming after a stick for Nessie, but the country people who live around the loch are familiar with otters, seals, and deer, and it is insulting to suggest that they would not recognize them for what they were. These local residents are at least as skilled observers of the larger local animals as are naturalists and zoologists, who, significantly, are among the least frequent recorders of Nessie. Indeed, some are more skilled than many scientists, who display a great tendency, when they cannot explain a sighting report, to say that "it must have been" some animal they are not themselves qualified to identify.

Thus, when Sir Brian Mountain showed his film to the Linnean Society in 1934, four members between them pronounced Nessie to be a seal, a whale, and an otter. Even professor Mackal accepted the Wilson (surgeon's) photograph of 1934 as a diving bird, which I, as a field ornithologist, assert categorically that it is not.[1]

[1] See Mackal's Comment on LeBlond addressing this topic elsewhere in this issue.—Editor.

Further, we surely cannot, as Campbell does, seriously propose mats of floating vegetation as an explanation for Nessie. This may well apply to lakes where there are sawmills, as in Norway, but at Loch Ness it is as far-fetched a speculation as are those on the plesiosaur and the giant mollusc.

Perhaps the least satisfactory part of the book is Campbell's assumption that, if Nessie does not exist, neither do any of the other "lake monsters" reported around the world. This cannot be sustained, if only because he does not analyze the reports from these other lakes at all. Many such reports are doubtless due to inanimate phenomena, as in Loch Ness, and others to mistaken identifications of various kinds of animals; but this must be demonstrated, not assumed.

So, we are still left with a mystery. What gives rise to the residual 5 percent or so of Nessie sightings? Why can Nessie be casually observed at long intervals of time, but not be consistently photographed, filmed, or recorded by sonar? Campbell quotes Ronald Binns as saying that, the more the loch is watched, the less Nessie shows itself. I myself stated much the same in my paper at the July, 1987, Nessie symposium held at the Royal Museum of Scotland, in Edinburgh: "One might almost conclude that taking part in an expedition to find Nessie militates against seeing the phenomenon which is under study."

Campbell claims this in support of his null hypothesis. But a negative is the most difficult thing to prove, and, indeed, is most often impossible. This is why we are all still struggling with the problem 55 years after Nessie burst on to the world stage. Shall we still be struggling in 2043?

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Sticking My Neck Out! By Nessie. By Edward H. Armstrong. Privately published, Langholm, Scotland. 1983. 39 pp. n/p. (p.).

The literature on Loch Ness and its Monster is substantial, and grows larger every year. Publications range from the most serious and scientific, the product of intensive scholarship and original research, to the most bizarre, and they come in a great variety of formats. This particular booklet consists of 22 double-sided A4 pages of duplicated typescript, and comes somewhat far down the list of serious contributions to the study of the Loch Ness problem. However, being privately published, and apparently with a very limited local circulation, it may previously have escaped the attention of many Loch Ness researchers. The author has two things he wishes to say,

one a reasoned attempt to estimate the size of Nessie from the 1934 Wilson photograph, and the second probably best described as light entertainment.

In brief, after a few preliminary pages describing Loch Ness and discussing possible reasons for the lack of firm evidence of Nessie, the author claims that certain prints of the familiar 1934 Wilson ("surgeon's") photograph show a "seagull" to the left of Nessie's neck, and that this gives a scale from which to calculate Nessie's dimensions. Although all previous attempts to measure Nessie from the Wilson photograph are now somewhat redundant, in view of the recent paper by LeBlond and Collins (Paul H. LeBlond and Michael J. Collins, 1987, The Wilson Nessie Photo: A Size Determination Based on Physical Principles, *Cryptozoology,* Vol. 6: 55–64), at the time this was, of course, a perfectly reasonable argument for Armstrong to advance. On the basis that the seagull has a wingspan of 2 feet, he concluded that Nessie's neck is about 7 feet long, and approximately 2 feet in diameter where it emerges from the Loch surface. (Note that LeBlond and Collins calculated a neck height of 4 feet.)

So far so good, but all this depends on whether or not a seagull is genuinely present in the photograph. The author goes to great lengths to explain the differences between the various possible prints and negatives. He identifies three negatives—numbers 2 and 3 being duplicates of number 1—made from either the original negative or a later print, and he also discusses, in some detail, which sources have been used for the numerous published versions.

Much of the author's booklet is taken up with simple repetition of already well-known sightings and quotations. These, and his rather extravagant style of writing, add nothing to his case, which centers quite simply on whether or not there is a seagull to be seen. He prints some startling enlargements of the photograph published in Constance Whyte's book *More Than a Legend* (1957), which, he claims, shows a seagull, but, to his credit, he also states quite frankly that other versions of the same photograph do not show this. He gives some ingenious reasons why this should be so, but the more obvious conclusion, that the "seagull" is a simple artifact which has crept onto the print in question, he does not apparently consider at all.

What are we to make of this, a seagull which nobody else has apparently seen during the past 50 years? The Scottish Natural History Library is fortunate in possessing virtually all the standard works on the Loch Ness Monster, often in several editions or later printings, and also has access to various other copies of the photograph in question. I have therefore had an unrivalled opportunity to examine all available versions of the photograph, including several copies of the actual print recommended by the author, and have done so both by the naked eye and also under high magnification. In none of these versions can I see anything bearing the slightest resemblance to a seagull; nor can anyone else whom I have consulted. Indeed, it is perfectly

clear that most photographs show nothing whatever in the place where the author indicates the seagull should be.

There is no doubt that the available copies of the Wilson photograph do vary, sometimes quite considerably. There is the well-known dot artifact (called the "floating cricket ball" by the author) to the left of Nessie's neck, which now appears in many, but not all, published versions. This is probably the most obvious artifact, but there are many more—dots, smudges, scratches, or other minor blemishes—and the explanation of the "seagull" seems only too clear. The irregular mark, which can, in the author's enlargements and to the credulous eye, appear to be not unlike the outline of a seagull, is nothing more than an irregular line artifact, probably a scratch, which has appeared on some print.

That minor damage can occur to photographs which have been repeatedly copied and reproduced over the years is very understandable, and can be well illustrated by examining the precise version to which the author refers, that in Constance Whyte's book. Over the area where the seagull is supposed to be, several copies I have examined show nothing at all, one shows a slight blemish, and another has an irregular line extending right back to Nessie's neck; clearly accidents in the copying or printing process. In the classic journalistic phrase, it's a pity to let the facts spoil a good story, but here the facts are quite simple—there is no seagull, merely an artifact.

With regard to the author's other theory, that Nessie is a swimming elephant—ingeniously illustrated with suitable drawings, including a cut-out from "daughter Melanie's Doll's cot"—this is entertaining, but perhaps the least said the better.

In short, a curiosity and an example of "Nessiana" for the collector, with a basically sensible theory about the seagull, which, unfortunately, is simply not borne out by detailed examination.

No price or place of publication is given in the booklet, but enquiry to the Dumfries Library (the staff of which the author thanks in his acknowledgements) gives the author's address as: Briershaw, Ewesdale, Langholm, Dumfriesshire DG13 0HJ, Scotland, from where copies could possibly be obtained.

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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.*

Fossil Ungulates and Archaeology: Two Kinds of Evidence

(Comment on Janis, *Cryptozoology,* Vol. 6: 8–23)

I would like to address briefly the interesting paper by Christine Janis on possible fossil ungulates depicted on archaeological artifacts. It seems that there are two kinds of evidence: one can be considered direct evidence, and the other indirect (or second-class) evidence.

The sivathere discussed by Colbert belongs to the former. No difficult interpretation of any sort is needed to establish the zoological identification of this statuette of the Sumerian period. On the other hand, the chalicothere from the Sakic culture (Siberia), the hyrax from China, and the *Mesembriportax* from East Africa need several reinterpretations to support the zoological identifications Janis provides. Atypical attributes such as the long tail of the chalicothere, the hooves of the hyrax (and general body morphology), and even the very stylized antelope, may be interpreted as "artistic license," as Janis herself states (p. 11) and emphasizes (p. 22). In other words, coincidence can be an explanation for these depictions.

This is not the case, however, with Colbert's sivathere, for which coincidence cannot be a reasonable explanation. Nevertheless, Janis's insightful article is a good addition to the dossier on Quaternary extinctions.

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A Review of Some Paleocryptozoological Hypotheses

(Comment on Janis, *Cryptozoology,* Vol. 6: 8–23)

Ancient images of animals, drawings or statuettes, discovered as a result of archaeological, historical, and ethnographic research, have long interested zoologists working on problems related to the evolution of recent fauna.

This specific interdisciplinary field also presupposes interest and knowledge of art history, and can provide useful information on the historical distribution of species. However, the interpretation of ancient images is a difficult task, and is frequently strewn with pitfalls; interpretations sometimes cannot be proven, and reality may give way to unchecked imagination.

For obvious reasons, ancient images of animals have become a subject of cryptozoological interest, and have led to "paleocryptozoological" hypotheses—the possibility that certain fossil species may have survived into periods much later than was earlier supposed.

Christine Janis's paper, "Fossil Ungulate Mammals Depicted on Archaeological Artifacts," deals with this problem. The hypotheses drawn in the paper are interesting. However, one cannot state that they are sufficiently convincing. Many of the images shown could have alternative interpretations, which I would like to address below.

On sivatheres: The flat and twisted antlers of the statuette from Kish (Iraq) remind one of the antlers of Mesopotamian fallow deer. However, on the whole, Colbert's statuette has an amazing resemblance to a sivathere, a fossil giraffid, and this argument indirectly supports the proposition of their persistence into the Holocene. Unlike Colbert's *Sivatherium,* however, the "*Bramatherium*-like" statuettes which Janis discusses are not as convincing, although the Egyptian "cow" is something that does draw one's attention. The presentation of the horns as a single growth, on the other hand, could be a form of stylization of some of the Egyptian breeds of long-horned cattle.

The medial cranial appendage of the Syrian "ram" may also simply represent a form of stylization, or perhaps it was required for practical considerations (sculpting techniques)—the shaping of two separate horns or antlers on small statuettes is complicated, and the horns break easily. The same could be said for the "Siberian stag." The antler-like structure on its head is indeed "*Bramatherium*-like," but it seems rather like a stylization of a Siberian roe deer—*Capreolus pygargus.* When we analyze an image in a case such as this, we should also undertake a more detailed study of the artistic traditions of the region involved.

On chalicotheres: The author's claim that the "fabulous wolf-like animals" depicted on Scythian treasures are probably chalicotheres is not very convincing. Similarity with chalicotheres could be found in the animals' claws and their somewhat horse-like appearance; however, the proportions of the body do not resemble those of chalicotheres. I see no other reason to link

the image presented by Janis with chalicotheres. Many similar Scythian ornaments depicting similar subjects are known. In fact, perhaps a number of other Scythian images deserve more attention from a cryptozoological perspective.

The explanation of a body combining ruminant features with claws requires a greater study of the social and religious nature of the Scythian-Sarmatian teratological style. The inherent symbolism of Scythian thinking, and the importance attributed to magic, could explain the creation of syncretic images of mythical creatures (A. Meljukov and M. Moshkov, eds., 1976, *Skifo-Sibirskii Zverinnii Stil v Izkoustve Narodov Evrazii* [Scythian-Siberian Teratological Style in the Art of the Peoples of Eurasia], Nauka, Moscow).

On the giant hyrax: The idea that the Chinese statuette (600–500 B.C.) represents a giant fossil hyrax is interesting. It is, however, still subject to debate. This "tapir-like" statuette could, in fact, simply represent a tapir. The head and the tail recall one small lagomorph, which still lives in Tibet—the pika, *Ochotona.* However, the limbs are much more like those of a tapir. As far as the Pliocene giant hyraxes are concerned (*Kvabebihyrax, Pliohyrax*), they probably followed a semi-aquatic way of life or lived near water basins; their morphology hardly resembled that of recent hyraxes.

On the Tji Wara: The case of the interpretation of this African antelope is very interesting indeed. Perhaps it is an oryx (oryx horns are slightly projected laterally, but to a lesser degree, and are practically straight), while the narrow, elongated snout is simply a stylization. Nevertheless, the parallel with *Mesembriportax* is an original hypothesis.

Articles like the one by Janis, however unusual the subject matter for zoology, are certaintly intriguing, and the interpretation of ancient images of animals, when done professionally, could lead to interesting zoological discoveries.

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PERSEPOLIS: A PUZZLING CASE IN ARCHAEOLOGICAL CRYPTOZOOLOGY

(Comment on Janis, *Cryptozoology,* Vol. 6: 8–23)

I read Janis's article with great interest, as I think in-depth research into ancient art will continue to provide evidence for unknown animals.

I would like to present here for Janis's evaluation a very puzzling case dealing with an ungulate mammal, a mammal which, incidentally, is now the logo of the ISC, and is thus the symbol of cryptozoology. In a palace in Persepolis (Iran), built in the time of Darius the Great, about 2,500 B.P., there is a frieze which surely represents an okapi, *Okapia johnstoni,* or, at the very least, a short-necked giraffid looking very much like an okapi: it is smaller—a scale is given by the men bringing the tribute to the monarch—and the shape of its ears is a little different from the well-known species from Central Africa (Steven C. Anderson, 1969, A Zoological Puzzle on a Persian Palace, *Pacific Discovery,* Vol. 22[5]: 11–15). Edwin H. Colbert, an authority on fossil vertebrates, also thought it was a true okapi.

As many readers know, the okapi was scientifically described from only a skin—and soon afterwards from a skull—in 1901. However, stories about the okapi had been recorded in Stanley's narrative of this expedition through Africa decades earlier.

How can one explain the animal represented on the Persian frieze? I see only two possibilities. The first is that the okapi was known to the Persian people—not directly, but probably through their relations with ancient Egypt. Excellent representations of Twide Pygmies in Egyptian art are available (Warren S. Dawson, 1938, Pygmies and Dwarfs in Ancient Egypt, *Journal of Egyptian Archaeology,* Vol. 24: 185–89), so that there is a link, thus: okapi/pygmies (okapi hunters)/Egypt/Persepolis. An okapi was certaintly a rare gift for a monarch, and we can hypothesize that one was taken back to Egypt from Central Africa by an Egyptian expedition or by African merchants who acquired it from the pygmies by barter. In addition, the geographical range of the okapi at that time may have been broader than it is now.

The second possibility is that the animal may have been another species of okapi-like girafid having survived into historical times in Iran. As a sivathere is believed to have survived until the Bronze Age in a not-so-distant region (Iraq), according to Edwin Colbert and Christine Janis herself, this second hypothesis is not unlikely, though I prefer the first one, which is at least supported by archaeological evidence.

In either case, Janis's opinion on this puzzle in archaeological cryptozoology would be welcome.

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Hurrah for Hyraces!

(Response to Tassy, Spassov, and Raynal)

Both Tassy and Spassov rightly point out that none of my archaeological artifacts carry the conviction of Colbert's sivathere depicted on a Sumerian chariot ring. In response, I can state that I gave Colbert's paper the clear priority as having been my inspiration for embarking on this particular project, and I pointed out that my additional observations were "fun examples" of other possibilities, rather than cases that bore the same degree of cryptozoological evidence.

The "additional sivathere" examples that I gave would have carried little or no weight had it not been for Colbert's unequivocal example, and I merely cited them as "other possibilities" in light of the existing cryptozoological evidence for the possible survival of the sivathere into historical times. Likewise, my "possible chalicothere" would have carried much less weight in the absence of other cryptozoological evidence for the possible recent survival of chalicotheres (e.g., the Nandi bear). Although there are problems with the interpretation of the supposed chalicothere artifact, and I would not presume to be acquainted with the mythologies and artistic styles of the Scythian culture, my attention was attracted to this particular artifact because—as explained in my paper—the other animals depicted on similar artifacts from the period were all clearly-recognizable living animals (horses, foxes, tigers, etc.).

Thus, I was not selecting one possibly recognizable animal out of an array of probably mythological beasts, and I thought that my explanation was at least more entertaining than the explanation in the British Museum catalogue of a "fabulous maned wolf" (this seemed a little lame to me, although perhaps the author knew something about cultural myths that I didn't).

I will not comment further on the possible extinct antelope *Mesembriportax*, as I myself did not feel that this was a particularly strong example—merely an additional possibility, as the statue happened to be sitting in my dining room when I was writing the paper! However, I must respond to the comments about the giant hyrax. Tapirs are not known in China either, nor have they been in that area since the Pliocene. Tapirs have small eyes, small ears, and a long snout. As someone who used to wake up each morning to a hyrax sleeping on my chest, I can assure readers that the head of this statuette cries out "hyrax!" It possibly does look a little like a pika (*Ochotona*), but the general body proportions suggest an animal the size of a large pig (i.e., 100–200 kg), and there are no known giant pikas in the fossil record.

The whole point of thinking that this animal might have been *Pliohyrax* is that it did not have the body proportions of a living small hyrax. Instead, this artifact obviously represents a much larger animal (explaining the less flexed limb posture) that was semiaquatic (because of the barrel-shaped

body). A number of living mammals have convergently acquired this type of body proportions in conjunction with a semiaquatic lifestyle and a body weight of around 100–200 kg, including tapirs, pygmy hippos, and capybaras (South American rodents). The skeleton of *Pliohyrax* has not been recovered, but semiaquatic habits have been inferred because of the relatively high position of the orbits. The size of the skull suggests an animal of a body weight of around 90 kg. In fact, the body proportions of this statuette are exactly what one would expect from a semiaquatic mammal of this size: if they had resembled that of a living hyrax, I would have seen no reason to ascribe this artifact to *Pliohyrax* (and there is no reason to suspect that the head would not have retained a hyrax-like appearance even with a difference in lifestyle).

The only possible anomalous feature of this artifact is the hooves, which look like a compact horse-like single hoof rather than the nail-like individual hooves of hyraces. Yet, it is quite logical to suppose that a much larger-sized hyrax, which would be bearing more weight on the feet, would have a more compact type of foot structure with a pad underlying the foot (compare, for example, the feet of a pig and the feet of a hippo). Thus, the feet of *Pliohyrax* might have borne a greater resemblance to the foot depicted in this artifact than to that seen in the small living hyraces. Combine this evidence with the evidence of geography and the fossil record, and I think that this cryptozoological example actually comes close to approaching the value of Colbert's sivathere.

The possible okapi depicted at Persepolis, mentioned by Raynal, is clearly a giraffid of some sort: the body proportions (front legs longer than back legs), the long neck, and the shape of the head (domed forehead with a rather roman-nosed appearance), clearly resemble a giraffid rather than a short-horned antelope. My problem with Raynal's preferred explanation is an ecological one. Okapis are highly specialized feeders: they do well in zoos today, but only if they are fed a very special diet. (For example, the Bristol Zoo okapis are fed entirely from imported oak bush leaves.) It seems unlikely that okapis could ever have survived in the Egyptian or Persian regions, as their preferred diet would not have been available. Even if one had been captured in the Congo and presented as a gift, it is unlikely that it would have survived the journey—let alone have been kept in captivity in Egypt or Persia—in the absence of a suitable diet.

As to Raynal's record possibility, the comparison with the sivathere is misleading, as the geographic distribution of sivatheres, and their skull and dental anatomy, suggests that they were much less specialized feeders. However, I think that this possibility—the survival of another giraffid species in that area into recent times—is the more likely explanation.

I showed the Persepolis depiction to my colleague Nikos Solunias at Johns Hopkins University, who is an expert on giraffe evolution, and he suggested

that the depiction might be a representation of *Paleotragus rouenii.* This animal was known to have lived in western Turkey about 7 million years B.P. (late Miocene), and was also much smaller than the okapi, about the size of a mule deer—thus in better accordance with the size of the animal on the Persian frieze. Solunias noted that the legs are rather too short for *Paleotragus,* but they are also short in comparison with the living okapi. Unlike the okapi, the dental microwear of *Paleotragus* suggests that it was a mixed feeder, and thus more likely to survive in captivity on a diet of whatever was fed to early domestic animals.

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A REEVALUATION OF *GIGANTOPITHECUS*

(Comment on Krantz, *Cryptozoology,* Vol. 6: 24–39)

Krantz argues that, based on his modeling, *Gigantopithecus blacki* is a large bipedal hominid, and thus is a good candidate for the reported Sasquatch. His reasoning follows that of most of the other researchers when confronted with the massive jaws and teeth of *G. blacki,* who have concluded that the species was a very large primate. As most Sasquatch reports have involved a large animal resembling a primate, *G. blacki* has been considered a likely candidate.

Krantz continues this line of reasoning further by modeling the head of *Gigantopithecus,* but with a bias towards a hominid jaw suspension that leads him to a bipedal reconstruction.

There are several problems with this approach. The modeling should have considered alternatives, including a reconstruction based on a pongid line. Initial modeling attempts by the senior author suggest that a reasonable case can be made for fitting *G. blacki* jaws to an ape jaw suspension, either gorilla or, preferably, an orang-utan. This reconstruction would tend to lean towards Simon's large knuckle-walking reconstruction of *Gigantopithecus* (Elwyn Simons and P. C. Ettel, 1970, *Gigantopithecus, Scientific American,* Vol. 222: 76–85).

Dental evidence suggests closer affinities of *Gigantopithecus bilaspurensis*—and thus *G. blacki*—to *Pongo* (orang-utans), with a joint lineage deriving from *Sivapithecus* around 12 million years B.P., with a further separation of *Pongo* and *Gigantopithecus* around 10 million years B.P. The African ape/human lineage diverged from the *Sivapithecus* lineage around 12 million years B.P., and further split from each other at some later date—a topic still hotly debated (Peter Andrews and Jack E. Cronin, 1982, The Relationships of *Sivapithecus* and *Ramapithecus* and the Evolution of the Orang-utan, *Nature,* Vol. 297: 541–46.; Leonard O. Greenfield, 1980, A Late Divergence Hypothesis, *American Journal of Physical Anthropology,* Vol. 52[3]: 351–65; Richard F. Kay, 1981, The Nut-Crackers—A New Theory of the Adaptations of the Ramapithecinae, *American Journal of Physical Anthropology,* Vol. 55[2]: 141–52).

Obligate bipedalism has appeared only in the Hominidae within the order Primates. There is no other indication of bipedalism as a terrestrial mode of locomotion in any other lineage of fossil primates. Postcranial evidence for any of the *Gigantopithecus* species is nonexistent. Previous arguments for bipedalism and other hominid characteristics of *Ramapithecus* raged through the literature in the 1950's and 1960's, until the recent (1982) discovery of GSP 15000 (*Sivapithecus/Ramapithecus*), which has a very close resemblance to an orang-utan (Mark Weiss and Alan E. Mann, 1985, *Human Biology and Behavior: An Anthropological Perspective,* 4th edition, Little, Brown, Boston, pp. 205–14).

This should caution against making grand, sweeping pronouncements without evidence to back them up. The paleontological evidence, such as it is, links *Gigantopithecus* with the orang-utans, and thus away from any lineage tending towards bipedalism. While this does not preclude a separate development of bipedalism in that lineage, there is no evidence at this time to argue for it.

If bipedalism developed in the hominids in a marginal or interfacing environment, then later adaptations led them towards the more open areas rather than towards the deep forests. Bipedalism, regardless of the selective pressures leading to it, certainly preadapted the early hominids to a life in the open. It isn't known when or in what environment bipedalism actually appeared in the hominid lineage, but its first appearance in the fossil record was with *Australopithecus afarensis* around 4 million years B.P. in East Africa, and is associated with a more open or at least patchy environment.

The *Gigantopithecus blacki* specimens are all associated with a forest-dwelling fauna (the *Ailuropoda-Stegodon* Fauna), which includes the giant panda. While the argument is a backward one, it is interesting to note that none of the forest-dwelling apes, either African or Asian, is bipedal. This would tend to suggest a quadrupedal mode of locomotion for *Gigantopithecus* rather than a bipedal one, moving away from Krantz's reasoning towards the bipedal Sasquatch.

The *Gigantopithecus* reconstruction and casting in the role of the Sasquatch hinges on the assumption that Krantz and almost all other researchers who deal with *Gigantopithecus* have made: that *Gigantopithecus* was a huge animal. While admittedly the teeth are huge, so are the teeth of the robust

australopithecines. A specimen of *A. boisei* scaled up based on teeth alone would produce a size significantly larger than it really was. However, there is a reasonable amount of postcranial fossil evidence to allow estimates of the size of the robust australopithecines. A cast of the Peninj (Natron) *A. boisei* mandible will easily fit over the outside of either of our own jaws, yet the primate was probably about 5′6″ by the current reconstructions. Large tooth size does not necessarily make a large body size.

The same reasoning can be applied to giant pandas, which have larger teeth than the more common *Ursus* sp. The difference in all of these cases is related to diet. It is just as plausible to reconstruct *Gigantopithecus* as a gorilla-sized ape with an extreme dietary specialization adapted to life in the bamboo forests of Asia, as a huge super ape with a more omnivorous diet. It would be interesting to compare the wear patterns of *Gigantopithecus* teeth to those of giant pandas and gorillas—both of which are bamboo eaters—which might give a clue as to the diet involved.

The modeling by Krantz, while interesting, does not answer the question of the origins of the Sasquatch. The evidence based on jaws and teeth alone can just as easily be argued towards the pongid line rather than the hominid line. Secondly, a satisfactory case has not been made for the extreme size estimates of *Gigantopithecus*. The primate may have been a megadont ape that ate bamboo or some similar tough vegetation, rather than a very large animal with more normally proportioned teeth for body size. Unfortunately, these questions cannot be answered without the postcranial fossil evidence that is currently lacking.

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A PROBLEM OF GIGANTIC PROPORTIONS

(Response to Buckley and Shelton)

Most of the points raised by Buckley and Shelton were considered when I made my reconstruction of the *Gigantopithecus* skull. We appear to have three major areas of disagreement that merit some discussion: posture, size, and affinities.

Posture was dictated quite simply by the enormous spread of the horizontal rami of the preserved fossil jaws. The reduction of the sectorial complex also indicates widely spaced condyles that alter the direction of jaw movements and canine occlusion. Anteriorly placed cervical structures would require just this great spread; no other explanation has been offered for it. I did indeed consider reconstructing the jaw along more pongid lines, but anatomical incompatibilities invariably resulted.

Size follows from that of the jaw, not the teeth. Tooth-to-body ratios vary greatly, depending on the types and uses of the dentition. The preserved jaw itself, however, cannot be reconstructed any smaller than I did without making it mechanically unworkable. Wu's reconstruction, and two others I've seen, are slightly larger than mine. No australopithecine jaw even remotely approaches the *Gigantopithecus* one in size. (Incidentally, the Peninj jaw mentioned by Buckley and Shelton that "will easily fit over the outside of either of our own jaws" actually matches almost exactly when inverted and set against the lower edge of my jaw.)

The affinities of *Gigantopithecus* are undemonstrated, and authorities are divided as to whether it is closer to the Pongidae or the Hominidae. I happen to lean toward the latter opinion, that of Weidenreich, Wu, Robinson, and Eckhardt. Of course, if *Gigantopithecus* is closer to the orang-utan, as Buckley and Shelton assert, then its reduced incisors and sectorial complex are unique for that affiliation. If an orang-utan relative could have such unusual teeth, such a great jaw divergence, and such an enormous size of mandible, then postulating erect bipedalism as well should not be too hard to accept. Yes, *Gigantopithecus* might be a member of the orang-utan clade, but assigning it to the African ape clade (along with hominids) would, in my view, involve fewer inconsistencies.

If anyone considers the issue worth pursuing, I urge them to build an actual reconstruction according to their own interpretations, which may then be compared directly with mine. The only requirements are biological consistency with the actual fossil, and mechanical effectiveness. How much leeway there is within these parameters remains for the reconstructors to discover for themselves; I found almost none.

Yet, this all may be an exercise in futility—as the final observation by Buckley and Shelton indicates—without the "evidence that is currently lack-

ing." But if we had that evidence in hand, we would probably only speculate on some other aspect that is still unknown.

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EVIDENCE FOR THE *TZUCHINOKO* EQUIVOCAL

(Comment on Dethier and Dethier-Sakamoto, *Cryptozoology,* Vol. 6: 40–48)

Dethier and Dethier-Sakamoto evaluate the various sources of evidence pertaining to the existence of the *tzuchinoko,* and present the hypothesis that it may involve a species distinct from the well-known *mamushi, Agkistrodon halys.* Their evidence for this comes from accounts given by local people, including snake hunters, and from certain morphological traits. While they take a healthily skeptical view of much of the more questionable information, they remain relatively uncritical of certain statements that are zoologically problematic.

As the authors discuss, the presence of a facial pit, triangular head, short thick body, and other features, unambiguously support the *tzuchinoko*'s identification as a crotaline, and almost certainly as a member of the genus *Agkistrodon.* The traits cited by Dethier and Dethier-Sakamoto as reminiscent of the Viperinae (we here follow Richard A. Seigel, Joseph T. Collins, and Susan S. Novak [eds.], 1987, *Snakes, Ecology and Evolutionary Biology,* Macmillan, New York, in applying subfamilial rank to the Crotalinae and Viperinae) in no way conflict with those cited for crotalines, as none are exclusive to the Viperinae.

After evaluating the evidence and lines of reasoning put forward by Dethier and Dethier-Sakamoto we propose, however, that these are not incompatible with the identification of the *tzuchinoko* as a subspecific, racial, or ecomorphic form of the *mamushi.* Indeed, the authors themselves allow this as one possibility, but appear to favor the hypothesis that the *tzuchinoko* is a distinct species.

After filtering out what are clearly exaggerations or embellishments on the part of observers, Dethier and Dethier-Sakamoto indicate that the remaining evidence points to a snake very much like *Agkistrodon halys,* except with respect to body proportions and color pattern. Here, however, uncritical evaluation of biologically significant data renders the final conclusions suspect. In reporting on a young specimen found by a girl in 1938, Dethier and Dethier-Sakamoto outline its dimensions as being 40 cm in length, 7–8 cm

in diameter, and weighing 3–4 kg. This is clearly an exaggeration, although the authors make no comment on this.

Calculation of weight by estimating volume of a simple cylinder (density $\cong$ 1.0 for vertebrate whole bodies in general, 1.05–1.16 for humans; Ellen Kreighbaum and Katherine M. Barthels, 1981, *Biomechanics: A Qualitative Approach for Studying Human Movement,* Burgers Publishing, Minneapolis) yields a weight of 1 (triangular body section)–2 kg (circular section), and these in turn are overestimates as the dimensions of the cylinders are based upon maximal diameter and do not allow for natural body taper. By comparison, a 40-cm rattlesnake has an approximate weight of 26.8 g (Lawrence M. Klauber, 1972, *Rattlesnakes: Their Habits, Life Histories, and Influence on Mankind,* Vol. 1, University of California Press, Berkeley).

This example may be symptomatic of exaggerations contained in the accounts of *tzuchinoko* observers. Nonetheless, the people involved clearly witnessed an animal that they recognized as distinct from *Agkistrodon halys,* independent of any misinterpretations or exaggerations on their part. It must be remembered, however, that *A. halys* is an extremely variable species in regard to both color pattern and body proportions (Leonhard Stejneger, 1907, Herpetology of Japan and Adjacent Territory, *U.S. National Museum Bulletin,* Vol. 58: XX + 577 + 35 pls.), and that folk or lay taxonomies are often more sensitive to these features than are those developed by systematists. Thus juveniles, males, and females of the same biological units may be recognized as distinct "taxa" by those people who most frequently encounter the animals in the wild. A case in point is that of the *kou-prey* and the *kou-proh* of northern Kampuchea (Cambodia), recognized by locals as two distinct kinds of wild oxen. When investigated by zoologists, it was found that the *kou-prey* is the calf of the *kou-proh, Bos sauveli* (Bernard Heuvelmans, 1958, *On the Track of Unknown Animals,* Hill and Wang, New York).

For the *tzuchinoko,* the case for the recognition of it as a separate "taxon" may really reflect the existence of a rare montane ecomorph, or a recent polymorphism affecting several morphological traits. Alternatively, sightings of the *tzuchinoko* may refer to gravid females of *Agkistrodon halys.* Information concerning the seasonality of *tzuchinoko* sightings would be useful in further consideration of this possibility.

In addition to the points raised above, we present information that may shed light on several of the statements made by Dethier and Dethier-Sakamoto. In support of the claims of jumping in the *tzuchinoko,* we cite the fact that escape leaps are relatively common in limbless reptiles (Aaron M. Bauer, 1986, Saltation in the Pygopodid Lizard, *Delma tincta, Journal of Herpetology,* Vol. 20: 462–463), and most notably in the viperid *Bitis caudalis* (Carl Gans and Heinrich Mendelssohn, 1972, Sidewinding and Jumping Progression of Vipers, *In* A. deVries and E. Kochva [eds.], *Toxins of Animal*

and Plant Origin, Vol. I, Gordon and Breach, New York). Although there are no previous literature records for jumping in *Agkistrodon,* it is certainly reasonable to assume that such behavior might occur.

The reported habit of jumping is, however, in mechanical conflict with Dethier and Dethier-Sakamoto's assertion that the tail of the *tzuchinoko* lacks vertebrae. Thrust from the surface is generated through the tail, and it is reasonable to assume that the caudal vertebrae and associated muscles would provide the lever system for the generation of this thrust. Caudal vertebrate would also be expected in a snake that is "able to hang from tree branches by its tail" and "stand erect on its tail before attacking." While the veracity of these behaviors cannot be evaluated at present, their acceptance by Dethier and Dethier-Sakamoto is in direct conflict with the structural attributes ascribed to the tail of the *tzuchinoko.*

The sightings of a *tzuchinoko*-like animal in New Guinea must be treated as being very doubtful. There are no known crotalines of any kind in New Guinea or on any of the Indo-Australian islands east of Sulawesi. While this area is unquestionably incompletely explored, the absence of pit vipers in the region is undoubtedly real, and dependent upon the historical biogeography of the Indo-Australian archipelago. The extent of crotaline distribution is coincident with that of many other organisms, and approximates the faunistic patterns designated by Wallace's line or Weber's line (Wilma George, 1962, *Animal Geography,* Heinemann, London). It appears that *tzuchinoko* sightings outside of the Palearctic and Oriental regions are attributable to the transference of characteristics (familiar to Japanese observers) to unfamiliar animals (probably viperines or even boids).

Regardless of any specific criticisms of their paper, we are pleased to see Dethier and Dethier-Sakamoto's contribution on the *tzuchinoko.* Far too often, smaller vertebrates, such as most amphibians and reptiles, are omitted from the mainstream of cryptozoological investigation, ostensibly because their reduced size and secretiveness in most cases make them less obvious (and less spectacular) than such cryptozoological mainstays as "lake monsters" and wild hominids or hominoids. While the track of a small unknown animal may not be as rugged, or (arguably) as potentially rewarding as that of larger forms, it is more likely to lead to a solution or resolution of the problem at hand. Smaller animals are more likely to survive than larger forms in even small patches of suitable habitat. They are also more likely to escape detection, and are more likely to be able to maintain viable population sizes by virtue of their smaller resource requirements.

The cryptozoological claim that not everything has been described nor every habitat explored thoroughly is brought home by reference to reptiles. In Australia, for example, the known number of reptile species grew from 339 in 1959 (Allen Keast, 1959, The Reptiles of Australia, *In* Allen Keast [ed.], *Biogeography and Ecology in Australia,* Junk, The Hauge) to 680 in 1986 (Harold G. Cogger, 1986, *Reptiles and Amphibians of Australia,* 4th edition, Reed Books, Frenchs Forest, New South Wales). Even in such a heavily populated area as California, nine new taxa of amphibians and reptiles have been discovered in the last five years (Theodore J. Papenfuss, personal communication).

Although very few of these animals are ethnoknown (J. Richard Greenwell, 1985, A Classificatory System for Cryptozoology, *Cryptozoology,* Vol. 4: 1–14) or are otherwise of cryptozoological interest, there are notable exceptions (Aaron M. Bauer and Anthony P. Russell, 1986, *Hoplodactylus delcourti* n. sp. [Reptilia: Gekkonidae], the Largest Known Gecko, *New Zealand Journal of Zoology,* Vol. 13: 141–148; 1987, *Hoplodactylus delcourti* [Reptilia, Gekkonidae] and the Kawekaweau of Maori Folklore, *Journal of Ethnobiology,* Vol. 7: 83–91).

Rightly or wrongly, many scientists outside the discipline of cryptozoology are generally unreceptive to, or skeptical of, much of the data supporting the existence of, for example, the Loch Ness Monster, Sasquatch and Mokele-Mbembe. This doubt stems principally—and perhaps rightly—from the circumstances in which dozens or even hundreds of sightings of these large animals have failed to yield an unambiguous photograph or a single specimen, or even part of a specimen. While we do not accept these negative results as justification for the cessation of cryptozoological work on large animals—witness the recent capture of an Onza (Onza Specimen Obtained—Identity Being Studied, *The ISC Newsletter,* Vol. 5[1]: 1–6)—we feel that "microcryptozoological" research provides a greater chance of successful identification of unknown animals. As a consequence, such work may be instrumental in justifying to skeptics the methodology and aims of cryptozoology, and in establishing the discipline as a recognized scientific activity.

The contribution of Dethier and Dethier-Sakamoto on the *tzuchinoko* is one example of such an approach.

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The *Tzuchinoko*: A New Hypothesis

(Response to Bauer and Russell)

We found the comments by Bauer and Russell on the *tzuchinoko* very interesting, especially coming from professional herpetologists, which, to be quite honest, we are not. Nevertheless, we would like to draw attention to several points. First, besides the fact that we are not herpetologists, we could hardly refer to a book published in 1987 in a paper first submitted for publication at the end of 1986. Second, we do not have a particular preference for either of the two proposed hypotheses.

The testimony of the little girl is cited, among others, without any analysis in that particular section of the article. It is, indeed, regrettable that we did not treat the matter further in our later evaluation. We do know that *Agkistrodon halys* is a very polymorphic species, and we by no means rejected the hypothesis of a rare ecomorph (see p. 47).

We do not rule out the jumping capacity of the *tzuchinoko,* but we still consider reports of 2–3-m leaps on flat ground to be probable exaggerations. In this connection, it is obvious that we have not been successful in explaining the lack of vertebrae in the tail. What we were really trying to emphasize, however, was the hypothesis that, in the tail of the *tzuchinoko,* the vertebrae are fused in a style or "terminal body," as is the case in crotalid snakes.

Observations of a snake evoking the *tzuchinoko* in New Guinea need further investigation. In the meantime, there is another hypothesis that we completely neglected, and that was not considered by Bauer and Russell either: that reports of the *tzuchinoko* (or at least some of the reports) may stem from the accidental introduction of a foreign species into Japan in shipments of lumber from Southeast Asia. We are seeking further information on this possibility.

We entirely agree with Bauer and Russell when they point out the importance of searching for small and discrete unknown animals. If they are not ethnoknown, however, they are, of course, outside the scope of cryptozoology. In any case, the adjective "microcryptozoological" seems superfluous to us.

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The Wilson Photo: Bird Explanation Now Untenable

(Comment on LeBlond and Collins, *Cryptozoology,* Vol. 6: 55–64)

I read with great interest LeBlond and Collins' independent estimate of size of the object appearing in the principal 1934 Wilson photograph. In my own analysis of both photographs, published in my book *The Monsters of Loch Ness* (Swallow Press, 1976), I concluded that the object in question was a diving bird. This conclusion was based on my own observations of various species of diving birds at Loch Ness, especially around Invermoriston, where Lt. Col. Wilson allegedly took the photographs.

Although the first photograph (the "classic" one usually referred to) does not look very much like a bird, the second (or, according to some, the first) one, taken a few seconds later, does indeed look like some of the fishing cormorants which I have observed. The bird's body is mostly submerged, so one sees only a head and neck. Without a comparative scale to judge distance, I found it very easy to imagine that I was seeing a great head and neck very far away, when, in fact, I was observing a small object fairly close by. This illusion is enhanced when there is an early morning haze or fog over the loch.

In light of of the recent work by Steuart Campbell (1984, The Surgeon's Monster Hoax, *British Journal of Photography,* April 20), and the new paper by LeBlond and Collins, my identification of the object in the Wilson photographs as a bird is now untenable. I now accept what many others have proposed all along: that at least one of the Wilson photographs represents a large unidentified animal in Loch Ness.

This conclusion is supported by Richard Greenwell's comparison of the Wilson and Mansi (Lake Champlain) photographs (J. Richard Greenwell, 1987, Tracing Monsters, *Cryptozoology,* Vol. 6: 137–140), which I find compelling, especially since the relatively large size of the object in the Mansi photograph has also been independently established (Paul H. LeBlond, 1982, An Estimate of the Dimensions of the Lake Champlain Monster from the Length of Adjacent Wind Waves in the Mansi Photograph, *Cryptozoology,* Vol. 1: 54–61).

The similarity between the two objects demonstrated by Greenwell supports my new conclusion that the unidentified animals photographed in Lake Champlain and Loch Ness represent one and the same species.

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The Tail of an Otter?

(Comment on LeBlond and Collins, *Cryptozoology,* Vol. 6: 55–64)

The laudable attempt by Paul LeBlond and Michael Collins to apply wave science to the Wilson photograph is flawed by one major defect, and perhaps by some minor ones too.

They assume that the wind was blowing along the length of Loch Ness, with a fetch of about 20 km. However, the wind data they obtained gave them no justification for this assumption. The nearest weather station reported no wind at all, and the one in Aberdeen reported wind from the north-northwest. At the time, a complex low pressure system covered the British Isles and southern Norway, and the forecast for the morning of April 19, 1934, was for a light or moderate northerly wind. Over the next day, this wind backed to the northwest and freshened. Any doubt about the wind direction, however, can be dispelled by examination of the photograph itself. It clearly shows waves either parallel to the shore or at a slight angle to it. As Fig. 1 shows, this indicates a west or northwesterly wind. In that case, the fetch reduces to 0.5 km or less, with a consequent reduction in the estimate for L_m.

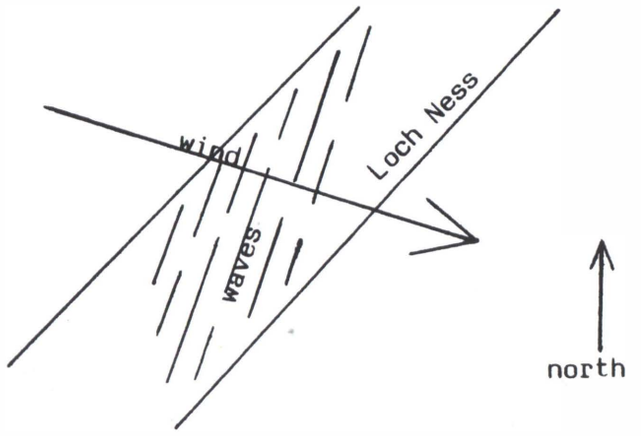

Fig. 1.—Depiction of angle of waves in relation to the shores of Loch Ness, indicating a west or northwesterly wind on April 19, 1934.

LeBlond and Collins state their their equation (4) is taken from Carter (1982: Appendix A). But Carter's Appendix A does not show that equation, although it does show some similar ones. Furthermore, Carter's equations do not give the wavelength; they give the significant wave height, the period corresponding to the peak frequency of the spectrum, and the zero-up-crossing period.

I leave recalculation of L_m to LeBlond and Collins, but it seems certain that the result must be a reduction in the height of the object in the photograph, down towards my own estimate of 0.7 m. As it is, I note that LeBlond and Collins's estimate has an error bound ranging from 0.6 m to 2.4 m, which includes my estimate. Nevertheless, their conclusion that the object "is of a dimension which certainly warrants all the interest which it has received and the mystery which surrounds it" is unjustified.

Uncertainty regarding the size, and indications that it is about the length of an otter's tail—and its resemblance to an otter's tail—justify the conclusion that it is indeed an otter's tail.

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The Tale of an Otter?

(Response to Mackal and Campbell)

I would like to thank both Mackal and Campbell for their interest in our calculations of the size of Nessie from the length of adjacent wind waves in the Wilson photo. Their comments illustrate both the need for—and the difficulty in—applying quantitative methods to cryptozoological problems.

Both commentators have published important and critical works (Roy P. Mackal, 1976, *The Monsters of Loch Ness,* Swallow Press, Chicago [I apologize to Mackal for the accidental ommision of this reference in our original paper]; Steuart Campbell, 1986, *The Loch Ness Monster: The Evidence,* Aquarian Press, Wellingborough, England) which have contributed to raise the level of discussion on the subject. We note, however, that the object seen in the Wilson photograph looked like a bird to the first, but like an otter to the second. Although appearances are not entirely to be denied,

conclusions based exclusively on morphological similarity are certaintly not convincing. We all recognize the need for more analytical approaches.

Obviously, quantitative methods do not lead automatically to unchallengeable conclusions. The information available to work with is often inaccurate and incomplete, and must be supplemented by a variety of assumptions. Results must reflect this uncertainty: honest quantitative estimates must include an assessment of error margins. The advantage of a quantitative approach is that the importance of every observation and assumption stands out clearly. Criticism may then be directed at specific points, and the discussion can rise above the defence of adversary positions, or the denigration of witnesses.

I will now address some of the specific points raised by Campbell. First, period and wavelength are related in water waves; being interested in wavelengths, I transformed Carter's results, expressed in terms of the wave period, to wavelengths using the dispersion relation (Paul H. LeBlond and Lawrence A. Mysak, 1978, *Waves in the Ocean,* Elsevier, Amsterdam). My apologies to Campbell, who took the trouble to look up the reference.

Secondly, since only a small band of the far shore is visible in the Wilson photo, and no near-shore feature is available to determine the orientation of the point of view, one may not conclude that the photo was taken looking directly across the lake. Hence, the direction of wave propagation may not be reliably assessed from the photo. Although no wind information was available for Loch Ness itself at the time of the observation, the surface atmospheric pressure charts provided by the National Meteorological Service for the morning in question show isobars running north-south, along the axis of the Loch. From that evidence alone, one would expect the wind to have been blowing nearly down-loch from the north. In addition, the presence of high hills on both sides of the Loch would tend to orient winds preferentially along the axis of the valley. This orographic steering is a common feature of winds in mountainous areas.

The most reasonable choice for the wind fetch is, then, the length of the Loch in the direction whence the wind blew, which is what we chose. I find no need to modify our estimate, nor the breadth of the error margins about it.

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A DOG IN WOLF'S CLOTHING?

(Comment on Buffetaut, *Cryptozoology,* Vol. 6: 88–90)

Buffetaut mentions the extraordinary case of the Beast of Gévaudan in his carefully considered book review of a work called *Les Survivants de l'Ombre.* The Beast terrorized parts of southern France for approximately three years in the 18th century by preying upon people in the fashion of a man-eater. Victims were numerous: somewhat less or somewhat more than 100 persons!

Buffetaut quite rightly discounts the idea that such attacks could have been done by individuals disguised as animals, which seems to leave—as he points out—trained dogs or wolves as the culprits.

There may be a third culprit in this odd affair: a hybrid mix of dog and wolf. This thesis was presented by C. H. D. Clarke in the April, 1971, issue of the American Museum of Natural History's *Natural History* magazine. In an article based on French sources, Clarke pointed out that: 1) there were at least two "Beasts"; 2) they were unusually large for wolves; 3) they had markings not usual for wolves (one had a white-colored throat, and the other had a "reddish" coat, as remarked by the naturalist Buffon); 4) they were not rabid; and 5) the people of the region were not afraid of ordinary (i.e., non-rabid) wolves, since they routinely sent children out to guard cattle and sheep in country known to contain large numbers of wolves.

It is true that a rigorous modern, examination could not be carried out, and the bodies of the "Beasts" were badly preserved; but Clarke makes a good case for a dog-wolf cross, rare and almost never known in the wild. Yet, the man-eating creatures called "Beast" had a man-eating habit *also* almost never known in the wild. The likelihood of these canids being "trained dogs" seems low, since apparently both had mates in the wild, one mate with pups according to Clarke's article. The two candidates for the "Beast" were both killed in the wild, and so were their mates.

It may well be that feral dogs are more dangerous to man than wolves. In this connection, I believe that some tales of attacks in France from earlier (16th and 17th) centuries may be seen to be at least possible *dog* attacks. The evidence that led authorities in the old days to point to "werewolf" attackers seems to also, in at least one direction, point to *dogs.* This clue is the evidence of the "wolf without a tail" (Charlotte Otten [ed.], 1986, *A Lycanthropy Reader: Werewolves in Western Culture,* Syracuse University Press, Syracuse, New York). It is clear that many more dogs than wolves have their tails cropped, so this particular description points to the domesticated species rather than to wolves, if we exclude the human criminal and the *loup-garoux.*

Certainly, dogs are less in awe of humans than are wolves, and Clarke's article, "The Beast of Gévaudan," implicates their progeny in a special case.

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MAD DOGS AND FRENCHMEN

(Response to Trotti)

I am glad that Trotti has raised the question of the Beast of Gévaudan for further discussion in *Cryptozoology,* as this is, in my opinion, a major problem in historical cryptozoology.

As Trotti rightly states, this mysterious animal (or animals) killed about 100 people within 3 years, and whatever it may have been, there is no doubt about the existence of this "man-eater" in 18th-century France. Much has been published about the Beast, ranging from preposterous occult speculations to well-documented and carefully reasoned hypotheses (see, for example, Xavier Pic, 1971, *La Bete qui Mangeait le Monde en Pays de Gévaudan et d'Auvergne,* Albin Michel, Paris).

One hypothesis involves the activities of a sadist, which, although not totally out of the question, certainly cannot explain most of the cases, those in which eyewitnesses had a good observation of what was undoubtedly a real animal, not a man in disguise. When all such hypotheses are discarded, there remain only a few reasonable possibilities.

Many authors have come to the simple conclusion that the Beast was merely a large wolf, which, for some reason, had turned to man-eating. The animal killed in 1765 by Antoine de Beauterne, who had been sent by the King to end the depredations of the Beast, seems to have been a large wolf, judging by the descriptions of the carcass. Its death did not stop the killings, however, and it was not until a second "Beast" was killed by a local hunter, a man sometimes described as being of dubious character named Jean Chastel, that they finally ceased.

As pointed out by Trotti, the second "Beast" was apparently not an ordinary wolf, to judge from the few descriptions available (the carcass was brought to Versailles, but was so decomposed by the time it arrived, that it was disposed of before it could be seen by experts). Nor was it something totally different from a wolf (such as a lynx or a hyena, as some have suggested), as this would have been noticed by the local people, who had direct experience of wolves, but were not unduly afraid of them.

Although I am definitely not an expert on canid anatomy and behavior, all this suggests to me that there is some merit to C. H. D. Clarke's idea, endorsed by Trotti, that the Beast may have been a dog-wolf cross. A feral dog could also be a good candidate. Several recent episodes in France—and probably elsewhere, too—have shown how dangerous some supposedly tame dogs can be to people, notably children (who were also the main victims of the Beast), whereas there seem to be very few authenticated reports of attacks on men by non-rabid wolves.

Perhaps a close study of the reports on the Beast's attacks on people by an expert on wolf and dog behavior would shed some light on this intriguing and much-debated question.

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THE YAHOO AGAIN MISREPRESENTED

(Comment on Groves, *Cryptozoology,* Vol. 6: 128–29)

In his response, Groves introduces two new errors of such a serious nature that they cannot be allowed to pass uncorrected. He asserts that full documentation for the Yahoo is still awaited, and that, meanwhile, the interim picture lacks substance. He also implies that there is no consistency, and, therefore, no associated evidential value to be found in these reports.

Documentation on the Yahoo is exceptionally full, and most of it—including everything of importance written by H. J. McCooey—has always been available since it was first published in the 19th and early 20th centuries. In addition, much of it was republished over a decade ago. While it is usually possible to learn more about something, to assert that the matter awaits full documentation is quite misleading.

The second criticism, that there is a lack of correspondence, is most easily met by identifying characteristics which show up consistently in two or more independent reports. Although Groves seems to require only one, here are six: tree-climbing, dual gait, hair black with reddish throat, shape of foot, possession of claws, and projecting face.

I hope these brief remarks help to make clear that the documentary evidence for the Yahoo is considerably more substantial—and more interesting—than Groves would have us believe.

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Explaining Away and Avoiding Errors

(Comment on Groves, *Cryptozoology,* Vol. 6: 128–29)

With some hesitancy, I would like to address a statement made by Groves in his Response to Joyner (Graham C. Joyner, 1987, The Yahoo: A Possible Solution, *Cryptozoology,* Vol. 6: 125–28).

First, I want to make it clear that I know little about the Australian Yahoo or Yowie; neither is it my intention here to participate in the evaluation of such evidence or reports. My concern is more related to the implications of Groves' statement for cryptozoology in general, and, indeed, for the pursuit of new scientific knowledge in general.

In responding to Joyner's criticism that he (Groves) was merely "explaining away" the historical Yahoo reports, Groves states: "I admit freely to Joyner that I have tried to 'explain away' the reports. It is important to try to do so; if they cannot be explained away, then what we are faced with is Evidence. . . ."

The problem presented by Groves' statement is the reader's inability to know precisely where—or when—Groves would decide that such reports *cannot* be "explained away." While I am certain that Groves knows in his own mind where this would be—and, indeed, he addresses the fundamental need for consistency in the reports—without imparting this information to others, his statement, however well intended, remains in the subjective realm, and is thus of little value.

If we were to let such subjective evaluations go unchallenged, it is possible that, at some future time, another investigator, perhaps less endowed than Groves—and perhaps with a less noble purpose—would utilize them for his or her own particular political purpose. Clearly, *anything* can be "explained away" if one tries hard enough. This is the technique of the rapid-fire debunker trying to score points, not the calm and objective true scientist trying to solve problems.

Now, I happen to know that Groves is a true scientist, and a good one. I suspect that, in "explaining away" the reports, he was simply trying to avoid a Type I error—a term borrowed from statistical science. A Type I error occurs when one thinks that something special is happening when, in reality, it isn't. However, strong avoidance of a Type I error can often lead to a Type II error. A Type II error occurs when one thinks that nothing special is happening when, in fact, it is, even if it is only faintly detectable.

Marcello Truzzi, a sociologist, has pointed out how workers in a given field who are less interested in the anomalous data generated in that field are more concerned about avoiding Type I errors. Those who concentrate on the *anomalous* data, however, are more concerned about avoiding Type II errors (Marcello Truzzi, 1981, Editorial, *Zetetic Scholar,* No. 8, July). Avoidance of either Type I or Type II errors varies from discipline to discipline. "In medical research," states Truzzi, "there is commonly interest in avoiding a Type II error because the outcome may be a matter of life or death, and we don't want to overlook something that may be hard to find but terribly important."

Finally, because of the abuse which the term "explaining away" could imply—and possibly Groves did not use it in the same context in his own mind as did Joyner—I think another term would be more appropriate, one which would clarify in the reader's mind exactly when the reports "cannot be explained away . . . [and] what we are faced with is Evidence."

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On Type I and Type II Errors in Cryptozoology; or, Was Proteus a Yahoo?

(Response to Joyner and Greenwell)

Graham Joyner has taken me to task for ignoring the full documentation available on the Yahoo (or Yowie, or Australian hairy biped), and Richard Greenwell for possibly falling into a Type II error, i.e., overlooking anomalies, faint though they may be, and so missing potentially significant evidence.

As Joyner is undoubtedly the leading authority on the Yowie, and as I have not seen H. J. McCooey's writings to which he refers, I have used his two compilations (1977, *The Hairy Man of South Eastern Australia,* published by the author; 1980, *More Historical Evidence for the Yahoo, Hairy Man, Wild Man, or Australian "Gorilla,"* National Library of Australia MS 3889) for my sources. Altogether, Joyner cites 33 reports (29 in his first publication, four more in the second); many of these state only that there is a Yahoo/Yowie, perhaps adding that it is large and hairy, but 15 do give extra descriptive information. In two cases (Joyner 1977, nos. 6 and 21, and nos. 10 and 11) the same animal, or the same story, is referred to, so I have combined the descriptions into one.

There are, thus, 13 reports which are independent, or as independent as can be hoped for under the circumstances. I have listed in Table 1 the characteristics of each under the six headings recommended by Joyner in his critique: tree-climbing, dual-gait, hair black with reddish throat, shape of foot, possession of claws, and projecting face. If there is consistency in Yahoo/Yowie descriptions, they should emerge in this tabulation.

TABLE 1.—Characteristics ascribed to the Australian "hairy man" (Yahoo).

Report Number	Tree-climbing	Dual-gait	Hair color	Shape of foot	Claws	Projecting face
(Joyner, 1977)						
1. George Osborne	yes	quadruped; lifts one arm when turning around	black; tan-colored streak from neck to abdomen	18″ long, shaped like iguana, with long toes		yes; with monkey features
3. Illawarra residents					yes	
4. Joseph Ward's granddaughter					nails of a tremendous length	
6, 21. "Yahoo" of Aborigines	yes		long white hair	turned backwards	great talons	
10, 11. Arthur Marrin		[bipedal]	tan-color	like man's hands	paws; overgrown nails	like a polar bear
13. Webb brothers				like man's, but with longer, spreading toes		
15. Harry Williams			grey hair all over			

TABLE 1.—Continued.

Report Number	Tree-climbing	Dual-gait	Hair color	Shape of foot	Claws	Projecting face
17. George Summerell		yes	covered with grey hair	long, ugly; human, but with only four toes		like ape or man, minus forehead and chin
19. Charles Harper		yes	brown-red on body, legs and arms; in the firelight shoulders and back seem black	metatarsals very short, but phalanges very long		head and face very small, but very human; large canine teeth
(Joyner, 1980)						
1. William Telfer			darkish color			
2. Aborigines at Belgrave		yes				
3. Walla Walla sawyers				one foot like horse hoof, other club shaped		similar to man's
4. Rocky Bridge fishing party		[?bipedal]	dark; back, belly and all down legs with hair lighter color			big, red face

Four reports—nos. 1, 10/11, 17, and 19—stand out as much more detailed than the rest. So, let us examine these first, and then see whether the others are consistent with any or all of them. But the first question is, are they consistent with each other? This is really the crucial point. Joyner is clear on this point, and implies that consistency is evident.

At first sight, no. 1 (George Osborne's description of 1871) and no. 19 (Charles Harper's of 1912) seem very consistent with each other: black colored on the back, but browner in front; long toes; small monkey-like face (if we suppose that the long canines of Harper's "gorilla" gave it a monkey-like appearance, and that the foot "shaped like an iguana" of Osborne's beast implied long phalanges, short metatarsals, as Harper so meticulously described). But the details are rather different: Osborne's had only a tan streak from neck to abdomen, whereas Harper's had long brownish-red hair on its body, legs, and arms, leaving the shoulders and back parts alone black; Osborne's was "slender-proportioned," though with well-developed arm and chest muscles, whereas Harper stated of his beast that "the body frame was enormous." Osborne was clear: "arms long, legs like a human being"; for Harper, too, the arms were "extremely long," but he, on the other hand, stressed how un-human the legs were, with a very short "fibula bone," but the "femur bone . . . very long, out of all proportion to the rest of the leg."

Now, although people's memories are fallible things, both Osborne and Harper infer that they took pains to get a good mental picture of their respective "gorillas." So, the apparent consistency of their descriptions has to be tempered with quite obvious inconsistencies.

The other two rather detailed descriptions are completely *in*consistent with these two. Arthur Marrin (nos. 10, 11), who killed an animal and described it as it lay on a table in his soft-drink factory in 1893, stated it was tan (i.e., all over, not parti-colored like Osborne's and Harper's); its feet were like a man's hands, with overgrown nails, but its hands were "great paws"; its face was "very much like a polar bear," and 11 inches (that's 28 cm!) across the forehead. The beast, when alive, had been bipedal, not quadrupedal like Osborne's, nor a tree-climber, nor of dual-gait like Harper's.

The fourth of the detailed descriptions is that of George Summerell (no. 17), of 1912, as reported by the poet Sydney Wheeler Jephcott. This was a bipedal creature covered with grey hair, with a very long foot of human shape except that, according to the footprints, there were only four toes. The face was "like that of an ape or man, minus forehead and chin." The color and the form of the feet, the most accurately described features, both render this beast quite different from Marrin's or Osborne's or Harper's.

On the basis of these four descriptions, if they are all of unimpeachable accuracy (not to mention veracity), we might postulate the existence of three separate kinds of Yowie, probably four. This seems to me an implausible proposition: we are discussing whether there is even *one*! At the risk of falling into a Type II error, I suggest it is more plausible that there is an aspect of people seeing different animals which they did not recognize, with a bit of conflation (e.g., were the footprints which Summerell saw, and took plaster casts of in the mud, really those of the creature he had just seen drinking? As for Marrin, I opine that he displayed the body of an exceptionally large wombat, and told a tall story about how it had viciously attacked him).

The other Yowie/Yahoo descriptions are more easily dealt with, as they are briefer. Nos. 3 ("various residents" of the Illawarra region) and 4 (a little girl, granddaughter of Joseph Ward) say only that the creature seen by them was hairy, and had long claws or nails; and nos. 6 and 21 (which together describe the Yahoo or Devil-Devil of South Coast Aborigines) mention claws, as well as the body hair being long and *white,* and the feet turned backwards. It is to be noted that, apart from Marrin's wombat-like beast, these are the only mention of claws at all: *contra* Joyner's statement (if this is indeed what he meant to imply), claws are never mentioned alongside dual-gait or hair black with reddish throat; only in the case of the Aboriginal legend are they mentioned along with tree-climbing, only in the case of Marrin's wombat-like creature are they mentioned along with a long-toed foot or projecting (in this case, polar-bear-like) face. No. 13, Joseph and William Webb's description, speaks only about the feet being like a man's, but with longer, spreading toes, the only other description of this kind of foot, and not associated with the requisite color, gait, face, etc. No. 15, Harry Williams' description, speaks of grey body hair—different from all other hair descriptions, except for Summerell's and just possibly the Aboriginal Yahoo.

Of the four cases described in Joyner (1980), no. 1 (William Telfer's) says only it was of darkish color; no. 2 ("persons frequenting the neighbourhood of Belgrave") mentions dual-gait (bipedal/quadrupedal), and nothing else; no. 3 (sawyers working in the Walla-Walla scrub) is an outrageous story of finding the cadaver of a nine-foot-high human-like creature of which "one of its feet resembled the hoof of a horse and the other was club shaped"; and no. 4 (a fishing party at Rocky Bridge), written by the same journalist as no. 3, describes an apparently bipedal creature of dark color, but "the back and belly and down the legs covered with hair of a lighter color."

Let us now review these again. Only two of the descriptions (Joyner 1977, nos. 1 and 19) are even remotely consistent with one another—and that not closely. Certain descriptive features certainly do tend to crop up in the descriptions, but in company with other features which are inconsistent: tree-climbing is described with, in one case, a black-and-tan creature, in the other with a white one; dual-gait, with a grey creature or a brownish-red one; the long-toed foot crops up in association with two black-and-tan creatures, but with different actual color patterns, and a wholly tan one; and so on.

Does this sound too negative? Does this sound as if I am rushing headlong

into a Type II error? I believe not. I believe that we are in the presence of a remarkable intellectual and cultural phenomenon, the *bête-humaine,* as Heuvelmans has called it in an African context: a widespread, perhaps global, belief, which sometimes has a zoological referent (the Great Apes, the Indri of Madagascar), sometimes not. In searching for the cultural phenomenon, it would be foolish to overlook the possibility of unsuspected zoological referents outside those already known; but we should avoid the sort of emotional commitment to the real existence of unknown hairy bipeds that blinds us to what actually is and is not acceptable evidence for them, and pushes us into Type I errors.

Sightings are, when all is said and done, poorish sort of evidence. The human mind kids itself endlessly about what it has experienced. When someone has read of someone else's report of a hairy monster, it is particularly difficult to maintain that his/her own report is truly independent. Where one finds, as in the Yahoo sightings, such a welter of reports that are inconsistent, beyond the bare descriptions of "large" and "hairy," plus a few that reek of tall stories in part or in whole, I cannot find anything that rates as Evidence.

Greenwell, in his Comment, describes a Type II error as "when one thinks that nothing special is happening when, in fact, it is, even if it is only faintly detectable." True. First, however, there must be some indication of an anomaly—in this case, some reason to suspect that there is an unknown hominoid or hominid in southeastern Australia. If as here, I find "nothing to explain," then the only way a Type II error could be involved is if a phenomenon exists *which has not yet proved detectable.* That is what I meant by "explaining away." Indeed, perhaps an unfortunate choice of phrase. What I intended to convey will by now, I hope, be clear; namely, what would genuinely be evidence (Greenwell), and what I mean by documentation and consistency (Joyner).

Cryptozoology is a remarkably interesting and rewarding field—as much in the cultural and methodological realms as in the truly zoological, though a zoological discovery at the end of the chase remains, in some cases, as a final reward for the careful practitioner. That I think the Yowie/Yahoo to fall, so far, at the cultural end of the spectrum in no way detracts from the extraordinary interest of the study.

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PARALLEL PRIMATES

(Comment on Hewkin, *Cryptozoology,* Vol. 5: 27–37; Vol. 6: 78–84)

One achievement of ISC and its journal is incontestable: they enable professional biologists with an interest in cryptozoology to come into the open. The latest case in point is wildlife biologist James A. Hewkin, who has conducted Sasquatch investigations "in an unpublicized low-key manner" since 1973, and now finds it appropriate to publish his findings. This is very welcome. First, hominology (the study of relict hominoids) is much in need of the expertise of wildlife biologists. Second, Hewkin's conclusions largely parallel our own, thus increasing the probability of their being correct.

Some examples follow (all from *Cryptozoology*). Hewkin: Sasquatch "is elusive and intelligent, more so than other mammals in the area, and more so than other living apes in Africa and Asia" (Vol. 5: 36). Bayanov: "As a result of this expedition, I am more inclined than ever to think that relict hominoids have neither 'supernatural' nor simply 'animal' status, but what I call 'superanimal' status. And this is for quite natural reasons: their large brains, coupled with upright walking and free hands. These factors are responsible, perhaps, for their occasional strange behavior, on the one hand, and their 'abnormal' elusiveness, on the other" (Vol. 3: 79). We can also underwrite such Hewkin findings as "its diet is probably omnivorous, with feeding habits similar to those of bears" (Vol. 5: 36), "it can use individual toe flexibility for dexterity in climbing steep terrain and scaling mountainous areas" (Vol. 6: 84), and many others. But the most striking parallel follows.

Hewkin: "One of the most revealing Sasquatch sightings reported took place in the Oregon Cascades in 1967. A logger described the activities of what he thought was a family of three Sasquatches (two adults and an infant) as they searched for hibernating rodents on a rocky ridge, digging up rocks and piling them up, finding nests of rodents, and eating them on the spot (Green 1968). On September 3, 1973, I located this site after spending 3 days searching the area. Further examination on the ridge indicated many old rock piles and diggings" (Vol. 5: 28–29).

Both Green and Hewkin are unaware that, on our part, we have the following information: there are reports of wildman sightings and footprints near the village of Sosnovka, in the Kirghiz Range of the Tien Shan Mountains in Soviet Central Asia. In 1951, a local resident discovered diggings and rock piles in the place of a marmot colony. "Apparently the diggings were made by wildmen because the rocks were not thrown helter-skelter as is the case when pits are made by wolves and bears, but were accurately piled on the lower side of the pit, with some rocks weighing up to a metric centner. At the bottom of pits there were marmot tails, paws, and fur. Judging by these, the wildman caught and killed 5–6 marmots in each pit" (1958,

Report of the Commission to Study the "Snowman" Question, Vol. 2, p. 98, Academy of Sciences of the U.S.S.R., Moscow, in Russian).

We are very pleased that Hewkin does not call for the killing of a specimen to prove the existence of Sasquatch. On the contrary, he states: "Once its behavioral peculiarities—which so far have kept it from scientific discovery—are better understood, this species will be susceptible to scientific field study just as other animal species have been eventually studied in their own habitat" (Vol. 5: 36). We wish every success to the author and his possible followers.

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SASQUATCH SNAPPED SAPLINGS (OR BIGFOOT BROKE BRANCHES . . .)

(Response to Bayanov)

I appreciate Bayanov's interest in my Sasquatch investigations, and the parallels between Soviet findings and our own are, indeed, striking.

I would like to take this opportunity to report on our latest activities. I received a report in June, 1988, of a Sasquatch sighting in the Oregon Coast Range. After checking it out quite thoroughly, it seems to be a reliable case. We found about six tracks in heavy forest duff, and a couple of snapped hemlock saplings. Unfortunately, I did not receive word of this incident until 3 weeks had elapsed; by then, the site had been visited by friends of the witness, and the place was scuffed up. We were unable to obtain good casts, because the footprints had deteriorated in the duff. There were widespread toeprints indicated in the few tracks noted.

The witness was a teen-age boy riding a dirt bike on a logging road. He sighted the animal at dusk as he rounded a bend. He reported that it crossed the road at a distance of about 100 yards. He could not provide a good description, only that it was big, heavy and dark, and man-like. I received preliminary information from the man who reported the incident, a neighbor whose son bike-rides the area with the witness quite often. He was taken to the site several days after the sighting, and made a cast of one track. He attempted a second one, but ran out of plaster. He also collected some hair samples from one of the snapped saplings.

This sighting occurred on a road not accessible to the public, as it is gated and locked. However, bike riders get around these gates, and take trails all

over the country. I checked the site with the neighbor on June 25, and interviewed the boy on August 8. The tree snapping is similar to that reported by Lonnie Somer (1987, New Signs of Sasquatch Activity in the Blue Mountains of Washington State, *Cryptozoology,* Vol. 6: 65–70).

In other fieldwork, Jack Sullivan and I examined a site in the Blue Mountains of Oregon on August 4 and 5, 1988. We found at least a dozen logs which had been torn up, with no sign of bear clawmarks on them. We finally found what appeared to be a nail scratch on two logs.

I am confident that this activity involves Sasquatch. A bear would have left claw marks on at least some of the logs. The site was brought to my attention 2 years ago by a deer hunger who had found supposed Sasquatch tracks in fresh snow on the opening day of the deer season. The animal is apparently still using the vicinity. No tracks were found, however.

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FIELDWORK, EVIDENCE, AND SCIENCE

(Comment on Zarzynski, *Cryptozoology,* Vol. 6: 71–77)

Project Champ Carcass put the Lake Champlain Phenomena Investigation (LCPI) in the forefront of cryptozoological research in 1987. The activities of Joseph Zarzynski and his colleagues are broad-ranging: encompassing a vigorous effort to seek out and document eyewitness sightings; public education on the importance of cryptozoological research; encouragement of other responsible expeditions; concern for environmental issues; an ongoing effort to obtain photographic and/or sonar evidence; and, now, a well-mounted attempt to obtain physical evidence of the alleged unknown animals said to exist in Lake Champlain.

I am concerned, however, that carcasses may be short-lived in the lake ecosystem. With eels and other scavengers being active—not to mention the effects of currents and sedimentation—physical remains may disappear in a relatively short time. Nevertheless, the finding of such remains—even one vertebra—would be well worth the effort. Project Champ Carcass already has been justified by the discovery of a boat wreck of historical interest.

Some readers may have noticed that, for several years, field reports in *Cryptozoology* involving lake monster research have issued predominately from Lake Champlain. Is this lake now the only location where such research is being undertaken? Are other reports, notably from Loch Ness, being rejected by the Editor?

Of course not. So, as the leader of a research effort at Champlain separate

from Zarzynski's, I wish to take this opportunity to address the important issue of publication.

Zarzynski is an author and educator. I am basically a journalist. We are both writers, and are both motivated to share our findings with others. In short, we are submitting reports to *Cryptozoology*. Those conducting similar work at other lakes are not.

This is unfortunate. The Academy of Applied Science (AAS) (Robert H. Rines, Charles Wycoff, Harold Edgerton, *et al.*) did publish reports on its Loch Ness work in *Cryptozoology* in 1982 and 1984 (these being beyond the landmark papers which appeared in *Nature,* Dec. 11, 1975, and *Technology Review,* March–April, 1976). AAS's recent lack of publication may be explained by an apparent lull in its underwater research activities while preparing for renewed efforts at the loch.

The Loch Ness and Morar Project (LNMP) (Adrian Shine, field leader), has, however, been doing major work at Loch Ness for the past several years. Indeed, in September, 1987, LNMP mounted Project Deepscan, a flotilla sonar-sweep of the loch basin. This effort attracted international media attention, and is certainly known to readers of this journal. One result of Project Deepscan which was widely mentioned in press accounts was the debunking of the 1975 "gargoyle" AAS photo, taken by an automatic underwater strobe camera, which the Academy had proposed was a close-up image of Nessie's head.

I read about this debunking with considerable interest. LNMP's Evidence Evaluation Committee (EEC) has taken a strongly revisionist view of the general body of evidence gathered at Loch Ness, being particularly skeptical of most photographs obtained to date. I had occasion to discuss with Shine some of the issues involved during a July, 1982, visit to the loch. On three subsequent occasions (most recently after Deepscan), I wrote to the Project asking for copies of the EEC reports. Unfortunately, I have received no response.

In November, 1987, I visited the Loch Ness Monster Exhibition in Drumnadrochit. The Exhibition is closely affiliated with LNMP. I again inquired about the work of the EEC. I was informed by knowledgable persons that my most recent letter had been forwarded to Shine. I was also informed that the Project has not published any reports containing evidence for its revisionist positions; that the object believed by the LNMP to be the true subject of the "gargoyle" head photo (variously reported in press accounts as a tree stump and a rock outcropping) was not at the Exhibition; and that no scientific publication on the subject was contemplated. Information on the Committee's activities will thus apparently continue to be made public only through statements to media representatives and writers.

All this came as a disappointment and a surprise. It was a disappointment because I have the greatest admiration for the work of Shine and the Project, and the greatest interest in their findings. It was a surprise because LNMP has publicly dedicated itself to being the most rigorously scientific group at Loch Ness.

Surely one of the most vital aspects of the scientific process is publication. After formulation and testing of hypotheses, with maintenance of objective and thorough research standards, data must be disseminated and conclusions offered for informed debate. The mere expression of expert opinion is not a substitute for scientific publication, and does not constitute scientific evidence.

This fact was underscored for me by an article in the London newspaper *Mail on Sunday* that appeared as a follow-up to Project Deepscan. According to this October 18, 1987, item, "an expert has cast doubt on the three sonar contacts which so excited Mr. Shine, pointing out that only top class military sonars would be able to give any idea of size."

Solid research at Loch Ness was again being dismissed out of hand by "an expert." *Ex cathedra* arguments, without supporting data, were again being reported in the press. I felt a great deal of sympathy for Shine and his team, whose important work was being denigrated by unsubstantiated statements.

How I wish my journalistic colleagues would get beyond "expert opinion," and ask for even the most basic evidence to support profered views. By the same token, I urge the Project to publish whatever data it has—including comparison photos, sonar tracings, mathematical calculations, and the results of repeating the procedures of other researchers—concerning Loch Ness photographic evidence. I'm confident that several journals would be greatly interested in such papers. *Nature, New Scientist, The Skeptical Inquirer,* and *The British Journal of Photography* have all published articles critical of evidence for the Loch Ness Monster. *Cryptozoology* also serves as a neutral forum for papers of a skeptical nature.

I would also welcome reports from other "monster" lake research groups to add to what is being regularly submitted to this journal by those of us working at Lake Champlain.

Far from being perceived by other research groups as an attack, a well-documented scientific publication by the Loch Ness and Morar Project on the classic photographic evidence for Nessie would be read with the greatest interest and respect. It would also serve notice to those who carry on debates in the mass media that opinions are only opinions. No matter how expert their source, only with refereed publication in scholarly forums do opinions rise to the level of evidence.

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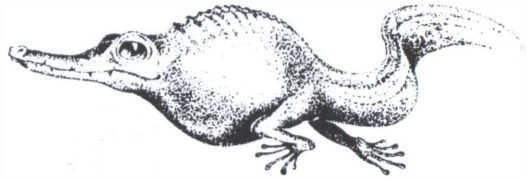
(Richard Smith is a journalist who has been leading a field research effort at Lake Champlain each summer since 1984.)

HAVE YOU EVER SEEN A TADPOLE OF REPTILE?



No?

And, furthermore, you doubt that you'll ever find one?

Then, you are a potential supporter of batrachology, as a distinct discipline.

The existence of a tadpole stage, followed by a metamorphosis, is one of the characteristics shared by most modern Amphibia, and which, among others, distinguish them from Reptiles. The Amphibia are considered to constitute a distinct class of Vertebrata by all zoologists, whatever their philosophy of classification. Therefore, why are the study of Amphibia and that of Reptiles usually referred to a single discipline, called "herpetology"? We believe it is simply a matter of historical tradition.

On the basis of this observation, a group of French zoologists in 1982 started the first batrachological society in the world. This society was founded in Paris, hence its name "Société Batrachologique de France" (S.B.F.), but it is open worldwide to all scientists and amateurs interested in the study and conservation of Amphibia. It publishes two quarterly journals. One, **Circalytes**, is mostly intended for amateur batrachologists. The other one, **Alytes**, is intended for professional scientists.

Alytes is now an international journal, with international printing standards and an international editorial board. It publishes papers in English and in French, dealing with all aspects of Amphibian biology. The following titles are drawn from the contents of the last two volumes (5 and 6):

G. ALBERTINI & B. LANZA: **Rana catesbeiana** Shaw, 1802 in Italy.

J.-L. AMIET: Aires disjointes et taxons vicariants chez les Anoures du Cameroun: implications paléoclimatiques.

M. BREUIL & G.H. PARENT: Essai de caractérisation des populations du Triton alpestre hellénique.

A. DUBOIS: Miscellanea taxinomica batrachologica.

A.K. HOTA & M.C. DASH: Growth and metamorphosis of anuran larvae: effect of diet and temperature.

R.F. INGER: Diets of tadpoles living in a Bornean rain forest.

P. JOLY: Le régime alimentaire des Amphibiens: méthodes d'étude.

R.F. LAURENT: The systematic position of the genus **Afrixalus** Laurent (Hyperoliidae).

Until now, the S.B.F. has developed mostly in France, but we think that the society and its journal **Alytes** must be much more widely opened to all batrachologists. We are therefore sending here a double call:

(1) for new subscribers to **Alytes** and to the S.B.F.;

(2) for papers for **Alytes**: good quality papers are wanted from all fields of study on Amphibians, not only systematics, faunistics, zoogeography and ecology, but also others, like behavior, ecophysiology, developmental biology, genetics, which are not so strongly represented in traditional "herpetological" journals.

Tariffs in French Francs for 1988 (volume 7 of **Alytes**) are as follows:

Subscription to S.B.F. (including CIRCALYTES) + ALYTES (only individuals)	**180 F.F.**
Subscription to ALYTES alone: individuals	**130 F.F.**
institutions	**260 F.F.**
Additional charge for airmail postage of ALYTES	**50 F.F.**

Payments should be made in French Francs by checks, payable to "Société Batrachologique de France", sent with the order to "Société Batrachologique de France, Laboratoire des Reptiles et Amphibiens, Muséum national d'Histoire naturelle, 25 rue Cuvier, 75005 Paris, France", or by international money order directly to one of the following accounts:

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Articles

Manuscripts must be submitted in triplicate, and must be prefaced by 50–100 word informative abstracts. The abstract should appear on the bottom half of the title page. The manuscript should begin on the second page. The citation of references is mandatory, and must conform to the referencing instructions appearing below. Manuscripts should contain relevant descriptions or interpretations related to cryptozoological matters. Topical or theoretical discussions or literature reviews, rather than specific research findings, are preferred. Illustrations and maps are welcome, but original artwork and black and white photographs are required (color slides are not acceptable). The author(s) should design subheadings in accordance with the structure of the article. Manuscript length is left to the discretion of the author(s), although a minimum of 1,000 words is expected. Manuscripts are reviewed by two referees for scientific content, originality, and clarity of expression, and the Editor may suggest deletions or revisions before acceptance.

Research Reports

Manuscripts must be submitted in triplicate, and must be prefaced by 50–100 word informative abstracts. The abstract should appear on the bottom half of the title page. The manuscript should begin on the second page. The citation of references is mandatory, and must conform to the referencing instructions appearing below. Manuscripts should detail specific research findings which can be quantified. Illustrations, tables, and graphs are welcome, but original artwork and black and white photographs are required (color slides are not acceptable). Examples would be results of laboratory analyses, photographic analyses, or social science or survey analyses. Subheadings should be: *Introduction* (which should give some background on the problem in general and the case in question in particular), *Method*, *Results*, and *Discussion*. Manuscript length is left to the discretion of the author(s), although a minimum of 1,000 words is expected. Manuscripts are reviewed by two referees for scientific content, originality, and clarity of expression, and the Editor may suggest deletions or revisions before acceptance.

Field Reports

These manuscripts should report on cryptozoological fieldwork. They should be limited to a maximum of 1,500 words (longer manuscripts will be considered in special circumstances). Only one manuscript copy need be submitted. Formal referencing should *not* be prepared, although references may be cited in parentheses *within* the text when appropriate (see examples in the journal); when so referencing, both the first (given) and second names of authors being cited should be spelled out, and the names of journals should also be completely spelled out and unabbreviated. Subheadings should be: *Introduction* (in which the type of cryptozoological event investigated is detailed, and the purpose or aims of the fieldwork, and the location and dates involved, are given; previous fieldwork by the author[s] in the same geographic region should also be mentioned); *Narrative Description* (in which the field activities are described, e.g., lake surveillance, equipment, interviews with aboriginal peoples); *Results* (in which the outcome of these efforts is described; the total lack of success of such fieldwork will not be used as a criterion in evaluating manuscripts); *Future Plans* (in which a brief discussion or statement on whether the author[s] plan[s] further fieldwork in the area is provided). Illustrations and maps are welcome, but original artwork and black and white photographs are required (color slides are not acceptable).

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The last name(s) of the author(s) being cited, followed by the year of publication, should be placed in parentheses within the text itself. If the authors already are mentioned in the text, only the year need appear in parentheses.

At the end of the manuscript, starting on a *new* page and under the capitalized heading of References Cited, the manuscript's formal references should be listed in alphabetical order by author(s) (note that only references actually cited in the text should be listed). If several publications by the same author(s) are cited, these should be listed in chronological order with the oldest publication appearing first. Both the authors' first (given) and second names *should be spelled out*, and the year of the publication should be set off under the author's name. The names of journals, popular magazines, and books *should be completely spelled out and italicized*. Volume numbers (in Arabic) and pages should be included (or dates in the case of popular magazines), and book titles should be followed by the city of publication, a colon, and the name of the publisher.

Notes and footnotes should *not* be included. Authors should attempt to include such comments within the text itself. Contributors are urged to consult an issue of the journal and/or consult the Editor when in doubt.

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