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HOW MANY ANIMAL SPECIES REMAIN TO BE DISCOVERED?

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ABSTRACT: Although it is impossible to count the number of organisms which remain undiscovered, and which, according to some, might not even exist, the study of the past discovery of new species in various zoological groups can throw some light on their numerical importance within each group, and on the rate at which new species have been described. It is then possible to estimate the number of species which will be discovered in the future. A historical study of those discoveries which led to reassessments of zoological classification indicates that the highest hopes in this regard may be entertained in *all* invertebrate phyla, and even in marine vertebrates. Cryptozoology, by its very nature, focuses on unknown species of large to moderate size. Its best prospects should be among some rare groups of marine invertebrates, as well as in marine vertebrates. Even though the probabilities of making discoveries of truly revolutionary impact on systematic zoology have become extremely low for land vertebrates, we should nevertheless continue to expect the regular discovery, each year, of numerous presently unknown species, to be classified within groups already known, even if only in the fossil record.

INTRODUCTION

It is obvious even to the layman that there is a much better chance of discovering a hitherto unknown insect than finding a mammal, a bird, or a reptile which has not yet been scientifically described. But what exactly are the respective chances of finding today new species in the various zoological categories? This is what we shall attempt to answer below.

CLASSIFICATION BEFORE CENSUS

To gain some idea of the number of animal species yet to be discovered, one must identify and count, in each of the major zoological groups, those species which *have* been discovered over the centuries; in this way, one may

measure the rate at which new species have been found. It is first necessary, however, to find a method of classifying all of these species in some convenient way, reflecting, if possible, the natural organization of the animal world.

In his *Systema naturae,* where he introduced a classification of living beings which developed into the present taxonomic system (the science of classification, and the rules on which it is based), Carl von Linné (1758) distinguished within the animal kingdom only six categories, with still rather imprecise boundaries: the mammals, the birds, the amphibians (which included the reptiles, and even some fish), the fish, the insects (which included all articulated animals: today's arthropods), and, finally, the worms, a veritable hodge-podge which included all soft-bodied animals lacking the jointed armor of the arthropods and the inner skeleton of the vertebrates.

Each one of these classes was subdivided into a certain number of orders, each order, in turn, into families, families into genera, these genera into species, and, finally, the species into varieties.

From the beginning of the Nineteenth Century, some of the six Linnean classes began, after more careful work, to be divided into more classes. The painstaking analysis of a meticulous observer such as Lamarck soon resulted in twelve classes of animals (1806), and later (1809) fourteen. The old classes of worms and insects—animals without backbones—literally exploded: they now came to include molluscs; annelids (aquatic and terrestrial worms); intestinal worms; cirripedians (such as barnacles or sea-acorns); crustaceans; arachnids; insects; radiates (organisms with radial symmetry such as starfishes); polyps; and infusorians.

At the same time, Cuvier (1817) was trying to reduce this proliferation of classes to four general forms, four groups with distinctly different organizations: the vertebrates, the molluscs, the articulates and, at the bottom of the scale, the radiates, in which were included the zoophytes, plant-like animals of equivocal appearance. Cuvier's fundamental types were to evolve into what we now call phyla, major groups of organisms whose common ancestry is evident to evolutionary biologists.

With the progress of anatomical knowledge, the four fundamental phyla, each one gathering an increasing number of classes, also became subject to a gradual division process. Today, we have at least twenty phyla, divided into seventy five classes, without even counting those which have become extinct during past geological ages.

A Modern Classification of the Animal Kingdom

The reader would obtain a good appreciation of the richness and diversity of the animal world from the outline of a recently proposed zoological classification. This classification, neither too revolutionary nor outmoded, is based on that presented by L. H. Hyman, an American invertebrate zoologist, in the 1962 edition of the *Encyclopedia Britannica.* One must

admit that there exist many others: as many, indeed, as there are classifiers. They all agree on essential points, however, and differ only in the way in which they group or distinguish between various branches of invertebrates, as well as by the position which they assign to particularly obscure groups of ambiguous invertebrates such as tardigrades (bear animalcules, or water bears), onychophorans (*Peripatus* or worm insects), and pentastomides (or linguatulids). As knowledge progresses, classifications are in a state of perpetual re-arrangement. To quote French zoologist Pierre-Paul Grassé (1968): "It would be a gross misinterpretation to believe that the present classification is definitive, and that it represents only natural lineages and the affinities between them."

Here is, then, a list of those phyla which are most commonly recognized today. Most of them are quite unfamiliar to the layman, who would consider them to be of interest to only a few eccentric specialists:

1) *Protozoa,* one-celled animals;
2) *Mesozoa,* minute parasites of some sea worms or of the kidneys of some cephalopods; these organisms are reduced to a central cell enclosed in a single layer of surface cells;
3) *Porifera,* or sponges;
4) *Cnidaria,* or common coelenterates (hydrae, jellyfish, sea anemones, and corals);
5) *Ctenophora,* or comb-jellies, coelenterates with rows of ciliated plates;
6) *Platyhelminthes,* or flat worms without an anus. They are free living (planaria) or parasitic (flukes and tapeworms);
7) *Rhynchocoela or Nemertina,* ribbon-like worms with both anus and protrudible proboscis;
8) *Acanthocephala,* spiny-headed parasitical worms;
9) *Aschelmintha,* a vast assemblage of diverse worms, the best known of which are the rotifers, with ciliated wheels, the nematodes, or roundworms, and the gordian worms;
10) *Entoprocta,* aberrant worms (formerly called vermidians, or wormlike; it is in a phylum of that name that Delage and Hérouard [1897] put all unarticulated invertebrates which they could not fit elsewhere.), whose mouth and anus are close together, and which are surrounded by a graceful crown of ciliated tentacles;
11) *Phoronida,* other vermidians with only the mouth crowned with cilia;
12) *Ectoprocta,* more vermidians, similarly crowned, but forming vast colonies (they were formerly in the Bryozoa, i.e., "moss animals");
13) *Brachiopoda,* or "lamp shells," vermidians enclosed like oysters in a bivalve shell;
14) *Mollusca,* from chitons to octopuses and squids, snails, clams and tooth shells;
15) *Sipunculida,* wormlike, with introversible mouth;

16) *Annelida,* worms with long jointed bodies, including those with many bristles and living in the sea (polychetes), the earthworms, and the leeches;
17) *Arthropoda,* or animals with jointed armor, the best known classes of which are the crustaceans, the arachnids (including myriapods, i.e., centipedes and millipedes) and the insects;
18) *Echinodermata*, sea-urchins, starfish, serpent stars, sea-lilies and sea-cucumbers;
19) *Chaetognatha,* or arrow worms, wormlike sea animals with grasping spines, but without either tubular cord or gill slits;
20) *Pogonophora,* other wormlike animals from the abyssal depths of the ocean, enclosed in a transparent mucous tube;
21) *Hemichordata or Stomochordata,* non-transparent vermidians with pharyngeal gill slits and a primitive nerve chord in the mouth;
22) *Chordata,* animals which in their adult state have either gill slits, a vertebral column (sometimes reduced to a dorsal nerve chord) or both; this, of course, is the phylum to which humans belong, together with other vertebrates, whether they be mammals, birds, reptiles, or amphibians.

The Small Fry Escape Cryptozoology

The layman dreaming of zoological adventures, the amateur naturalist, and indeed all those who may be keen to explore the animal world but have not delved into the many detours and labyrinths of the zoological sciences, may rest assured: they may immediately forget this unappealing inventory, this rather boring list of barbaric names. Within the scope of cryptozoology, the science of animals yet to be discovered, there is no need to keep track of minuscule specimens. Each year brings an impressive harvest of small new life forms, a fact which surprises no one (Anonymous 1952). All that can be done to discover those new organisms is to continue, as in the past, to harvest them blindly, relying mostly on chance. It is, of course, possible to improve the instruments by which they may be captured, or to organize the search for new species on a more systematic, broader, and more energetic basis. This is, indeed, one of the traditional aims of zoology. One may not expect in those cases, however, to orient the search for certain species in the hope of hastening their discovery, as is the essential aim of cryptozoological work, where some notion of a particular species' existence is reported but not fully documented.

This cryptozoological aim may be realized only through the stubborn collection of a rich fund of eye-witness reports, indigenous lore and traditions, tracks, and other material evidence, and through the elaboration of circumstantial evidence.

For an unknown animal to be reported, or for it to bring attention to itself, it is necessary that it should be *highly visible,* and hence be of an *appreciable size.* This concept is, of course, subjective and rather relative, and should perhaps be supplemented by that of *abnormal size* within a certain group. Few except professionals would consider reporting unexpected observations of a bird of the size of a sparrow or of a fish of the length of a minnow; however all would marvel at an ant as large as a vole, or at a spider spanning a long-playing record. In fact, those elusive animals with which we are concerned, and around which legends are soon woven, permitting them to be sought and tracked down, are those which are characterized by some trait which is truly singular, unexpected, paradoxical, striking, emotionally upsetting, and thus capable of mythification. Even if that trait is not necessarily linked to an impressive size, it remains that a minimum size is essential.

Thus, cryptozoology generally does not deal with most of the phyla listed above. There are a few rare exceptions: a gigantic medusa, the lion's mane jellyfish, whose gelatinous bell has a diameter of nearly three meters, and whose trailing tentacles hang down nearly 40 meters; a marine worm, *Lineus longissimus,* no thicker than one's thumb, but reaching up to twenty meters in length; a few gigantic marine molluscs, mostly octopuses and squids; some sea scorpions of ages past which exceeded the size of a man; and finally some very rare present-day crustaceans such as the Japanese stilt-crab. Generally, however, most animals of medium to large size are, and have always been, to our knowledge, members of the last phylum, the chordates, and, further, of a single one of its sub-phyla. These sub-phyla are three in number: the Urochordata, or tunicates, of which the best known representative is an edible sea-squirt, the "Vioulet" or "Biju" of the Provencal fish markets; the Cephalochordata or Acrania (without skull), of which the famous *Amphioxus* (now in the genus *Branchiostoma*), a transparent marine dart, is held to be very close to the ancestral type of the vertebrates, but which does not exceed half a dozen centimeters in length; and the Vertebrata proper, which are of great interest to cryptozoology.

Thus, except in the marine area, where we shall have to take into account the exceptional but nevertheless possible existence of giant invertebrates most probably belonging to one of four definite phyla (Cnidaria, Nemertina, Mollusca, and Arthropoda), those hitherto unknown animals whose existence and classification one might hope to determine by the methods of cryptozoology are almost certainly members of one of the following eight classes which make up the sub-phylum Vertebrata:

a) *Agnatha,* jawless fish, represented nowadays by lampreys, hagfish, and slime eels;
b) *Placodermi,* mainly armored fishes of the Devonian period, but possibly also including the chimeras, most often included in the next class;
c) *Chondrichthyes,* cartilaginous fishes, including sharks, skates, and rays;
d) *Osteichthyes,* bony fishes (i.e., all other fishes);
e) *Amphibia,* most of which lead a double life, first aquatic, then terrestrial;

f) *Reptilia,* cold-blooded "crawlers," mainly in tropical zones;
g) *Aves,* warm-blooded, feathered flyers;
h) *Mammalia,* with hair and mammary glands.

Normally, each time a new animal is collected or captured, it is rather easy for a trained zoologist to determine, after a brief examination (often a mere glance suffices), which phylum of the animal kingdom it belongs to. For parasitic worms, or for wormlike sea creatures, some difficulties of identification may arise, requiring special expertise. If the unknown creature is a member of the sub-phylum Vertebrata, however, it is almost childs-play to recognize the class into which it should be placed. Specialists of the fundamental classes of vertebrates—ichthyologists, herpetologists, ornithologists and mammalogists—can usually recognize at a glance the order and often the family to which a given specimen belongs. Determination of the genus, and especially of the species, usually requires a more detailed study.

Where to put the Unclassifiables?

It soon became apparent that there exist "deviant" animals, which do not correspond to the definition of their name: exclusively aquatic amphibians; reptiles which, far from crawling, run, jump and even fly; birds which cannot fly; and mammals without real breasts. Such "deviants" usually exhibit most of the *other* traits of their class, however, and it is usually easy to recognize their true nature. Might it not happen, though, that one would not know exactly where to place an unknown animal because it does not fit into *any* of the known groups?

This could well happen, and has actually occurred often in the past. There was a time, not so long ago, when it was not even certain in which kingdom to place certain creatures. Up to the Eighteenth Century, coral was thought to be a petrified marine tree, intermediate between the plant and the mineral kingdoms. Such a double nature was also attributed to what were appropriately named zoophytes, such as sponges and sea anemones, of which it was not known, in the words of Pierre Belon (1555) "whether they were plants or animals."

More careful studies have, of course, dissipated such superficial ambiguities. For centuries now, whenever a new animal has appeared onto the zoological stage, it has always been possible to fit it into one or another of the known phyla, or even classes, because the limits of these broad categories long remained poorly defined. The existence of catch-all categories, such as those of worms, or vermidians, was also of providential usefulness in this respect. However, even after the diagnosis of fundamental categories had been rigorously established, classification problems still occurred from time to time, and it was sometimes necessary to create new groups to accommodate the unclassifiables. The unearthing of fossil forms has often required

the creation of broad new categories. And sometimes, when a modern form was discovered, it so happened that it could be placed within an established group whose other representatives were known *only* from the fossil record. Such animals have been called "living fossils," an expression invented, perhaps in jest, by Darwin himself, and which has been much abused as it really has no objective meaning.

As the exploration of our world has progressed, the number of new forms being discovered has gradually diminished, whether they be true fossils or "survivors" from the past. More importantly, the number of new classes created to accommodate these new creatures has also gradually decreased over time. Nowadays, the description of new species is a routine matter, and that of new genera usually equally unexciting. Much more rarely, one may have to introduce a new family. Innovation at the level of the higher categories—orders, classes, phyla—has become, as one would expect, quite exceptional.

Nevertheless, however improbable this may appear, major reclassifications of animals are still sometimes necessary in order to accommodate completely unexpected new life forms.

Upsetting Discoveries Come from the Sea

Limiting ourselves to the present century, we shall briefly review the most dramatic revolutions which occurred in the classification of animals. The order which we shall follow is that of the phyla listed above.

In 1959, a completely new type, requiring the creation of a new family, was added to the coelenterates of the phylum Cnidaria, when the British oceanographic vessel *Sarsia* brought up, from a depth of more than 3,000 meters in the Bay of Biscay, a plum-colored jellyfish trailing tentacles which, when fully extended, measured two meters from tip to tip. Viviparous, although reproducing asexually, this animal was baptized *Stygiomedusa fabulosa*: the fabulous jellyfish from the deep.

It is also through the exploration of the abyssal depths that we have considerably enriched out knowledge of the phylum Mollusca.

In 1903, a black ciliated octopus which appeared common at great depths, but with *ten* tentacles rather like a cuttlefish or a squid, was described by the German scientist Carl Chun, and given the impressive name *Vampyroteuthis infernalis*: the devilish vampire-squid. After an in-depth study of this ambiguous monster, Grace Evelyn Pickford created the new order Vampyromorpha for it in 1946.

An even more sensational incident occurred in the world of molluscs. Their classification was completely disrupted in 1940, following a reinterpretation by N. Odhner of a small cap-shaped fossil shell, long known under the name *Pilina*: a new class, the Monoplacophora, had to be created for this organism. One may well imagine the surprise and the emotion of the Danish specialist Henning M. Lemche when he recognized, in a haul of

specimens dredged by the *Galathea* in 1952 in the Pacific off Costa Rica, a living specimen of this class, thought to be extinct for 350 to 400 million years. He baptized it in 1957 with the name of *Neopilina galathea.* Since then, three new species of this genus have been discovered.

The phylum Arthropoda is that to which the greatest number of important additions have been made in this century, among the crustaceans, as well as insects, and even recently in the arachnids.

Entire orders had to be added to the class Insecta: the Protura, in 1907, minuscule insects, without either wings or antennae, which do not even go through metamorphosis; the Zoraptera, in 1913, which includes about twenty small tropical species; the Notoptera, in 1915, fairly large insects which live in the snow of high mountains in Japan and North America, at a temperature close to freezing. Quite recently, in 1978, a strange male cricket was collected in Queensland, Australia; it combined the characteristic traits of three different families of Orthoptera, and a new family will probably have to be created for it. Until more specimens are caught, and a more appropriate and pompous scientific name is chosen, this cricket goes under the nickname "Cooloola Monster."

In 1983, the discovery in a tropical rain forest of the smallest known spider, *Micromygale diblemma,* led to the creation of a new sub-family of arachnids by its scientific sponsor, Norman I. Platnick. One must admit that this creature is completely abnormal: not only does it lack lungs, breathing directly through its skin, but it has only two eyes, which is truly monstrous in a group where everyone has from six to eight!

It is undoubtedly among the aquatic arthropods, particularly the crustaceans, that the most radical taxonomic changes have occurred, and invariably continue to happen.

Among the peracarids, a fossil subclass, a new order, the Thermosbaenacea, had to be introduced in 1927 by Theodore Monod, following the capture by L.-G. Seurat, in the Tunisian hot spring of Hel Hamma, of specimens of a unique species which Monod had named *Thermosbaena mirabilis* in 1924. Nine other species of this well-hidden group have subsequently been discovered, all in the Mediterranean area, and as far as Texas in the New World. They had to be accommodated into three new genera: *Monodella,* described in 1949 by Ruffo, which now contains seven species, and the genera *Halosbaena* and *Limnosbaena,* described by Stock in 1976.

The sub-class of the minute mystacocarids was created only in 1943, following the discovery, in the interstitial waters of a sandy beach, of the first specimen of the genus *Derocheilocaris,* whose various species are now known the world over.

In 1957, another new sub-class, the cephalocarids, reminiscent of the trilobites of the Paleozoic Era, had to be created to include the genus *Hutchinsoniella,* the first specimens of which had been fished out by Howard L. Sanders two years earlier from the muddy bottom of Long Island Sound,

New York. That same year, the new order of Speleogriphacea was added to the crustaceans, following the collection of a completely unclassifiable species in the subterranean waters of a cave in Table Mountain, overlooking Cape Town, South Africa.

In 1975, two French scientists, Jacques Forest and Michèle de Saint-Laurent, recognized in a jar stored at the Smithsonian Institution a representative of the family Glypheidae, believed extinct since the Eocene. It had been marinating there for nearly seventy years, since it had been collected in 1908 by the oceanographic ship *Albatross* off the Philippines. In the spring of 1976, the same two scientists brought up a live specimen of this animal, named *Neoglyphea inopinata,* a presumed ancestor of crabs, shrimps, and lobsters.

The phylum Echinodermata has also experienced a revolution, without even the need to track down a new species. In 1962, the New Zealander H. Barraclough Fell announced that a relatively rare starfish from the Pacific coast of Mexico, *Platasterias latiradiata,* described by Gray in 1871, was really the last survivor of the fossil class Somasteroidia, believed extinct since the Ordovician, i.e., 400 million years ago.

A Completely New Phylum of Invertebrates and Many Important Fishes

Even more extraordinary, almost unthinkable, would be the discovery of a whole unknown phylum of the animal kingdom. But that is precisely what happened not very long ago.

This astounding revelation was the result of a gradual process. In 1914, Maurice Caullery had given the name *Siboglinum weberi* to a kind of thread-like and transparent worm dredged from abyssal muds by the Dutch oceanographic expedition ship *Siboga.* The French biologist described it prudently as "a new type of invertebrate," and tentatively classified it as an addendum to the phylum Stomochordata. Returning to it in 1944, he was of the opinion that its affinities were still imprecise. In the meantime, in 1933, the Russian P. V. Ushakov had described a similar creature, which he named *Lamellisabella zachsi,* as an unknown form of the polychete worms. A few years later, in 1937, the Swede K. E. Johansson carried out a careful histological study which cast doubt upon the validity of that interpretation. For the latter, the species in question justified the creation of a new class of worms: the Pogonophora, or "beard wearers." Then, in 1944, the Russian V. N. Beklemishev went further in suggesting that this class of "bearded worms" was really a distinct phylum, to be placed in the vicinity of the Stomochordata, as Caullery had brilliantly suspected. This interpretation has since been fully justified, mostly from the rich catches brought back from the oceanographic cruises of the Soviet oceanographic vessel *Vitiaz* (1949–58), and, to a lesser extent, by the Danish ship *Galathea* (1952).

Today, the phylum Pogonophora still includes a single class, which is now,

however, divided into two orders, containing five families, for a total of eleven different genera. Of the 43 known species, 37 have been described by the Soviet scientist Artemii Vassilievitch Ivanov, who has become the greatest specialist on this phylum, having written in 1960 a now-classic monograph on that subject. These pogonophorans literally swarm in the abyssal muds, forming masses resembling Chinese noodles. Their most remarkable feature is their complete lack of a digestive tube; they are the only multicellular organisms in the world without one. Zoologists are still speculating on just how they feed.

One will have noted that the revolutions which have occurred in the Twentieth Century in the classification of the animal kingdom have, so far, all been concerned with invertebrates. The most sensational, the most stupefying of all the zoological discoveries of our time is thus undoubtedly that of the coelacanth, *Latimeria chalumnae* in 1938. This fish, of the order Coelacanthiformes, a survivor from the Devonian, is the only living representative of the class Crossopterygia, believed extinct for the past 65 million years.

Among newly discovered fish, one whose novelty led to the creation of a systematically important group is a mid-depth stingray from South Africa, *Hexatrygon bickelli.* In contrast to all other batoid fishes (electric rays, guitar fishes, sawfishes, skates, stingrays, eagle-rays and devil-rays), this ray has six rather than five gill slits. When it was described, in 1980, it was placed in a new sub-order within the order Myliobatiformes: the Hexatrygonoidei.

Finally, a large shark, 4.5 meters long and more than 700 kilos in weight, caught quite fortuitously by a U.S. Navy ship off Hawaii in 1976, seems to justify by its aberrant anatomy the invention of a whole new family. This fish, known for years only under the nickname "Megamouth," for its extremely large mouth, has just been given the scientific name *Megachasma pelagios* (Taylor *et al.* 1983).

On Land: Much That Is New, But Nothing Revolutionary

To find readjustments of comparable magnitude in the classification of modern land vertebrates, one must go back to the last century.

In 1831 and 1842 John Edward Gray successively described under two different names, *Sphaenodon* and *Hatteria,* a cranium and a complete specimen of the same species of small New Zealand lizard. This animal was called tuatara, or "spiny-back," by the Maoris. A quarter of a century later, in 1867, Gray's assistant, Albert Günther, realized, while studying the anatomy of this unorthodox lizard, that it was really a survivor of the order Rhynchocephalia, known only from the fossil record, and more ancient than the dinosaurs.

The platypus, an extraordinary Australian animal with webbed feet and duck's bill first described by George Shaw in 1799, had first been placed by Johann Friedrich Blumenbach in the hodge-podge order of Edentata. Etienne Geoffroy-Saint-Hilaire, having noticed that this pseudo-otter, together with his pseudo-hedgehog cousin the echidna, the latter with a long beak and sporting a venomous spur, both had a cloaca, as reptiles and birds do, thought it more appropriate to set up especially for them the order of monotremes, "those with a single hole." These alleged mammals, which seemed to be lacking breasts and even appeared to be egg-layers, were too bizarre for Lamarck, who refused to include them with mammals, and who introduced for them an independent class which he called the Prototheria, or "first mammals." In 1834, Ducrotay de Blainville divided the class of mammals into three sub-classes: the Ornithodelphia (or monotremes), the Didelphia (or marsupials) and the Monodelphia (or placentals). It was only a half-century later that it was unequivocally determined that monotremes, those "unacceptable" mammals which nevertheless gave milk to their young, did indeed lay eggs. From now on, anything seemed possible in the animal kingdom.

In fact, it still happens, even nowadays, that a new family of terrestrial vertebrates must be introduced.

Thus, it was as recently as 1954 that it was noticed that the world's rarest lizard, a small earless monitor lizard from Borneo, *Lanthanotus borneensis,* discovered in 1878, exhibited so many of the traits of snakes that it had to be closely related to their common ancestor. It was thus placed, by itself, in a special family, the Lanthanotidae. Known until then only through a dozen mummified or preserved specimens, it was not until 1961 that a live specimen was captured and examined.

The family Craseonycteridae was created in 1974, following the discovery in Thailand of a very peculiar kind of bat, *Craseonycteris thonglongyai.* Some may state that this is really not so exciting, since it is the smallest known mammal. Its body, head included, hardly exceeds 3 centimeters, and it weighs no more than 2 grams; it has been nicknamed "the bumblebee bat." Nevertheless, this small animal represents a real significant zoological novelty.

All told, then, when one reviews the story of the major modifications of the zoological classification since the beginning of this century, one must recognize that they have been mostly concerned with unknown types of invertebrates, usually small and generally from the depths of the ocean, or from the equally dark waters of caves. What this means is that there is really very little chance of discovering, at least on land, large vertebrates *of a truly original body plan, and thus unknown even in the fossil record.*

This does not imply that we might not hope to discover such new forms in the oceans, or even in some inland waters. We shall see that one should expect to find, even on large land masses, numerous new species, some of

which may belong to unknown genera. It is certain that such species exist, and that some may be of appreciable size. I have already reviewed the numerous large land animals which had been discovered within the past 120 years (Heuvelmans 1955, 1958). Since such reviews, discoveries have continued to occur. It will suffice to recall a few: in 1965, of an unknown genus of cats, *Prionailurus iriotomensis,* in one of the Ryukyu Islands (Japan); in 1969, of a current Asiatic representative of the human Neanderthal wave, *Homo pongoides* (Heuvelmans 1969); and, in 1975, of the largest of the peccaries, *Catagonus wagneri,* known only as a Pleistocene fossil, and discovered live in Paraguay (Wetzel *et al.* 1975). Compared to those important discoveries made in the oceans or in caves in the past decades, these are really commonplace, at least from the perspective of systematic zoology, and one should perhaps be surprised that some of them have led to vehement and ongoing controversies.

Contrary to what one might be led to believe, one should not prejudge the size of animals still to be discovered, imagining that the smallest are necessarily those which can hide best. Paradoxically, in many groups, it is often the largest specimens which have been discovered last. The largest of the sharks, the whale shark, was discovered in 1828; the largest of the cephalopods, the giant squid, in 1856; the largest of the Procyonidae or raccoons, the giant panda, in 1869; the largest of the bears, the Kodiak bear, in 1899; a local race of the largest of rhinoceroses, Cotton's white rhino, in 1900; the largest of all apes (and the largest known non-human primate), the mountain gorilla, in 1901; the largest of the boars, the giant forest hog, in 1904; the largest of the lizards, the Komodo dragon, in 1912; and, as we noted above, the largest of the peccaries, the tagua, in 1975.

Have We Completed Only A Quarter of the Inventory?

It is now possible to calculate, with a fairly satisfactory degree of approximation, the number of new species which one should expect to discover in the decades to come. This calculation is based on the number of species already known.

We do not know, of course, and have no means of knowing exactly, how many animal species inhabit our planet. On this point, only conjectures are possible, and although some of these may appear outrageous, they are based on the continuing abundance of the yearly crop of new species. The count of living species is now well over a million. That, however, might be only a modest fraction of the total. French zoologist Lucien Berland (1942) wrote: "On the whole, it does not seem exaggerated to think that there must exist about 5 million species on the globe, and perhaps even more, of which 9/10 are insects." If that is truly the case, then *less than a quarter* of all species presently living have been collected and scientifically described. According to a new calculation by Terry L. Erwin (1982), as many as 30 million arthropod species may exist. If this is correct, we have not yet completed even one thirtieth of the planet's zoological inventory!

What basis is there for such risky estimates? The extrapolation is based, quite straight-forwardly, *on the rate of increase through the years and decades of the number of known species, assuming, quite legitimately, that the rate of discoveries will vary in the future according to a trend already perceptible in past centuries.*

To carry this out (Table 1), one should take into account those general inventories which have been presented in the past, beginning, of course, with that of Linnaeus (1758). In 1817, Cuvier contributed a useful survey of animal species recognized up to that date. These compilations may then be compared to those of Johannes Leunis (1860), inspired in part by the inventories of Wagner, Göppert, and mainly Bronn, presented in 1848 and 1850. We shall then take into account the numerical estimates included in the revised edition of Leunis by Hubert Ludwig (1886), and those of Karl August Möbius (1898), based on the records of the Museum of Natural History in Berlin up to 1896. In the Twentieth Century, we shall note H. S. Pratt's (1912) recapitulation, followed by the painstaking censuses conducted by R. Hesse (1929), carried out with the collaboration of numerous German and foreign specialists, and based on the most careful literature surveys. Finally, we shall incorporate the estimates of Ernst Mayr (1946), which may be compared to his own precise census of the number of bird species.

It is also useful to compare these numerical data sets to more restricted censuses bearing on specific groups of animals. One finds, among the latter, the very old surveys by Henshaw of various classes of vertebrates, by Sharpe of the birds, and the more recent ones by Schultz and Stern of the fishes, by Angel and Cochran of the amphibians, by Guibé and Carr of the reptiles, by Fisher and Peterson of the birds, and by Poole, by Morris, and finally by Honacki, Kinman and Koeppl of the mammals. Henshaw (1912) proposed the following numbers of species: fishes: 12,000; amphibians: 2,200; reptiles: 6,000; birds: 20,000; mammals: 7,000. Schultz and Stern (1948) estimated the number of fish species at 40,000. Angel (1947) enumerated 1,415 species of amphibians, while Cochran (1961) gave a higher estimate, around 3,000. Guibé (1962) spoke of approximately 6,000 species of reptiles, while Carr (1963) gave the more precise count of 5,924. While Sharpe (1909) had recorded 18,937 species of birds, Fisher and Peterson (1964) counted only 8,616. Finally, the early count by Poole (1926) of 10,041 forms of mammals was reduced by Morris (1965) to 4,237 species, and by Honacki *et al.* (1982) to only 4,170.

When all these rows and columns of numbers are presented graphically, a number of striking contradictions appear. The result is not the stepwise

Table 1.—Change in the number of registered species in all major groups of animals and in each class of vertebrates during modern times. The data appearing under the names of the various authors do not necessarily refer to the year of publication, but to the period of time concerned. Whereas the numbers given by all other authors are based on countings or estimates (expressed graphically in Figs. 1 and 2), the numbers given by Heuvelmans are based on the graphical expression of their data taken altogether, and occasionally corrected in the light of more recent or more accurate numberings of specific groups (Figs. 3 and 4).

	1758 (Linné)	1817 (Cuvier)	1850 (Leunis)	1850 (Heuvelmans)	1886 (Leunis & Ludwig)	1896 (Möbius)	1900 (Heuvelmans)	1911 (Pratt)	1928 (Hesse)	1946 (Mayr)	1950 (Heuvelmans)	2000 (Heuvelmans)
Protozoa	3	10	1,400	1,300	4,130	6,000	6,500	8,000	—	15,000	16,000	21,100
Porifera	11	59	528	520	600	1,500	1,700	2,500	4,500	5,000	5,300	6,000
Coelenterata	141	92	3,000	2,700	3,000	3,000	3,400	4,500	9,084	10,000	10,500	12,200
Vermes	47	101	1,270	1,250	6,300	9,000	9,700	12,700	19,650	25,000	26,200	32,000
Mollusca & Brachiopoda	701	965	11,400	10,000	21,320	50,000	64,000	61,000	103,820	88,000	102,400	125,300
Crustacea	101	360	1,500	1,430	5,600	8,150	9,500	16,000	15,500	25,000	26,300	33,400
Arachnida	78	168	3,000	2,700	4,000	20,000	10,200	16,000	28,000	30,000	31,000	34,600
Myriapoda	16	24	700	600	800	3,000	2,400	2,000	8,100	10,000	10,700	13,000
Insecta	1,937	1,393	64,300	62,800	200,000	281,050	300,000	360,000	750,000	750,000	765,000	967,000
Echinodermata	36	143	1,232	1,200	2,370	3,000	3,000	4,000	4,200	4,700	4,800	5,500
Protochordata	—	—	—	—	300	400	600	1,300	1,600	1,700	1,800	2,200
Vertebrata	1,335	2,421	18,567	8,500	24,800	33,500	24,000	34,400	69,319	35,600	40,000	47,700
Pisces	415	1,031	8,000	4,000	9,000	12,000	11,100	13,000	20,000	18,000	20,000	25,600
Amphibia	20	27	400	300	1,000	1,450	1,500	1,400	2,858	1,600	2,600	3,100
Reptilia	162	212	1,100	800	2,500	3,550	3,000	3,500	5,461	3,900	5,000	5,900
Aves	554	765	7,000	2,500	10,000	13,000	6,300	13,000	28,000	8,600	8,700	8,870
Mammalia	184	386	2,067	900	2,300	3,500	2,100	3,500	13,000	3,500	3,700	4,230
Totals	4,406	5,736	106,897	93,000	273,220	418,600	435,000	522,400	1,013,773	1,000,000	1,040,000	1,300,000

increasing trend which one might have logically expected, but a chaotic profile, studded with peaks, looking like the temperature chart of a malarial patient (Figs. 1 and 2).

Why is it, for example, that the number of species in many groups increased abruptly from 1886 to 1896, in just ten years? And, on the contrary, how is it possible that for some other groups—the arachnids and the myriapods, for example—there should be many less species known in 1911 than in 1896? What can possibly explain the dramatic fall in the number of vertebrates between 1928 and 1946?

A number of factors are responsible for these irregularities and apparent absurdities.

First, it was realized that a number of species had been described many times, under different names, which reduced the total number by the elimination of these synonyms. Second, one should note that some inventories (such as Hesse's for all animals, or those of Sharpe for the birds, Poole for the mammals, and Schultz and Stern for the fishes) included *all* known forms, including sub-species as well as species. For mammals as well as for birds, the number of sub-species or geographical races is about three times larger than that of species. Such compilations, in particular that of Hesse, which is responsible for the extremely high peaks of 1928, are to be interpreted as qualitative indicators. Finally, and most importantly, a great number of former species have turned out to be only local varieties or subspecies, and sometimes mere individual variations.

Two opposing tendencies have long divided systematists into two groups: the "splitters" and the "lumpers." Today, it is fortunately the lumpers who have the upper hand. This explains the relatively modest number of species given by Mayr, who took great care to include only so-called "good" species. Given his authority in the matter, one should undoubtedly place the greatest confidence in his results, even though he seems to have exaggerated slightly in the direction of lumping when dealing with vertebrate classes outside his own specialty of ornithology.

A Numerical Estimate of Our Expectations

Taking into account all the data at my disposal—and there are surely others—I have tried to show graphically the gradual enrichment of some of the main groups in the animal kingdom (Fig. 3), and in particular of the various classes of vertebrates (Fig. 4). To ensure uniformity, I have had to follow, on the whole, the older, less detailed classifications in the figures as well as in the tables. This is why such old catch-all categories such as Vermes (worms and vermidians) have been resuscitated; why all chordates other than the vertebrates and the hemichordates have again been lumped into a heterogenous group, the Protochordata; and, finally, why such unrelated

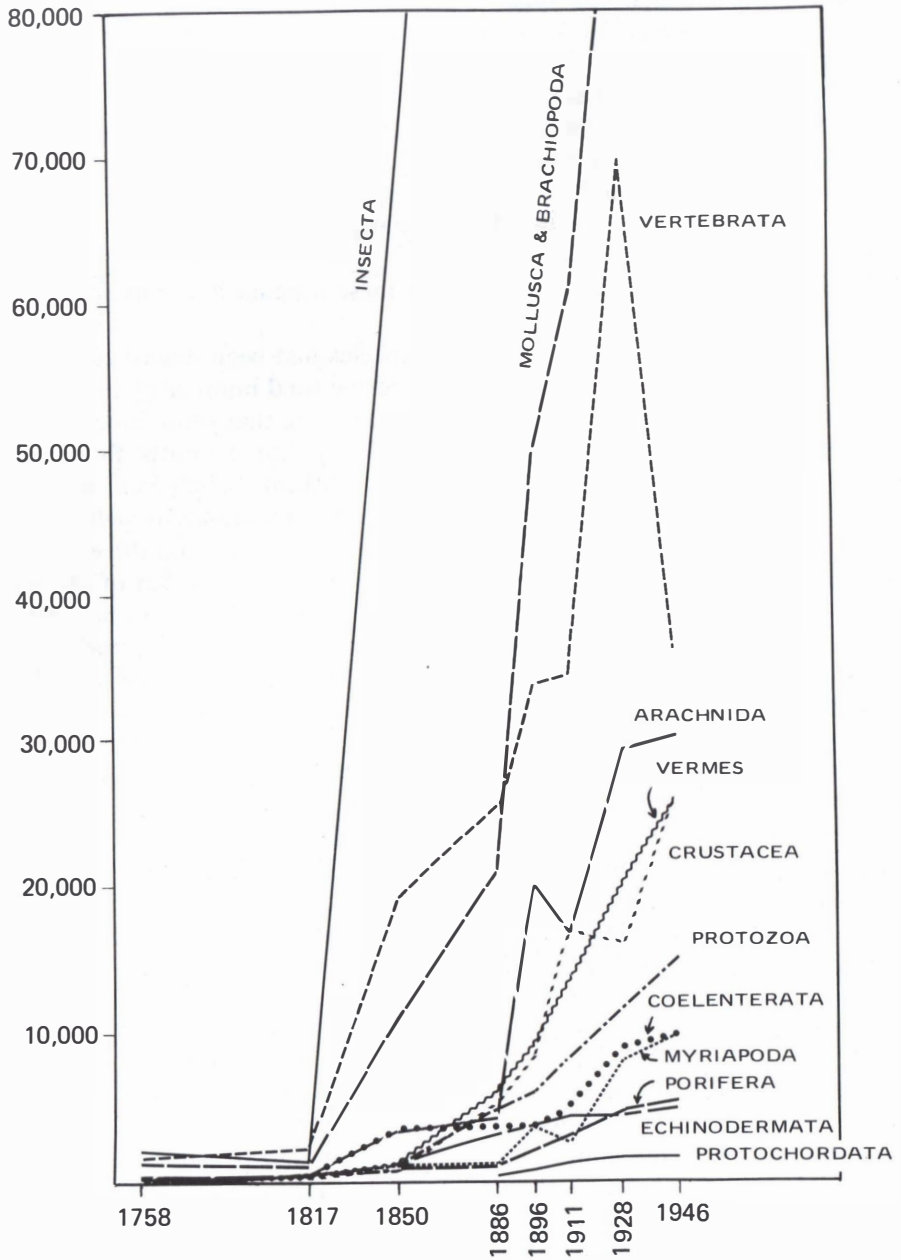

FIG. 1.—Change in the number of registered species in all major groups of animals during modern times according to various authors (see Table 1).

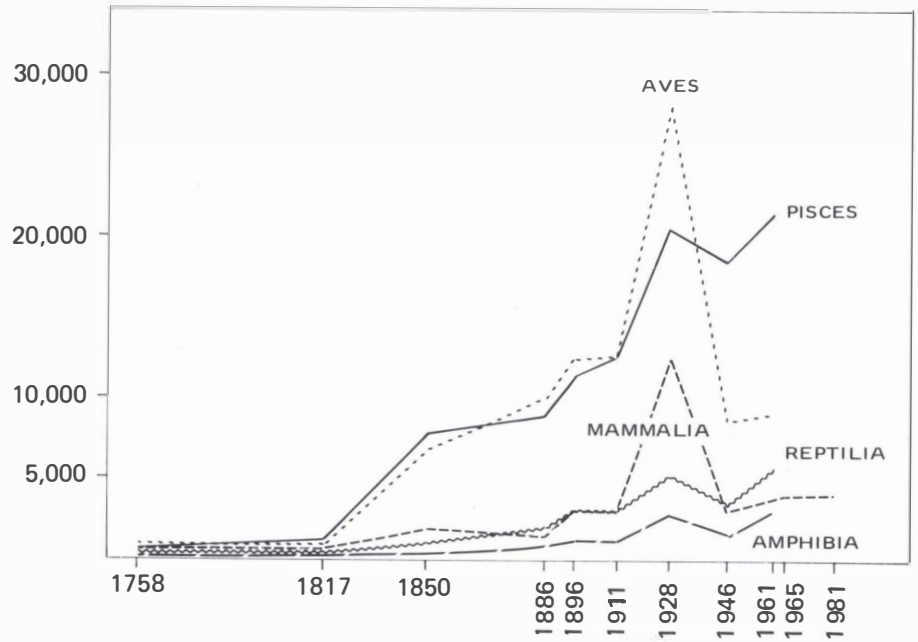

FIG. 2.—Change in the number of registered species in all classes of vertebrates during modern times according to various authors (see Table 1).

animals as molluscs and brachiopods have been grouped together, as they formerly were.

The curves obtained by a judicious smoothing of the original zigzag plots probably represent as faithfully as possible the real increase in "good" species contained in the zoological catalogs of the past two centuries. They should certainly help to determine any real trend from the days of Linnaeus to our own. Continuation of the early Twentieth Century part of the curves should, by extrapolation, yield rather precise estimates on the status of species in 1975 (which it does, on the whole, for those groups which have been the object of an accurate census) and provide useful approximations for the year 2000.

These predictions are of course less precise for curves which rise more steeply, and thus for which the number of species discovered annually is higher. Hence, it is the results on the molluscs, the brachiopods, and especially the insects which must be interpreted most carefully. In tracing the curves, I have always favored a more conservative approach, as shall become apparent below.

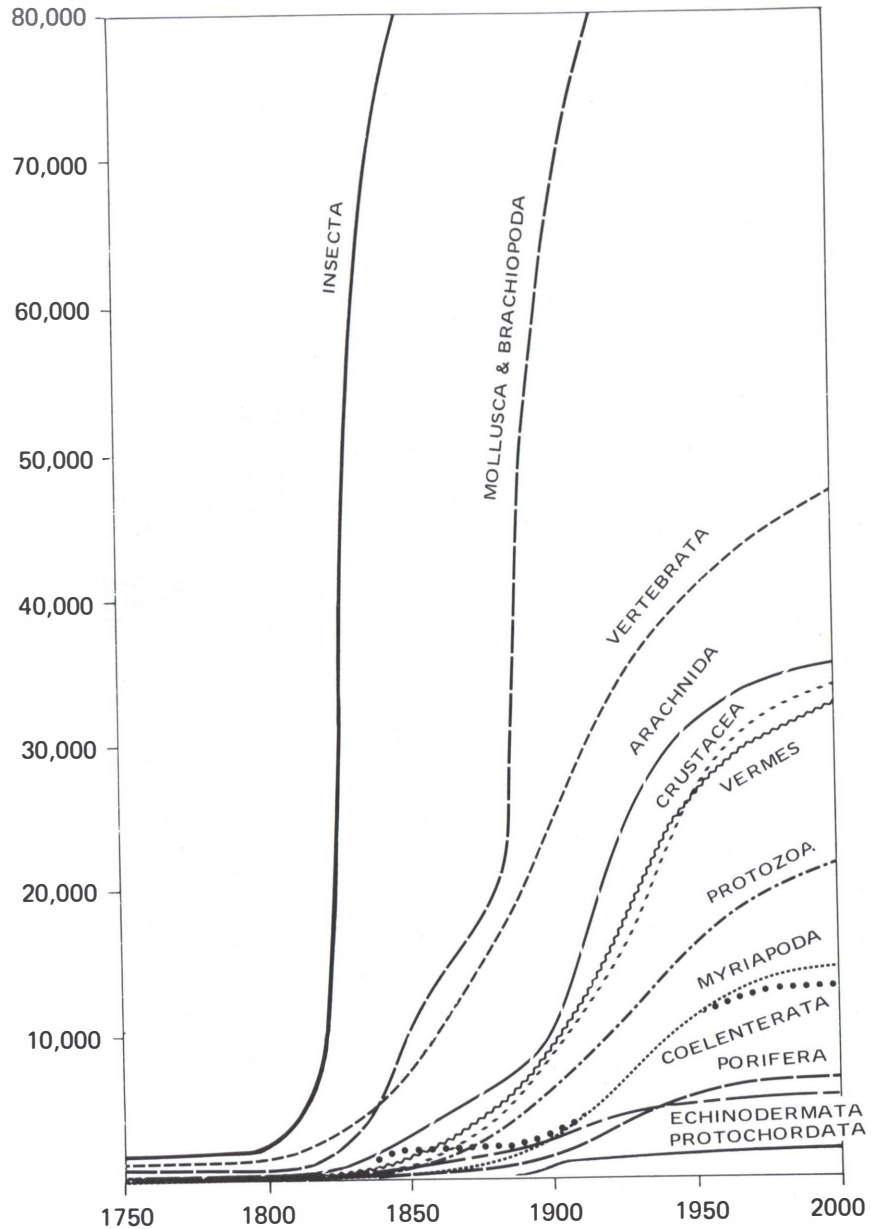

FIG. 3.—Increase in the number of known species in all major groups of animals during modern times, as proposed by the author.

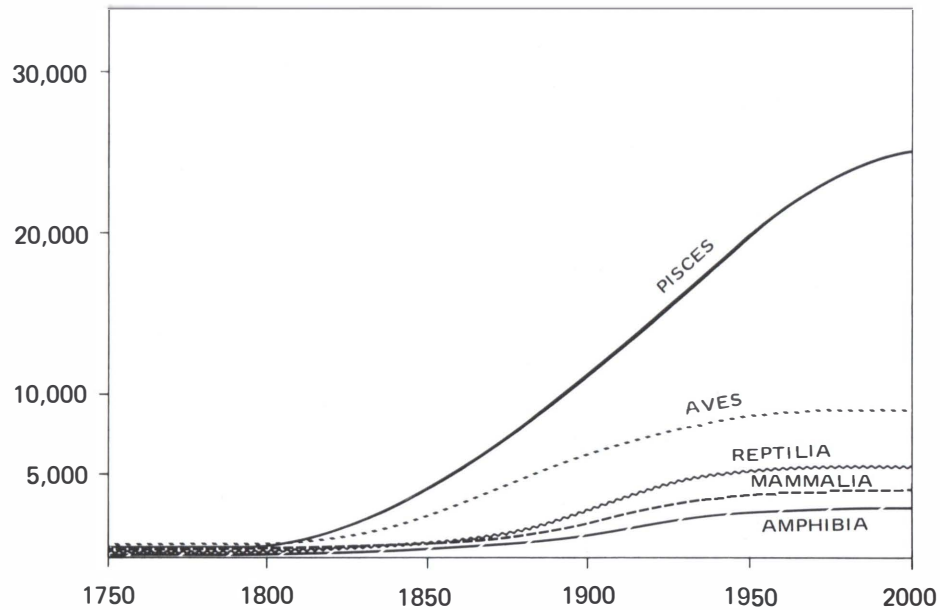

FIG. 4.—Increase in the number of known species in all classes of vertebrates during modern times, as proposed by the author.

Numerical estimates taken from the smoothed curves for the years 1850, 1900, 1950 and 2000 have been entered in Table 1 for the sake of comparison of these personal estimates with earlier results.

To determine the rate of increase of the number of known species in each group of organisms, one takes differences between data half a century apart. Dividing by 50 then yields the average number of species described each year in each half century, or, equivalently, assuming that the rate of discovery does not vary too quickly, the number of species discovered each year at the mid-point of each period, i.e., in 1875, 1925 and 1975 (Table 2).

The final result of all these calculations is that the number of animal species increased by nearly 350,000 during the second half of the last century and by about 600,000 during the first half of the present century; it should increase again by at least 250,000 (from our conservative point of view) during the second half of this century. The inventory of the animal kingdom has been enriched annually, on the average, by nearly 7,000 new species between 1850 and 1900, and by about 12,000 between 1900 and 1950; this gradual increase will continue at about 5,000 per year for the period 1950–2000. The insects contribute the majority of the annual figures: nearly 4,750

TABLE 2.—Rate of discovery of new species of animals in all major groups and in each class of vertebrates during modern times. As the average number of species discovered per year during each half of a century corresponds approximately to the number discovered about the middle of these periods, the relevant figures have accordingly been situated in 1875, 1925 and 1975 in Fig. 5.

	Between 1850–1900	Average per annum	Between 1900–1950	Average per annum	Between 1950–2000	Average per annum
Protozoa	5,200	104	9,500	190	5,100	102
Porifera	1,180	23.6	3,600	72	700	14
Coelenterata	700	14	7,100	142	1,700	34
Vermes	8,450	169	16,500	330	5,800	116
Mollusca & Brachiopoda	54,000	1,080	38,400	768	22,900	458
Crustacea	8,070	161.4	16,800	336	7,100	142
Arachnida	7,500	150	20,800	416	3,600	72
Myriapoda	1,800	36	8,300	166	2,300	46
Insecta	237,200	4,744	465,000	9,300	202,000	4,040
Echinodermata	1,800	36	1,800	36	700	14
Protochordata	600	12	1,200	24	400	8
Vertebrata	15,500	310	16,000	320	7,700	154
Pisces	7,100	142	8,900	178	5,600	112
Amphibia	1,200	24	1,100	22	500	10
Reptilia	2,200	44	2,000	40	900	18
Aves	3,800	76	2,400	48	170	3.4
Mammalia	1,200	24	1,600	32	530	10.6
Totals	342,000	6,840	605,000	12,100	260,000	5,200

a year during the second half of the Nineteenth Century, more than 9,000 for the first half of the Twentieth Century, and at least 4,000 during its second half.

This is where the conservative nature of my estimates is confirmed, in the light of those offered by others. Edward Chapin (Anonymous 1952) evaluated at about 5,000 the number of new insect species discovered each year; Maurice Burton (1961) gave a higher estimate, 6,000, and Joan Arehart-Treichel (1978), 7,000.

An analysis of the numerical results of Table 2 leads to the following conclusions.

First, that it is during the first half of our century that the rate of discovery of new species reached its peak in the majority of phyla of the animal kingdom. The only exception to this rule is the heterogeneous group of molluscs and brachiopods, where the rate of discovery peaked in the last century and has since continuously declined.

The rate of discovery of new species has dropped, more or less abruptly, in all phyla. This deceleration is most noticeable nowadays, following the former period of abundant discoveries during the first half of this century.

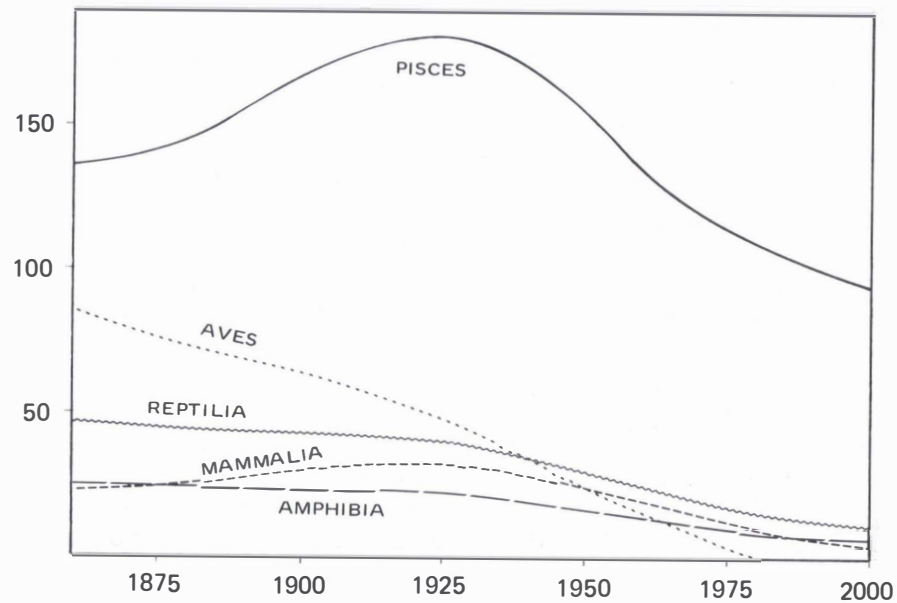

FIG. 5.—Rate of discovery of new species in all classes of vertebrates during modern times (see Table 2).

This is of course not unexpected: the faster the rate of discovery, the more rapid the decrease of the remaining, yet-to-be-discovered stock.

What has happened for the various classes of vertebrates should be of particular interest to cryptozoologists. Quite recently, in the mid 1970's, there were discovered, each year, around 112 new species of fishes, 18 new species of reptiles, about ten new species of amphibians, the same number of mammals, and 3 or 4 new species of birds. Here, too, the rate of discovery has slowed down at different rates in the various classes. A graphical presentation of the data of Table 2 gives a more exact idea of this variety (Fig. 5).

One may note from Fig. 5 that the deceleration of the annual discovery rate had already begun in the last century for most classes of vertebrates, but not for the fishes, well hidden in the waters, particularly those of the marine environment, nor for the mammals, more elusive than had been imagined, perhaps because of their higher intelligence. Today, the rate of discovery is slowing down only very gradually for reptiles and amphibians, of which one should still expect to discover 12 and 7 species a year, respectively, by the year 2000. At the same time, it should decrease much

more rapidly for the mammals, with only 3 or 4 new species a year by the end of the century, and extremely rapidly for the fishes, with only about 90 species a year by then, which is *relatively* little given the number of species in that class. It is for the birds that the decline in the rhythm of discovery is most dramatic; one can even predict that the inventory of that class will have been practically completed between 1985 and 1990. This is, of course, quite a logical expectation, given the generally conspicuous nature of these avians.

This study is founded on quite controversial premises, about which there exists no general consensus, and it is to be expected that the numerical conclusions which follow are subject to a certain lack of accuracy. Our essential aim has been to obtain orders of magnitude and to make comparisons.

Bearing this in mind, and barring unforeseen developments, the main thrust of our conclusions is that one may entertain high hopes for future discoveries, providing full justification for cryptozoological research. By its very nature, such research should accelerate noticeably the rate of discovery of animals of medium to large dimensions. Most importantly, it should make it possible to discover many of them before they disappear without our even having noticed them under the pressure of the irreversible destruction of many parts of the biosphere.

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THE EVIDENCE FOR WILDMAN IN HUBEI PROVINCE, PEOPLE'S REPUBLIC OF CHINA

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ABSTRACT: Wildman investigations were conducted in Hubei Province, People's Republic of China, in 1982. The distribution of Wildman reports, current research, the so-called "monkey babies," morphological traits of the supposed Wildman, and the evidence (footprints, nests, fecal remains, eyewitness accounts, and hair) are discussed.

Interviews with other investigators, interviews with eyewitnesses, translations of Mandarin-language manuscripts, and viewing of some of the evidence were conducted. It is unlikely that a bipedal primate still unknown to science is involved, but the possibility cannot be entirely ruled out.

INTRODUCTION

Reports continue to appear, arousing considerable public interest and scientific skepticism, concerning the presence (or the survival) of human/ape-like creatures in the forests, mountains, or snowscapes of such far-flung places as the Pacific Northwest Coast of the United States and Canada (Sasquatch or Bigfoot), Nepal (the Yeti or the Abominable Snowman), and now the People's Republic of China (Wildman). What is most interesting is that such reports from these different regions have many striking similarities, despite the fact that, until rather recently, there has probably been minimal contact among the cultures and the peoples making the reports. In China, reports of Wildman appear in T'Ang Dynasty (A.D. 618–907) texts, and perhaps even earlier (Zhou 1982).

Why the striking similarity among such reports? A full answer to this question awaits further analysis. One or a combination of four facts may be a basis for an explanation.

1. There is something basically similar to all the cultures reporting such creatures. Perhaps a fear of, or fascination with, relatively inaccessible mountain and forest regions leads to similar reports.

2. Despite the high degree of improbability, especially in ancient times, reports of such sightings in different areas of the world are fueled by the

diffusion of reports from elsewhere. This is more likely to happen now, with sophisticated means of communication, than was previously true.

3. Perhaps such sightings reflect reality; perhaps some of the reports are true. Perhaps the Yeti exists in Nepal, perhaps Sasquatch exists in the Pacific Northwest, and perhaps Wildman exists in the People's Republic of China. Although this is the most stimulating of all the possibilities, it is also the least probable.

4. The similarities of such reports merely represent people's conceptions of what such creatures would look like if they existed. After all, the model for such creatures is a combination of traits of living humans and living non-human primates (monkeys and apes). In some areas with reports of such creatures, however, it is unlikely that many people have ever seen an ape or a monkey.

The co-authors approach this subject from different academic backgrounds and experiences. Hu was part of a Chinese scientific team that investigated reports of Wildman in the Shen Nong Jia Mountains of Hubei Province. Poirier interviewed several Chinese Wildman investigators while he worked in Hubei in 1982. Chen also worked in Hubei in 1982, and he adds the necessary cultural anthropological perspective and provides English translations of Chinese reports. Hu is a biologist, while Poirier is a physical anthropologist and Chen a cultural anthropologist.

Distribution of Wildman Reports

Wildman reports come mainly from three areas of China: Szechwan, Kweichou (Guizhou) and Hubei Provinces. Zhou (1982) states that encounters with Wildman have also been reported in Yunnan, Shanxi, Zhejiang, Fujien and Anhui Provinces, and the Autonomous Regions of Tibet and Xinjiang. Szechwan and Hubei Provinces share a western (Hubei) and eastern (Szechwan) border. Kweichou's northern border is shared mainly with the southern border of Szechwan. The main focus of our investigation, and of other recent investigations by teams of Chinese researchers (one team including 110 people), is the Shen Nong Jia mountains in Hubei Province (Fig. 1).

In 1976, six community leaders were driving their vehicle near the village of Chungshuya, in the Shen Nong Jia District, when their headlights shone upon a "strange, tail-less creature with reddish fur" standing in the road (Topping 1981: 66). It is important to note that the creature had reddish fur, and we will return to that point later. Five members of the party approached this creature to within a few feet. The creature is reported to have been as curious about the observers as they were of it. Eventually, it disappeared into the darkness.

This report was forwarded to China's prestigious Academy of Sciences in Beijing (Peking). Scientists from the Institute of Vertebrate Paleontology and

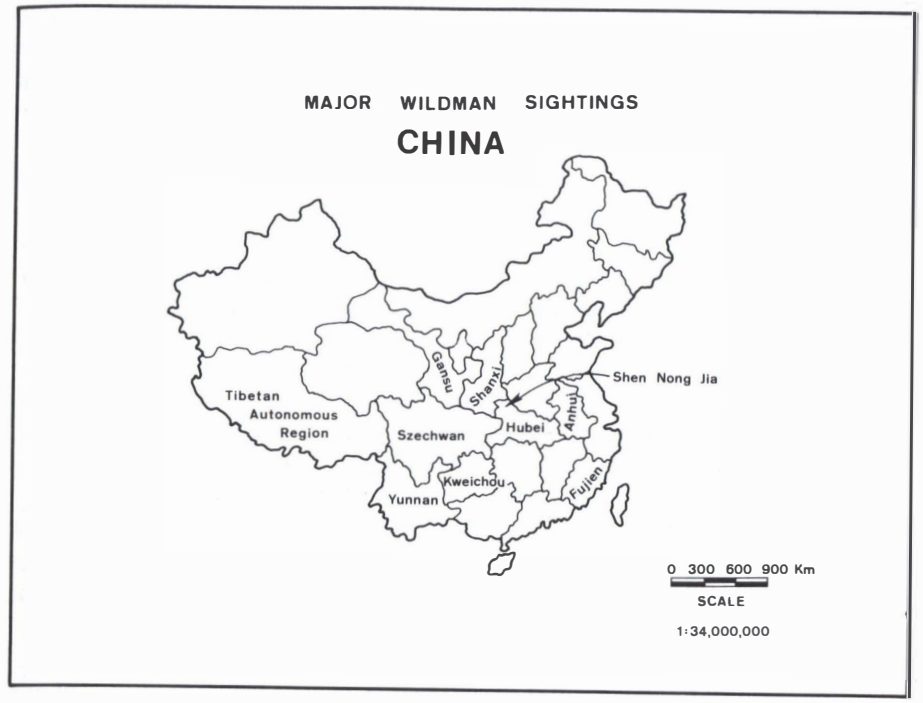

Fig. 1.—Location of Shen Nong Jia in Hubei Province, People's Republic of China.

Paleoanthropology launched a full-scale investigation that has since turned up hundreds of Wildman reports in central China.

Ecology of Shen Nong Jia

Shen Nong Jia, one of the largest forest reserves in central China, is home for many forms of wildlife, including several species of rare birds, many forms of cats, the wolf, the black bear, a number of kinds of deer and wild goats, and two species of macaque monkey (the rhesus and stump-tailed macaque). Shen Nong Jia is also one of the few areas in China to have the rare and endangered golden monkey, *Rhinopithecus roxellanae,* which lives at 2,000–3,000 meters (6,400–9,600 feet) above sea level. Since 1975, the Chinese government has listed *Rhinopithecus* as a first priority endangered species, meaning that this animal is among the most endangered of China's wildlife (Poirier in press, Poirier and Hu in press).

Shen Nong Jia is also the home of a number of albino animals, including bears, deer, monkeys, and an unidentified carnivore. Six albino bears have

been captured since 1949. Chinese scientists are unable to explain this apparently high incidence of albinism. A search for radioactive emissions, for example, has proved inconclusive. Poirier and Davidson (1979) reported a high incidence of albino monkeys (*Macaca cyclopis,* the Taiwan macaque) and bears on Taiwan. They were also unable to explain this phenomenon.

Shen Nong Jia is located at about 30–31° north latitude and 110–111° east longitude. The maximum elevation is about 3,200 meters (10,240 feet), and the area covers about 3,200 square kilometers. The area is classified as subtropical or temperate, depending upon the altitude. The flora and fauna have eastern Asiatic traits, and exhibit tropical, subtropical, and temperate elements. Shen Nong Jia is the eastern distribution of animals from Szechwan and Kweichou provinces. While some virgin forest remains, exploitation and forest fires in the past 30 years have greatly damaged the region. Virgin forests are today found only at the highest elevations and in narrow inaccessible gorges. Unfortunately, descriptions of the area's flora and fauna in old texts are only historical reminders of what the area once looked like.

Utilizing ancient Sung Dynasty (A.D. 960–1279) texts, Hu suggests that there has been continual ecological change at Shen Nong Jia. The climate has become cooler since the time of the Sung Dynasty. The high evergreen forests and continuous tropical/subtropical forests are being destroyed. The flora and fauna are now in transition.

At about 2,000 meters (6,400 feet), there is snow as early as September. Rainfall is mostly concentrated in the months of July and August. The area is affected by the southeastern monsoons in the summer months, and the Siberian air mass affects the weather during the winter. Shen Nong Jia is hotter and more humid than western Chinese mountain areas, and is relatively drier than the hill areas of southwestern China.

The eastern part of Shen Nong Jia extends into Szechwan Province, where the forest is dense and difficult to penetrate. The southern part exhibits less dense vegetation, and a main road divides the area. The northeastern part is penetrated by many roads and the forest is less dense.

Brief Review of Wildman Research

Perhaps the first recorded Wildman report was made over 2,000 years ago when the statesman-poet Qu Yuan made frequent references in his poems to the "Shangui" (mountain ogres). Seven centuries later, during the T'Ang Dynasty (A.D. 618–907), the historian Li Yanshow described a band of Wildmen in the forests of Hubei Province. During the Ming Dynasty (A.D. 1368–1644), the pharmacologist Li Shizhen mentioned several kinds of Wildman in the 51st volume of his *Compendium of Materia Medica.* In the 1700's, the poet Yuan Mei described a strange "monkeylike, yet not a monkey" creature in Shanxi Province (Topping 1981).

In 1974, three scientists went to Shen Nong Jia to search for Wildman.

This precipitated the more extensive expeditions in 1977, when Chinese scientists first began to seriously investigate Wildman reports in Shen Nong Jia. This team was headed by the then governor of Hubei Province, and it included 110 people. From 1979 to 1980, a team of 20 people investigated Wildman sightings. Neither the 1977 nor the 1979–80 teams reported seeing the creature. As of 1982, three self-supporting adventurers were in Shen Nong Jia continuing the research.

A conference was held in Wuhan, Hubei Province, in 1980 to discuss Wildman sightings. This conference was apparently a battleground where the two opposing camps, supporters and non-supporters, clashed. The conference failed to produce a consensus, a situation that still exists today. A second conference was held in December, 1982. A Wildman Study Association was founded in 1981, which is headquartered in Wuhan, and which consists almost entirely of supporters of the existence of Wildman.

Various investigative teams have amassed much data on Wildman, but the creatures themselves have not been seen by them. Photographers from the Beijing Scientific and Educational Films Studio spent almost two years searching the forest regions, but never even caught a glimpse of a Wildman. A film crew from Japan also went to Shen Nong Jia in 1980 in search of this elusive creature. They had no better results.

Zhou (1982) reviews Wildman investigations in other areas of China, e.g., the Tibet Autonomous Region and Yunnan, Shanxi, and Zhejiang Provinces.

The So-Called "Monkey Babies"

There is a human birth defect that produces a condition that the local people refer to as "monkey babies." Afflicted individuals are often claimed to result from a mating between a male Wildman and a local female villager, or to be the result of a mating between some members of a relict population of the extinct primate *Ramapithecus,* whose remains are found in China and date to about 8–10 million years ago. Other remains of this primate occur in India, Pakistan, southern Europe and Kenya, to list a few locales. The taxonomic position of *Ramapithecus* is in dispute (Poirier 1981). Investigators consider *Ramapithecus* to be an ancestral form of 1) an orangutan, 2) both apes and humans, or 3) an ancestral human. We will discuss the supposed relationship between fossil primates and the Wildman below.

In 1970, it was reported that a woman in Hubei Province who had seven children had given birth to three so-called "monkey babies." Two of these infants were females, and one was a male. The children exhibited the following traits: the fontanelles were closed at birth, the head was small and strangely shaped, the intelligence level was low, the children had no speech, they acted strangely, and they often lost their sense of balance. Despite these infirmities, the children were said to be physically healthy, and they did not mind the cold.

Are these so-called "monkey babies" the result of mating between what all scientists consider an extinct primate, i.e., *Ramapithecus* (which would have had somehow to have survived in China for millions of years), and village women, as some suggest? Most certainly the answer would be no.

It must be remembered that the Chinese are generally hairless people, and they have a distaste for body hair. Many Chinese once thought that hairy people were a reversion to an ancestral prototypical ape-man. Fraser (1980: 417) notes that:

> Chinese scientists, for example, are fascinated by the so-called ape babies that are born from time to time. Such infants are covered with body hair from their heads to their toes and are thought to be interesting as a link to our Darwinian past. Much is made in propaganda magazines about them and their "equal opportunity to make contributions to socialism," but they are clearly regarded as freaks and maintained as such. No efforts appear to be made to remove the unwanted hair. I got the distinct impression they were required to go through life with their flowing manes to disprove religious conceptions of the origins of humanity.

Suggestions have been made that Wildman is an atavistic outcast who survived through the ages, and developed societies in the forests. Wildman would, therefore, be a genetic throwback to an earlier form of humanity. The atavism theory implies that Wildman represents humans born with an abnormal amount of hair on their faces and bodies.

While conducting research on rhesus monkeys, *Macaca mulatta,* in Xhin Xhan County, Hubei Province, Poirier and Hu were witnesses to a so-called "monkey baby." This individual walked with a shuffling gait, had a slouched back, had a low misshapen forehead, could only make sounds with no articulate speech, and grinned constantly. He did not appear to have an unusual amount of body hair. This person was considered something of an amusing novelty in the town. He was not, however, a result of a mating between a human and nonhuman primate, as some suggested. Such a mating is genetically impossible because of differences in chromosomal numbers. (Even our closest ape relative, the chimpanzee, has a different chromosomal number than we have. Therefore, mating between humans and chimpanzees is considered practically impossible). Instead, this unfortunate individual suffered genetic mental deficiencies and several physical maladies.

Zhou (1982) reports on his skeletal investigation of a "monkey child" in Szechwan. In his view, the skeleton belonged to a deformed human—a view strengthened by the higher than normal occurrence of human genetic deformities in the area of investigation.

What Does the Wildman Look Like?

An answer to this question relies upon reconstructions of eyewitness accounts. There are no live specimens, no photographs, and no reliable scientific descriptions. The available data consist of footprints, hair, feces, and nests, all of which are controversial and hotly contested evidence.

The morphological traits of the Wildman are based on some 300 "sightings." There is great diversity among the sources of these eyewitness reports. For example, eyewitnesses include government officials, forest workers, People's Liberation Army soldiers, and area natives. Most of the sightings come from the last group. Because of the discrepancies among these reports, there is no doubt that many different phenomena are being called Wildman. In fact, in 1980, a hunter shot what he thought were two Wildman individuals. These turned out, however, to be two of the rare and precious golden monkeys. It also appears that some of the reported sightings of Wildman are descriptions of bears. Perhaps the discrepancy in eyewitness accounts has had something to do with the plethora of names attributed to Wildman, e.g., "red-haired mountain man," "mountain ghost" (perhaps a reference to the existence of albino animals reported in Shen Nong Jia), and "manbear."

Topping (1981: 66) provides the following eyewitness account given by Pang Gensheng, a 33-year-old commune leader, in 1977. This report comes from an encounter that supposedly took place within five feet of the Wildman and lasted about one hour (unlike this close encounter, most encounters are long-range sightings).

> He was about seven feet tall, with shoulders wider than a man's, a sloping forehead, deep-set eyes and a bulbous nose with slightly upturned nostrils. He had sunken cheeks, ears like a man's but bigger, and round eyes, also bigger than a man's. His jaw jutted out and he had protruding lips. His front teeth were as broad as a horse's. His eyes were black. His hair was dark brown, more than a foot long and hung loosely over his shoulders. His whole face, except for the nose and ears, was covered with short hairs. His arms hung down to below his knees. He had big hands with fingers about six inches long and thumbs only slightly separated from the fingers. He didn't have a tail and the hair on his body was short. He had thick thighs, shorter than the lower part of his leg. He walked upright with his legs apart. His feet were each about 12 inches long and half that broad—broader in front and narrow behind, with splayed toes. He was a male. That much I saw clearly.

Reports of a preserved hand and foot supposedly stored in Shanghai could not be confirmed by Poirier in 1982. Zhou (1982) investigated Wildman hand and foot specimens from Zhejiang Province. He concluded that they belonged to a higher primate, and showed morphological traits of both monkeys and apes. Zhou concluded that the hands and feet were not from a Wildman, but might possibly have belonged to an enormous, heretofore undescribed, species of macaque.[1] Based on Poirier and Hu's macaque study in Xhin Xhan County, there are too many so-called unreported macaques in China (Poirier and Hu in press).

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[1] A Research Report by Zhou on his analysis of this evidence has been submitted for publication.—Editor.

Over 60 years ago, in 1922, a member of a local militia reportedly caught a Wildman and sent it to a nearby county. There are, however, no records of this creature. A Wildman was reportedly killed in 1940. None of the reports can be substantiated by current Chinese scientists.

The following description has been pieced together from personal interviews, and from unpublished manuscripts by Shang (1981) and Hu (1981). The creature is erect and supposedly walks bipedally. The tallest creature is said to be over 3 meters (over 9 feet) tall. Most of the reports, however, estimate a height of 2 meters (just over 6 feet). Zhou (1982) reports a height range of 1.2–2.5 meters, representing, perhaps, two types of Wildman. The legs are longer than the arms, and the forelimbs are not used for locomotion, such as swinging through branches. The hands are free, and the forelimbs look human (except that Pang reported that the thumbs were only slightly separated from the fingers). The creature walks flat-footed; one foot (measured from a cast) is 48 centimeters (19 inches) long. The first and second toes are separated from one another by 30°. There is no tail.

One of the most distinctive Wildman traits is its long hair, variously described as reddish, brown, or white. Apparently, this variation cannot be explained by citing seasonal, sex, or age differences. The hair on the back is especially long (contra the report by Pang). Males are said to have a bare spot on the chest about the size of a human hand (but Pang, who reported hitting the Wildman in the chest with a rock, made no mention of this trait). The face is long, but the lower part of the face is narrow. The jaw juts forward (what scientists call prognathism). Although there is a low nasal bridge, the nose is relatively long. The incisor teeth are small, and the canines long. (Pang, however, said the front teeth were as broad as those of a horse.) The ears are large and flop over on the side towards the front. The stomach is large. Females have pendulous breasts, and males have a pendulous penis.

These creatures are said to travel alone (quite unlike other primates generally), to have no permanent home, and do not attack humans. In fact, they are said to flee when encountering humans (although Pang reported that the creature he faced watched him for one hour). When they meet humans they "smile" and chatter—both primate submissive traits. A 1960 report cites a Wildman sitting down, clapping its hands, and making a chattering noise. This report also has the Wildman picking up and eating chestnuts, one of its foods, along with corn and other vegetable matter.

Let us now take a closer look at these traits, dividing them into three broad categories: human, nonhuman, and nonhuman primate (monkey and ape). Some traits are not exclusive to one or another category, but they are illustrative of the mixture of traits said to be characteristic of the Wildman.

Human traits.—Looks human, has no tail, walks bipedally, does not use its arms for locomotion, has free hands, the forelimbs look human, the legs are longer than the arms, sits upright, pendulous breasts and penis.

Nonhuman traits.—Does not make tools, has no language, does not use fire, eats raw food, does not store food, has hair all over its body, has no social life, has excessive prognathism.

Nonhuman primate traits.—Has long hair, has large canines, smiles (what ethologists label a grimace), has a chatter vocalization, eats vegetable matter, sits upright.

Although this is a rough categorization of Wildman traits, it is readily seen that this creature possesses a mixture of human, nonhuman, and nonhuman primate traits. Can such a creature exist?

Claims for the existence of Wildman come from the following kinds of physical evidence: footprints, hair, feces, cave deposits, and nests.

Hair.—Over 100 hairs have been collected that are said to belong to Wildman. The longest of these hairs measures 53 centimeters (21 inches). Some hair was supposedly collected by eyewitnesses; the rest was collected from a tree that a Wildman was believed to have rubbed against. The hair was studied by the Hubei Provincial Medical College and the Institute of Vertebrate Paleontology and Paleoanthropology in Beijing. The general consensus is that the hair belongs to a higher primate (monkey, ape, or human).

Footprints.—According to one report, written on the spot and substantiated with photographs: "The prints are of an elongated foot, wider (approaching 4 inches) in front and narrower (approximately 2 inches) at the back. The toe marks are oval in shape, with one somewhat separated from the others. The footprints follow each other in single file, the distance varying between 20 inches and a yard" (Topping 1981: 113). The authors do not know how closely Topping's report approaches the original (the Chinese use the metric system).

More than 1,000 footprints have been reported (Fig. 2). Some of these were cast and then photographed on the spot. Some of the prints suggest that claws tipped the digits, definitely a nonprimate trait. Higher primates have all of their fingers and toes tipped by nails, and not claws. The longest distance measured between two prints is 2.6 meters (8.3 feet). One footprint impression measured 48 centimeters (19 inches) long; the first and second toes were separated by 30°.

Zhou (1982) suggests that there are two types of footprints. One is large, 30–40 centimeters, remarkably similar to human footprints, with the big toe pointing slightly outward. The other type is smaller, about 20 centimeters, and more like the footprints of an ape or monkey, with the big toe evidently diverged.

Feces.—The fecal remains are usually found beside its footprints or in caves. The feces are shaped like a tube, and have an average diameter of 2.5 centimeters (1 inch). On cross-section, the outer layer is brown and the inner layer yellow—indicating the vegetable nature of the diet. Most fecal remains are found at lower altitudes, at about 1,600 meters (5,120 feet). The

FIG. 2.—Casts of supposed footprints of Wildman.

feces contain the remnants of roots, fruit skins, and some small insects which were perhaps ingested along with the vegetable matter.

Caves and nests.—The lowest-lying cave was found at 1,500 meters (4,800 feet). Animal bones are often found associated with these sites. This contrasts with reports that Wildman eats only vegetable material, and casts doubt on the association between the Wildman and the nests/caves. Cave sites average 5 meters (16 feet) deep. No fecal material was found in the caves.

The supposed nests of Wildman were made by pulling seven or eight bamboo trees together. The nests look something like a chair with one high side. Pulling seven or eight bamboo trees together is a prodigious effort, so the creature making this structure must be very strong; bamboo is hard to shape.

Wildman does not hibernate, but is said to change its routine seasonally. In the spring, the creature inhabits gorges; in the summer, it moves to the higher mountains. This movement allows it to take advantage of seasonal food supplies, and to escape the winter snows in the high mountains.

ANALYSIS OF THE EVIDENCE

Let us now take a closer look at some of the specific criticisms of stated facts about Wildman.

"*Monkey babies*".—The so-called "monkey baby," the result of a reputed cross between a human female and a Wildman (itself either a descendant of *Ramapithecus* or a cross between a relict population of Ramapithecines and modern humans) is simply not tenable. *Ramapithecus* is presumed to have been extinct in China for at least 8 million years. Furthermore, at no time since recorded history has the habitat of Shen Nong Jia resembled that now recognized as suitable for *Ramapithecus* habitation. The so-called "monkey babies" simply exhibit genetic disorders.

Footprints.—Because the footprints are made in mud or in bamboo forests, they are distorted, hard to find, and it is difficult to determine what type of animals made the prints. This makes identification of the prints difficult, and interpretation even more difficult. A strong element of subjective judgment is necessary to decide what is or is not a footprint of the Wildman. Casts of the prints are made with plaster of paris, and the final rendition requires considerable touching up. Any final product is highly suspect, and there is considerable room for interpretation. Furthermore, a print of 48 centimeters (19 inches) in length would indicate a very large creature indeed! Considering all the prints, there is great size variation. Some have argued that this is due to individual size differences in the animals making the print. This argument is yet to be made convincingly.

Nests and fecal remains.—This evidence is of dubious value. Conditions surrounding the finds are usually vague, and reports are often contradictory. Chinese panda experts who have examined Wildman nests and fecal remains suggest a close resemblance to the giant panda. However, we know of no sightings of giant pandas in Shen Nong Jia. Topping (1981) reports that a fecal sample of six piles supposedly made by a number of Wildman was found on a steep rock halfway up a mountain. Although it was dry and hard, analysis revealed bits of undigested fruit skins and wild chestnuts. There is no evidence of animal fur or bone fragments. The researchers concluded that the feces could not have been from humans as we know them, nor from a carnivorous animal. Yet the quantity of the feces and the size of the remaining food particles were too small to be from a hoofed animal or a bear. The sample most resembled that of an omnivorous primate (possibly the golden monkey).

Eyewitness reports.—The veracity of eyewitness reports is the greatest problem. How does one interpret the legitimacy of these 300 sightings from such a diversity of sources? Having interviewed several of the original eyewitnesses, Hu is particularly critical of some of these sightings. Few, if any, reports are made by reputable scientists. Most reports come from untrained observers. The wide publicity that reports of the Wildman have received could certainly have colored the reports and/or led to some misinterpretation and possibly even deception.

Eyewitness accounts are very inconsistent. Wildman sightings range from

close up to far away. Despite the inconsistencies, these 300 accounts have been generalized as *the* Wildman. Descriptions vary; despite a wide range of discrepancies concerning such things as hair color, for example, all creatures are called Wildman. Some eyewitnesses who see a supposed Wildman are so frightened by the experience that they are bedridden for a few days. Despite this state of anxiety, they seem to have provided a remarkably lucid accounting of the creature days after the sighting. Is this likely?

Zhou (1982) is particularly critical of some reports. He notes (1982: 23) that:

> . . . it has to be admitted that most . . . [research participants] are not well trained in faunal ecology, primatology, vertebrate paleontology, paleoanthropology, physical anthropology and other disciplines concerned with this topic. Consequently, when they collect and describe evidence, or visit witnesses to obtain first-hand reports, they are not always in a totally objective and scientific frame of mind. This, in turn, can affect the accuracy and reliability of the evidence which they collect. Caution therefore must be exercised when using their materials.

Wildman sightings can be detrimental to local production goals. For example, a 1979 sighting so frightened village women that they refused to work for several days. Local government authorities had to dispatch a special team to search for the Wildman to assuage their fears. Ten days of searching failed to find the creature. It was finally concluded that the women had spotted a bear.

In 1980, in Shen Nong Jia, a hunter saw two golden-haired animals, and shot them thinking they were a Wildman pair. As it turned out, the hunter had killed two rare golden monkeys. This is most distressing, given the endangered status of these monkeys. A trained biologist investigated reports of Wildman in Kweichou Province, and identified the animal as a monkey. (A reported unicorn from the same area was also investigated. This animal turned out to be a Tibetan ram, *Pantholopus hodgsoni,* whose two horns extend upward together. From the side, these two horns resembled one large horn. A fertile imagination, combined with a mistaken impression, led to a sighting of a unicorn. Could the same thing have happened a number of times as concerns Wildman?)

Hair.—In 1982, Poirier was graciously given a hair that supposedly was left by a Wildman while it scratched its back against a tree. The hair is long and of a reddish-orange color, similar to that described as typical of the Wildman in some areas. This hair, however, matched exactly with the long shoulder hair of the golden monkey, *Rhinopithecus roxellanae* (Fig. 3). The longest Wildman hair measures 53 centimeters (21 inches). One museum specimen of a golden monkey we measured had hair 32 centimeters long. Have there been other times when the golden monkey of Shen Nong Jia has been mistaken for Wildman? Certainly, the hair color of the monkey matches many descriptions of Wildman, and the golden monkey has long and flowing

FIG. 3.—A young adult golden monkey, *Rhinopithecus roxellanae,* from Shen Nong Jia. Such animals are sometimes thought to be the Wildman.

hair, as is also described for the Wildman. It should be noted that some of the major areas reporting Wildman—Shen Nong Jia, Szechwan Province, and Kweichou Province—are all areas containing the golden monkey.

The golden monkey is a rare animal, and few people have seen it. Therefore, it is quite likely that it would arouse the curiosity of any observers. Not knowing what the golden monkey is, but perhaps being aware of reports of the reputed Wildman, people might mistake it for the Wildman. In 1980, for example, a golden monkey was captured in Shen Nong Jia. Apparently, some people thought this was a strange animal, and could not recognize it. It immediately acquired human traits (even to the point of being reported that it liked to drive a vehicle!). It was eventually taken to a local hospital, where it died, apparently of an intestinal obstruction. Some people called this monkey a Wildman, and it later led to rumors of Wildman in the area.

A more personal experience of how a strange animal can be generalized into Wildman sightings happened to co-author Poirier. He was the first foreign visitor to Xhin Xhan County, which borders on the Shen Nong Jia District, for a very long time. While not unusually hairy by Western standards, Poirier nevertheless looked strange to the local populace. Standing

about 5′11″ tall, he was also much taller and heavier than local residents. A number of local children stared at him from a safe distance, and ran away horrified at their encounter with what they screamed to others was the Wildman in their midst. Will this encounter become embellished and fit into reports of Wildman in the future?

Conclusions

No definite answer can be given concerning the existence or nonexistence of Wildman in China. Clearly, the reports are inconsistent. The proposed existence of such a creature would undermine current scientific consensus that such a creature is highly unlikely to exist. The burden of the proof lies, therefore, on those who hold that the Wildman exists. The survival of *Ramapithecus,* or of the large extinct primate *Gigantopithecus* (a fossil ape that became extinct in China about 600,000 years ago and perhaps stood 6 feet tall) is highly unlikely. One might counter, however, that the giant panda, which was a contemporary of *Gigantopithecus,* is still very much alive today. Zhou (1982) discusses the *Gigantopithecus* and *Ramapithecus* possibilities in more detail. Even more unlikely—practically impossible according to current genetic information—is the possibility of the mating of human and nonhuman primates.

Given that conventional scientific wisdom denies the existence of a creature such as Wildman, why do such reports continue to appear and have such fascination? As noted previously, reports of creatures similar to Wildman occur on several continents. Do humans have a "need" to believe in the existence of such mysterious creatures? Does the human mind need some imponderables? It has been suggested that, in China, sentimentality and nostalgia may help to explain the continuance of tales of such creatures that are described in ancient texts. The fact that such creatures reside in underpopulated, relatively inaccessible areas must be considered. Such areas are mysterious, frightening, and compelling.

Other factors must also be considered, at least concerning the sightings in Shen Nong Jia. For example, the ex-governor of Hubei Province is an avid supporter of Wildman research. Has his support in the search for this creature pressured the populace of Shen Nong Jia to report findings that they feel would be acceptable? A sighting of Wildman would also garner a considerable amount of public attention. How many people, particularly those in isolated regions, would pass up a chance for some public recognition of themselves or their village? There are other considerations. For example, if someone is so frightened when encountering a Wildman that they are given a few days rest from work, perhaps that is an incentive to "see" such a creature. Finally, there are also economic considerations. Shen Nong Jia is currently being developed as a nature reserve, ostensibly for the golden monkey. However, will reports of the possible existence of the Wildman bring more tourists to the region? Economic gain from such increased tourism is certainly an understandable goal.

The centuries-old riddle of the Chinese Wildman remains unresolved. There is currently not enough evidence to prove the existence of such creatures. Scientific discretion dictates that we either reject claims for the existence of such creatures, or at least withhold judgment until some valid, verifiable evidence is produced. Until contrary evidence is absolutely convincing, however, there is always a possibility that the Wildman exists—but one should not hold one's breath.

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THE MONGOLIAN ALMAS: A HISTORICAL REEVALUATION OF THE SIGHTING BY BARADIIN

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ABSTRACT: Research into the Almas, a supposed man-like creature of Mongolia and Central Asia, is said to have begun when Badzar Baradiin saw one while on an expedition to Tibet in 1906. The sighting was said to have been reported to Tsyben Zhamtsarano, who thereupon began a long-term research study of the Almas. Examination of Zhamtsarano's archive in Leningrad, and in particular of Baradiin's unpublished expedition diaries, casts doubt on the accuracy of this version of events.

The Sasquatch and the Yeti are the most renowned of the various "man-like" creatures supposed to inhabit different regions of the Earth, but in many ways they are not at all man-like. All they apparently share with man is a bipedal gait and some indication of a primate ancestry. Other hirsute bipedal creatures which have been reported have a much better claim to be termed "man-like," and one of them, the Mongolian Almas, has been described in terms which suggest it is little different from man except in the possession of a hirsute body, and the lack of articulate speech and culture.

The possible existence of the Almas was first brought to the attention of the Western world in a systematic manner by the Commission for the Study of the Snowman Question of the Academy of Sciences of the USSR in its *Information Bulletins,* published in 1958 and 1959 (Porshnev and Shmakov 1958). The Commission was established during the Abominable Snowman "flap" of the 1950's, largely at the instigation of Boris Fedorovich Porshnev, and the material in it relating to the Almas in Mongolia was collected by him mainly from information supplied by the Mongolian scholar Rinchen. Rinchen, stimulated by Porshnev's interest, published some of his own researches in September, 1958 (Rinchen 1958). Shortly afterwards, E. Vlček drew attention to some Oriental literary sources which appear to describe a hairy biped in Tibet and Mongolia (Vlček 1959). This was followed in 1960 by an article by G. Démentiev and D. Zevegmid reviewing the various travellers' tales about creatures resembling the Almas from the Middle Ages to the present day, and reporting on their own 1959 investigations (Démentiev and Zevegmid 1960). Four years later Rinchen published a further article (Rinchen 1964).

Most other authors have largely confined themselves to relating the evidence contained in the above works and in Porshnev's two major personal contributions to the literature, which will be discussed below. In 1964, Ivor Montagu published an English-language account of the history of and current

thinking on the subject (Montagu 1964). Much of this material was compiled yet again, with long extracts from Porshnev's works, by Odette Tchernine (1970), and Porshnev's last major work was incorporated, after his death, in French translation, in the book published by Heuvelmans under the title *L'Homme de Néanderthal est Toujours Vivant* (Heuvelmans and Porchnev 1974).

Little really new about the Almas in Mongolia has been published since the early 1960's, though further reports continue to trickle out. In 1981, Damdinzhavin Maidar, First Deputy Chairman of the Council of Ministers of the Mongolian People's Republic, related that tracks had been found in 1973, and an encounter claimed in 1974 (Maidar 1981). Myra Shackley, a British archaeologist, has followed Porshnev in suggesting that Almases may, in fact, be the successors to Neanderthal man: she has had the opportunity to conduct archaeological research in Mongolia and interview the local population about the Almas (Shackley 1980, 1982). However, she modifies this view to some extent in her forthcoming book, *Wildman.*

The above survey of references to the Mongolian Almas has deliberately been cursory, and has not elaborated the details of the evidence claimed in support of the existence of the Almas, because I want to concentrate in these pages not on the Almas itself, but on a particular aspect of the history of the subject. Porshnev wrote two major treatises on the question of the existence of "relict hominoids," as he termed them. One of them, *The Current Position with Regard to Relict Hominoids,* was published by him in a very small edition in 1963 (Porshnev 1963). It is a consolidation and interpretation of the evidence presented in the Commission's *Information Bulletins.*

The other work, "The Struggle for *Troglodytidae,*" is a more popular account, not only covering much of the same material, but bringing the story of world-wide research up to date, and giving Porshnev's own attitude and reaction to many of the practical problems he had encountered in doing his research. It was issued in a general-interest journal published in Alma-Ata (Porshnev 1968). In many ways, it is his testament. As it gives Porshnev's final and most readable account of how he discovered the existence of research into the Almas, the relevant passages are reproduced here in detail (all translations are the author's):

> A small boy once asked me: "Aren't those Almases in [M. K.] Rozenfel'd's book *The Ravine of the Almases* connected with the Abominable Snowman?" The boy deserves all the credit. I flicked through the absurd fantasy story. There, among other things, is portrayed the figure of the Mongolian scholar [Tsyben] Zhamtsarano, who studied the mystery of the Almas. A fictional scholar? It transpired that some time earlier, in 1930, the same Rozenfel'd had published a collection of true reports, *By Car through Mongolia.* The same name cropped up in it—Professor Zhamtsarano. Rozenfel'd noted what he said about strange creatures—wild men, Almases—who live in Mongolia, according to a wealth of data collected by him from the populace, and according to a report of a personal observation by Professor [Badzar] Baradiin of St Petersburg, a Buryat Mongol by birth.

Being no Mongolian scholar, I still doubted the existence of Zhamtsarano and Baradiin, but the experts soon dispelled my ignorance. Professor Zhamtsarano was a scholar of world-wide reputation on Mongolian studies, and was the founder of a national scientific school.

How could I find his material in fuller form than in Rozenfel'd's literary adaptation? The efforts of Rozenfel'd's widow to find the corresponding travel notebook, in the hope that there might be something in it, proved unsuccessful. Zhamtsarano himself, it was explained, was dead, his archive gone. Students? Professor Rinchen, a doctor of linguistics, was said to have been the closest to him. And, at last, I got a reply from him in Ulan Bator. "Yes," wrote Rinchen, "you are not mistaken. I am indeed the last man left alive who knows all the details of the interrupted researches of the esteemed Professor Zhamtsarano into the Mongolian Almases. I also know all the details of Professor Baradiin's sighting which were never published. The last conversation I had with him about this was in Leningrad in 1936."

Everything that I managed to gather about the traveller B. B. Baradiin's discovery (he was an eminent Soviet Orientalist) I later published in the following words:

> It happened in April 1906 in the Alashan desert, at the spot known as Badyn Jaran. One evening not long before sunset, when the caravan had to stop for the night, the caravan leader suddenly gave a startled cry. The caravan stopped and everyone looked at the figure of a hairy man, like an ape, on a sandy mound. Stooping, with long arms dangling, he stood on a ridge of sand, lit by the rays of the setting sun. For a minute he looked at the people, then turned and disappeared among the mounds. Baradiin asked the drivers to go after him. No-one made the attempt, except a Lama from Urga accompanying the caravan, Shirab the Hoarse, a first-class athlete. He tried to set off in pursuit of the Almas, as the Mongolians call this creature, intending to tackle him and overcome him. But in his heavy Mongolian boots Shirab could not catch up with the Almas, who swiftly disappeared over a ridge. This invaluable sighting of Baradiin's evoked lively interest in educated Russian circles. However, the discussion was purely verbal. In the account of his journey published in 1908, Baradiin was obliged to omit this event at the insistence of the head of the Imperial Geographical Society and Permanent Secretary of the Imperial Academy of Sciences, S. F. Ol'denburg, "to avoid embarrassment." In this way, conservative official science buried this noteworthy discovery for a long time . . .

Nonetheless the seed was not lost. Baradiin told his friend Zhamtsarano of the event, and also said that his Mongolian travelling companions considered a meeting with an Almas to be about as rare as a meeting with a wild horse or yak. For many years, Zhamtsarano prepared for an expedition. But to where, and with what aim? According to Rinchen, Zhamtsarano questioned a multitude of Mongolians. Every instance of a meeting with an Almas from the end of the Nineteenth Century to 1928 was noted on a special map. "Also," writes Rinchen, "we noted in the margin the names of our informants, for the most part caravanners and nomadic monks who traversed these places and heard or saw these strange creatures or their tracks." Data relating to the observations were noted. Zhamtsarano also devised the following approach: each witness would describe the appearance of the Almas, and Mongolian Science Committee artist Soyoltai, a researcher present at the interview, would sketch a color picture of it. In this way, a large sheaf of pictures, forming a sort of composite portrait, was built up. Alas, neither sketches nor the map have come down to us. The Mongolian Academician Dorji Meiren, who collaborated in the research, summarised the main results . . .

In 1937 the last tongues of this premature flame in Mongolia flickered and died. The actors disappeared one by one (Porshnev 1968, IV: 101–103).

One short confirmatory note about Badzar Baradiin occurs in Rinchen's first article:

> In 1936, Professor Baradiin asked me in Leningrad whether his companion Sharab [*sic*] the Hoarse, who tried to chase and catch the Almas, was still alive. The Professor regretted not having taken a picture of the Almas, and that this had prevented his noting his wonderful meeting with the wild inhabitant of the sand dunes of Badyn Jaran in his report (Rinchen 1958: 35).

In the main passage quoted above, we have the basic description of the nature of Zhamtsarano's work on the Almas, and of the event which awoke his interest. The account can be summarized as follows: Baradiin saw an Almas while travelling in the Alashan desert. He was prevented from publishing the fact himself, but reported it to Zhamtsarano, who began investigating the subject, and compiled a map and pictures. However, Zhamtsarano's archive is now "gone." The word used for "gone" ("propal") can mean either that the archive has disappeared or that it is known to have been destroyed. In *The Current Position with Regard to Relict Hominoids,* Porshnev (1963: 37) uses a slightly different phrase: "is not available to us."

Why did the flame "flicker and die" in 1937? The answer is to be found in the political history of Mongolia and the Soviet Union. Zhamtsarano was not a native of Mongolia proper. He was, like Baradiin, a Buryat Mongol, coming from the same district as Baradiin. The local administration in Buryatia allocated funds on the same day for both of them to go to university (Tudenova 1969: 142). Despite, or perhaps because of, his Buryat background, Zhamtsarano rose to occupy an important position in the new Mongolian People's Republic, being instrumental in founding the Mongolian Science Committee (forerunner of the Mongolian Academy of Sciences), and becoming its first Secretary (Rupen 1964, I: 204–206).

In 1932, he was ordered to return to Leningrad, where he spent his last years working at the Institute of Oriental Studies. He had long had a close connection with the Institute. The manuscript material collected on his many expeditions between 1903 and 1930 was deposited in the Manuscript Department of the Institute, and he continued to deposit manuscripts there up to 1936. These were not his personal papers, but Mongolian manuscripts from monasteries and other places, copied, bought, or otherwise acquired by Zhamtsarano on his travels. In Leningrad, however, his views were apparently still thought to be too "bourgeois-nationalist," and in 1937 he was arrested. He died while in prison. The date is not certain, but it was most probably in 1940. This is the year of death written beneath his picture in the Leningrad Branch of the Institute of Oriental Studies today. Badzar Baradiin suffered the same fate, disappearing in 1937.

What happened to Zhamtsarano's archive, his own papers? In 1963, Porshnev said it was "not available," and in the political circumstances of the

time this was quite true. But Porshnev's search had been carried out in 1958. In the early 1960's came the Soviet thaw, and many who had disappeared in the Stalinist era suddenly became mentionable again. A conference celebrating the eightieth anniversary of Zhamtsarano's birth took place in Buryatia, and a rehabilitatory article appeared (Baldanzhapov 1962, Tsibikov 1962).

Porshnev does not seem to have been aware of these developments. Rinchen, however, plays a more enigmatic role. Although Zhamtsarano was still a "non-person" in 1958, Rinchen knew at that time precisely what had happened to Zhamtsarano's archive. He examined the calendar of the archive and some of the documents in it in January, 1958, and published a description of the archive in June, 1959 (Rinchen 1959). This brought the wrath of the Soviet authorities down upon him, and he was denounced for making unauthorized use of the archive (Rupen 1964, I: 256). It seems strange that he should not have told Porshnev of the existence of the archive. (An immediate consequence of Rinchen's action in publishing an attack on the Soviet Union for keeping the archive, and listing its contents, was that the entire collection was re-sorted and re-numbered, rendering the list obsolete.)

Zhamtsarano's papers are preserved as collection no. 62 of the Archive of Orientalists in the Leningrad Section of the Institute of Oriental Studies of the Academy of Sciences of the USSR. Rinchen's list of its contents reveals how it reached the Archive:

> 104. Lists of Ts. Zh. Zhamtsarano's materials compiled by L. Puchkovskii, original and typescript; also the decree of April 21, 1938, on the receipt of Zhamtsarano's other materials by the Manuscript Department of the Institute of Oriental Studies (Rinchen 1959: 205).

In 1938, of course, Zhamtsarano was in prison, if not already dead. It would seem, therefore, that all his working papers were confiscated after his arrest and handed over to the Institute. When, thirty years later, Zhamtsarano's edition of Mongolian laws was published from materials in the collection, the editor, S. D. Dylykov, noted that "all of Zhamtsarano's scholarly materials are to be found in a complete state of preservation in the manuscript collection of the Institute" (Khalkha Dzhirum 1965: 7n). This statement is not entirely correct. Some of Zhamtsarano's papers are kept in the Institute of Social Sciences of the Buryat Branch of the Siberian Section of the Academy of Sciences at Ulan-Ude. But the limiting dates of that collection are 1890–1917, and Zhamtsarano's work on the Almas lasted at least until 1928 (Lichnye arkhivnye fondy . . . 1962, I: 261). Tudenova (1969: 138) states that the collection was handed over by Baradiin in the 1930's, and that the limiting dates are 1890–1909.

In September, 1982, I was privileged to be allowed to study the Zhamtsarano collection at the Institute of Oriental Studies in Leningrad. Although

I was not able to examine every item in the collection, there are no maps in it other than a collection of historical maps compiled by Grumm-Grzhimailo; and no sketches of Almases. Nor is there any evidence in his diaries or collected materials that he collected tales about the Almas. Admittedly, these materials are incomplete, but they include his diary and materials from his trip to eastern and southern Mongolia in 1909–10, when we might expect him to have been interested in the Almas (AV MSa,b). It may be that, on returning to Leningrad in 1932, Zhamtsarano left part of his collection in Mongolia, and only took with him the materials on which he wished to work in Leningrad. In general, there is very little in the collection from the 1920's and the early 1930's. Most of the material dates from the first fifteen years of the century, and from the mid-1930's when he had returned to Leningrad.

One interesting item which *is* preserved in Leningrad is the letter which Baradiin wrote to Zhamtsarano on returning from the expedition on which he is said to have seen the Almas (AV MSc). Baradiin had set off to travel to Urga in 1905, spending some time at the court of the Dalai Lama, who had taken refuge at Van-Kuren in Mongolia; then returning to Urga in March, 1906, to travel to Gumbum and Lavran in north-eastern Tibet, which he reached in June, 1906. In January, 1907, he set off on the return journey, and reached his home, Aga, in Buryatia, in May, 1907. On July 17, 1907, he wrote to Zhamtsarano from Aga. He gives the main impressions of his journey: The collection of books and manuscripts; how he was threatened by a large crowd while taking pictures of a religious ceremony; how he "made the wearisome journey across the endless Gobi" on the return journey. He gives Zhamtsarano news of his family. If he saw an Almas, it does not warrant mention in the letter.

The absence of a mention of the Almas in Baradiin's published account was explained by Porshnev as arising from pressure from Ol'denburg. In the published account, the entire journey from Urga to Lavran is sketched only in the barest outline, covering only one page of text (Baradiin 1908: 196–97). However, as Baradiin met his eventual end in much the same way and at much the same time as Zhamtsarano, it is not entirely surprising to find that his archive is also preserved in the Archive of Orientalists. It is collection no. 87 of the Archive.

Among the Baradiin papers are the diaries of his journey to Lavran and back. The diaries of his return journey illustrate his method of working. First, there are the actual diaries, written day by day while in Lavran, and covering the start of the return journey (AV MSe). On his return to Aga, Baradiin rewrote the diaries, adding at appropriate places long and detailed discourses on particular aspects of events, places and people encountered on his journey (AV MSd). Nothing from the original diary was omitted and much was added. This formed the rough draft for a final full manuscript report (AV MSf). For the outward journey we do not have the actual diaries,

but we do have the written-up diaries in their rough form, comprising over 321 pages of closely written text, on foolscap paper (AV MSg). The journey described on one page of printed text occupies 128 pages of the manuscript. Each day Baradiin notes the distance covered, the nature and usually the name of the stopping place, and records every event of the slightest interest in great detail. Meticulous attention is paid to recording animals seen and persons met. The diary was written up immediately on Baradiin's return to Aga, before he could consult with Ol'denburg or anyone else: the preface is signed, and dated "Aga. 1907.VIII.5(n.s.)" (p. 11).

Baradiin left Urga on March 29, 1906 (p. 175), reaching the borders of Alashan on April 18 (p. 221). The approximate route of his travels is shown in Fig. 1. A gale blew during the first two days of his journey across the Alashan desert. On April 19 Baradiin wrote:

> With the greatest difficulty we managed only five versts from the well from which we had just replenished our water, and stopped for the night before we had escaped the mountain ridge, at a spot called Alag Usu (there was a well there once, but not any more). The hurricane raged as before (p. 223).

On April 21 he wrote:

> Up to now we had not seen a single living soul for a whole week, but today we spotted a herd of camels at the head of a watercourse, which signalled the presence of human habitation nearby (p. 229).

On April 27, the caravan reached the camp of the Alashanian guides, where they remained until May 3 (pp. 233–37). From there, they proceeded without further incident to the capital of Alashan, Alasha-yamyn', reaching it on May 8 (pp. 237–41). They left Alasha-yamyn' for Gumbum on May 14, reaching the southern border of Alashan on May 24 (pp. 252–58).

At this point in the written-up diary, Baradiin breaks off the daily account to give a "General note on Alashan" (pp. 259–80). He describes the wildlife in detail:

> As soon as the heat of summer arrives, hordes of lizards and snakes (a species of grass snake) appear. Poisonous scorpions can also be seen hiding under slabs of stone. There are several species of swarming insects, beetles and ants, among which the golden ant is particularly noteworthy. But, in general, Alashan is rather poor in different species of insects, thanks to the sparseness of the vegetation. Wild animals are represented by wolves and foxes, and there are small rodents and hares. There are no marmots, whose area of distribution ended while I was still travelling through the Northern half of Khalkha-Mongolia. There is game: wild sheep, "argali," shelter in the rocky mountains; zerens and steppe gazelles on the steppe belts. In the Alashan Ula mountains can be found the beautiful "blue goat." There are hazel-grouse, and geese and ducks on the lakes in summer (p. 261).

Baradiin nowhere relates that he saw an Almas. The supposed sighting took place in the Alashan desert in April of 1906. As we have seen, this effectively limits the sighting to the period April 19–27. The germ of the

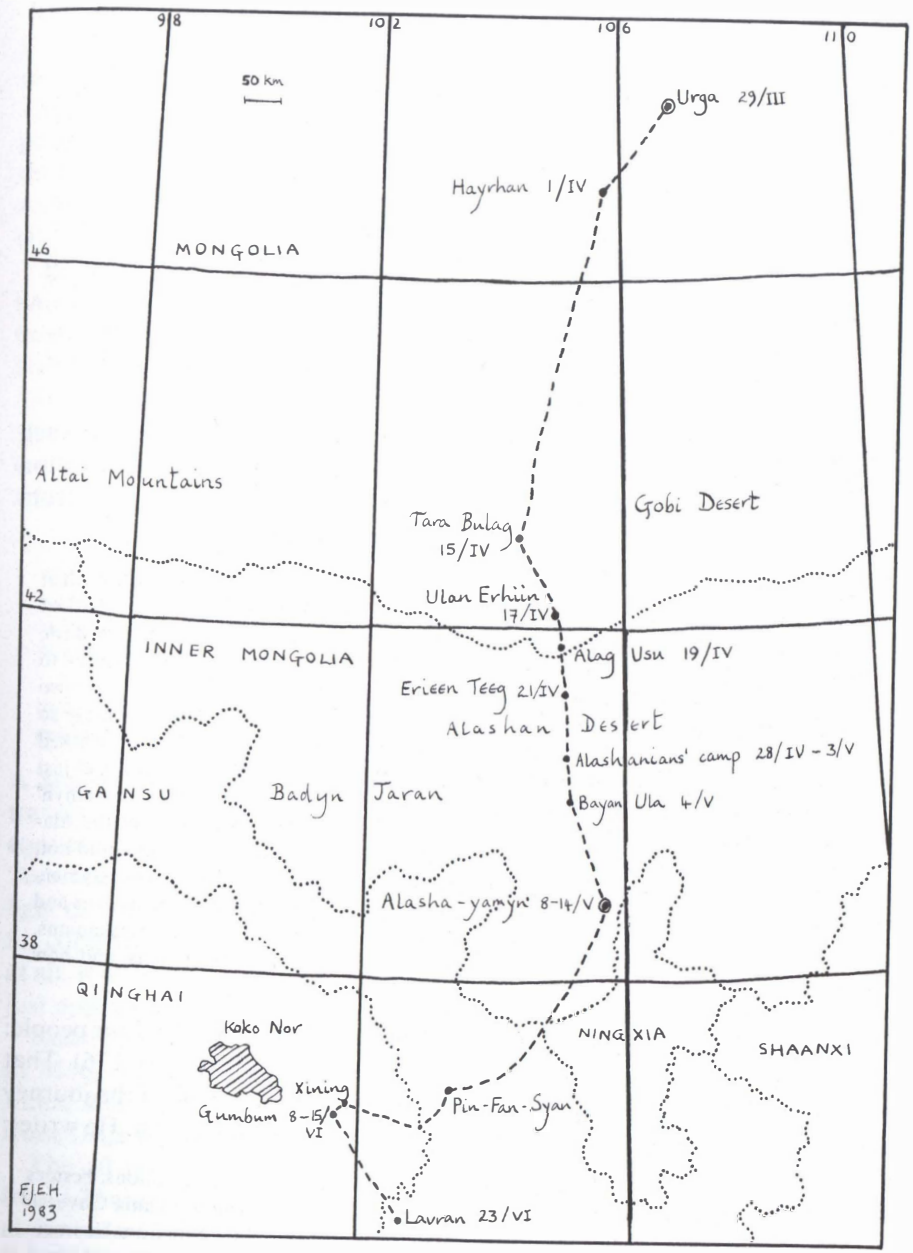

FIGURE 1. Baradiin's journey from Urga to Lavran, 1906

story may lie in a curious note Baradiin makes in the diary entry for April 21:

> The spot where we found a camp with a well was called Erieen teeg. It seems that people live even among these sandy dunes. The point is that these dunes represent a sandy layer above soil, and not a sandy stratum on bedrock. And so between the dunes a healthy earth soil can be seen, beneath which water can be found at a depth of about thirty feet. Yellow clumps of the thick local scrub (dzag) serve as excellent food for camels and sheep and goats, so attracting the local nomads to stop there at the right time of year with their animals. Our Alashanians told us that in the sandy regions of Alashan, especially its Western half, there are sizable oases with brackish and sometimes even fresh water lakes with splendid pastures. Alashanians live with their herds very wealthily in these oases, which are lost in the most inaccessible depths of wide expanses of sand, and so successfully avoid the persecutions of the authorities, and heavy taxes and duties. The people of these places are very wild and avoid the outside world, they say (p. 226).

Shirab the Hoarse is supposed to have been on the caravan. No such person is mentioned in Baradiin's narrative. He set out on his expedition with his brother Zhap and Demzhin Namzhilai (p. 11), and it is clear from his description of events in Urga that no Lamas joined them:

> March 15–22. The time of our departure from Urga for Tangutia depended on when the first Alashanians arrived in Urga with camels carrying millet. In view of this, we asked daily about Alashanians. We found out that some Alashanians would arrive in a few days, who would take four or five days to sell their millet and set off on the return journey to their native Alashan. In Urga we found two fellow-countrymen, Lamas, who were also en route to Tangutia. We became acquainted and agreed to travel together, the more so as one of them was a man who had travelled to Tangutia before. In view of this, we asked our experienced countryman to conduct negotiations with the Alashanians who had just arrived about how much they would require to take us on their camels to Alasha-yamyn' (Fu-ma-fu). This was not so easy a matter for novices ignorant of the ways of the Alashanians. But it turned out that the Alashanians who had arrived first in Urga could not, with the number of camels they had, take us all, and could only take our countrymen. To our great disappointment, our companions hired the Alashanians for themselves and had to leave before us. But our turn came to hire drivers from newly arrived Alashanians. By now, I already knew how to conduct negotiations with these Mongolians, and how much they would want in order to take us (pp. 173–74).

Baradiin later mentions that the Alashanian party consisted of four people: the caravan-leader, his nine-year-old son, and two employees (p. 176). That Baradiin had only his two Buryat companions on the first part of the journey is confirmed when he leaves Alasha-yamyn' on a new camel train. He writes:

> Now we were a regular company, fourteen strong in all. Our new companions, besides the three Buryats from Dooramba Gonchon already mentioned, and the Lama Gavesan with the young monk Gonbo, were the entire family of a rich pilgrim Lama from Uzumchi [*sic*—M. H.]. The head of the family was a middle-aged man, a happy, well-dressed Lama . . . with two wives . . . a thirteen-year-old girl . . . two more employees of his younger brother, a young Khalkha Mongol (pp. 252–53).

This lists eleven companions, with Baradiin and his two Buryat companions making up the fourteen. There is no room for Shirab.

Where did Shirab come from? He first appears in Mikhail Rozenfel'd's novel *The Ravine of the Almases,* an adventure story of anti-Soviet intrigue in which the Almases serve merely to get the heroes out into the desert. They are revealed at the end, almost as an afterthought, as barbarized Chinese peasants whose ancestors had fled to the mountains to escape a cruel overlord three hundred years before. Rozenfel'd's book first appeared in 1936 (Rozenfel'd 1936), and was reprinted several times in the 1950's and 1960's (Rozenfel'd 1955, 1957a, 1957b, 1962).

Porshnev is guilty of a slight inaccuracy in saying that Zhamtsarano is named in the novel: the Zhamtsarano character in the novel is called Dzhambon. To find Zhamtsarano, Porshnev had to go to Rozenfel'd's earlier 1931 book (not 1930 as stated by Porshnev), the travelogue *By Car through Mongolia* (Rozenfel'd 1931), in which Rozenfel'd relates several experiences which were later incorporated into his novel. The deeds of Shirab in the novel are as related by Porshnev, but in the travelogue the story is somewhat simpler:

> In 1906, Professor Baradin was travelling with a caravan on the sands of Oloshan. One evening, shortly before sunset, when it was already time to stop for the night, the caravan leader looked at a hillock and suddenly let out a cry of fear. The caravan stopped and everyone saw on a sandy knoll the figure of a hairy man, like an ape. He stood on a ridge of sand, illuminated by the rays of the setting sun.
>
> For a minute the Almas looked at the people, but noticing that the caravan had seen him, disappeared among the hillocks. Baradin asked them to chase him, but none of the guides ventured to do so (Rozenfel'd 1931: 73).

A Shirab did, however, accompany Rozenfel'd and Zhamtsarano on their travels in *By Car through Mongolia*: he was an ex-Lama who had been converted to work for the new Mongolia. It seems clear that Rozenfel'd "borrowed" this Shirab to add color to the story of Baradiin's Almas. In the light of this, Baradiin's query of 1936 as reported by Rinchen, as to whether his companion Shirab was still alive, takes on a new meaning. If it happened at all, it was clearly a joking reference to the *fictionalized* account which had just appeared that very year in Rozenfel'd's novel.

Rozenfel'd is also the first to mention Badyn Jaran. Although place names in Mongolia tend to recur frequently, Baradiin does not mention Badyn Jaran. Badyn Jaran is an area of the Alashan desert which is on a direct line from Urga to Lavran, but Baradiin's route took him 150 miles east of it.

The oft-repeated tale of how Baradiin saw an Almas and so inspired his friend Zhamtsarano to conduct a long-term research project appears to have at its base some rather shaky foundations. Of the two main elements in Baradiin's encounter, the sighting and the pursuit, the pursuit is fictitious and the sighting itself in grave doubt. It is inconceivable that in a diary of such comprehensiveness, conceived as a complete record of events, describing a monotonous journey across the desert, an incident of such a startling nature should go unrecorded. The merest hint is given of "wild people" in

the desert, and while it may just be possible that Baradiin recorded the legend of the secluded Alashanians of the western half of the desert as a direct consequence of a sighting, it seems much more likely that the tale of the sighting is an elaboration of the vaguer tales of "wild people" in the desert. The absence of any mention of the sighting in Baradiin's letter to Zhamtsarano on his return to Buryatia is another, albeit negative, indication that the encounter did not take place.

The role of Rinchen in Porshnev's research of the late 1950's and early 1960's remains a problem. Rinchen knew of the existence of the Zhamtsarano archive. Why did he not tell Porshnev that the collection was in Leningrad? Was it because he believed, as he wrote in his description of the collection, that the Soviet Union had no right to it? He did not actually see most of the collection, and would have been unwise to presume that the map and sketches were not there. And is his account of his meeting with Baradiin in 1936 deliberately misleading, or did Rinchen himself misunderstand Baradiin?

Whatever the foundations of Mongolian Almas research, the shakiness of part does not bring the whole edifice tumbling down. Although Zhamtsarano's map and sketches are not in his major collection in Leningrad, there is nothing to suggest that they did not at some stage exist, and may exist still. In addition to Rinchen's testimony, Porshnev apparently had the testimony of Dorji Meiren. There are no grounds for doubting Rinchen's word about his own researches, or the word of other Mongolian researchers. Porshnev may have been unwise to relate "evidence" at third and even fourth hand, but the mass of other evidence for the Almas, adduced by him and others, is not thereby invalidated.

My visit to Leningrad was undertaken under the terms of an exchange agreement between the British Academy and the Academy of Sciences of the USSR. The help and support provided by both sponsoring and receiving institutions is gratefully acknowledged, as is the generosity of the Curators of the Bodleian Library in allowing me study leave to accomplish the visit.

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ANATOMY AND DERMATOGLYPHICS OF THREE SASQUATCH FOOTPRINTS

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Abstract: Casts of three large human-like footprints were made by U.S. Forest Service personnel in June, 1982, in southeastern Washington State. The fine-grained soil preserved many impressions of dermal ridges and sweat pores. Careful study by dermatoglyphics experts shows these impressions are perfectly consistent with the friction skin found only in higher primates. The foot size of 37.5 × 17 cm rules out any known primate; the nonopposed first digit indicates a hominid. Physical circumstances suggest a body weight of 300 to 400 kg. It is believed that all possible methods of faking this evidence have been considered, and ruled out.

Introduction

New animal species become generally accepted when a physical specimen is collected and properly described. Most commonly, living species are initially demonstrated by the skin and skull of one individual. Extinct species are based on fossilized bone or shell, often on rather small frements. Some are proposed even on the basis of such indirect evidence as burrows or tracks. In all cases, something tangible is required, which is deposited in a collection somewhere, and which can be examined later by persons other than the discoverer. Only in the case of relatively unimportant subspecies might acceptance be based only on unsubstantiated observations by qualified authorities.

There are no clear rules about the kind and amount of evidence that must be provided in each case. Instead, there is an unstated consensus that this evidence depends on the nature of the animal, or more properly, on our emotional reactions to it. The more unlikely or unexpected the species, the more proof is required to establish its existence. A new subspecies of chipmunk might be seriously considered on the eye-witness testimony of one competent zoologist. But if the giraffe were unknown to science, five expert accounts, supported by photographs and footprints, would hardly suffice. Similarly, the first platypus skin brought to Europe was suspected of being faked.

There have been persistent reports for over a century that a species of large bipedal, higher primate, known as Sasquatch or Bigfoot, lives wild in North America, and other areas as well. (This is not the place to present detailed information on these reports, but the interested reader might consult Green [1978], and Hunter and Dahinden [1973].) Quite rightly, zoologists and anthropologists have not accepted this animal on the testimony of claimed eyewitnesses, however many these may be. The supposed species is too

unexpected, as well as potentially too significant, to base any firm conclusions on unverified accounts. If we accepted every creature that is supported by the testimony of ten or more claimed sightings, then we would have a truly unwieldy zoological inventory of mostly unclassifiable creatures, including unicorns, goblins, griffins, fairies, moth-men, and the like.

Most scientists would agree that only good skeletal evidence would establish the reality of a species like the Sasquatch (a North American Indian name). A few specialists would be satisfied with such esoteric evidence as a test-tube full of blood. Likewise, a few specialists would be convinced by anatomically clear footprints recorded under satisfactory circumstances. If we were simply dealing with an over-sized subspecies of an accepted animal, such footprints would probably gain general acceptance. As it is, however, we should not be surprised if the best-of-all-possible foot impressions of a Sasquatch were ignored or denied by most of the scientific establishment. That is exactly what we are now confronted with.

Background

On June 10, 1982, U.S. Forest Service patrolman Paul Freeman was surveying elk in the Umatilla National Forest, on the border of Washington and Oregon states. That morning, he reported seeing at relatively close range an animal of human shape, hair covered, standing about 2.5 m tall, with an estimated weight of 400 kg. This event is typical of the hundreds of descriptions of encounters with the supposed but unverified Sasquatch. Other Forest Service personnel from the Walla Walla Ranger District Office in Walla Walla, Washington (where Freeman was employed), were called to the scene that day, and they observed many apparent footprints that were consistent with an animal of that description. Many photographs were taken, and a plaster cast was made of one of these impressions (Fig. 1, right).

The following day, a search and rescue team on an unrelated mission came upon the scene, took more photographs, made another cast (Fig. 2), and attempted to track the creature. One week later, on June 17, Freeman and other foresters encountered more footprints a few miles away, at a place called Elk Wallow, this time of two individuals. One of these sets of tracks matched the tracks at the sighting location, and a third cast was made (Fig 1, left). The second individual left slightly different tracks, and three casts were made of these (Figs. 3 and 4). The analysis in this article centers on these three new tracks (made by the second individual) from Elk Wallow, in the Mill Creek Watershed. (For more background information, see *ISC Newsletter* [1982a].)

During the following winter, Freeman and other investigators found additional tracks on several occasions. They made more plaster casts indicating the existence of at least two more individuals. In all, 11 casts of four distinguishable types have been made. Five of these casts were made by Forest

FIG. 1.—Two footprint casts made by Forest Service personnel of the animal reportedly seen by Paul Freeman. Both are imprints of the same left foot. The cast on the right was made on the day of the sighting, June 10, 1982. The cast on the left was made at Elk Wallow one week later, where it was accompanied by tracks of the prime (second) individual.

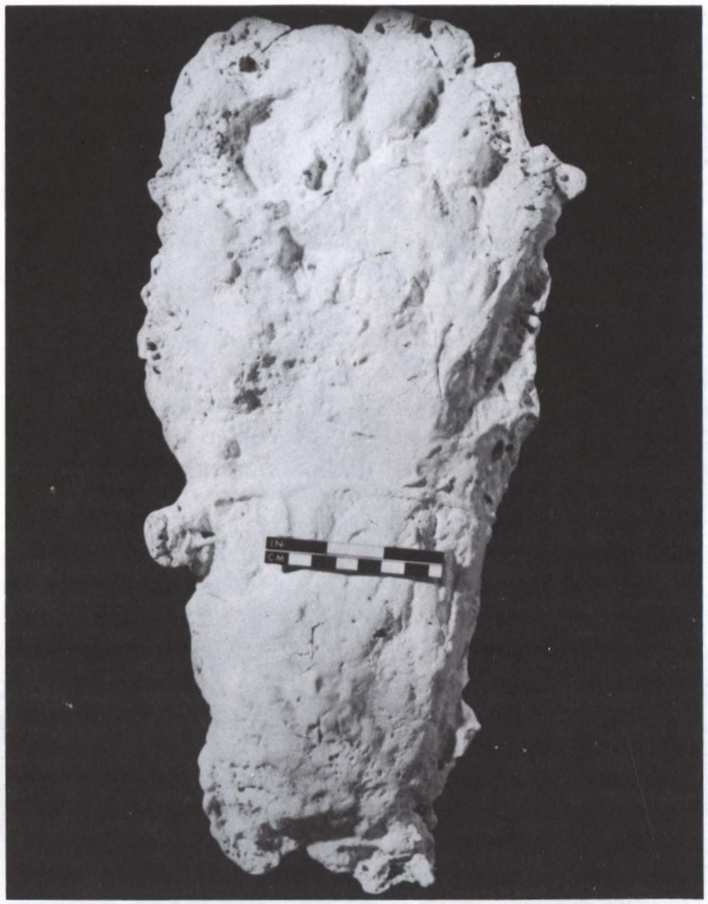

FIG. 2.—Footprint cast made by Art Snow, of the search and rescue team from Oregon, at Paul Freeman's sighting location, on June 11, 1982. This is the same left foot, as in the other casts. It is 35 cm long.

Service workers in June of 1982, and the originals are still in my possession, pending their ultimate disposition. One made by Art Snow of the search and rescue team, and five more made by Paul Freeman himself, have been copied with latex or Silastic molds and returned to their owners.

This study is not concerned with the truth or accuracy of Freeman's claimed sighting. It is also not concerned with the full range of tracks and the differences between them. Nor do I stress the physical circumstances of the supposed Sasquatch trails themselves, convincing as they may be. Instead, the emphasis here is on the morphology of the three Elk Wallow track

FIG. 3.—Two of the footprint casts of the prime (second) individual made by Forest Service personnel at Elk Wallow on June 17, 1982. These represent the bottom view of the feet, so the right foot called "full right," is seen on the left (second digit cut short), and the left foot is seen on the right (with extra plaster beyond the toes). These are 37 and 38 cm long.

FIG. 4.—Another cast of the prime (second) individual from the Elk Wallow tracks found on June 17, 1982. This cast is called "short right," because its heel was lost when an elk stepped on it before removal.

casts, which show the greatest detail, especially in terms of the pattern of dermal ridges that was observed in the actual tracks, and that has been preserved in parts of the plaster casts.

The prevailing soil type in this region is wind-blown loess, a very fine-grained substance with typical particle sizes of around 0.01 mm diameter. When this soil is damp and cool, and pressed into by a warm body, a detailed imprint commonly remains. Most, if not all, of these tracks were cast within a day of the time of impression, so they had not dried out, nor had they received any intrusive material. The well mixed casting plaster that was poured into these impressions was able to record any degree of detail that was held by the soil. Variations down to less than 0.1 mm are faithfully preserved. This is fine enough to show individual dermal ridges and their sweat pores.

While the above-described circumstances seem perfectly reasonable in hind-sight, I was initially unconvinced that such detail could transfer from skin to dirt to plaster. A simple test demonstrated its feasibility. The top soil on my property in Pullman, Washington, is of similar loess. I experimentally depressed my own thumb into a piece of this topsoil, and made a plaster cast of the impression. It faithfully recorded the dermal ridges and some of the sweat pores of my skin. I have since found out, through discussions with police officials, that footprint patterns in dirt are actually used for criminal identification in India and New Zealand.

A U.S. Border Patrol tracker who was called in by the Forest Service to help in the Walla Walla investigation declared the tracks to be fakes because of the presence of these dermal ridges (among other things). He pointed out that, of all North American mammals, only humans have fully developed friction skin on the soles of their feet. He did not allow for the possibility of the track maker being *another* higher primate—all of which have the same kind of friction skin with virtually identical dermal ridges.

Illustrating these tracks proved to be a difficult problem. Photographing an entire cast for general shape was simple enough, but the detail was another matter. Given the significance of these fine lines, and the controversy over their source, it was obvious that drawings would not be satisfactory. Some objective method was needed to transfer the pattern of ridges directly into black and white images for publication.

Attempts at inking and rolling were not very successful on cast copies, and it was decided not to try inking any of the originals. Fingerprint dust (commonly used by fingerprint specialists) and tape removal also worked well only on small areas at a time on cast copies.

The relief of these ridges is generally under 0.2 mm, so they are best seen on the casts with only oblique lighting that throws the furrows into shadow. A number of close-up photographs of small areas were made with this

method. These are shown here with their locations on the full casts clearly indicated. Some of these photographs have already been published (*ISC Newsletter* 1982b). This method of illustration is limited because of the curved surfaces of a rigid structure. Beyond a small area, details are either thrown into total shadow, or else they are lost when the light enters all depressions.

Analysis of Gross Anatomy

In overall morphology, these prints resemble human feet, considerably expanded. Individual footprints vary in size due to differences in depth of imprinting, motions of the foot in stepping, and apparently from flexibility of the padding in the sole. The primary individual under consideration here left imprints 37 to 38 cm long, 10 cm wide at the heel, and 17 cm wide at maximum on the forefoot. (The length and heel breadth are based on just two of the three prints because an elk stepped on the heel of one cast before it was retrieved from the ground.)

One investigator, Edward Palma, of the Sheriff's Department of Laramie County, Wyoming, contacted major shoe manufacturers about human feet of this size. John Robinson, of Niki Shoes, informed him that an extraordinary human could have a foot of this length, but not of this breadth. The same conclusion is evident in data from Red Wing Shoes (Cook 1981).

Palma was able to trace the dermal ridge pattern across the ball and interdigital areas of one cast without break, thus showing that the cast adequately represented the breadth of the foot. This fact alone appears to rule out a human foot being involved. The accomplishments of stride length, slope climbing, and depth of impression also rule out a human. This would be especially true of any really "giant" humans, who are far less adept at moving themselves around than are normally sized persons.

The imprints indicate that there was no longitudinal arch in the foot. This should be expected for a biped of such a size, where the strengths of supporting structures (all based on cross-sectional surfaces) do not keep up with the body weight (based on volume). In addition, there is reason to believe that the ankle is set relatively farther forward on the foot than in humans (Krantz 1972a). This observation was supported by Benny Kling, of the Wyoming Law Enforcement Academy, who was the first to examine latex lifts from the prints. He interpreted the imprint as showing that a disproportionate weight was carried on the front of the foot as compared with a human, and asked if the creature walked hunched over. My suggestion of an anteriorly set ankle struck him as an even better explanation for the apparent weight distribution. From the thin and totally flexible lifts, he also deduced that it had collapsed arches.

The depths of imprints were indicative of great body weight, but exact figures are difficult to calculate. The tracks were impressed much deeper

than the investigator's boots, in spite of the fact that they covered over twice the area of these boots. At the location of Freeman's alleged sighting, the Forest Service conducted a test at the urging of an individual visiting from Seattle. A piece of metal the size and shape of one of the footprints was cut, and this was placed on the dirt road near a track. An auto jack was then put on it, and this was used to raise the rear end of a pickup truck. They found that this metal plate was impressed into the ground less that the actual footprints. From this fact, and given the weight of the half-supported truck, the Seattle individual concluded the "thing" weighed 4,000 pounds (1914 kg). None of the foresters agreed with this weight (widely quoted in the press), allowing only one-fourth of it at most.

This excessive weight figure is easily dismissed because it is based on a gradual pressing of weight into the ground, in contrast to the impact of striding. A simple experiment illustrates this: One can walk with long and firm strides on fine moist soil, leaving clear footprints, then stop and walk slowly back to examine the tracks that were just made. One may then back-off and compare the "striding" tracks with the "walking" tracks, and see that the latter are far less impressed. I have found that, in most cases, even hard stamping in-place will not match the impressions just previously made in striding. Full striding impacts the body weight, at some speed, entirely on the heel area initially, and with a slight forward motion as well. In leaving the ground the body weight is last impressed on the toe area, with the force of stepping off, and also with a slight backward motion.

Applying this knowledge to the tracks in question does not give an exact figure of weight, but it moves the reasonable estimate back to nearer Freeman's guess of 800–1,000 lbs, or about 400 kg. Of course, this all refers to the first individual from the sighting event of June 10. The second individual's tracks, found on June 17, were slightly larger and impressed to a similar degree. Thus, the tracks that are considered in detail here were quite possibly pressed into the ground with a weight of as much as 400 kg.

The foot shape is essentially human, in spite of the large size and flat arch. The toes are unusual in being more nearly all the same size than in humans; the first digit is the largest, but only slightly so. The toes are also arranged nearly straight across the front of the foot. This contrasts with the more tapered human foot, where the lesser toes are progressively much shorter. (All these characteristics also apply to the other Walla Walla footprints, as well as to most reported Sasquatch tracks.)

The two right footprints (Figs. 3 and 4) are especially instructive in respect to differences in the depths of toe impression. At the metatarsal ends, the "full right" footprint has a deeply impressed digit I, while the "short right" one (missing the heel) has a more deeply impressed digit II. The toes of "short right" are impressed into the substrate only sightly less than the metatarsal ends; but digits II–V of "full right" turn somewhat upward and

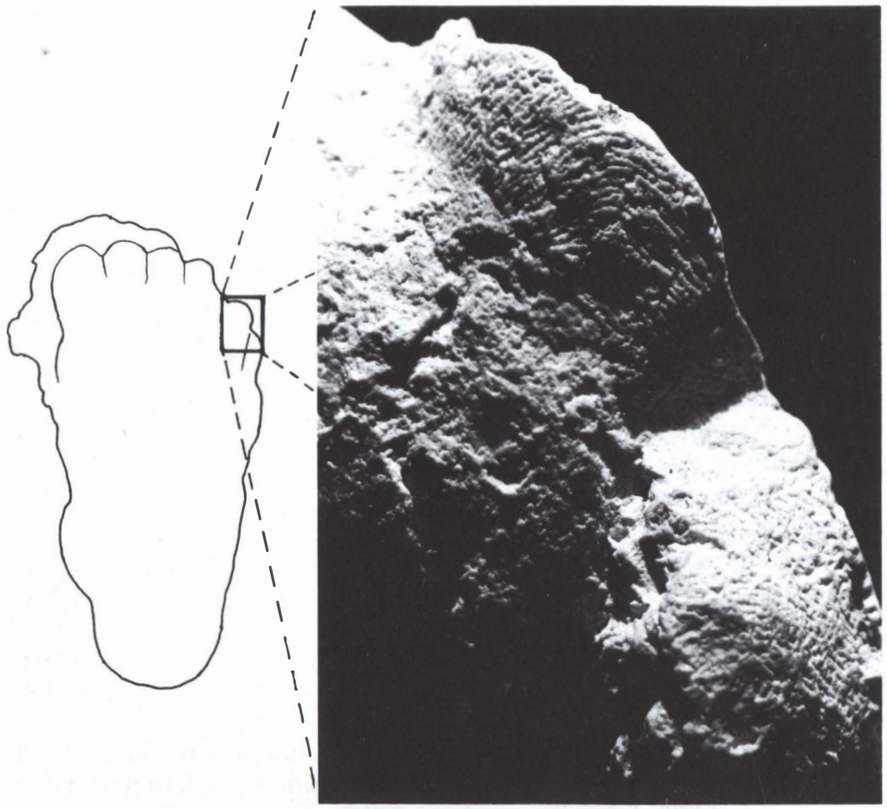

FIG. 5.—Enlargement of the fifth digit of the "full left" track shown in Fig. 3. The area of the cast in this photo is shown on the outline drawing. Dermal ridges can be seen clearly, and are continuous onto the extra bulge of flesh or incipient sixth digit, seen on the lower right.

some of their tips are not recorded. This is also evident in digit V of "full left," where the arched ridge pattern, first taken to be the tip of the toe, actually appears to be from the metatarsal pad, and the tip does not show.

A peculiarity of the "full left" track (Fig. 5) is the extra pad of flesh to the lateral side of the base of digit V. Dermal ridges pass directly from digit V onto this pad, so it is not a double strike of the fifth digit. Since this pad does not appear on either of the "right" tracks, its exact nature is not clear. It may result from connective tissues in the sole pad that were under abnormal tension. Possibly it is an incipient sixth digit.

The "short right" track shows what may be a double strike of digit II, where that toe was first pressed against digit III, then glanced off a small stone and into a more normal position. A cast of that stone is preserved in

FIG. 6.—Anterior part of the "short right" track seen in Fig. 4. The suggested first-strike position of digit II, and the stone cast, are directly above the "in cm" marks on the scale. The second-strike position of digit II is directly above the third cm space. The V-shaped meeting of dermal ridges is just to the left (as seen here) of the first strike. See Fig. 7 for a closer view.

the track (Fig. 6). In order to make such a movement, the digit had to shift to the side after it first pressed into the ground, roll over the stone, and press in again. The first pressing of digit II against digit III is indicated by the set of V-shaped ridges meeting in a cleft (Fig. 7). This cleft might also be taken as a deep scar between the toes, but this would not explain the apparent two impressions of digit II.

Perhaps the most interesting of the gross characteristics of the prints is the large stone impression in the middle of "full right." This stone was photographed in the actual track before the cast was made (Fig. 8). It measures about 9 cm long, by 7 cm wide, and stands up into the footprint cast to a maximum of 2 cm toward the rear part. The footprint maker simply stepped on a large stone and pressed it down into the dirt around it on all sides. This was not a double action, hitting on one side then twisting over to press down on the other. Such a double imprinting would have left cor-

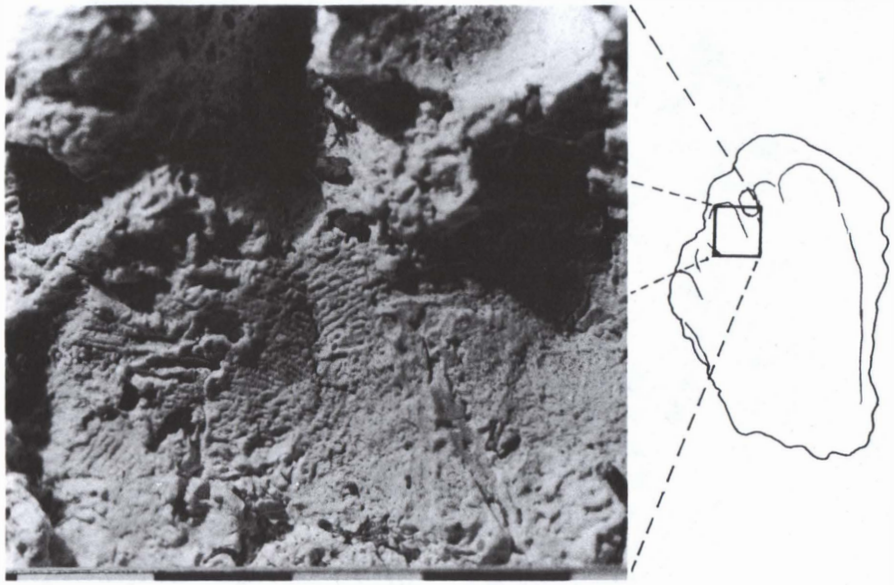

FIG. 7.—Close-up view of dermal ridges on "short right" track between digit III and the first strike of digit II. The line of separation could also be interpreted as a scar.

responding breaks in the imprint to the front and rear of the stone—but there are none.

The significance of this stepped-on stone is that it shows the sole of the foot had a very thick padding that was also very flexible. Mobility of bone connections cannot account for most of this indentation. The deepest part is close enough to the back of the foot that only the calcaneous, or heel bone, is involved. It is obvious that the sole pad must have had a thickness somewhat greater than the 2 cm depth of the stone's indentation. This applies to the osteologically more rigid rear part of the foot, and may be much less the case in the forefoot, where there is more movability between the bones.

A flexible sole pad is further supported by the configuration of the edges of the footprint indentations. These are nearly vertical in many areas, and actually overhang by as much as 1 cm in one place. This overhang (or undercut of the impression) might have been caused by sagging of the surrounding dirt after the imprints were made, although personal observation of many tracks makes this seem unlikely to me. A rigid structure could not have pressed laterally against the walls of its impression, then withdrawn medially in removal. A flexible foot, especially one with a thick sole pad, would make just these movements, leaving an impression that can be wider at the bottom than it is nearer the top. Detailed skin impressions are visible

FIG. 8.—Imprint of the "full right" track in the ground before the cast was made. The stone the supposed Sasquatch stepped on is visible near the middle of the track.

on some of these vertical and overhanging walls, with dermal ridges clearly showing in one case near the heel of "full right" (Fig. 9). *These details are directly imprinted into the side walls, and thus could not have been made by a rigid structure with any combination of movements.*

The presumption of a thick sole pad was also indicated by four footprint casts from Gray's Harbor County, Washington, made earlier in 1982 by the Gray's Harbor Sheriff's Department. These tracks show such identity of detail with each other that a single foot was almost surely involved. Yet they varied in breadth by as much as a centimeter from one print to another. Varying pressure on a thick, flexible, sole pad was clearly suggested in that case, too.

The logical expectation of such a thick sole pad has long been indicated by the apparent ratio between body weight and foot surface area. With increasing body size, surfaces increase by the square of linear dimension, while body volume (and weight) increases by the cube. Thus, the larger hominid puts more weight on a given area of sole than does a smaller one.

The reported unusually heavy-set Sasquatch bodies only add to the problem. A typical man, 170 cm tall, will have a foot 26 cm long. His 65 kg body weight will be distributed over about 200^2 cm of one foot (at times), or about 0.325 kg per cm^2. Our reconstructed Sasquatch, 2.4–2.5 m tall, has a foot 37.5 cm long. Its 400 kg body weight is distributed over 486^2 cm (actual measure) of one foot, or about 0.822 kg per cm^2.

The above figures assume the same ratio of stature to foot length in the human and the Sasquatch. Evidence on this point is mixed. If we follow the measurements estimated from the Patterson film of 1967 (Krantz 1972b), a 37.5 cm foot indicates a smaller Sasquatch with a body weight of only about 250 kg. This would put 0.514 kg on each cm^2 of the foot—still more than half-again as much weight per unit area on the sole of the foot as in a normal human. This is probably the minimum possible figure.

Such a thick sole pad would presumably be made largely of fat, but would have to be strongly interlaced with, and partitioned by, connective tissue running in many directions. This design would allow for the retention of a typical higher-primate friction-skin surface with a minimum of internal restructuring. This conclusion, which was based only on Sasquatch footprint morphology and estimated body sizes, is supported by data on the gorilla foot. Raven (1950: 217) describes the gorilla sole as follows: "The subcutaneous connective tissue . . . is very thick and fatty and similarly has a framework of heavy collagenous septa." And (1950: 71): "The fatty pad of the sole is everywhere covered by dense connective tissue. The fat is thin along the middle of the foot but is very thick, 2.5 cm under the heel." Given that the Sasquatch may weigh twice as much as the gorilla, and is entirely bipedal, it is reasonable to expect the same sole structure, but to an exaggerated degree.

ANALYSIS OF DETAILED ANATOMY

I have already alluded to the dermal ridges and sweat pores that characterize friction skin on the palms and soles of all higher primates, and not of other mammals. These ridges are clearly evident in the three casts in question. They cover most of the toe areas, and can be seen in scattered parts of the rest of the sole. (The following comments on the skin details are based on long discussions with various experts in the field, [see Addendum], and such sources as Cummins and Midlo [1943], and Olsen [1978].)

From ridge to ridge, these lines are spaced about ½ mm apart—more in some areas and less in others. This kind of spacing is typical for almost all higher primates, regardless of body size. Within a primate species, large individuals (usually males) have ridges somewhat farther apart than in small individuals. The number of ridges in the fetus is geared to the average adult body size of the species, so those individuals that grow to larger sizes have the given number of ridges more spread out, and vice-versa. Friction skin

tends to have the optimum density of ridge spacing for best adhesion to smooth objects. The number of lines laid down in the fetus varies according to the adult size for each species. There is no adjustment for sexual size differences. Thus, it is not clear whether our 38 cm tracks were deposited by a male or female.

The near constancy of ridge spacing in primates rules out one method of faking. Latex molds of real skin, soaked in kerosene, will expand greatly; there may also be other methods of expanding molds. *This procedure could produce gigantic skin patterns (in some respects), but the ridge spacing would also be expanded, and thus easily recognizable as abnormal.*

The dermal ridges in these track casts show bifurcations, terminations, and isolated short segments in various places. These are the same kinds of variations that are seen in human dermatoglyphics, and they occur with normal frequencies.

If a dermal ridge running across a toe is counted as "one," and a ridge of similar length on the sole as another, then these three track casts display almost a thousand dermal ridges. Each of those that has been carefully observed is a smoothly rounded ridge in cross section, and not a scratch or V-shaped groove as would be produced by engraving. The furrows are also rounded valleys between the ridges, and are relatively narrow. The spacing between ridges varies only gradually from one location to another; there are no abrupt breaks in pattern density.

Ridges can be seen in some places well off the sole, more than 2 cm up the side of the foot. This shows most clearly around the outside edge of the heel on the cast of "full right" (Fig. 9). The greater extension of friction skin up the edge of the foot, as compared with a much lesser degree on the hand, is normal in primate dermatoglyphics.

The ridges are clearest, and with the deepest relief, on those areas that bear the least weight. Around the edges of the foot itself, and around individual toes in particular, the skin should normally strike the substrate only briefly. In these areas, ridge to furrow depths are at a maximum. On the more weight-bearing surfaces of the sole and middle of the toe pads, the ridges appear to be somewhat worn down (Fig. 10). This is normal for a primate that walks a great deal on hard surfaces. I am told by a primatologist (preferring anonymity) that only a big gorilla, living in a concrete-floored cage, would wear down the ridges completely. Interestingly, the footprints at the Freeman sighting location (the first individual) show no ridges, except for a few traces around the edges in some places. Perhaps this individual had done considerably more walking than the second individual being studied here.

In the early fetal development of dermal ridges (and presumed evolution), each sweat pore has a tiny cone, or pebble, of skin surrounding it. As growth progresses, these cones line up in fixed patterns and merge together into

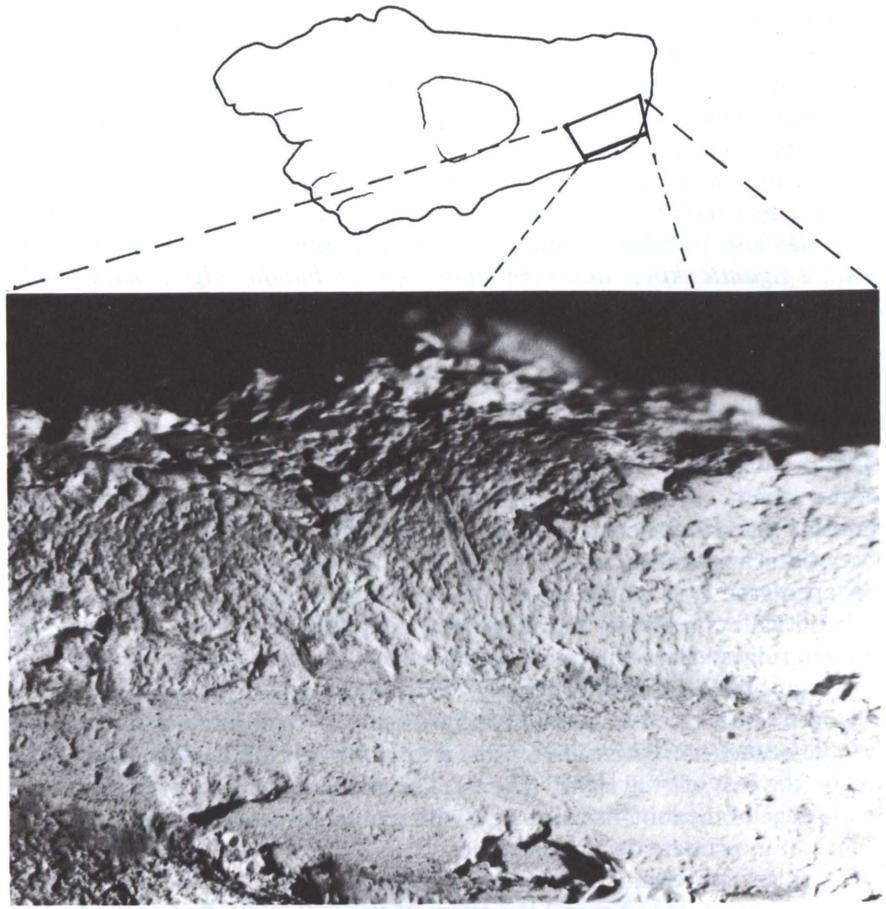

FIG. 9.—Lateral edge of "full right" track just ahead of the heel. The position of this photo is shown on the outline drawing of the full cast. The cast is shown with its bottom side up—that is the usual way it has been studied. The smooth streak across the edge represents damage to the original cast at some early date. Dermal ridges can be seen above this streak (below, in the normal position). They are best developed to the right, but show poorly in this photograph.

unbroken ridges. Sometimes, this development partially fails, and many of the ridges consist only of a jumble of very short segments. This condition is known as displacia (or dysplasia), and tends to affect mostly those central areas of greatest weight support where the ridges are also most worn down. While the two conditions topographically coincide, they should not be confused, because they have separate origins and morphology. In affected areas,

the directions of dermal ridge sets can still be made out by observing the general trend of orientation of the short segments.

Our second individual shows this form of displacia as it occasionally occurs in humans. The pattern in some toe tips and over the fore-part of the sole (in "full right") is somewhat confused and hard to follow. Nevertheless, the trend on this one recorded instance of a midsole print is transversely across the sole. This is the hominid pattern, as opposed to the pongids, where the trend in this area is diagonally from anterior-medial to posterior-lateral (Biegert 1961). This contrast should be used with caution because apes show high variability, and some of them approach the human orientation in parts of the sole. The first (Freeman-sighting) individual's sole was too worn to determine if it showed displacia.

In at least one place the ridge-trend abruptly changes direction, with no apparent reason. While this has puzzled three examiners, it is actually a perfectly normal phenomenon, though rare. This is another form of displacia, or dissociation, of dermal ridges, and is discussed by Olsen (1978). Even more extreme examples have been reported in palm prints. In one human case, a 1 cm circular patch, sharply outlined, has its ridges turned 90° from those of the surrounding skin (Olsen, personal communication).

The ridge alignment is slightly broken up on some toe tips, but the basic print pattern can still be determined. The toe tip patterns are mostly arches (Figs. 5 and 10), while digit I of "full left" appears to have a loop (Fig. 11). On the fingertips, arches occur rarely in humans—showing in just over 10% of individuals only among Congo Pygmies and South African Bushmen. On the toes, they occur somewhat more frequently, but are still in the minority.

On the shafts of the toes, the ridge pattern begins to follow the primate norm of running straight across. At the bases of the toes, which are surprisingly short, new patterns again can be seen on the metatarsal pads. None of these can be followed through clearly in their entirety, but an "arching" design is the most common in the distal areas of these pads. The best cast, "full left," seems to show the beginnings of a loop pattern at the base of digit II (Fig. 10), and perhaps on digit I (Fig. 11), while digit V (Fig. 5) simply arches as far as the lines can be traced. These are normal patterns for apes or humans.

In addition to being unusually short for such a foot size, the toes show another peculiarity that does not occur in primate feet. They have no flexion creases (see especially Fig. 10). Ideally, there should be three such creases, each corresponding to one of the three phalangeal joints, but in this case none are visible. I do not know how to interpret this absence, except to make note of it. It may not be a normal Sasquatch trait, because the toes on this particular individual are relatively even shorter than on other presumed Sasquatch footprints. The main significance, for now, may relate to the

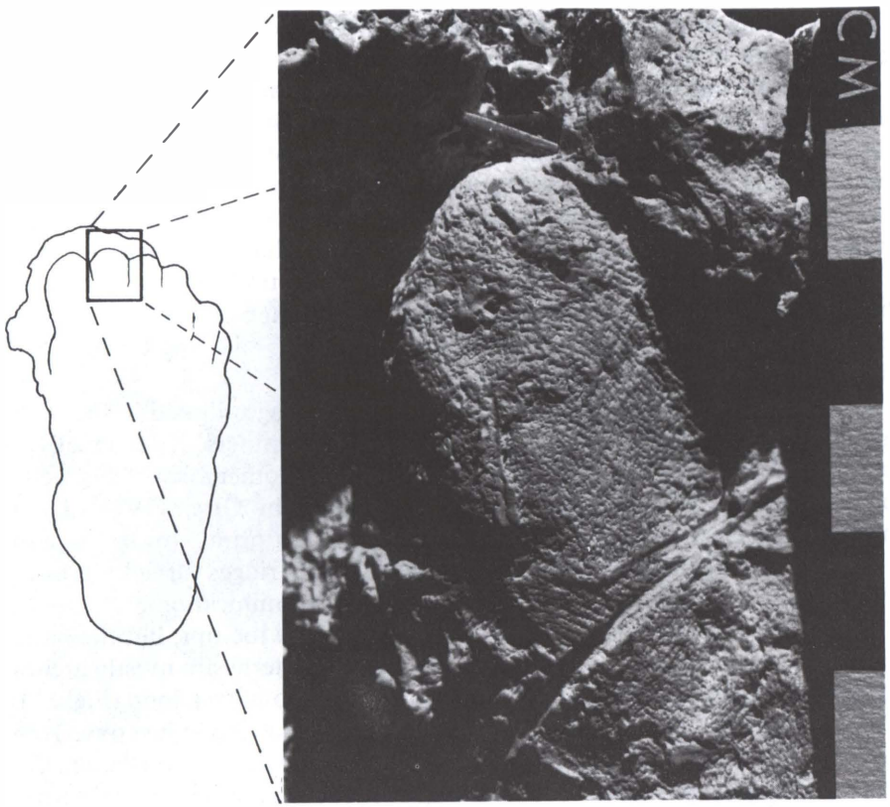

FIG. 10.—Enlargement of digit II of "full left." The position of this photo is shown on the outline drawing. The arch pattern is clearly seen on the tip, and a slight tendency to change to straight-across ridges below that. Just above the thin diagonal streak the pattern again arches, signaling the base of the very short toe. Note that there are no flexion creases; there should be three running across this digit in normal primates. The central area of this toe print shows both use wear and displacia of the ridges.

possibility of these tracks being faked with lifts and casts from real primate skin. How does one locate a group of people with arched thumbs of various sizes, and uniformly short, with none of them showing any flexion creases? While there are many pattern breaks with large blank areas on the soles, some of the toe prints are continuous from tips to metatarsal pads. No patching is indicated. I could find only one case in the literature of a human finger without flexion creases. Haylock (1983) illustrates one of a brachydactylous individual, where a very short digit II lacks creases and has a continuous arch pattern from the tip to the metacarpal end.

Dermal ridges were first suspected on the cast of a hand print, ascribed to a Sasquatch, that was recovered in Northeastern Washington in 1970

(Krantz 1971). In this case, parallel ridges are evident on the edge of one finger, near the tip. They are spaced 1 mm apart, thus making their identification as dermal ridges somewhat uncertain. Other footprint casts have been sought out and examined since the Walla Walla discovery, but nothing more than suggestive traces could be found in a few cases. This is not surprising because of the fortunate, and rare, circumstances in this instance.

In many places in the specimens considered here one can see small indentations, or pores, located along the ridges. These are typically spaced about 0.5 mm apart, and are centered on the ridges. They vary in size from barely visible, less than 0.1 mm, up to a diameter of 0.2 mm. The ridges visibly widen around each of these pores. This is most clearly seen at the base of digit I of the "full left" track (Fig. 10). The margins of these pores curve gradually inward to the centers—there is not a sharp edge. Several forensic specialists who have examined this material agree that these are sweat pores.

The sweat pores are generally lined up regularly on adjacent dermal ridges, as opposed to having alternating or random positions. In other words, the pores also occur along lines drawn perpendicular to the ridges. This pattern is not regular, but it is a strong tendency—just as in human dermatoglyphics (see Figs. 10, 11, and 12).

The possibility that air bubbles might have mimicked sweat pores was suggested by physical anthropologist Tim White, at the University of California, Berkeley, who otherwise thought the casts appeared to represent legitimate footprints. To settle this point, I made impressions of false ridges (with a fine comb) in similar soil, and cast them in paster. I compared the results with the actual casts, and found that there are, in fact, occasional air bubbles from casting. These bubbles, however, are sharp-edged, and are not as small as the apparent sweat pores. They are rather few, and not regularly spaced or lined up. In some cases, they also bulge out the ridges around them, but only slightly, and with a much thinner wall between the hole and the ridge edge than with the presumed pores (Fig. 13).

One fingerprint expert (with the Vancouver, B.C. Police Department) pointed out that sweat pores often have irregular edges, while these pores sometimes appear quite circular. This discrepancy results from the method of observation. Inked fingerprints used by law enforcement agencies record the very outermost edges of the pores, and these are usually not quite circular. Photographs of many of these cast pores show only shadows, thrown deeper into the holes, where active sweat pores are in fact perfectly rounded. Others, with more oblique illumination, show the typical irregularities of outline.

DISCUSSION AND IMPLICATIONS

Thus far, every specialist who has examined these casts agrees that their detailed anatomy has all the characteristics and appearance of being derived from an imprint of primate skin. These now include thirty police fingerprint

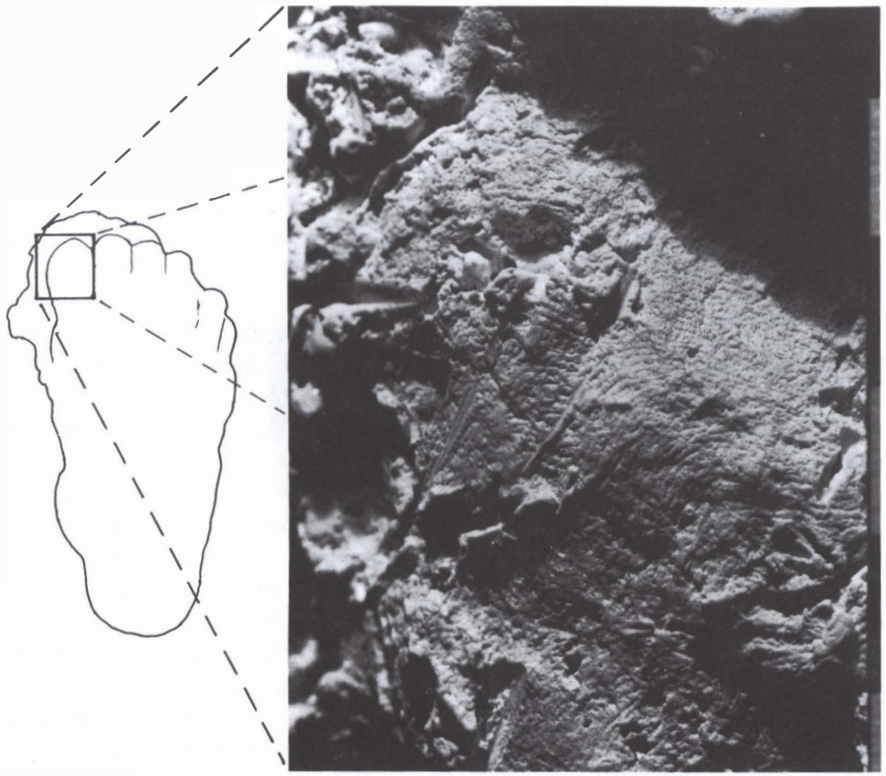

FIG. 11.—Enlargement of digit I of "full left." The position of this photo is shown on the outline drawing. This toe displays a loop pattern, only partly seen here. Running roughly down the center is a disruption that may represent an old injury.

workers, mostly from the Western states, twelve of whom might be considered experts. Also included are six physical anthropologists with expertise in this area, as well as four pathologists and two zoologists. At present, two of the police experts are willing to state categorically that the prints actually represent the existence of a real but unknown animal, regardless of the implications (see Addendum). Interestingly, these two are not only among the most highly qualified; they are also the two who have studied the material more thoroughly.

Even the strongest critic, a mammalogist at the Smithsonian Institution, agrees that the casts represent primate skin, but he thinks that it must have been transferred from known animals by silicone rubber casting and combined somehow to form the tracks. His examination of the casts was brief. Another strong critic, a leading medical authority on skin in the Pacific

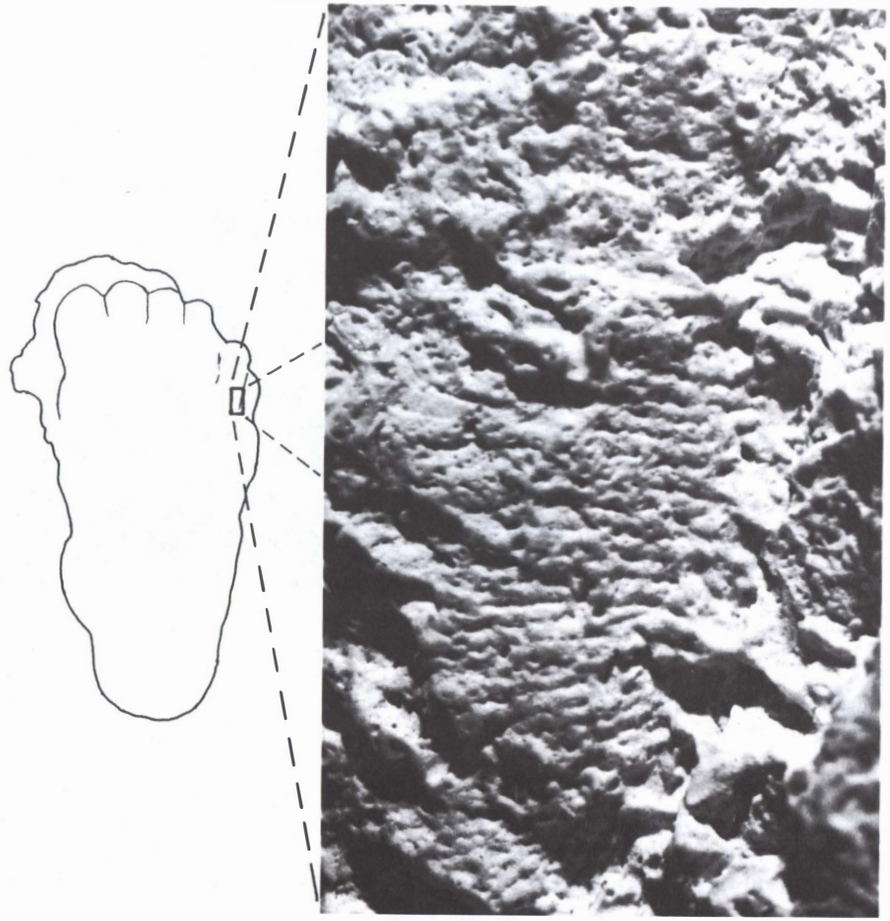

FIG. 12.—Enlargement of a patch of dermal ridges from the sole of "full left." The position of this photo is shown on the outline drawing. This section is typical of many small areas that preserved the skin imprints, while debris and adherence of dirt to the foot obscured the detail on most of the footprint.

Northwest, also agreed that the impressions were undoubtedly of primate friction skin, but left open the question of how they came to be located on these tracks. His examination of the casts was somewhat longer. Neither of these experts attempted to specify the source of the skin that might have been transferred to these tracks.

In their gross anatomy, the tracks could not have been made by any known animal. They are too large to be human, and the length of stride and depth of imprinting are also beyond human capability. Human feet do not have

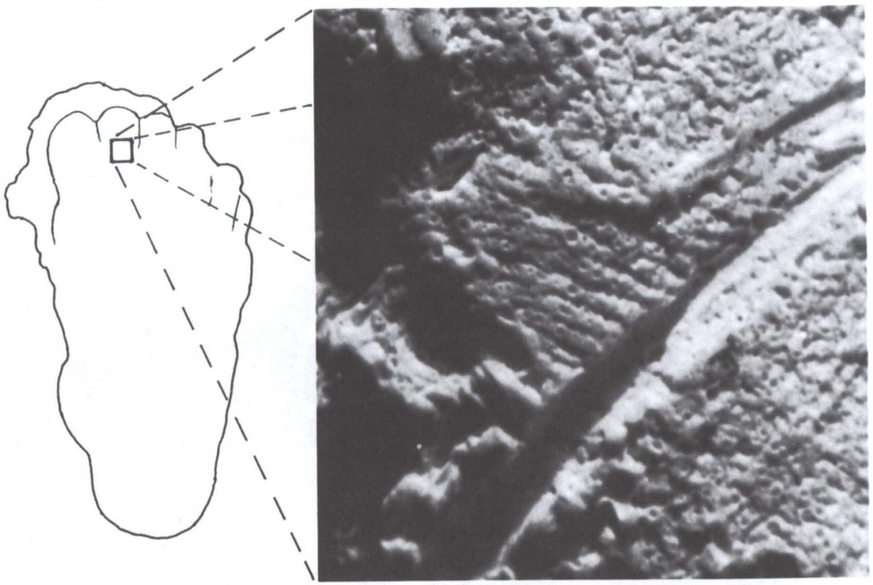

FIG. 13.—Extreme enlargement from the base of digit II of "full left." The position of this photo is shown on the outline drawing, and may be compared with Fig. 10, just above the thin diagonal streak. This best shows the sweat pores and their tendency to line up along adjacent ridges. Some of the larger "pores" seen here are probably exaggerated artifacts caused by air bubbles in the cast.

the flexibility of sole to conform to the large stone that was stepped on in one case. The toe design is also beyond the range of human feet in that they are relatively too short, toes II through V are too wide, and they line up too nearly straight across the front of the foot.

For all of the above reasons, no ape foot comes anywhere near matching these casts. In addition, all apes show some degree of opposability of the first digit, which is not seen in these casts. The lack of a longitudinal arch in these feet, interestingly, is not an argument against either human or ape identification.

Bear feet can easily be eliminated on gross anatomy. Again, these tracks are too large for the hind foot of any bear, in which the first toe is the shortest and the third is the longest. Bear tracks also generally show claw marks, and have especially narrow heels. No other animal feet need seriously be considered.

There are only two possible explanations to account for these footprints, short of denying their existence. Either they were fabricated somehow by a modern human hoaxer, or else they are the tracks of a presently unrecognized animal.

Previous studies have already dealt with most of the problems faced by a potential hoaxer of tracks like these in terms of physical circumstances. They need only be listed here for this case as reminders. The hoaxer had to penetrate a watershed area that is closed to the public by the U.S. Forest Service. This had to be done without leaving any noticed evidence of vehicles or human footprints into or out of the area. The unlikelihood of the tracks being discovered at least suggests that dozens or hundreds of sets of tracks must have been made in order to insure the discovery of some of them. The long stride, commonly 1.2 m, is difficult to manage even without big fake feet. Stepping once on the heel while coming down a 2 m embankment should have toppled any hoaxer on his face at the bottom. The depth of imprint means that, even if the hoaxer weighed 150 kg, he was carrying an additional weight of maybe 200 kg—and still managing long strides. He also managed to walk for at least three-quarters of a mile with all this equipment—that is how far the search and rescue team from Oregon was able to backtrack the first individual.

Accounting for the gross morphology of the tracks presents an equally baffling set of obstacles. The hypothetical hoaxer somehow knew how to make the soles flat, the toes short and wide with only a modest emphasis on digit I, the forefoot almost squared off, and the heel abnormally wide. These would not have been too difficult to execute if some of my published descriptions of tracks were consulted. But he also knew enough to introduce two other variations that I noted back in 1970; these have never been written down nor told to anyone.

Some innovations added here include the apparent double strike of one toe that must have been difficult to manage. One cannot help wondering why the extra pad of flesh was included next to digit V of the "full left" track, which looks almost like an incipient sixth digit. The deep indentation in midsole of the "full right" track shows a surprising degree of flexibility of the foot and thick cushioning. How the hoaxer managed to walk on top of these flexible pads is another problem—unless he was long accustomed to doing this.

Earlier descriptions of authentic-seeming Sasquatch tracks often included the "double ball." This is seen clearly in the partial left track, but for various reasons it did not show in the other two tracks. Differences in the relative depths of imprinting of the metatarsal ends between the tracks requires some explanation—perhaps separately articulated pseudo-bones in faked feet. Also, different inclinations of the toes between the two right feet implies still more structural complexity in the "feet."

The hoaxing problems recounted up to now strain the imagination. Yet, similar circumstances have been reported by reliable observers many times in the past, for which no good explanation has been forthcoming. The usual approach of the debunkers is simply to ignore the situation, or to insist that

the facts have not been correctly reported. When the actual tracks have eroded away, and memories have faded, all that remain are some big plaster casts. Little argument is left.

In this case, however, the three plaster casts show a degree of detailed anatomy that cannot be dismissed so easily. The supposed track hoaxer somehow managed to include microscopic detail that matches the foot skin of a higher primate. There are clearly formed dermal ridges, with sweat pores, over large parts of these three imprints. These details are of the correct size, shape, and orientation in each area for gigantic hominid feet. Some peculiarities include arched toe-tip prints, and very short toes that lack flexion creases. All possible sources of primate friction skin have been considered, and none can account for these characteristics. In addition, the hoaxer had the ridge relief slightly worn down in exactly the areas where the most weight would be supported on feet of this size and design. Finally, the incomplete pattern formation of displacia occurs on just the areas where it would be normal for feet and toes of this size.

Our track maker is an excellent outdoorsman of incredible size and strength. He is able to move about, largely unseen, over vast wilderness areas, leaving many tracks. He does not depend on vehicles to get in and out of the areas where the tracks are found. He is able to imprint the ground with tracks that are anatomically correct for a gigantic hominid. And he is able to leave, on occasion, clear impressions of primate friction skin. Such a track maker is not really impossible—just not *Homo sapiens.*

After removing any possibility of a human hoaxer, we can consider the footprints on face value. Using only the three prime footprints described above, the track maker can be reconstructed as follows. The skin makes it a higher primate, without specifying monkey, ape, or man. It is bipedal (no hand or knuckle prints show), and thus with upright posture. The stride indicates a stature of almost 2.5 m if it has a human body-to-leg ratio, where stature is twice the striding gait. (If it has an ape or monkey ratio it must be somewhat taller.) A body weight of about 400 kg is consistent with the depth of impressions, and with the indicated sole cushioning pad. The stature and weight would indicate a body build much more heavy-set than is usual for humans. The short toes, nondiverging first digit, and transverse midsole ridges all indicate a hominid—a member of the zoological family Hominidae that also includes humans. Granting the hominid status, it would have broad shoulders like any other ex-brachiator. One could also presume from the track locations, and the lack of any communications between them and ourselves, that they are not "human" in any cognitive and social meanings of the word.

The above description is consistent with Paul Freeman's sighting report of the animal that started this particular investigation. Many hundreds of other people have described seeing essentially the same kind of biped. All

such reports also include reference to a full covering of body hair, usually dark brown in color. This might be a reasonable expectation for a nonhuman higher primate, especially in a temperate climate. Most observers say the arms are relatively longer than is usual for humans—they are also more massive, especially at the shoulders. It is often noted that the head is set low, with no constriction showing at the neck. The face is described as being nearly flat, with little muzzle projection, and with deep-set eyes under projecting brow ridges. It has a head that rises only gradually behind the brows to a low peak toward the rear.

These last named traits are entirely consistent with the animal described from the footprints. It does not automatically follow that our track maker has all these additional traits, but it would be a most peculiar situation if it did not. That possibility would presuppose the existence of a giant hominid that leaves footprints and has never been seen, and another species of giant, hairy, gorilla-faced hominid that is often reported, but which leaves no footprints! The simplest resolution to this is that the footprints and descriptions may be ascribed to the same animal.

It may seem as if a large edifice has been erected on the base of just three footprints that were cast in plaster by the U.S. Forest Service in 1982. Actually, the edifice has been there all along, based on many hundreds, perhaps thousands, of footprint events and observations accumulating for over a century. The fact that direct, substantial proof of the animal has not been produced, combined with the fact that a proportion of the evidence has been demonstrated to be false, has led most biological scientists to deny the existence of the species. One prominent physical anthropologist has told me that, if these tracks were the first and only evidence available, the animal would likely be accepted. But as it is, with so much publicity and fakery known and suspected in the past, such acceptance will not occur. It has been far easier for most scientists to assume that *all* the evidence is false, than it is to believe that a giant hominid has eluded verification for this long a time.

The evidence of these three track casts has been presented (directly or by photographs) to many experts in the appropriate fields. The reactions of two major groups are significantly different. In the fields of physical anthropology and zoology, most of the responses have ranged from mild interest to flat denial that they could be real. Among fingerprint experts and pathologists, the responses have ranged from mild interest to enthusiastic acceptance.

To most anthropologists and zoologists, the implications of these tracks, if authentic, are profound. It would mean that their sciences have completely missed one of the biggest and potentially most important mammalian species on earth. They may even fear that the public might ask, if they were so wrong in this case, what else might they be overlooking? I have heard of instances in which scientists have sincerely expressed the hope that Sasquatch will never be proven, exactly because of these implications. It has also been

suggested that anthropologists are still smarting from the exposé of the Piltdown hoax, and that their public image would be severely damaged by another such instance. To them, I need only point out how almost all geologists firmly opposed the idea of continental drift until the late 1960's—they survived.

Police fingerprinters and pathologists generally have no stake in whether or not such a creature exists. It is outside their normal area of concern, and few have made any past pronouncements on the subject. They risk no professional embarrassment by its demonstration. If anything, they might be more concerned about the possibility that a hoaxer has developed a technique that can be used to fake fingerprints.

The evidence of this new set of tracks should be treated as conclusive by all authorities, but it is not. If and when a Sasquatch skeleton or fresh specimen is recovered, it would require little intelligence to conclude that the species has existed all along. What does require some reasoning power is to move from the presently available data to a clear conclusion as to either the method of hoaxing or to the reality of the animal.

ADDENDUM

(*Opinions by Other Experts*)

Tatyana Gladkova, Dermatoglyphics expert at the USSR Institute of Anthropology. Saw photographs of casts, including enlargements of key areas. (Response provided through Dmitri Bayanov, Moscow, USSR.)

"I see dermal ridges of the arch type distally directed. I see sweat pores. If it's a fake, it's a brilliant fake, on the level of counterfitting, and by someone well versed in dermatoglyphics."

Anthropologists *Mikhail Urisson* and *Vladimir Volkov-Dubrovin* (Deputy Director of the Institute) agreed with the above opinion.

Henrietta Heet, Candidate of Biological Sciences and Senior Scientific Worker, Institute of Ethnography of the USSR Academy of Sciences. Saw several photographs and brief description of circumstances of discovery. (Response provided through Dmitri Bayanov, Moscow, USSR.)

"Regarding photographs of skin imprints sent over by G. Krantz. I fully agree with his opinion on these footprints, as well as the opinion of Benny Kling. The structure of the dermal ridges is very much like that of man. The sweat glands have large openings because the ridges are much bigger than in man. It was great luck that the footprints were left in the soil that revealed fine details of the imprints. As for the patterns of ridges, some irregularity in ridge lines in separate places in the photos may be connected with the peculiarity of the material in which the imprints were made (unevenness of soil, various inclusions, such as small pebbles, pine needles, etc.). Another possibility is scars and skin injuries.

"Incidentally, even in ideally made human imprints there can be such irregularities. There is even a whole branch of dermatoglyphics studying genetic irregularities in ridge lines, i.e. medical and genetic dermatoglyphics.

"In the imprints shown by the available photographs, I cannot detect anything unusual, except digit I, left foot, which shows, apparently, a pattern of the arch type (in man the whorl type is more frequently found)."

Douglas M. Monsoor, Supervisor, Criminalistics Unit, Department of Public Safety, Lakewood, Colorado. Certified Latent Print Examiner, and fellow of the Fingerprint Society of the United Kingdom. Was sent detailed photographs in late 1982, examined original casts in December, 1982, and again in June, 1983, for two hours on each occasion.

"I see the presence of ridge structure in these track casts which, in my examination, appears consistent with that type of ridge structure you would find in a human. Under magnification, they evidence all the minute characteristics similar to human dermal ridges. The sizes, distributions, and orientations of the ridge patterns are consistent with those found on a human foot. Of the ridge structure visible in the impressions, I believe it was produced concurrent with the creation of the overall impressions, and not added later.

"If hoaxing were involved, I can conceive of no way in which it could have been done. They appear to be casts of original impressions of a primate foot—of a creature different from any of which I am aware."

Robert D. Olsen, Sr., Criminalist, Kansas Bureau of Investigation, Topeka, Kansas. Certified Latent Print Examiner, Fellow of the American Academy of Forensic Sciences, Fellow of the Fingerprint Society of the United Kingdom, Member of International Association for Identification, etc. Was sent detailed photographs in late 1982, and Silastic lifts in early 1983. Examined original casts and two-color lifts in June, 1983, for several hours.

"Based on everything I see, there is nothing in these tracks that is inconsistent with the impressions of an actual living primate foot. Ridges and pores are consistent with real primate skin. I'm convinced that this represents real friction skin, and shows no inconsistencies in structure or orientation.

"If they are faked, the individual would have to know an extraordinary amount about fingerprinting. I could not have done it. A faker would have to be an accomplished artist as well as an expert on dermatoglyphics. He would also need a knowledge of gross anatomy of feet. The amount of time needed to do all this work is beyond the realm of believability."

Edward Palma, Fingerprint examiner for the Laramie County Sheriff's De-

partment, Cheyenne, Wyoming. Had latex lifts from the footprints in late 1982—inked these and traced print pattern extensively. Had cast copies to examine at leisure. Saw original casts for several hours in early 1983—made Duplicast impressions of critical parts, and took photographs for further study.

"My professional opinion of the three casts is that they represent footprints of a living higher primate of an unknown species. The over-all configuration of the foot is roughly human, but it is too wide—a human foot would not be over five and one half inches [wide] for this length, and thus these impressions could not be human. The actual width is represented and supported by ridge pattern.

"My study of the tracks concentrated more on the sole than on the more conspicuous details of the toes. I traced the ridge pattern over the entire breadth of the forefoot, finding triradius landmarks appropriate in their respective positions with intervening ridges flowing in proper directions. It could not have been patched together from smaller parts that were copied from skin of a known primate.

"The detailed morphology of the ridge and furrow structures and patterns are especially convincing to me. In all details, they conform perfectly in design and size to real friction skin. The sweat pores are clear and are lined-up and spaced just as expectable, and can be distinguished from occasional air bubbles in the casts.

"I began this investigation with the goal of showing how these prints were, or might have been, faked. All evidence now tells me that any faking would be impossible."

Benny Kling, Instructor, Law Enforcement Academy, Douglas, Wyoming. Had latex lifts from the footprints in late 1982, and cast copies shortly thereafter. Saw original casts for several hours on two occasions in early 1983.

"These track casts show all the characteristics of real friction skin derived from a higher primate footprint. The ridge details, in all respects, duplicate that found in human feet. Parts of the pattern on right and left feet are near-mirror images; some displacia is indicated in the areas where it could be expected; smoothing by wear shows on the weight-bearing areas. In addition, the footprints indicate that an unusual proportion on the body weight fell on the front of the foot, and the arches are evidently flat.

"This kind of print could not have been made by a human foot, nor that of any known animal. It could not have been manufactured by any hoaxer; the design is too dermatoglyphically correct, and the engraving job would be beyond the capabilities of the best forger. Descriptions of the supposed Bigfoot, or Sasquatch, are consistent with the traits found in these footprints."

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A PREVIOUSLY UNREPORTED "SEA SERPENT" SIGHTING IN THE SOUTH ATLANTIC

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ABSTRACT: A casual conversation has resulted in a new, previously unreported "sea serpent" sighting being introduced into the historical record. Captain J. Koopman wrote in his memoirs about a "sea serpent" sighted in 1906 about 40 nautical miles off the coast of Brazil. The observation occurred less than 100 kilometers south of the location of the sighting reported by Meade-Waldo and Nicoll in the same year. Whether the same kind of animal was involved in both incidents is not clear.

Cryptozoology is characterized by a paucity of solid data. Every shred of direct observation, every new sighting, adds to the weight of evidence, but the reporting of new evidence has always been difficult. The International Society of Cryptozoology was formed to provide a sympathetic environment for the dissemination of knowledge about creatures of doubtful existence. That the Society is already playing a useful role is illustrated by the manner in which the sighting described below was brought to light, and is now part of the historical record.

I was explaining my interest in cryptozoology to Jean Paelinck, a professor of economics at Erasmus University, Rotterdam, Holland, and a colleague on a scientific commission of the University of Quebec, when he exclaimed: "But, my grandfather saw a sea-serpent!" After a life at sea with the Dutch merchant marine, Professor Paelinck's grandfather, Captain J. Koopman, had returned home to write his memoirs. Professor Paelinck later sent me a copy of the relevant passage. Here is the story.

It was 1906. Captain Koopman was at that time an officer on a merchant vessel steaming from the Mediterranean to Montevideo, Uruguay. He writes in his memoirs:

> On a Sunday afternoon, at about 3 o'clock, I was on watch. The sea was flat as a mirror and seemed to reflect the heat as well as the light. Except for the engine room crew and the officers on watch, the whole ship's crew lay asleep on deck, half undressed, under an improvised tent. In the distance, there was a sailing vessel, with its sails loosely flapping in the wind. I changed course by a few degrees so as to be able to read its name and home port with my telescope, in order to report its position upon arrival in Montevideo.

Koopman goes on to relate how difficult it was to see the ship's flag with the sails flapping, but that he could manage to read its name (which he couldn't recall) and its home port (Hamburg). He then writes:

> I was still standing in the middle of the bridge, with my telescope pointed at that sailing ship when the wheelman suddenly shouted: "Sir! Sir! Look over there, on starboard!" The canvas screen which normally protected the watch officer from wind and spray was lowered over its entire length, on account of the oppressive heat, so that I had an unobstructed field of view. I saw, about one hundred metres away, obliquely on starboard, an enormous beast whose length I approximated at about 60 metres. It was overtaking our ship, which appeared to be standing still, with the speed of an arrow off a bow. With the help of my telescope, I could form some idea of the monster, although only in an approximate fashion. The monstrous head and a number of enormous dorsal fins sticking out above water level, as well as its wide wake, showed the nearly horizontal posture of this giant sea-dragon or serpent.
>
> We were, at that time, about 40 sea miles (about 74 km) offshore from Pernambuco. The coast thus lay well below the horizon; nevertheless, I saw that town, upside down, profiled sharply against the sky: an impressive *fata-morgana*. The wheelman and I pointed out to the officers and crew the charming optical illusion. However, we chose to hide the matter of the sea-serpent, for fear of ridicule.

The pattern is familiar. The mirage, backed by a solid body of refraction theory (Fraser and Mach 1976) is accepted and admired by all. The "sea-serpent," on the other hand, is an anomalous event, beyond the borders of science, best filed away in silence.

No further details are given, and Koopman assures us that "he would never have brought the matter up again, had not a study related to the sea-serpent quite accidentally come into his hands." The study in question was the description published by Meade-Waldo and Nicoll (1906), of an unknown marine creature off the coast of Brazil. This sighting is also discussed in Heuvelmans (1968: 372). Koopman was greatly encouraged by the attention given to such a phenomenon in the scientific press, and concluded his description of the incident as follows: "Is it not quite remarkable that Meade [-Waldo] saw this animal at the end of 1905 on the Brazilian coast, at nearly the same point on the globe where we would, a few months later, observe an identical phenomenon?"

The two observations are indeed very close in space. Pernambuco (usually called Recife) is at 8°06′S, 34°53′W, so that Koopman's sighting, approximately 75 km to the east, would have occured at about 8°06′S, 34°13′W. Meade-Waldo and Nicoll give their position as 7°14′S, 34°25′W, less than 100 km to the north. That the same kind of animal was seen is not clear from the descriptions given. Koopman's report is very much less detailed than that provided by the trained naturalists; it also mentions a "number of dorsal fins" rather than one "large fin." However, there must have been some similarity in order for Koopman to speak in terms of an identical phenomenon.

Any attempt at classification of the Koopman sighting must remain very tentative. The multiplicity of fins observed suggests that the creature might belong to Heuvelmans' (1976: 562) "many finned" class, in contrast to other

observations in the same area (Heuvelmans 1968: 554), which fall into the "super-eel" category.

I thank Professor Jean Paelinck for sending me the extract from Captain Koopman's memoirs, and for granting permission for the publication of this information. Professor W. Opechowski provided invaluable assistance in the translation of the sighting from the Dutch original.

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VERTICAL FLEXURE IN JURASSIC AND CRETACEOUS MARINE CROCODILIANS AND ITS RELEVANCE TO MODERN "SEA SERPENT" REPORTS

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Abstract: Reports of "sea serpents" often mention that these animals move by vertical undulations of the body. It has been pointed out that, among living vertebrates, only mammals and birds can flex their body in a vertical plane, while fishes, amphibians, and reptiles can only undulate in a horizontal plane. However, the vertebral structure of Mesozoic marine crocodilians of the infraorder Thalattosuchia made vertical flexure possible. It thus appears that reports of vertical flexure do not necessarily demonstrate that the "hidden animal" in question cannot be a reptile.

A noteworthy feature of many "sea serpent" and "lake monster" reports is that the animal's body is said to undulate in a vertical plane. Almost a century ago, in his famous study of the "great sea serpent," A. C. Oudemans (1892: 523) stated that "generally it swims with vertical undulations." Modern popular imagination apparently also considers this type of locomotion as one of the most striking features of "sea serpents," to judge from the number of newspaper cartoons which show their bodies describing graceful loops above the surface of the water.

Bernard Heuvelmans (1965) devoted several pages of his exhaustive monograph on the "great sea serpent" to this question, and he mentioned this point again in his *Derniers Dragons d'Afrique* (1978), in connection with a "lake serpent" report from Lake Victoria. In both instances, he rightly pointed out that such vertical flexure of the body is of some importance in determining the zoological affinities of "hidden" aquatic creatures. In 1965, he remarked that all fishes, amphibians, and reptiles swim by horizontal undulations of the body, because the structure of their vertebrae does not allow flexure in a vertical plane (the amphisbaenians being the only exception among reptiles). Mammals and birds, on the other hand, are able to bend their vertebral column in a vertical plane, as exemplified by the movements in the water of aquatic mammals, which propel themselves largely by vertical undulations of the body. Heuvelmans also stressed the fact that the supposed vertical flexure in "sea serpents" is not merely a result of poor depiction of these animals by unskilful illustrators: innumerable witnesses concur in describing this particular mode of locomotion.

My aim here is to point out that, although Heuvelmans is correct as far as *living* reptiles are concerned, some extinct ones were, in fact, capable of vertical flexure. This has been shown by Bernard Krebs in a series of pa-

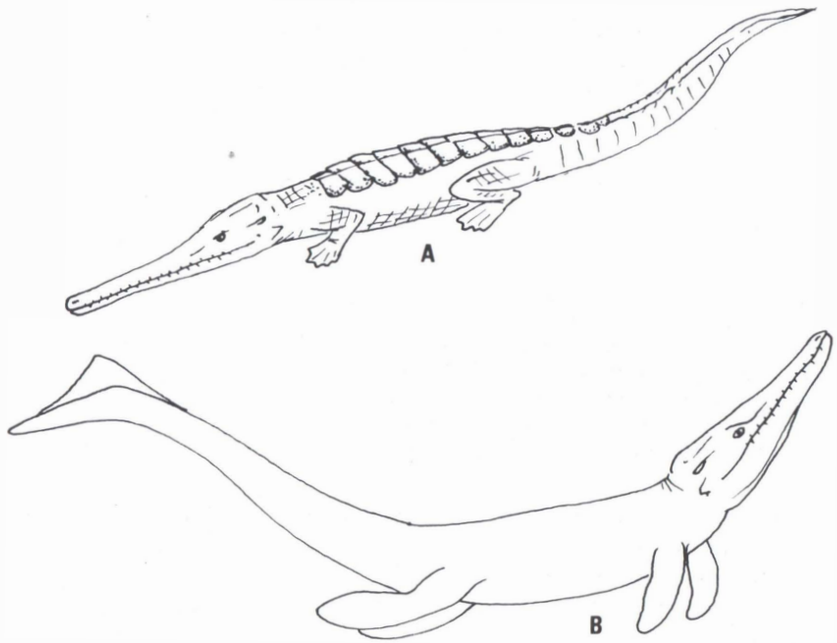

FIG. 1.—Reconstructions of two Jurassic thalattosuchians. A: the teleosaurid *Steneosaurus*. B: the metriorhynchid *Metriorhynchus*. Each animal could reach a length of about 4 meters.

leontological studies (1962, 1967a, 1967b, 1968) on Jurassic marine crocodilians belonging to two families of the archaic suborder Mesosuchia, the Teleosauridae and the Metriorhynchidae. These families of long-snouted crocodilians (Fig. 1), which lived during most of the Jurassic and disappeared in the early Cretaceous, were actually closely related, and can be included in a common infraorder, the Thalattosuchia (Buffetaut 1980, 1982).

Krebs based his inference on studies of the vertebral structure of these animals. The plane in which the body of an animal can bend is determined mainly by the orientation of the zygapophyses of its vertebrae. As pointed out by Alfred S. Romer (1956: 225), "a flat horizontal apposition of zygapophysial surfaces permits relatively free lateral movement within the column but sharply restricts vertical flexure; an increasing degree of tilt of the surface tends to restrict lateral bending but allows greater vertical movement." Krebs (1962: Fig. 3a) has very clearly illustrated the difference in the orientation of the zygapophysial facets between an animal which swims by horizontal flexure of the body, such as the alligator, and one which moves by vertical bending, such as a whale. While it is true that most known reptiles

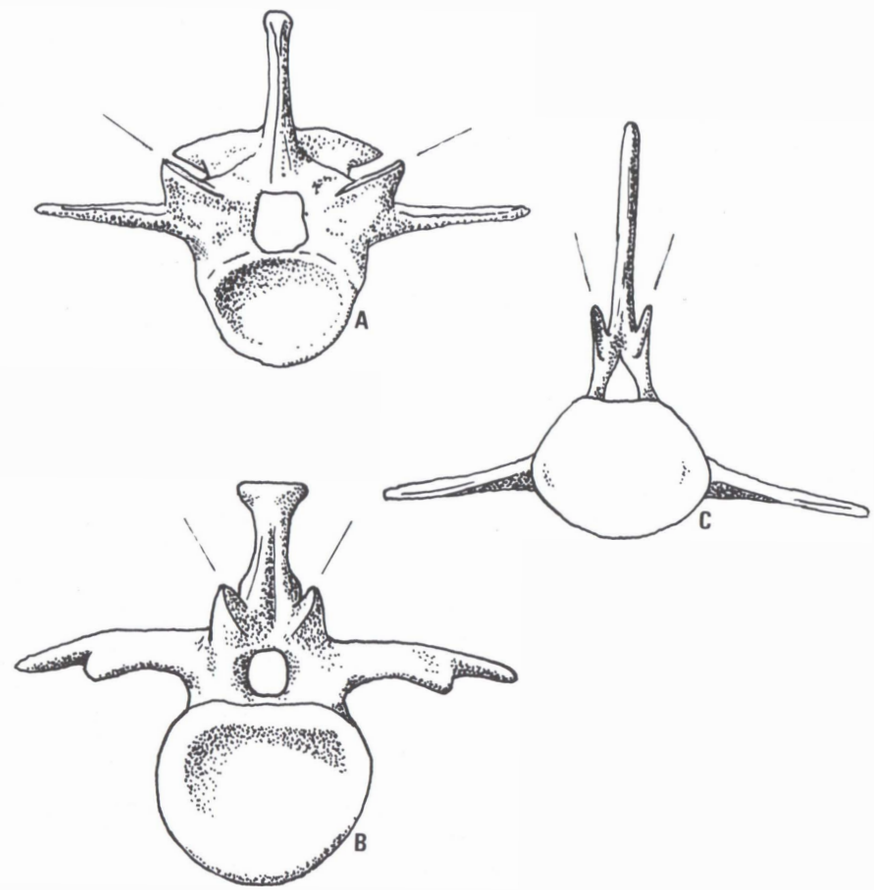

FIG. 2.—Anterior views of dorsal vertebrae of (A) the living alligator *Alligator mississippiensis*, (B) the Jurassic teleosaurid *Steneosaurus* sp., and (C) a living whale, *Balaena* sp. The straight lines indicate the tilting of the zygapophysial facets. The convergence between *Steneosaurus* and the whale in this respect should be noted; it indicates that vertical flexure of the body was possible in thalattosuchians. Redrawn after Krebs (1962).

are similar to the alligator in having zygapophysial facets in a subhorizontal or feebly tilted plane, Krebs showed that both the Teleosauridae and the Metriorhynchidae had strongly tilted zygapophysial facets, which are reminiscent of the condition exemplified by whales (Fig. 2), and which prevented lateral bending of the body while allowing vertical flexure.

According to Krebs (1962, 1967a, 1967b, 1968), the locomotor consequences of such a condition were that the Teleosauridae and the Metrio-

rhynchidae could not swim in the same fashion as modern crocodilians (which undulate in a horizontal plane), but were able to hold their body rigid while propulsion was effected by rapid movements of the end of the tail (which in metriorhynchids bore a small vertical fin). However, Krebs states that vertical flexure was also used, especially when these animals were diving.

Krebs' research on the vertebral structure and locomotion of thalattosuchians is obviously important to discussions of the zoological affinities of "sea serpents," because they show that, in some reptiles, a type of vertebra could evolve which permitted vertical flexure of the body while at the same time restricting horizontal bending. This does not imply that all (or any) reports of "sea serpents" undulating in a vertical plane are based on observations of surviving thalattosuchians. It is obvious that many features of most "sea serpent" reports are hardly compatible with the morphology of a crocodilian, which is rather different from that of a snake or an eel. Even such a specialized form as a thalattosuchian would be unable to bend its body in such a way as to produce the many vertical undulations mentioned in some eyewitness reports. Also, the thermic physiology of crocodilians, which restricts their range to the warmer parts of the world, is not compatible with the numerous sightings of "sea serpents" in cold waters. Nevertheless, Heuvelmans (1965), in his classification of "sea serpents," considers that a few reports of crocodile-like "sea monsters," from tropical areas, may be based on surviving thalattosuchians. In my opinion, this is by no means impossible; coelacanths were also supposed to be extinct since the Cretaceous until one was found alive in late 1938.

Whether or not some reports of "sea serpents" are based on crocodilians, the example of the Mesozoic Thalattosuchia shows that *some* reptiles have been able to acquire by convergent evolution a vertebral structure relatively similar to that of mammals, enabling them to bend their bodies in a vertical plane. This, in turn, shows that one should be cautious when using reports of vertical undulations as evidence for the mammalian nature of a "sea serpent." Vertical flexure may be a strong hint in favor of identification as a mammal, especially if it is accompanied by other corroborating evidence, but the possibility that it could be the result of convergent evolution in a non-mammal should also be kept in mind, all the more so that, in the Thalattosuchia, the evolution of strongly tilted zygapophyses is very likely to be the result of adaptation to a fully (or almost fully) aquatic life.

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FURTHER NOTES ON THE ROLE OF FOLKLORE IN HOMINOLOGY

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ABSTRACT: Dmitri Bayanov has pointed to folklore as an important source of information on possible relict hominoid creatures. He suggests that one may infer from such a creature in a tale the existence, present or former, of a real hominoid in the environment of the tale bearers. In fact, however, since the elements in a folktale serve largely an emotive or expressive function, a folktale is only useful in suggesting facts about relict hominoids just in so far as its hominoid creatures fail to show folkloric traits, exhibiting instead mundane details that have crept into the tale from the real world. Often, such fact-like details are relatively obvious, but for others a team of experts is needed to ensure proper evaluation. Two tales, one from the Ubykh of the Caucasus, the other from the Bella Coola of British Columbia, are briefly examined for both fact-like and fantastical elements regarding hominoid creatures.

Dmitri Bayanov, one of the world's foremost experts on "relict hominoids," has recently called attention to the value of folklore for the study of such relict forms (Bayanov 1982). This is only his latest restatement of a position previously put forth by him and his colleague, Igor Bourtsev (Bayanov and Bourtsev 1976). In short, their position is this: given the recurrence of hominoid figures in many of the world's folkloric traditions, these bodies of lore should be scrutinized more carefully by a host of experts to extract what, if any, information may lurk therein regarding relict men or man-like creatures.

A very simple and seemingly compelling example which they put forth is that of the figures of wolves and bears in folklore. Their conclusion is that such figures exist in a given lore precisely because such animals are part of the experience of the bearers of this lore. A similar inference must be made for hominoids. In part, I think that they are clearly right, and are performing a highly useful function in calling the attention of experts to a source of information which, in many (if not most) cases, may be the only source of information on relict hominoids in a particular area. On the other hand, their example of the wolves and bears of folklore is, I feel, misleading. One needs to look very carefully at exactly what types of information folklore can provide regarding matters of the real world and the creatures in it.

First, put quite simply, there are many figures that are frequently found throughout the world's folklore which no one would assume directly refer to a counterpart in the real world. Thus, one finds witches and sorcerers, demons and ogres, angels and good fairies. The list could be quite long.

Some of these figures, such as those of witches, sorcerers, and ogres, may reflect deep seated fears, perhaps arising during childhood, as in our own lore, or, in some cases, active daily concerns on the part of more "primitive" peoples. Thus, a witch may stand for the fear and revulsion a child feels toward a particularly unfortunate old woman that he may have met. Many "primitive" cultures call strangers "demons" or "devils." For such peoples, strangers actually are demons in that they violate the customs of the tribe, customs used to define humanness in the face of inhumanity. The racial slurs and epithets of our own time are dim reflections of such earlier loathings.

In short, a monster with man-like properties need not suggest that the lore in which it appears in some way bears testimony to the existence, present or former, of a relict hominoid in the environment of the lore-bearers. In certain cases, as with "primitive" peoples, demons, ogres, etc. may actually exist as perceptual categories into which certain peoples are placed, but such existence is of interest to the student of culture, not to the student of relict hominoids. Roughly speaking, folklore is expressive, not assertive. It depicts the feelings and reactions of a people to a multitude of things in their world. Such lore, be it the heritage of a "primitive" or a "sophisticated" people, does not come with an index listing the things in the world to which specific folkloric figures stand as emotional expressions. For example, when we find tales containing wolves, we infer that wolves are, or were, part of the fauna with which the tale tellers must have dealt. We do so because we all know, from firsthand experience, that wolves are real animals. Even if the wolf talks and carries on a wily dialogue with Little Red Riding Hood, we are not surprised because we realize that this is part of the expressive, narrative role of folklore. Thus, folklore represents a mode of expression in which fact and feeling, animal and man, animate and inanimate, nature and society, are all made to assume *dramatis personae* so as to take part in the narrative. As characters on a stage, the creatures, people, and elements of the world do not fall into the categories that we ("sophisticated" peoples) have come to feel reflect the objective world. They may bear traces of their objective properties, but as *dramatis personae* they carry non-natural (to us) features and traits, aspects directly related to the feelings and fears of the people bearing the lore.

Given this emotive and expressive aspect of folklore, one may feel that no useful information may be obtained from tradition. Perhaps we should simply ignore the mass of material on the Sasquatch that Suttles has listed (1972, 1980). In short, this seems to be exactly what many investigators have done. Both Suttles and Bayanov suspect that this is just the reason that so little of the lore on relict hominoids has found its way into enthnographic accounts and compilations. In my own case, working as a linguist, I did not know what to do when I came upon material dealing with a montane forest

man, as he is called in Circassian, and if it had not been for the purely fortuitous fact that a conference on human-like "monsters" was held a few years after I had come upon this account, I might never have published my own paper on the topic (Colarusso 1980).

However, despite its fundamentally expressive form, folklore does offer a source, sometimes the only one, for interesting information about a people's world, and it should be recorded and scrutinized not merely for its literary value, but also for its factual content, however masked that may be. Such scrutiny is done simply by looking for non-emotive factors in the tale. Such fact-like elements will be there not because they in some way serve the basic function of the tale, but because they have leaked in, so to say, from the world of brute fact, albeit filtered through the perceptual sieve of the culture involved. I shall now give a simple example of a fact-like hominoid account and an emotive hominoid account taken from the Caucasus.

The Ubykh are a people, now living in Turkey, who formerly lived on the east coast of the Black Sea, up against the Caucasus Mountains, with their kinsmen the Abkhazians to the south and the Circassians to the north. Their language is on the verge of extinction, and consequently has been the object of intensive efforts to record it during most of this century. By chance, one of the tales recorded contains accounts of both a fact-like hominoid and an emotive one (Dumézil and Namitok 1955: 30–33). A hunter goes up into the mountains, slays a deer, and prepares to cook it over his campfire. After placing his food on the fire, he hears a loud cry from the depths of the forest. Climbing into a nearby tree for safety, and not forgetting to take his food with him, the hunter nevertheless leaves behind his shaggy wool cloak, which he has draped over a tree. To his great shock and fear, a wild man emerges from the forest. This being is said only to be covered all over with hair. Nothing else is said by way of description. This wild man pounces upon the cloak of the hunter, at which point the hunter fires his pistol at the wild man. Whether or not he wounds the thing, its pelt catches fire and it runs off into the forest from whence it came.

There are a number of presumptive facts that one can extract from this account. First, there are hair covered man-like hominoids that live in the deep forest of the Caucasus, or lived there until recently. They are attracted by either the light of a campfire or the smell of cooking meat. They announce their approach by screaming loudly. Perhaps some territorial behavior is involved. They engage in actual combat. The wild man seems to have mistaken the shaggy cloak of the hunter for either a man in a cloak, or, more interestingly, for another wild man. In the latter case, this would imply that wild men use fire and cook meat.

The reaction of the hunter is not that of a hero, but merely of a man badly frightened. Sensibly, he climbs a tree for safety. He does not exhibit masterful marksmanship, but, in fear, sets off his pistol, perhaps missing or merely

wounding the wild man. The wild man seems to have caught fire from the campfire, and this case of a badly singed pelt, quite naturally, drives him off back into the forest. In short, both the hunter and the wild man act quite naturally, exhibiting aggression and fear in a manner conconant with a rare but real encounter. This account is only slightly more dramatic than that of the wild man to be found in the recently translated novel of the Abkhazian writer, Fazil Iskander (1983: 31). In this account, a forest woman jumps out of a thicket up in the mountains, runs past the Abkhazian hero, Sandro, and disappears into a rhododendron thicket before he can catch her. She is said only to have knee length hair, and to be beating her forehead (she at least has one!) in what appears to be lamentation. In Sandro, Iskander is trying to present not merely a heroic and satiric figure, but also an apotheosis of the beauties and lore of Abkhazian culture (*ibid.*: vii–viii). Sandro's encounter sounds like an anecdote that Iskander must have heard back in his native Abkhazia.

Such accounts are similar to the Circassian one of men trading with the wild men (Colarusso 1980: 257–58). My own Circassian informant first heard of the "montane forest man" when his father was teaching him hunting techniques (*ibid.*: 256–57). All this points to a near-man, perhaps a Neanderthal or another recent "sub-sapiens," or perhaps an early modern man, or even a relict *Homo erectus*. In a biological sense, the Caucasian wild man is quite mundane. This biological plausibility contrasts sharply with the emotive hominoid of Caucasian lore.

In the same story, the Ubykh hunter, once having driven off the wild man (and once the story teller presumably has his audience's complete attention following this hair-raiser), wanders lost in the forest until he enters a great wide plain. There, he encounters a raging giant bound in stone. This is the famous image of the Caucasian bound giant, apparently the source for the Greek Prometheus legend (Olrick 1922: 133–290). While the wild man is called "montane forest man," the giant is simply called "ugly big one." He is characteristically one-eyed, carries a weapon (in the present story a hook-like implement), is bound to a stone or rocky summit of a mountain, and is set upon destroying mankind and the whole order of nature should he ever be set free. He is the last of a wicked race, chained for his evil doings, and determined to destroy the world in revenge for his fate at the hands of man. He is Doomsday incarnate. His anatomy is implausible. He is not hairy, merely one-eyed and huge. He carries a weapon, usually magical, with which he will destroy the world. By refusing to listen to the giant's pleas to be released, the hero exhibits his knowledge of and respect for the old beliefs. The second half of the tale thus bears religious overtones of pagan origin. It has little content from which one might extract information about a possible relict hominoid. In its content and function, it stands in stark contrast with the first part of the story.

The tale of the wild man at the campfire sounds like it could be a narrative of an actual encounter. If anything, it serves to frighten the listener and attract his attention. The tale of the giant would seem to relate old religious beliefs, perhaps ancient fears about the inclement elements so typical of the mountain summit. Clearly, the giant's wicked intent is strongly contrasted to the bounty and fertility found in the wide plain, full of fruit and livestock. Thus, the giant probably has no hominoid prototype at all except for his personification based along the lineaments of man himself.

The Ubykh tale offers a relatively clear cut contrast between emotive and fact-like hominoid accounts. There is an analogous Bella Coola tale from the coast of British Columbia (Davis and Saunders 1980: 192–99), concerning a Sasquatch (called a /puq'ws/ in Bella Coola, a Salishan language). In this tale, the same figure plays both a fact-like and emotive role. Two encounters are depicted. In the first, a man named Almtsi is digging for clams when he hears what he thinks are two Sasquatches digging nearby. He goes and spies two of them, one digging toward the water and the other carrying armfuls of clams back into the woods. After a bit, he decides to try and shoot one, but his gun fails to fire. The Sasquatches, however, hear the "click," and, apparently knowing it for the sound of a trigger, run off. In the second encounter, a couple with a child are sitting by the campfire near the shore. They hear loud deep noises in the forest, which they think are thunder. Then, what they take to be a man suddenly appears, but to their fright it proves to be a Sasquatch, and the man throws a firebrand into its face. While the Sasquatch is writhing in pain, the family makes good its escape. The head of this family is named Qaaklis.

The encounters of both Almtsi and Qaaklis with the Sasquatch are considered rare and unusual events, but they are both conspicuously lacking in emotive content, except for the fright of Qaaklis and his family. One might infer that this tale relates noteworthy encounters with some sort of relict hominoid. This hominoid dwells in the forest, but will come down to the water. In Almtsi's case, the Sasquatches exhibit cooperative effort of a simple sort, as well as a taste for clams. In Qaaklis's encounter, it is not clear what the Sasquatch is after (perhaps it would like to eat the child, or perhaps steal their belongings, or drive them off its territory), but at least it exhibits no fear of fire. It slaps the trees, making a thunderlike sound as it does so. This the Bella Coola narrator explains as part of common knowledge about the habits of Sasquatches, and as his way of explaining an error on the part of Qaaklis and his wife, who took it as thunder. Bayanov (1982: 47) mentions tree-striking as a Sasquatch feature, and offers the ingenious interpretation that this may be the creature's way of knocking down dead trees so that it can feed upon the larvae therein.

This last feature of tree-slapping is an excellent example of ambiguous or opaque data. To a folklorist with no training in primate behavior, such a feature might seem purely mythical, with little factual basis. To a hominologist, this feature seems to be an account of a straightforward feeding habit, albeit the Bella Coola narrator seems quite unaware of this possible interpretation. Clearly, as Bayanov urges, a host of experts should scrutinize such lore.

Other features in the Bella Coola account bear an emotive quality. Qaaklis and his family find, in the course of their escape, that the channel they have taken seems to be running shallow, so that they have to drag their canoe along. Almtsi's gun fails to fire. The significance of both happenings is not stated. The listener is left to infer some mystical or magical significance, should he or she so choose. These emotive features, however, may be explained. The gun simply failed to fire. Perhaps as he was near water Almtsi let the gun get wet. Qaaklis nearly grounds his canoe with his family and all his belonging on board. Though he should have been familiar with the channel, in his great fear he may have run afoul of some shallows in the dark. Nevertheless, the narrator has left open the *possibility* that the strange figures of the Sasquatches may have had some ill-defined role in causing these odd mischances. The narrator concludes the story by saying that these are unusual events.

Despite this eerie quality, the mischances have nothing about them that in any way interferes with the extraction of mundane information about a possible hominoid from the two encounters. No weird anatomical aspects are involved, such as a single eye. In fact, the tale is quite disappointing in providing very little information about the creatures, except that they may be mistaken for people, but in fact are beasts (Davis and Saunders 1980: 196). In fact the creatures are not even considered half-men or some such, but simply are taken as animals. No strange lasting effects are experienced by the men. The encounters are eerie and associated with odd happenings, but these are explicable. This story stands in stark contrast with, for example, another one in Davis and Saunders (1980: 247–51), in which a man who habitually wanders about the woods at night meets "Death." The man faints, and is revived by a "spirit" who gives him a magical implement that can be used to revive the dead. Back in his village, the hero does just that and becomes a famous shaman. I would not waste my time looking for hominoid creatures in this story. Against this shaman's tale, the first story takes on a very mundane aspect.

I would assert the following about the usefulness of folklore in the study of relict hominoids. In so far as a tale lacks mystical, numinous, magical features, in short, in so far as it contains features that do not appear to serve an emotive, expressive function, then these features may be scrutinized for possible data regarding relict hominoids. Such scrutiny is worthwhile even if a portion of the story lapses into a highly magical tone. One is looking for noise from the real world that has found its way into an otherwise fantastical

drama. Such scrutiny should be conducted by numerous experts with wide-ranging knowledge, preferably by a linguist to ensure that the words of the tale are glossed accurately; by a folklorist so that elements with typically emotive function may be identified with a high degree of probability; by a cultural anthropologist who knows the patterns of the society involved, so that features that might seem peculiar to an investigator, but are really just aspects of the culture at hand, may be identified; and finally, by a primatologist, so that traits typical of (or possibly related to) hominoid behavior may be singled out and not taken for a residue of fantastical features, such as may be the case with tree knocking.

In effect, the analysis of a tale proceeds from prior knowledge brought to bear upon it from a wide spectrum of experts. In this sense, the analysis is basically like that of inferring a wolf in the world from a wolf in a tale. It is more complex, however, in that the traits belonging to a wolf in the real world are fairly well known to everyone, whereas the traits of any putative surviving hominoids must be inferred from those of their near kindred, ourselves (presumably), the great apes, and primates in general. One must err on the side of caution in such an undertaking, and not seek a Sasquatch behind every ogre, no matter how often ogres are encountered in the world of folklore.

Surely many more creatures lurk in the forests of the mind than in the forests of the real world.

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Research Reports

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ATTITUDES OF BIOLOGICAL LIMNOLOGISTS AND OCEANOGRAPHERS TOWARD SUPPOSED UNKNOWN ANIMALS IN LOCH NESS

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ABSTRACT: Three hundred American and Canadian Ph.D.-level scientists, including 100 biological limnologists and oceanographers, were surveyed on their attitudes toward the existence of supposed unknown animals inhabiting Loch Ness, Scotland, and the supposed Sasquatch (Bigfoot) of Northwest America. More than a third of the biological limnologists and oceanographers responding accepted "Nessie" as an animal "still unknown to science," and about 65 percent believed the subject should be studied by scientists; only 23 percent, however, supported the use of government funds for such research.

INTRODUCTION

Over the decades, there has been much speculation concerning the supposed animals inhabiting Loch Ness, Scotland (heretofore referred to as "Nessie"), first as to whether such animals actually exist, and second, as to what kind of animals they might be.

There has also been speculation as to what scientists, particularly freshwater and marine biologists, think of Nessie. It has generally been assumed, not altogether incorrectly, that most scientists do not take such reports seriously. This perception has probably resulted from scientists' on-the-record comments to newsmen during interviews following Nessie sightings. But what about their off-the-record personal beliefs, which they may not necessarily divulge to newsmen? A study was undertaken to investigate these attitudes, and the relationship between attitudes and disciplinary fields, at least in North America. The self-report method using specially designed questionnaires was used. We present here those parts of the survey results which will be of particular interest to students of the Loch Ness Monster problem.

TABLE 1.—Responses by biological limnologists and oceanographers on the cause of Nessie/Sasquatch reports.

	Nessie	Sasquatch	χ^2
Living animals "still unknown to science"	38.7%	12.5%	4.409 $p < .05$
Ordinary animals misidentified	48.4%	34.4%	nonsignificant
Imagination, hoaxes, myths	38.7%	78.1%	4.581 $p < .05$

METHOD

Three hundred Ph.D.-level American and Canadian scientists, all employed at universities or research centers, were surveyed on their attitudes toward Nessie and the supposed Sasquatch (Bigfoot) of Northwest America. One hundred of these were biological limnologists and oceanographers randomly selected from the 1976 *Membership Directory* of the American Society of Limnology and Oceanography. Half of the target individuals were mailed a Nessie questionnaire; the other half received a Sasquatch questionnaire. The other two groups (also with 100 individuals each) were physical anthropologists and physical chemists. All were guaranteed anonymity.

RESULTS

Of the 100 biological limnologists/oceanographers surveyed, 64 responded, but only 63 returned questionnaires; of these, 31 were on Nessie and 32 were on Sasquatch. Table 1 shows that almost 39 percent accepted Nessie as a real animal "still unknown to science," while an equal number believed it is simply a result of imagination, hoaxes, and myths; a majority, over 48 percent, believed it is a result of honest misidentifications of ordinary animals. Only 12.5 percent accepted Sasquatch, while over 78 percent attributed Sasquatch reports mainly to imagination, hoaxes, and myths.

The reasons why the majority of the respondents rejected the existence of Nessie is made clear in Table 2: the lack of specimens or osteological material. Table 3 shows that slightly over half of the respondents believed that the

TABLE 2.—Reasons for rejecting Nessie/Sasquatch.

	Nessie	Sasquatch
Lack of fossil evidence	-0-	46.9%
Lack of specimens (or parts of)	48.4%	84.4%
Lack of bones	32.3%	78.1%
Too large/too tall	3.2%	6.3%
Lack of environmental nutritional resources	12.9%	12.5%
Could not remain so long "undetected by science"	25.8%	46.9%
"Too bizarre" to consider	3.2%	-0-

TABLE 3.—Impact of Nessie/Sasquatch discovery "on science."

	Nessie	Sasquatch
Severe impact	6.5%	21.9%
Moderate impact	38.7%	53.1%
Slight impact	54.8%	21.9%

$\chi^2 = 7.807$, $df = 2$, $p < .025$.

TABLE 4.—Responses on whether scientists should undertake Nessie/Sasquatch research.

	Nessie	Sasquatch
Certainly should	29.0%	31.3%
Probably should	35.5%	21.9%
Uncertain	3.2%	12.5%
Probably should not	22.6%	28.1%
Certainly should not	6.5%	6.3%

TABLE 5.—Responses on whether government funds should be made available for Nessie/Sasquatch research.

	Nessie	Sasquatch
Certainly should	6.5%	15.6%
Probably should	16.1%	9.4%
Uncertain	12.9%	18.8%
Probably should not	35.5%	31.3%
Certainly should not	29.0%	25.0%

TABLE 6.—Biological limnologists and oceanographers who have seen/read scientific literature on Nessie/Sasquatch.

	Nessie	Sasquatch
Only seen scientific literature	9.7%	12.5%
Read scientific literature	77.4%	21.9%
Neither	12.9%	65.6%

TABLE 7.—Biological limnologists and oceanographers who are aware/have read Mackal/Napier books.

	Roy Mackal's book on Nessie	John Napier's book on Sasquatch
Aware of only	16.1%	21.9%
Have read	9.7%	-0-
Neither	74.2%	78.1%

discovery of Nessie would have only a "slight impact" on science, while only 22 percent believed the discovery of Sasquatch would have no more than a "slight impact."

As shown in Table 4, about 65 percent of the respondents indicated that scientists either "certainly" or "probably" should undertake Nessie research, while 53 percent supported research on Sasquatch. However, as shown in Table 5, only 23 percent supported government funding for Nessie research, and only 25 percent supported government funding for Sasquatch research. Thus, although Nessie is viewed as a more credible phenomenon than Sasquatch, support for research and government funding are about the same for both.

A majority of 77 percent of the respondents had read some scientific literature on Nessie (as opposed to popular literature), as shown in Table 6, but only 22 percent of the respondents had read any scientific literature on Sasquatch. Table 7 shows that only about a quarter of the respondents had read or were aware of Roy Mackal's book on Nessie (Mackal 1976), and that less than a quarter were aware—and none had read—John Napier's book on Sasquatch (Napier 1972). Over half of the Nessie respondents provided personal information on themselves (including name), and over one-third provided informative comments.

DISCUSSION

We can conclude that there is far less skepticism about the existence of Nessie than there is about the existence of Sasquatch among North American biological limnologists and oceanographers, although the existence of both is strongly doubted. Because of the lack of physical evidence, the majority believe that Nessie reports are based on honest misidentifications of ordinary animals, imagination, hoaxes, and myths, although they seem to bend over backward to support scientific investigation of the topic, provided government funds are not involved.

A similar survey of British marine and freshwater biologists has not been carried out. It would be of interest to compare the results of such a possible future survey with the results presented above.

An analysis of the data focusing on the Sasquatch responses by physical anthropologists is also available (Greenwell and King 1981a, 1981b). More complete survey results, including response rates by physical chemists and a selection of informative and abusive comments by the respondents, may also be found elsewhere (Greenwell and King 1980).

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Field Reports

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RESULTS OF THE FIRST CONGOLESE MOKELE-MBEMBE EXPEDITION

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INTRODUCTION

Interest in the existence of an extraordinary animal called Mokele-Mbembe has continued since the two recent American-Congolese expeditions (see 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; see also Herman A. Regusters, 1982, Mokele-Mbembe: An Investigation Into Rumors Concerning a Strange Animal in the Republic of the Congo, 1981, *Munger Africana Library Notes,* Vol. 12(64): 1–27). Evidence found by these expeditions concerning the existence of this animal has greatly interested the international scientific community, particularly since the described morphology of the animal is similar to that of the sauropod dinosaurs of the Mesozoic. As a result of this, a meeting was held in early 1982 to discuss the feasibility of conducting a Congolese government-sponsored expedition to determine the existence of Mokele-Mbembe.

The meeting included representatives from the Ministry of National Education, the Ministry for the Promotion of the Arts and Scientific Research, the Ministry of Tourism and the Environment, the Ministry of Water and Forests, and the author, who participated in one of the American expeditions (see Mackal, Greenwell, and Wilkinson 1982, above) as a biologist representing the Congolese government.

The new expedition was originally planned for 1982, but a series of technical, organizational, and financial difficulties rendered this impossible. Consequently, the expedition was rescheduled for April–May of 1983. The expedition, under the leadership of the author, left Brazzaville for the Likouala region on April 3, 1983.

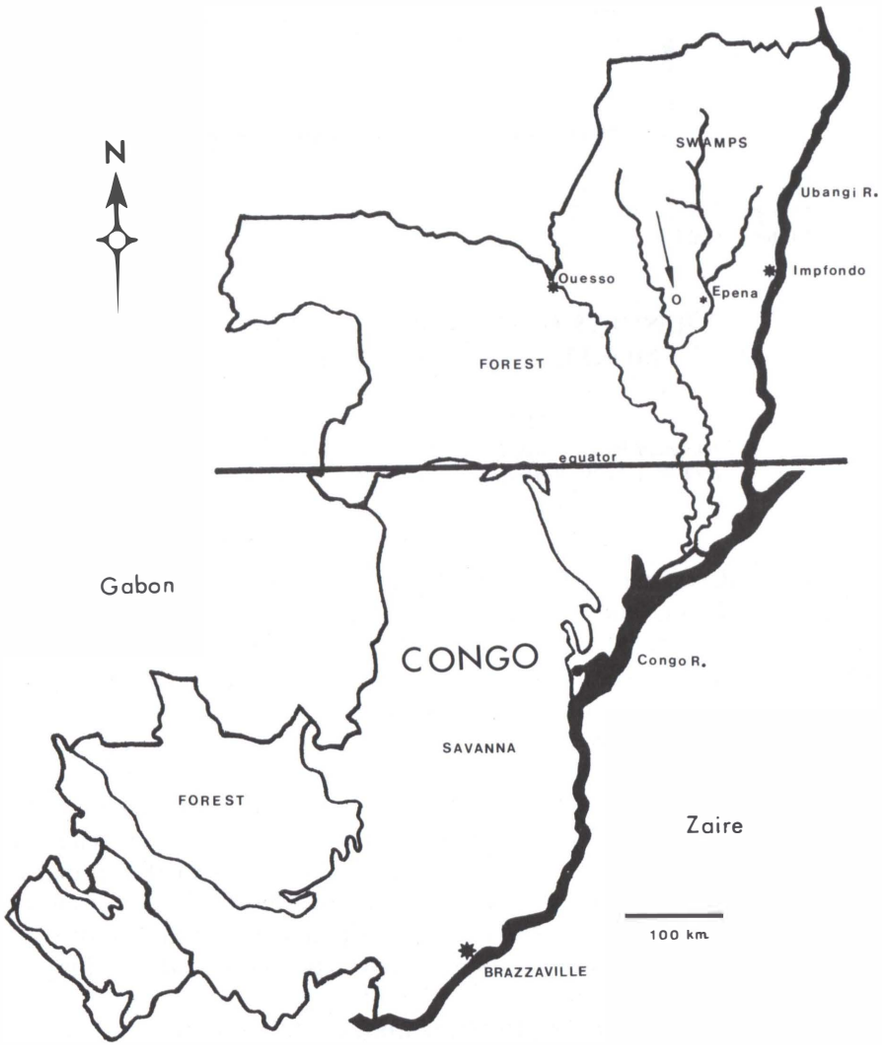

FIG. 1.—Map of the People's Republic of the Congo. The expedition flew from Brazzaville, in the south, to Epena, in the north, located on the Likouala River. Approximate location of Lake Telle indicated by arrow. Most of the northern part of the country is comprised of almost impenetrable, seasonally inundated swamp-forest.

NARRATIVE DESCRIPTION

The expedition consisted of the following individuals: Marcellin Agnagna, biologist, Ministry of Water and Forests; Jean Marcel Abesse, zootechnician, Ministry of Water and Forests; Auguste Mfouka, engineer, Ministry of Water and Forests; Serge Etienne Mingouolo, chief technical agent, Ministry of Water and Forests; Marcel Ontsira, Ministry of National Defense; Honore Basso, Ministry of the Interior; and Romuald Ntekissa, Ministry of Information.

FIG. 2.—Typical small village on the banks of the Likouala River, as seen from dugout.

The expedition departed from Brazzaville for Epena, located on the Likouala River, where several days were spent preparing for the journey downriver (Fig. 1). The district commissioner loaned us a motorized dugout, and we also purchased 200 liters of motor fuel. The team then proceeded by dugout down the Likouala (Fig. 2), past the confluence of the Likouala and Bai Rivers, to Bouanila, where we initiated our fieldwork. Following discussions with villagers and dignitaries at Mokongo, we examined the Bolelo pool, in which Mokele-Mbembe is said to be found on occasion. Certain indications of the existence of a large animal were uncovered. The second point of study was Botongo, but no further evidence was uncovered there.

We then proceeded north to Edzama, Djeke, and finally Boha, the inhabitants of which "own" Lake Telle, one of the reported habitats of Mokele-Mbembe. The stay at Boha lasted one week, due to the reluctance of the villagers to cooperate with our expedition concerning our proposed work at Lake Telle. This was partly due to their negative experiences with one of the previous American expeditions in 1981. Finally, after a week of discussion, the villagers acceded to our requests, stating that they were only doing

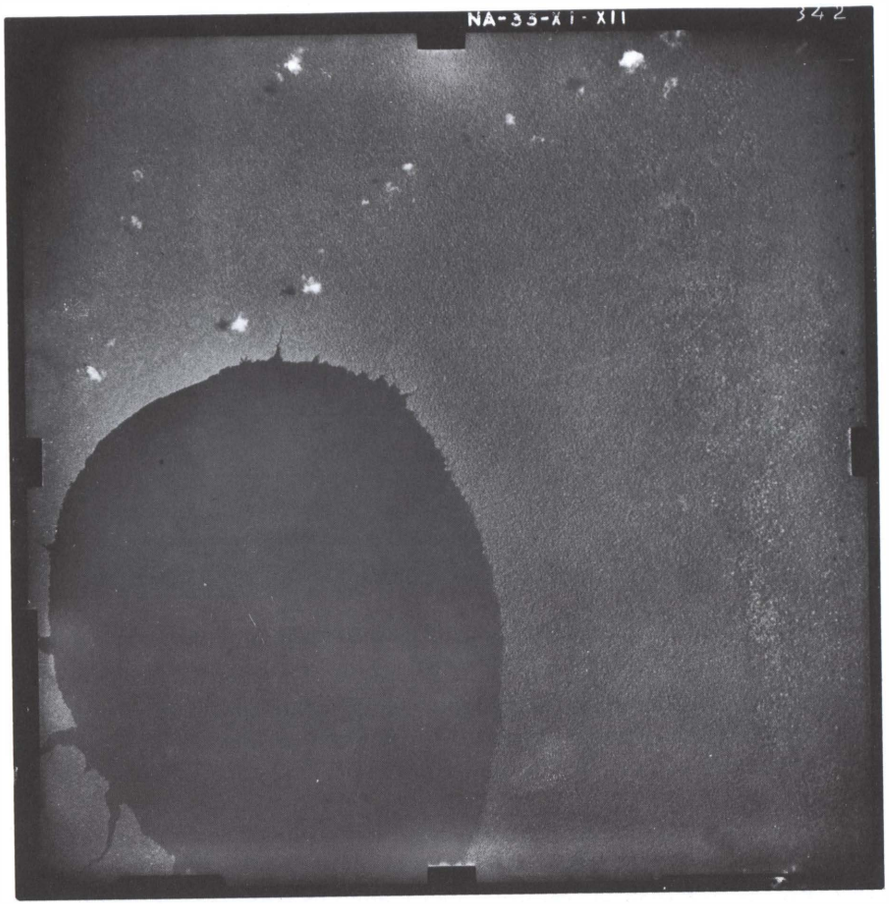

FIG. 3.—Aerial photo of Lake Telle, one of a long series of aerial photographs of the Likouala region taken by the French government. Note swamp-forest reaches the very edge of the lake. (*Institut Géographique National, Paris.*)

so because our expedition was an official project of the Congolese government. Even so, these disruptions had caused morale problems among expedition participants.

On April 26, the drums were beaten at Boha, while village elders made their ritual prayers invoking the spirits of their ancestors to protect the expedition members and ensure the success of the mission to Lake Telle. The expedition then set out on foot, accompanied by seven villagers from Boha who were to act as guides in the forest. The trail through the forest proved to be quite difficult and it was usually necessary to cut through the

FIG. 4.—Illustration by the author of the animal seen by him and two other witnesses in Lake Telle, from a distance of about 240 meters. The total length of the animal, presumed to be Mokele-Mbembe, was about 5 meters visible above the water.

foliage to allow passage. It being the dry season, water was scarce, and it became necessary to drink from muddy pools.

The 60-kilometer trek to Lake Telle was completed in 2 days, and it was with some emotion that we finally looked across this little sea, located right in the heart of the equatorial forest of Central Africa. The lake is oval in shape, about 5 kilometers by 4 kilometers (Fig. 3). A base camp was established at the water's edge, and one of the Boha villagers caught a large turtle which served as dinner that first night. Two days of intensive observing of the lake produced no sightings of the supposed Mokele-Mbembe, although there were frequent observations of a large turtle, with a shell reaching 2 meters in length.

On May 1, 1983, the author decided to film the fauna in the low-canopy forest surrounding the lake. This forest is a habitat for many mammalian and bird species. The author and two Boha villagers, Jean Charles Dinkoumbou and Issac Manzamoyi, set out early in the morning. At approximately 2:30 p.m., the author was filming a troop of monkeys. One of the villagers, Dinkoumbou, fell into a pool of muddy water, and went to the edge of the lake to wash himself. About 5 minutes later, we heard his shouts to come quickly. We joined him by the lake, and he pointed to what he was observing, which was at first obscured by the heavy foliage. We were then able to observe a strange animal, with a wide back, a long neck, and a small head (Fig. 4). The emotion and alarm at this sudden, unexpected event disrupted the author's attempt to film the animal with a Minolta XL-42 movie camera. The film had been almost totally exposed already, and the author unfortunately began filming with the lens cap on. By the time this was realized, the film had been totally exposed, as determined by subsequent processing in a French laboratory.

The animal was located at about 300 meters from the edge of the lake, and we were to able to advance about 60 meters in the shallow water, placing us at a distance of about 240 meters from the animal, which had become aware of our presence and was looking around as if to determine the source of the noise. Dinkoumbou continued to shout with fear. The frontal part of the animal was brown, while the back part of the neck appeared black and

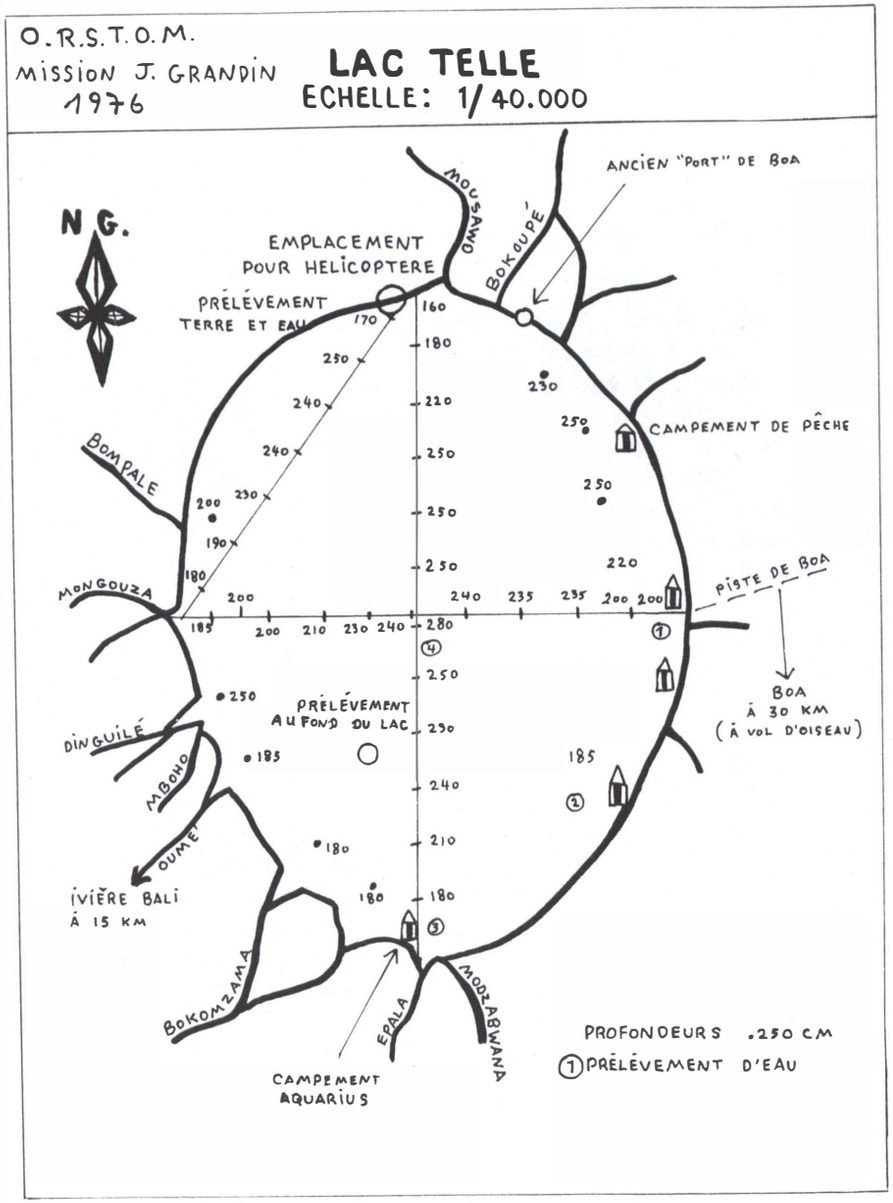

FIG. 5.—Field sketch of Lake Telle showing the measured depths obtained in 1976 by a French government expedition. Depths are in centimeters. Maximum depth, in the center, was 280 centimeters. (*Jean Grandin, Centre de Brazzaville, Office de la Recherche Scientifique et Technique Outre-Mer* [*ORSTOM*].)

shone in the sunlight. The animal partly submerged, and remained visible for 20 minutes with only the neck and head above the water. It then submerged completely, at which point we trekked rapidly through the forest back to the base camp, located 2 kilometers away. We then went out on the lake in a small dugout with video equipment to the spot where we had observed the animal. However, no further sighting of the animal took place.

It can be said with certainty that the animal we saw was Mokele-Mbembe, that it was quite alive, and, furthermore, that it is known to many inhabitants of the Likouala region. Its total length from head to back visible above the waterline was estimated at 5 meters.

The expedition's return to Boha through the same forest route took place on May 3, 1983, and we then proceeded back to Epena. Because of the remoteness of the Likouala region, we had great difficulty in returning to Brazzaville. After waiting for a week for the plane to pick us up at Epena, we were finally forced to trek over 100 kilometers through dense forests and swamps to Impfondo, on the Ubangi River, where there is a regular flight to Brazzaville. The trek to Impfondo proved to be a difficult and exhausting experience. The expedition formally terminated on May 17, the date of our return to Brazzaville. The entire expedition had taken 45 days.

RESULTS

The fauna of the Congo is still relatively unstudied. No thorough research in the area has ever been undertaken, and no comprehensive publications on the subject have appeared. The Likouala region, and more specifically the Epena district, with all its swamps and forests, remains almost entirely unfrequented by man. Because of the inaccessibility of the area, the zone has become a refuge for certain animal species, many of which were observed during the course of the expedition. Herds of buffalo (*Syncerus caffer*), ranging from five to 20 individuals, are found in the area of Mokongo. The buffalo population is quite dense in this region compared to other parts of the country, where it is becoming rare. The forest elephant (*Loxodonta africana cyclotis*), is still abundant, as it has been able to avoid most human contact. At Edzama, herds of between five and 15 individuals may be seen. Most primates withdraw into the forests and rarely show themselves in the areas of the villages or the rivers. The Western lowland gorilla (*Gorilla gorilla gorilla*) is still quite abundant in the Likouala region, including the swampy areas around Lake Telle.

The avifauna is quite varied and largely represented by ciconiiforms (herons, storks, and bitterns), falconiforms (fish eagles and black kites), pelicaniforms (cormorants), and some anseriforms. Marabou storks are present in significant numbers, and groups of up to 100 birds were seen on the sand banks at Mokongo. Reptiles are abundant in the Likouala, and crocodiles

may be seen sun-bathing on the sand banks. It is worth noting that crocodile hunting has diminished in this area because of a decrease in the purchasing of skins. In summary, the Likouala region remains one of the richest areas in terms of fauna, almost all known Congolese species being represented there, and the density of the populations is quite high. It is also suspected that the size of certain specimens observed in the Lake Telle area surpass that of the standard, turtles being one example.

It was verified that Lake Telle in generally quite shallow, with a depth of about 2–3 meters (Fig. 5). In certain areas, deep holes are found, reaching approximately 6 meters in depth. The water is dark, due to rotting vegetation, and visibility underwater does not exceed 50 centimeters. The bottom is muddy, with the layer of mud estimated at 70 centimeters in the middle of the lake and, at most, 150 centimeters around the sides. Various inlets of the lake branch off into small forest rivers, but these eventually terminate in thick vegetation.

Besides the known fauna of the region, there are many stories of encounters between local inhabitants and strange animals. It appears that the accounts of Mokele-Mbembe in the past have been based on actual occurrences, but the witnesses have left no written accounts for subsequent generations, oral accounts being the main form of information transmission in the area. With this current expedition, the situation has changed in that a trained biologist can now report his observation of one of these animals. And this individual is also acquainted with the known fauna of the Likouala.

Besides the sighting of Mokele-Mbembe by the author and two other witnesses, additional information of interest was obtained from villagers in other areas. One account concerned an event which took place in May, 1980, near the village of Mokongo. The witness noticed that the water had become murky in the area in which he was fishing, and that branches and leaves were floating on the surface of the river. Upon inspection, he found that trees and bushes had been dislodged from the riverbank, as well as large lumps of earth. A large animal was then observed in the water, but the morning mist made it impossible to distinguish its form. The destruction supposedly caused by this animal was observed by the expedition members. Apparently, according to different villagers, this phenomenon has occurred a number of times over the years.

Another event related to us occurred near Edzama just 2 weeks before our arrival. A village girl was returning home when her dugout became stuck on a sand bar. She pushed against the obstruction with her paddle, and found the dugout being forced against the sand bar once more. A large animal then appeared, creating a large disturbance on the water's surface. She was unable to distinguish the head or tail of the animal, but later indicated that the bulk

observed was the size of four forest elephants (which are smaller than the more commonly known African bush elephant). The girl was in a state of shock and was crying for help, and she was later rescued by her parents. Our inspection of this sand bar indicated that it had been well-swept by some agency, as if an animal had, in fact, been resting and moving a bulky body about on the sand.

At Djeke, we found unidentified footprints in the same location as where the Mackal expedition found the trail of a large, unknown animal (see Mackal, Greenwell, and Wilkinson 1982, above). The prints were very recent, no older than 4 days, and we were able to follow them for over 200 meters right to the edge of the water in the company of an elephant hunter, Immanuel Mongoumela, the same individual who had shown the trail to the Mackal expedition. This same individual reported that, since the Mackal expedition had been there, he had observed, on three separate occasions, a long-necked animal eating vegetation while submerged in the water. Only the neck of the animal had been visible as it consumed leaves and fruit. He identified the plant eaten as the Mabondji, and also the fruit of the Molombo (see Charles W. Weber, James W. Berry, and J. Richard Greenwell, 1982, Mokele-Mbembe: Proximate Analysis of Its Supposed Food Source, *Cryptozoology,* Vol. 1: 49–53). Mongoumela is one of the few witnesses who remains unmystified by the presence of these animals. He considers them as natural to the ecology of the area as others he encounters. It should be noted that he has been an experienced hunter for over 30 years.

In summary, it can be concluded that the first Congolese expedition in search of Mokele-Mbembe was highly successful. The animal itself was observed at Lake Telle, and evidence supporting the existence of other individuals in the Likouala region was obtained. Fieldwork was conducted under relatively favorable conditions, it being the dry season; this increased our ability to observe, unlike with the Mackal expedition, which operated during the rainy season. The scientific team, however, was incomplete, since only one biologist was present, and he had to undertake all the faunal studies on his own. Other expedition members were concerned with botanical aspects and with security.

There was also the problem of obtaining correct information from the local inhabitants, who are always reticent about giving this for fear of falling ill or even dying; their tradition dictates this. It is clear that the inhabitants of the region are well aware of the existence of these extraordinary animals. If these animals are indeed surviving sauropod dinosaurs, it would be further indication that the Likouala region and other similar regions in Central Africa have undergone no important geomorphological and ecological changes since the Cretaceous.

Future Plans

The results obtained by the first Congolese expedition confirm those of the prior American expeditions, one headed by Roy Mackal, and the other by Herman Regusters. The author believes that it is now necessary to undertake a new expedition, on a larger scale and involving more competent individuals. Adequate means, both material and financial, would have to be provided, and an expedition lasting about 3 months would be more productive. It is hoped that the Congolese government will pursue this matter further, and sponsor a systematic inventory of the animal species to be found in the Likouala region.

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FURTHER INVESTIGATIONS INTO THE BIOLOGICAL AND CULTURAL AFFINITIES OF THE RI

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Introduction

Between mid-June and mid-July, 1983, the authors conducted fieldwork on the east coast of central and southern New Ireland, an island province of Papua New Guinea, located northeast of New Britain and mainland New Guinea at about 3°S. latitude and 152°E. longitude. The purpose of the fieldwork was to further investigate reports of an aquatic animal the Barok natives of the area call the Ri. The existence of the reports was first published by the senior author (Wagner) after conducting fieldwork in New Ireland in 1979–80 (see Roy Wagner, 1982, The *Ri*—Unidentified Aquatic Animals of New Ireland, Papua New Guinea, *Cryptozoology*, Vol. 1: 33–39).

The Ri is described as a marine animal with a "fish-like" lower trunk, terminating in a pair of flukes, but with a "human-like" upper trunk and head. The size is about that of a human. The description generally resembles that in reports from many other parts of the world, including Western cultures in Europe. Various medieval writings and artwork in Europe depict such supposed animals, known to us today through folklore as the mermaid.

While it would be unwise to give serious consideration to the possible existence of mermaids as portrayed in Western literature and mythology, it was felt that further investigation of such New Ireland reports was warranted, particularly as the natives claim to distinguish the Ri from other marine mammals, including the Indo-Pacific dugong, which, together with the Caribbean and African manatee, has long been regarded as the animal giving rise to mermaid beliefs. It was thought that the Ri reports might be generated by a marine mammal still unknown to science, the discovery and description of which would be of significant zoological importance.

The expedition program had three general objectives: 1) to conduct further interviewing of the Barok natives to elicit additional details regarding Ri behavior and ecology which might aid in the development of observation

and capture strategies; 2) to locate physical remains of the Ri, which are sometimes said to be caught by accident in fishing nets and eaten by villagers; and 3) to attempt to observe, film, or photograph the animal in its own aquatic environment, and to capture a live specimen if possible.

It was thought that the natives could identify particular beach vantage points. If such observations proved to be successful, attempts would then be made to approach an animal in a small boat. Should the Ri allow itself to be approached in such manner, attempts would then be made to obtain close-up film footage of the animal, both above and below the water surface. A rubber dinghy, snorkeling gear and underwater cameras were included in the expedition's equipment. Ideally, the expedition would capture a live specimen unharmed. Nets could prove useful, but it was recognized that the capture of a specimen would prove to be very difficult—perhaps impossible—on a first expedition, as capture strategies would have to be formulated on site, following a study of the animal's habits. Spearguns or other equipment which could harm or kill such an animal were not included in the expedition's equipment.

Before departure, we consulted with several marine biologists to aid us in a possible on-site identification of the Ri. The consensus by the authorities consulted was that the animal being reported was a mammal. Three kinds of mammals are partly or totally marine: the cetaceans, the sirenians, and the pinnipeds. Various species of cetaceans and one sirenian (the dugong) are found in the New Guinea area. No pinnipeds are known in that part of the world.

Narrative Description

Three expedition members (Wagner, Greenwell and Raymond) initiated work in Kavieng, the capital of New Ireland. Visits were made to provincial officials, including Premier Robert Seeto. The purposes of the expedition were discussed, and the premier offered his encouragement, as well as logistical support in the event that a live specimen was acquired and it proved to be an unknown animal. Provisions were acquired in Kavieng, and a vehicle was rented.

The team drove south for seven hours to Namatanai, the only town in the southern part of the island. The dirt road, the only real one on the island, follows the coastline for about 200 miles. Most villages are located along this road, or between the beach and the road. Contact was first made with Mr. Bernie Gash, manager of the Namatanai Hotel. Mr. Gash manages several coconut plantations in the area, and kindly provided the Ramat Plantation house as a base of operations. The plantation is about 18 miles north of Namatanai, overlooking Ramat Bay, where Wagner had lived in 1979–80.

Initial interviewing was done at the closest village, Korapun, where Wagner had once observed what a native (now deceased) had identified as a Ri. Some villagers knew some English, but all spoke New Guinea pidgin, which Wagner also spoke fluently. Kurus, a local villager who had previously served as Wagner's anthropological informant, was retained to assist the team. Interviewing continued at other villages, and various new Ri accounts, of varying degrees of interest, were obtained. One villager from Giligin (real name!), for example, claimed to have once seen six Ri in line on a reef in Ramat Bay.

It became apparent that villages further north in central New Ireland regard the dugong and the Ri as the same animal, as was determined by Wagner when he uncovered "Ri bones" buried in the sand. In the area of Ramat Bay and further south, however, the distinction becomes clearer, and two separate animals are described in the native accounts.

Of particular interest was the reported instance of a Ri killing in the mid-1970's (see Wagner 1982, above). Villagers at Korapun identified two individuals who had been present when the animal had been hit by an axe, thrown in the back of a truck, and taken down the coast to Namatanai. There it was killed, butchered, and eaten. One of the two witnesses, still living in Namatanai, stated that the animal was not a Ri, which he had never seen, but a dugong. A younger relative of the witness, who had been present at the butchering as a boy, later informed one team member that it was not a dugong at all, but a Ri, and that his uncle ("my father but not my father") knew this; he emphasized that he would never contradict his uncle in front of others. The other witness identified by the Korapun villagers was also located. He was a Western-trained medical orderly (a "doctorboi"), and he stated emphatically that the animal was definitely not a dugong, which he claimed to know well, and was as other villagers described: human-like in the upper torso, with arms closed in or "fused" to the sides of the body, smooth skin below (no scales), and light brown in color.

One may well ask why one witness insisted that the animal eaten was a dungong. The "doctorboi" and other villagers implied that this witness was purposefully giving incorrect information, perhaps related to the fact that he and other inhabitants of Namatanai were originally from the island of Manus, and did not want to be involved in further controversy. A dispute had apparently arisen with villagers at Ramat Bay at the time the animal was taken to Namatanai concerning the appropriateness of killing it, with some villagers demanding that it be set free. This hypothesis was later strengthened during a sing sing—a ceremonial dance—when another villager, a relatively well-educated stranger, approached one of the team and, stating that he knew both witnesses, claimed that the "doctorboi" was the more reliable witness of the two, and that the man from Manus Island had his

own private reasons for denying the animal was a Ri. That is as far as our own inquiries reached in that particular case.

A visit was also made to the head of the Nakela clan, in the village of Pire. The Nakela hold the Ri as their masalai (tutelary spirit), as an animal distinct from the dugong (see Wagner 1982, above). The clan head related an incident, about 40 to 50 years ago, when a mother Ri was killed, and the infant was kept alive a short period before it died. He also stated that the Ri were caught more often in the "old days," before the introduction of modern nets. The old nets, he stated, had bamboo reinforcers and were stronger.

Besides interviewing, the team investigated the reef at Korapun, an area of Ramat Bay where the Ri supposedly entered shallow water at high tide. The villagers spoke of a small opening in the reef through which the Ri enter. One scenario called for the placing of a net at this opening to either trap an animal upon entering or, as the tide receded, upon leaving. An inspection of the passage was necessary, but was not immediately possible because of inclement weather and rough seas.

Another scenario called for a net to be used in one of the small rivers which flow into Ramat Bay. Some villagers talked of Ri entering the rivers at night to fish in very shallow water. Due to the lack of detailed information, this project was not attempted.

A few days after the team's arrival, a Balile villager died of an undetermined malady, and the communal "men's house" was consequently burned down. These were serious matters, which necessitated a postponement of overt expedition activities. Most local people, and the expedition members, participated in the subsequent funeral activities. This was followed the next day by a mortuary pig feast, which included lengthy Barok debates concerning protocol and inheritance.

Following these functions, and as the weather cleared, work at Korapun continued. The passage in the reef was located with the help of Kurus. It was, indeed, as the natives had described, just wide enough for a small marine mammal (or human, as we determined) to negotiate underwater. A 100-foot-long net was acquired in Namatanai, but difficulties arose in conceptualizing a method of installing it at the head of the passage; the continual battering of the waves would probably render any scheme ineffective, and possibly dangerous to any animal which might become entangled in the net.

In the meantime, the team was informed by a villager that Ri were being sighted almost daily at a village named Nokon, about 50 road miles to the south. With a lack of information on current sightings in Ramat Bay, the team proceeded south to Nokon, which is located on Elizabeth Bay, on the western side of Cape Matanatamberan (Fig. 1). This peninsula, the site of extensive military activity by Japanese forces during World War II, is primarily grassland, and it proved to be very difficult to negotiate in a vehicle.

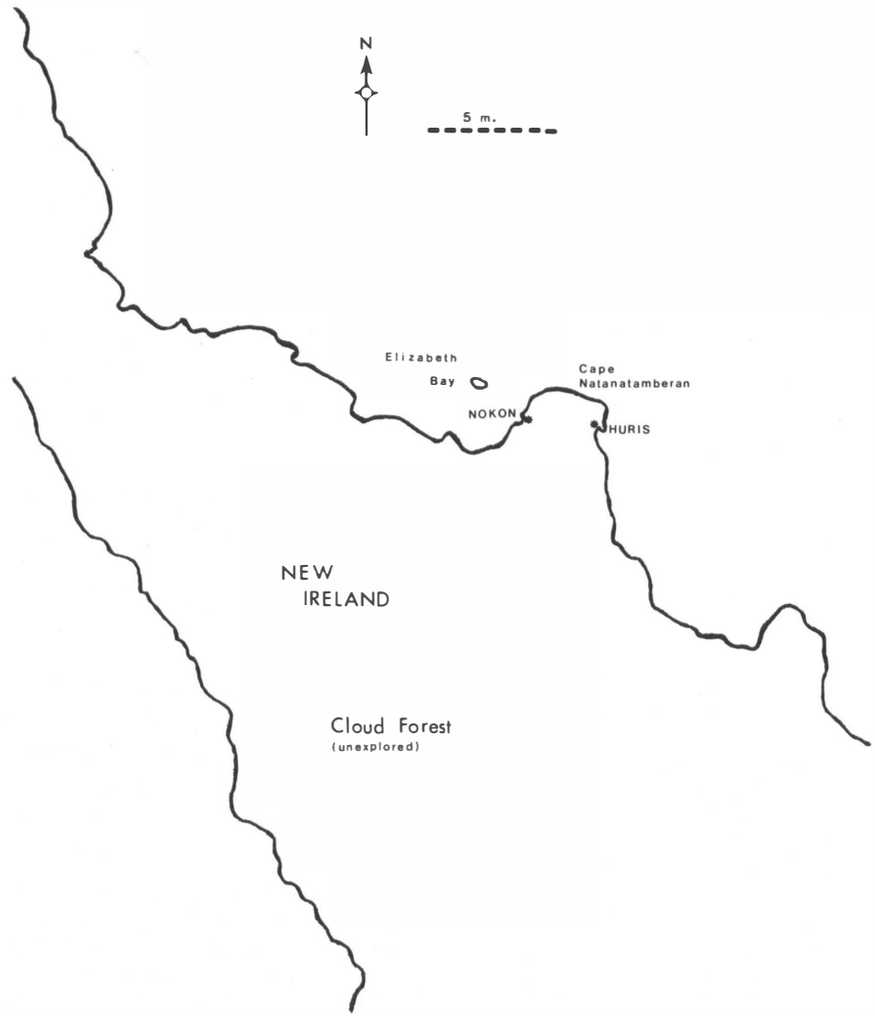

FIG. 1.—Location of Nokon, a Susurunga village, in Elizabeth Bay.

Nokon is populated by Susurunga people, and although they describe the same sort of animal as the Barok, they refer to it as Ilkai (the "k" here is a glottal consonant). The Ilkai (like the Ri) is described as having a human-like upper torso and head, with the eyes set to the front of the head. The mouth protrudes and is "peculiar." The arms are "fused" to the side of the body, and the pseudo-hands serve as flippers. The legs are fused, one across the other, terminating in flippers instead of feet, which provide aquatic propulsion. When pressed on the claim that they have eaten Ilkai, which

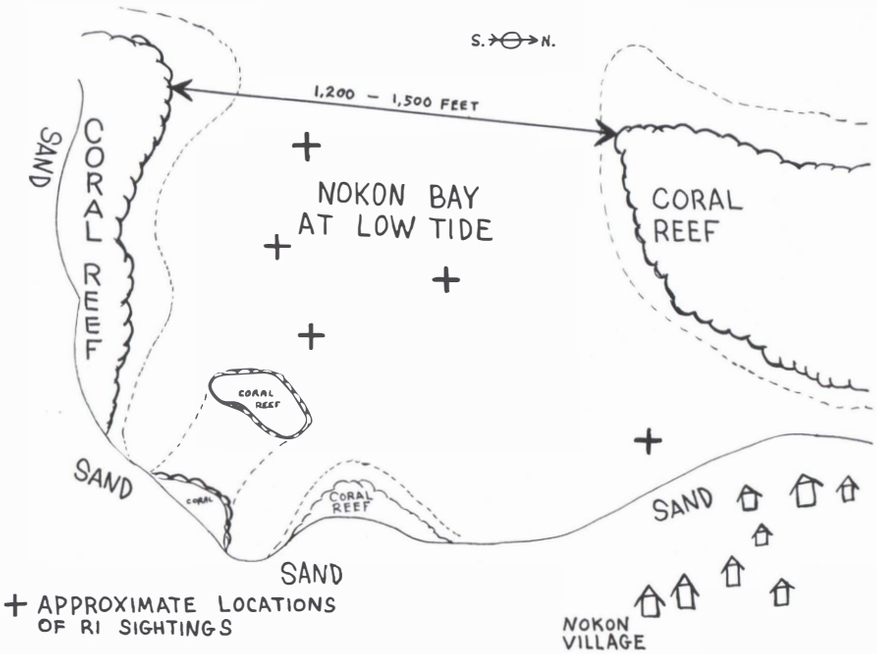

FIG. 2.—Field sketch of Nokon Bay at low tide, indicating the locations of Ri sightings.

they believe have human affinities, several villagers conceded that the Ilkai is really "not man," but "like man." We were informed that an Ilkai could be seen almost daily at dawn and dusk in their inlet (which we named Nokon Bay), so we decided to stay the night and observe early in the morning.

At dawn on July 5, 1983, we proceeded to the beach. Raymond stayed at the northern end of the bay (which is about 1,500 feet across) near the village, while Wagner and Greenwell walked down to the southern end of the bay and onto a reef, from which they surveyed the bay with binoculars (Fig. 2). Raymond almost immediately observed an unidentified animal enter shallow water about 100 feet from the village shore. It swam rapidly, breaking the surface with its back, but showed no dorsal fin. Its color was described by Raymond as light brown. Neither head nor tail was visible. Fish were seen to jump from the water nearby, as if avoiding capture. This observation lasted about 20 minutes, until the animal swam away. Because of the low light conditions and the speed of the animal, the photographs taken by Raymond show nothing of scientific interest.

About half an hour later, Wagner and Greenwell, who were still unaware of the sighting by Raymond, were called back from the reef to the southern end of the bay by some shouting boys. The Ilkai was then observed from

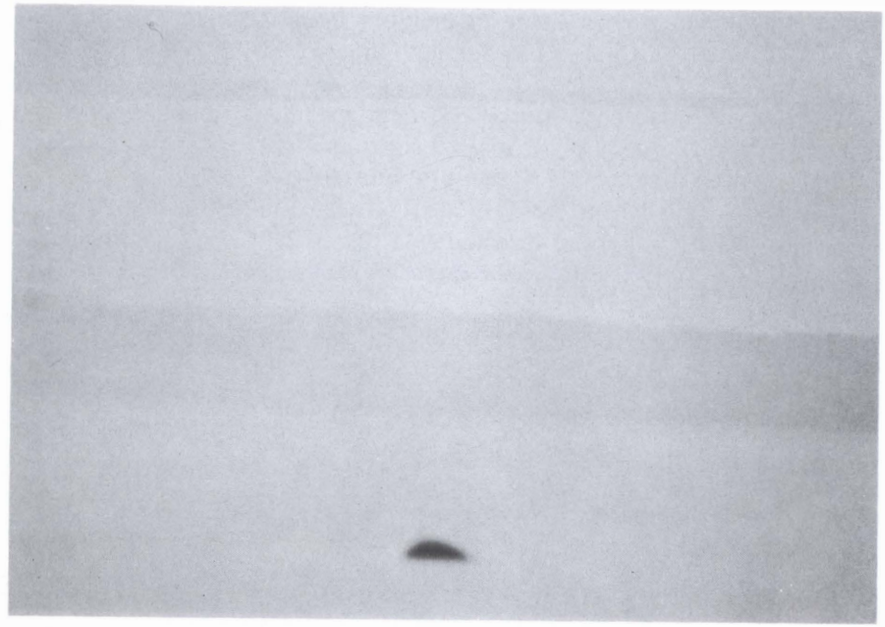

FIG. 3.—Photograph of the Ri, known locally as Ilkai, rolling at the surface. Surfacings occurred about every ten minutes, and lasted for less than two seconds.

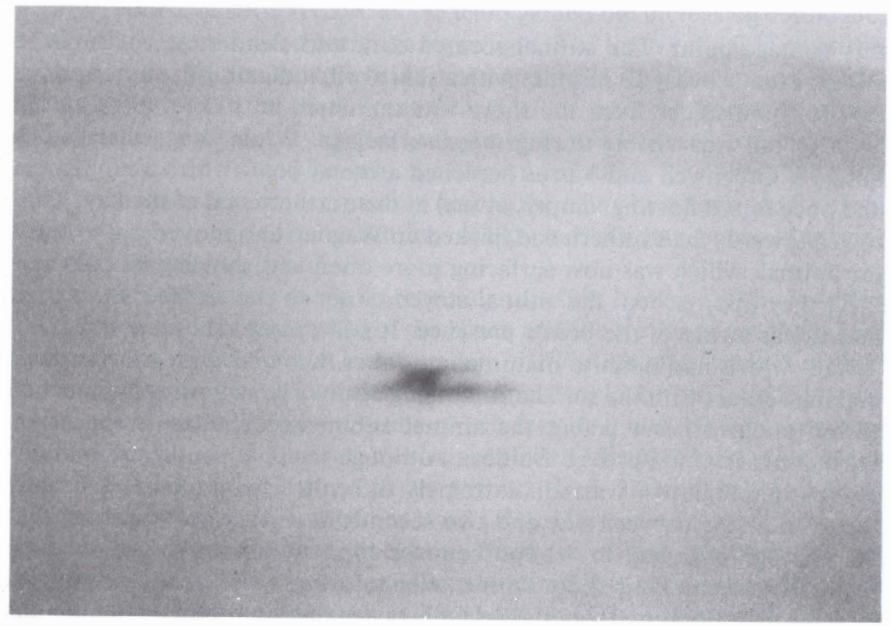

FIG. 4.—Photograph of the Ri's tail, which hovered above the water when approached by a boat.

FIG. 5.—Net placed in Nokon Bay in an attempt to capture a Ri, with no results.

this vantage point. The animal seemed dark and slender, and it surfaced almost exactly every 10 minutes with a sharp roll, indicating extreme vertical flexure. Its distance from the shore was estimated at 300 to 400 feet. No head or tail was visible during these surfacings. While Wagner stayed to observe, Greenwell and Kurus launched a metal boat (which the villagers had once found floating, empty, at sea) at the northern end of the bay. They rowed towards the southern end, picked up Wagner, and moved out towards the animal, which was now surfacing more often and showing its tail.

As they approached, the animal stayed closer to the surface. It was felt that it was aware of the boat's presence. It still did not show its head, but its tail, which had definite mammalian flukes, hovered above the surface, making contact with the surface, and rising again. The witnesses approached within about 50 feet before the animal submerged without reappearing. Further observation proved fruitless. Although the light conditions had improved, photography was still extremely difficult. The animal was usually only visible for between one and two seconds at the surface, meaning that the photographer had to "shoot from the hip." When on shore, the time required to frame and shoot with a telephoto lens was usually more than the time that the animal was visible. Only one of the shore-based photographs caught the animal rolling (Fig. 3). In the boat, the photographer had to

contend with the additional problem of small waves rocking the vessel. Nevertheless, an out-of-focus photograph of the tail was obtained (Fig. 4). No movie film was exposed.

After the sightings, the team members returned to Ramat Bay, where Von Nieda had arrived from California to join them. They then attended the long-awaited Barok sing sing, and returned to Nokon with more provisions and equipment. Despite many hours of vigil, no further significant sightings were made. It appeared that, while team members had positioned themselves at different parts of the bay at dawn and dusk with negative results, villagers were, in fact, observing the animal from time to time during their routine activities, without informing the team members. This occurred on at least two occasions, and served as a reflection of the casual, non-metaphysical attitude they hold toward the Ilkai, and the New Ireland cultural trait of not volunteering information that is not specifically requested.

On another occasion, two easily identifiable dolphins were observed entering the bay (with a behavior and visible morphology quite different from the Ilkai), and one of the team (Greenwell) approached some villagers and excitedly pointed to the cetaceans, exclaiming "Ilkai! Ilkai!" This was done purposefully to determine if the villagers would refer to any marine mammal as an Ilkai in order to please the Western visitor. The villagers simply stated that the animals were dolphins, not Ilkai, strengthening the hypothesis that they know the difference.

In the course of several days, as the team gained the increasing confidence of the villagers, new information came to light. The team was taken to Huris, a Susurunga village on the other side of the peninsula. An Ilkai had reportedly been caught and eaten there some years before, and the bones were supposedly still there. Huris villagers took the team to a large tree around which lay the bones of many pigs, which villagers consume. The Ilkai had once hung from a tree branch, but a search among the many bones proved fruitless. Other villagers reported daily sightings of Ilkai in Huris Bay, but no observing was done by the team.

An attempt was then made to capture an Ilkai in Nokon Bay by lowering a net between the northern end of the beach and the reef opposite the village (near the Raymond sighting location) (Fig. 5). The net was checked at night to ensure that any captured marine mammals could be safeguarded or released unharmed. The net was about 120 feet long and 10 feet wide, and was weighted down at one end by an unexploded Japanese land mine which the villagers occasionally use as an anchor. (New Ireland still has much Japanese military equipment from World War II scattered about its beaches and jungles.) Numerous fish, including three reef sharks, were caught, but no mammals. Snorkeling was conducted around the bay's reefs, and about the reef off an island 2 miles out in Elizabeth Bay. The variety and numbers of coral and fish species were remarkable, and marine productivity seemed

high. Other dolphins were also observed, but they were easily identifiable for what they were. Operations were then terminated at Nokon, and the team returned to Namatanai and Ramat Bay.

Upon returning to the Ramat Plantation, on July 12, 1983, two local Barok men appeared at the plantation house late in the afternoon, stating that a Ri was being observed very close to shore in the small inlet below. We immediately went down to observe, and two team members (Wagner and Greenwell) saw the animal rolling at the surface on the other side of the inlet (about 500 feet distant) on two separate occasions. It appeared to be the same kind of animal seen previously at Nokon. The roll observed by Wagner was in clear sunlight, and he described the animal's color as tan to light green. Further observing took place in the inlet for the two days prior to our departure, with no additional sightings.

Results

The expedition spent almost a full month visiting villages and communities in central-southern New Ireland, and succeeded in two of its objectives: 1) it gained further information on the Ri (or Ilkai) from the aboriginal peoples; and 2) it had direct visual contact with the Ri, or at least with an animal villagers identified as the Ri (or Ilkai), and was able to photograph (but not film) it. The expedition failed to locate tissue or osteological remains, and also failed to capture a live specimen.

Capturing a Ri alive would require considerable effort, and involve a substantial expenditure of funds. Before such a project is undertaken, there would have to be a fairly high probability that the animal involved is not a species already scientifically recognized. The descriptions given by the natives are in no way similar to known species of marine mammals. Nevertheless, the question arises as to whether they are reporting an *unknown* animal, even if the descriptions are embellished, or a *known* animal which the natives have completely metamorphosed into their own version of the mermaid. At this time, there is no way to conclusively answer this question.

As to the animal the team members saw, which the natives claim was a Ri (or Ilkai), not enough of the animal was seen to support or negate the native descriptions. Even so, the various marine mammalogists consulted have been unable to identify the animal seen and photographed. One of us (Greenwell) visited the Hawaii Laboratory of the Naval Ocean Systems Center soon after the sightings, and observed various marine mammals maintained there by the U.S. Navy; none resembled the New Ireland animal, nor were the descriptions or photographs immediately identifiable (Peter Schroeder, personal communication).

It has been speculated that the animal we saw was a dugong, but certain evidence makes this unlikely. The Indo-Pacific dugong (*Dugong dugon*), together with three species of manatee, in the Caribbean, the Amazon, and

FIG. 6.—Sketch of typical dugong roll at the surface, based on Anderson and Birtles (1978). Compare with typical Ri roll in Fig. 3.

West Africa, comprise the living order of sirenians (the only other recognized species being the Steller's sea cow, presumed extinct). The dugong is the only known species of herbivorous animal that lives exclusively in the sea (see George E. Heinsohn, 1972, A Study of Dugongs [*Dugong dugong* (sic)], in Northern Queensland, Australia, *Biological Conservation,* Vol. 4[3]: 205–13). The animal the team members saw, however, appeared to be a predator, although this was a subjective interpretation based on its locomotion and predator avoidance behavior by nearby fish.

Although faster than the manatee, the dugong is a relatively slow mover when compared, say, to cetaceans. The animal observed by the team, however, moved fairly rapidly. The dugong has a bulky body with a limited degree of vertebral vertical flexure. The animal we observed appeared to have a high degree of vertical flexure. Various typical dugong surfacing behaviors have been identified, the closest to what the team observed being a low-profile roll (length approximately 10 times the height), consistent with more limited vertical flexure (see Paul K. Anderson and Alastair Birtles, 1978, Behavior and Ecology of the Dugong, *Dugong dugon* [Sirenia]: Observations in Shoalwater and Cleveland Bays, Queensland, *Australian Wildlife Research,* Vol. 5: 1–23). This typical dugong roll (Fig. 6) may be compared with the high-profile roll consistently observed by the team (Fig. 3) (length less than four times the height).

Extensive fieldwork has shown that the average duration of submergence for the dugong is about a minute. In 204 timed dives over a month's period, the average duration was just over 65 seconds. The maximum average for 18 dives on one day was 126.1 seconds (see Anderson and Birtles 1978, above). The animal the team observed, however, stayed submerged for periods of about 10 minutes, consistently.

Paul Anderson has examined the photographs taken by the team, and states that there is nothing in the photos themselves that clearly rules out a dugong, although some of the behaviors observed are very un-dugong like (Paul Anderson, personal communication). He also provided one author (Greenwell) with super 8 mm movie film of dugongs surfacing. No morphological or behavioral similarities between the Nokon animal and the animals seen in the movie film (dugongs) were found. It was noted that, on occasion, a dugong would surface with a more pronounced vertical roll, but

this was not a consistent behavior, nor was it rapid. Anderson (personal communication) suggests that dugongs diving in deeper water (about 30 feet) may flex more on their surface rolls, and may also stay submerged for longer periods of time, possibilities which he is currently researching in Australia.

Nevertheless, the rapid movement of the animal, its consistently extended duration of submergence, its consistently extreme vertical flexure, and its apparent predatory behavior lead us to the conclusion that the animal observed and photographed at Nokon was not a dugong.

If known sirenians can be ruled out, and pinnipeds are not known in the northern Australasian region (the animal's morphology and behavior did not resemble a pinniped, either), the cetaceans are the only other candidates. Only two small cetaceans are finless, the right whale dolphin (*Lissodelphis*), represented by two species, and the finless porpoise (*Neophocaena phocaenoides*). The northern right whale dolphin (*L. borealis*) is found in the eastern north Pacific, as far south as California, usually travels in large schools of hundreds or thousands of individuals, and leaps out of the water, exposing all of its body. The lesser-known southern species (*L. peroni*) inhabits the Antarctic Ocean. The right whale dolphin can be eliminated from consideration as a candidate for the Ri (Forrest G. Wood, personal communication).

The only other small, finless cetacean, the finless porpoise (*Neophocaena*), is found in the Indian and Pacific Oceans, from southwest to southeast Asia, and it is conceivable that its range may extend as far as New Guinea and New Ireland. These animals inhabit mangrove estuaries, which is consistent with the New Ireland sighting location, and travel alone or in pairs (see Lyall Watson, 1981, *Sea Guide to Whales of the World,* E. P. Dutton, New York). The flukes, however, are reportedly never visible above the water (see Watson 1981, above), which would rule out this candidate, as do the diving times, which are similar to those of other small cetaceans and sirenians, but unlike those of the New Ireland animal. Intervals between blows are 10 to 20 seconds, with longer dives lasting 40 seconds. In one study, the average of 62 timed dives was 17.5 seconds, and the longest dive recorded was 65 seconds (see Zhou Kaiya, G. Pirelli, and Yuemen Li, 1979, Observations of the Baiji (*Lipotes vexillifer*) and the Finless Porpoise (*Neophocaena asiaeorientalis*) in the Changjiang (Yangtze) River between Nanjing and Taiyangzhou, with Remarks on Some Physiological Adaptations of the Baiji to Its Environment, *Investigations on Cetacea,* Vol. 10: 109–20).

Furthermore, according to published illustrations, this short but bulky animal (1.5 meters in length) has a limited vertical flexure capability, and is reportedly sluggish and slow-moving (see C. A. Gibson-Hall, 1950, The Whales, Porpoises, and Dolphins Known in Sarawak Waters, *Sarawak Museum Journal,* Vol. 5[2]: 288–96). After a review of the pertinent literature, the finless porpoise can also be eliminated as a possible candidate for the Ri (Forrest G. Wood, personal communication).

Having considered all possibilities, the authors have not been able to identify the Ri or Ilkai as part of the known inventory of zoology. None of the marine mammalogists consulted so far are convinced that the animal we observed and photographed is one they are acquainted with. We are therefore left with the tantalizing possibility that the animal we observed is indeed new to science. Whether or not it corresponds to the descriptions given by the Barok and Susurunga natives remains to be seen. But so far, they are the only people who *do* claim to know the animal.

Future Plans

Attempts to identify the Ri or Ilkai as an animal known to zoology, even one outside its normal geographic range, will continue. If such efforts are unsuccessful, a new expedition to conduct further field observations would be warranted. Such an expedition would have to include better equipment, such as a sonar unit, more sophisticated photographic equipment, and possibly a powered boat. Adequate funding would have to be made available. At the same time, due to the disruptive nature of such activities in the local villages, and the confidence which has to be gradually developed with the native people, equipment and personnel would have to be kept to a minimum.

In the meantime, numerous Barok and Susurunga villagers have been alerted to the potential importance of acquiring physical evidence, and have agreed to notify the authors or the proper local authorities if they obtain such evidence in the future.

Cryptozoology, 2, 1983, 126–131

LCPI WORK AT LAKE CHAMPLAIN, 1983

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INTRODUCTION

The Lake Champlain Phenomena Investigation (LCPI) continued its work in 1983 in an attempt to identify the community of unknown animals in Lake Champlain, most commonly referred to as "Champ." As in previous years (see Joseph W. Zarzynski, 1982, LCPI Work at Lake Champlain: 1982, *Cryptozoology,* Vol. 1: 73–77), the intent of the 1983 fieldwork and research was: (1) to maintain periodic field operations through surface surveillance with binoculars and cameras, and the employment of underwater surveys using sonar and scuba monitoring; (2) to proceed with documenting sightings of these "Loch Ness-like animals"; and (3) to pursue further efforts to ensure the passage of a legislative resolution that encourages protection for the Champ animals, and encourages scientific inquiry into their existence.

The known evidence on Champ can be outlined into several categories. Reports of large, unknown aquatic animals in Lake Champlain predate French explorer Samuel de Champlain's 1609 expedition upon the 109-mile-long lake. Although the belief in giant underwater serpents/dragons is almost universal in Indian mythology, several Indian legends, particularly Iroquois and Algonquin lore, reinforce a possible parallelism between this Indian mythology and Champ eyewitness accounts (see Joseph W. Zarzynski, 1983a, Champ—A Zoological Jigsaw Puzzle, *Adirondack Bits 'n' Pieces,* Vol. 1[1]). Besides Indian lore, there are Champ-related art and place names, eyewitness sightings of Champ, several purported Champ surface photographs, and some sonar evidence (see Zarzynski 1983a, above).

LCPI fieldwork in 1983 consisted of 29 days of camera, sonar, and scuba monitoring sessions. Exercises were coordinated by the author. M. Pat Meaney assisted during most of the 1983 sonar and scuba exercise. Further assistance was given by Jack Sullivan, Ted Straiton, Bruce Hallenbeck, Mike Connery, and Jean-Pierre Sylvestre, a visiting French cetacean researcher.

NARRATIVE DESCRIPTION

The 29 days of 1983 LCPI-sponsored field operations at Lake Champlain consisted primarily of monitoring with cameras and binoculars from shoreline and vessel vantage points. Camera equipment included: several 35 mm cameras, a super 8 mm movie camera, telephoto lenses, camera tripods, a long-range television video camera and recorder, and other miscellaneous photographic gear.

Five of these 29 days were devoted to the use of a modified Raytheon

FIG. 1.—The author operating a modified Raytheon DE725C sonar unit during the 1983 LCPI fieldwork.

DE725C sonar unit (Fig. 1), connected to a 4.5-foot-long camouflaged aluminum tripod (200 kHz). This was set into position on the lake's bottom by Zarzynski and Meaney by means of scuba diving. This sonar-rigged tripod has been put in action each year since 1980 for sonar data collection (see Zarzynski 1982, above). Sonar dates were August 22–26, 1983.

Lake surface surveillance was conducted at the following locations: Big Snake Bay, New York; Bulwagga Bay, New York; Panton Bay, Vermont; Button Bay, Vermont; Kimball Dock Pier, Vermont; and along other shoreline and lake sites. Vessel surveillance was done from an outboard motor-powered 13-foot-long Avon inflatable boat.

Lake surface surveillance, sonar, and scuba searches were conducted on the following dates: January 8 (consisted entirely of a search for the 1977 Mansi "Champ photograph" site); April 10 (photography and Mansi site search); April 26 (photography); April 27 (photography); May 21 (photography); May 28–30 (photography); June 25–July 2 (photography and scuba); July 25 (photography and Mansi site search); August 6 (photography); August 20–27 (photography, sonar, and scuba); September 4 (photography and Mansi site search); September 17 (photography); and September 18 (photography).

Further interviewing of eyewitnesses and other associated research was

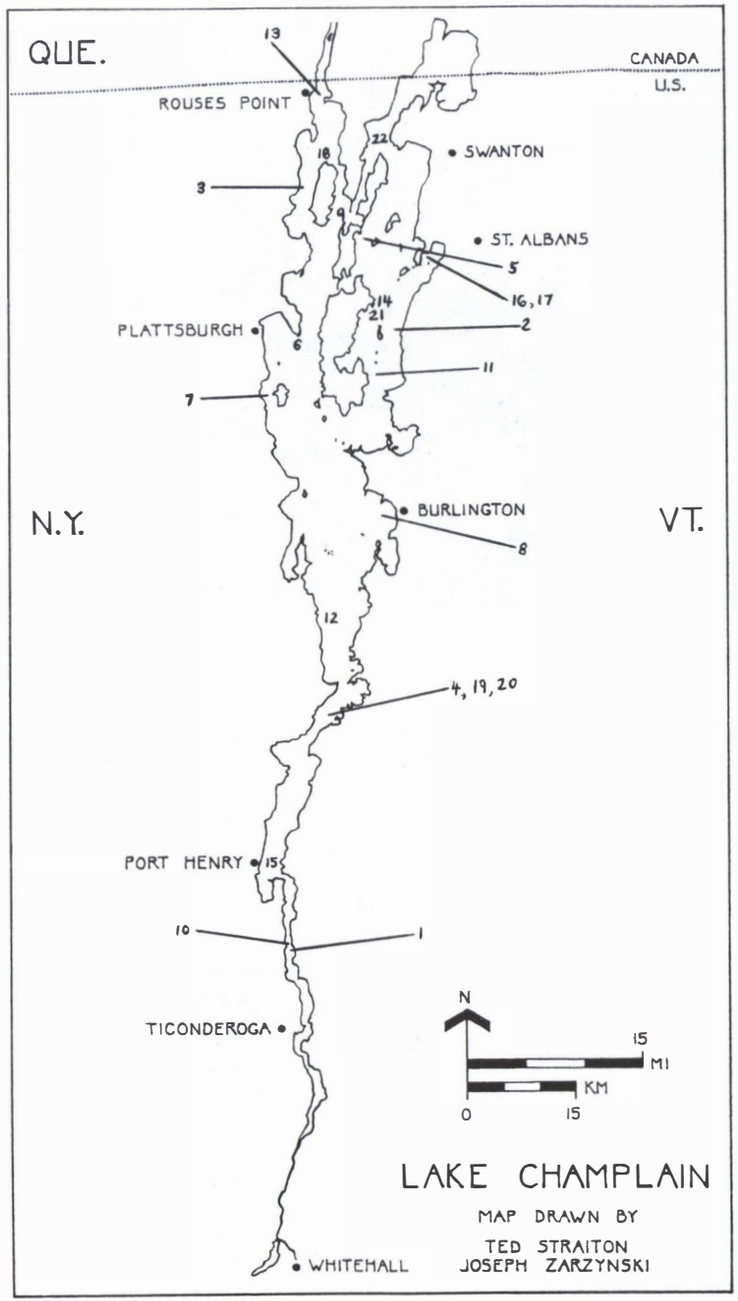

FIG. 2.—Map of Lake Champlain, with numbers indicating the locations of the 22 eyewitness sightings logged by the LCPI during 1983.

conducted throughout the year as part of the LCPI's continuing investigation. Ten scuba dives were made into Lake Champlain during 1983. Of these, seven were directly related to Champ reconnaissance and follow-up sonar deployment/retrieval. These scuba dives were made by Zarzynski and Meaney (June 27 and 30, August 21, 22, 23, 25, and 26).

On June 12, 1983, a Lake Champlain dive was conducted by Sullivan and Zarzynski from a 13-foot-long Boston Whaler boat. This dive was performed as an underwater survey for the State of Vermont's Division of Historic Preservation to conduct a reconnaissance of remains of a wooden vessel found in a bay on the Vermont side of Lake Champlain. This shipwreck had been accidentally located by Zarzynski and Meaney on July 3, 1982, when concluding a week-long cryptozoological expedition at Lake Champlain. A total of three dives in 1982–83 were made at the site by Zarzynski, Meaney, and Sullivan. A report was written and then filed with the State of Vermont, ISC, and other interested parties (see Joseph W. Zarzynski, 1983b, Report on Potash Bay, Lake Champlain Shipwreck-Reconnaissance Survey).

RESULTS

Field operations in 1983 did not result in visual surface sightings of unidentified animals. Sonar and scuba sessions did not result in possible Champ targets or physical remains.

Nonetheless, the LCPI did obtain a number of new reported Champ sightings, most of which took place in 1983. As of this writing, 22 sightings from 1983 have been collected by the LCPI (Fig. 2). In addition, four 1983 sightings await further documentation on the part of the witness(es). To date, a total of 198 sightings of Champ have been collected and catalogued by the LCPI. A brief summary of the 22 recorded sightings in 1983 is outlined below:

—May 3, 1983: Barbara Drinkwine, Theresa Drinkwine, and Ernest Carpenter; a head and humps off Crown Point, New York.

—May 28–30 (weekend of), 1983: Jane and Harry Atkinson; from their boat while halfway between Burton Island, Vermont, and the Sand Bar Causeway.

—June 13, 1983: Beverly Fraser; east of Chazy, New York; "two were present underwater . . . swimming at a very steady rate of around 3 mph."

—June 14, 1983: Dick L. Noel and Tim L. Noel; from a North Hero, Vermont, restaurant while having dinner; 20–25 feet long, dark or black, 3–4 feet high; head, neck, and two to three humps.

—June 15, 1983: Suzanne Starr, Greg Wildasin, and several others; off Fort Cassin—mouth of the Otter Creek, Vermont; four humps (dark); 30 to 40 feet long; "It could have been one large creature or four smaller ones."

—June 16, 1983: Robert and Kevin Algeri; off Cumberland Head—near Lighthouse, New York; Robert Alger believes he saw two animals, one behind the other.
—June 19, 1983: Joe and Toni Krupka; off of Peru Boat Dock—south of Plattsburgh, New York; three to four black ridges totaling 20–30 feet long.
—June 21, 1983: Jane Rowe and husband; three dark humps beyond the Burlington, Vermont, breakwater.
—July 2, 1983: Mr. and Mrs. Philip Jordan; 30 to 40 feet of "large black 'humps'"; at Rutland Railroad causeway, near old railroad bridge and southern point of land, North Hero, Vermont.
—July 2, 1983: Ronald S. Kermani and Susan Kopp; while fishing from a boat; two dark humps 1 mile north of Crown Point, New York, boat launch.
—July 7, 1983: 35 people; at YMCA Camp Greylock, South Hero, Vermont; two brown humps totaling 50 feet long off camp docks.
—July 14, 1983: Mrs. Jane Marsh, Jr., and Mrs. Kimball Prince; six humps totaling 20 feet between Elizabeth Point and Sandy Beach Point, south of Willsboro, New York.
—July 17, 1983: Betty Hebert and husband, with family and friends; two blackish humps off Rouses Point, New York; approximately 10–15 feet long.
—July 17, 1983: Steven and Norma Deforge and their two sons, Darren and Dana; 30 feet in length with a "head . . . like one of those plant-eating dinosaurs"; off South Hero, Vermont.
—August 6, 1983: Beatrice M. Jochum and Mabel R. Merrihew; a black creature, "about 40 feet" with a "head-three humps-tail," from their house near Hospital Creek, Vermont.
—August 10, 1983: Dr. E. James Swinyer, Avis Swinyer, and Irene Domina; St. Albans Bay, Vermont; vicinity on west side of Shantey Point opposite the cove on the east side of Butler's Island, Vermont; two–three dark humps.
—August 12, 1983: Dr. E. James Swinyer and Avis Swinyer; two "wakes" at same place as the August 10, 1983, sighting (Dr. Swinyer felt these may have been two animals).
—August 14, 1983: John W. Herbert and family, from their house on the north shore of Isle La Motte, Vermont; range 1 mile, using binoculars and a telescope; "rather like a submarine moving through the water with deck just awash" (Jennifer E. Herbert claimed to have seen the head of Champ using a telescope).
—August 16, 1983: Richard Alther and 13 other people; from Fort Cassin Point, Vermont; three to four humps, 40 to 50 feet long; vertically 3 feet out of the water.

—August 18, 1983: By several people from Richard Alther's camp at Fort Cassin Point, Vermont; several claim to have seen head and neck of animal.
—August 27, 1983: Mrs. Eva Gauvin and friend, off Camp Marycrest, South Hero, Vermont; 20 to 25 feet long; "small head followed by a long neck shot out of water, quickly followed by two humps" (Mrs. Gauvin thought that possibly a second animal appeared 100 yards away from the first).
—August 27, 1983: Graham Reynolds and two others; off east shore of Alburg, Vermont; 30 to 40 feet long; four humps.

One of the significant aspects of the 1983 sightings is that five reports chronicle the possibility of two or more animals being observed at the same time by the same witness(es).

Future Plans

The LCPI will continue its field operations at Lake Champlain, employing similar strategies as in previous expeditions. It is hoped that research and groundwork will serve to attract professional organizations to donate equipment and/or services toward the search for Champ. Furthermore, the LCPI will pursue more historical research, will continue public education activities, and shall strive to get the Vermont Senate and the Quebec authorities to pass the "Champ resolution," which was passed by the New York State Assembly on April 18, 1983. The resolution encourages further scientific inquiry into the phenomenon. This resolution had been passed in 1982 by the Vermont House of Representatives and the New York Senate.

Future research by the LCPI shall also be directed at a study of the possible food source(s) of Champ. With a more comprehensive data base from recorded sightings (dates, times, locations, weather and lake surface conditions, etc.), combined with an indication of Champ's possible food source(s), Champ's behavioral characteristics may become better understood. A more empirical approach, applying sonar, scuba and photography, may then be better utilized in search procedures. The data and experience from Champ research and fieldwork are providing a clearer perspective on when, where, and how to pursue Champ, and this may eventually lead to successfully locating and identifying the animals.

Book Reviews

Cryptozoology, 2, 1983, 132–146

Les Bêtes Humaines d'Afrique [The Human Beasts of Africa]. By Bernard Heuvelmans. Plon, Paris, 1980. 672 pp. 100 Fr.

Could large, unknown primates still lie hidden within Africa, land of big reserves and photo safaris? Bernard Heuvelmans answers in the affirmative with this lengthy book, which, nevertheless, talks more about what has been discovered than what is still unknown.

Firstly, the book is about African great apes and *Homo sapiens*. From this perspective, it is a history book: the history of relations between man and African apes from antiquity to their scientific discovery, and a history of the more complex relations between white Westerners and African negroids, bushmen, and pygmies. This zoological and anthropological history is particularly well written. It is clearly shown how Western man has made frequent practice, paradoxically, of anthropomorphizing all things belonging to apes, and dehumanizing unstudied peoples of the time—such as the pygmies—in such a way as to confuse chimpanzees, satyrs, and pygmies. The author also creates a delightful "short dictionary of different peoples of ancient Africa."

The book is remarkable for its references concerning the history of the discoveries of African apes. The author provides original information on their earliest sightings (by Valentin Ferdinand between 1495 and 1516), the first descriptions of baboon herds and chimpanzee nests (Richard Jobson in 1623), and, finally, the first observations of the different apes by Monboddo (1774). The story of vernacular names and binominal nomenclature, which lead to the current systematics of the living apes, is given with skillfulness.

As one expects of Heuvelmans, there are strange elements. The questions about the bonobo and the mountain gorilla are elucidated. The problems of an unknown species or sub-species of chimpanzee or pygmy gorilla (*koolokamba*) are nearly classics of primatology. But the supposed survival of Neanderthal Man in the ancient Maghreb (the *gorillaï* discovered by the Carthaginian Hannon), and the supposed present-day occurrence of australopithecines in the Congo basin (*kakundakari* and *kikomba*) and southern Africa (*tokoloshe*, a mythical figure like our goblin), are the most provocative (and truly cryptozoological) elements in the book.

The two questions lie in different fields. In the case of Hannon's *gorillaï*, only the various interpretations of his story can be evaluated. Even if there

are "Neanderthal-like" fossils (*erectus* or *sapiens*) in Morocco, where Hannon sailed (Heuvelmans' opinion), one must say that the gorilla-like aspect of fossil *Homo sapiens* (Neanderthal or not) is at least conjectural. On the other hand, some believe that Hannon reached Senegal and the Cameroons, and that the *gorillaï* were actually gorillas (an opinion refuted by Heuvelmans). Whatever the case may be, Heuvelmans' account is worth reading.

Concerning australopithecines in the Congo basin, one may note that nothing new has come from Zaire after the late 1950's–early 1960's, when C. Cordier followed the tracks of living apes which were supposedly not chimpanzees or gorillas. This chapter is rather short, being limited to 28 pages. A South African bushman rock painting less than 2,000 years old, published by D. N. Lee & H. C. Woodhouse in 1970, is very evocative, and shows tall and slim bushmen hunting robust and probably hairy creatures (the relict australopithecines?). We also learn that paleoanthropologist Phillip V. Tobias, now on the Board of Directors of the International Society of Cryptozoology, once told Heuvelmans that one of his colleagues had set traps to capture living australopithecines. I wonder what J. T. Robinson, now a member of the Editorial Board of *Cryptozoology*, has to say about this. Anyway, partly because of the author's own style, and partly because of his inclination to extract the possibility of mystery from the most academic and detailed bibliographic research, the book is not written as a paleoanthropological study.

It is a genuine pleasure to be repeatedly lost in the labyrinthic and swarming African folklore. From the perspective of cryptozoology, this unique work is an exhaustive set of documents. It is a major work. A last remark: the book's beautiful cover is taken from "La Pensée Lointaine," a painting by Alika Lindbergh.

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Alien Animals. By Janet and Colin Bord. Stackpole Books, Harrisburg, Pennsylvania, 1981. 258 pp. $12.95.

At first glance, this seems to be another compilation of reports of animals that have not been scientifically described, and to some extent it is. Indeed, in the introduction (p. xiii) the authors state: "It contains reports of un-

identified animals and of known animals where they should not be." A few lines later, however, they add: "In fact, much of the behaviour of both the identified and unidentified creatures is uncharacteristic of 'normal' animals." This view, that the subjects in question are not just unexpected, but in a separate category from all known animals, is developed throughout the book. It soon becomes clear that the authors do not think they are working within zoology, or even cryptozoology, but in an entirely different field of investigation.

The book concentrates on five groups of creatures, which would represent a broad taxonomic spectrum if they were considered relatives of the animals they seem to resemble most—reptiles, birds, dogs, cats, and hominoids. Despite this variety of reported alien animals, the authors note (p. 185) that "they possess features in common, which fact suggests that each creature is but one manifestation of a single phenomenon." The authors do not claim to have determined the exact nature of this phenomenon, but indicate that it may be associated with psychic activity, hidden energy sources, or unidentified flying objects (UFOs). Among the characteristics that are regularly reported, regardless of the over-all physical form of the creatures, are glowing eyes, the ability to suddenly appear and reappear, invulnerability to human weapons, and a connection with electrical and mechanical disturbances. Although this line of thought will seem ludicrous to many persons, the book is generally interesting, well organized, and apparently well researched and documented. It covers both folklore and modern reports up through the late 1970's. Much emphasis is on the British Isles, where a surprisingly large number of mysterious monsters have been reported, but many other parts of the world also receive comment.

Chapter 1, "Elusive Lake Monsters," is about the Loch Ness situation, and similar occurrences on five continents. Alleged recent sightings at Loch Morar in western Scotland, and along the coasts of Cornwall and Wales, are related in detail. Several photographs, which have not received wide distribution in North America, are shown. The authors state, however, that despite the numerous records of lake monsters, there are few good photographs, and a remarkably high rate of camera or film failure or loss. They suggest the following explanation (pp. 37–38): "*If* water monsters are some kind of psychic phenomenon rather than being solid, physical creatures, and *if* they are more readily seen by psychic individuals who somehow can tune in to the same wavelength on which a monster is appearing, then perhaps that person at the same time initiates some kind of involuntary telekinetic effect on the equipment he is operating . . . ."

Chapter 2, "Cats that Can't be Caught," deals mainly with a profusion of recent reports of large felines in Great Britain. The animals are said to resemble any of several big cats that do not occur naturally on the island, but are most commonly compared to the puma (*Felis concolor*) of North

and South America. One local police force compiled 362 reports between September, 1962, and August, 1964. Livestock has allegedly been attacked and plaster casts of tracks have been collected.

As I work with rare and endangered species, and have spent time gathering and reviewing reports of the nearly extinct subspecies of puma in eastern North America, this chapter has special significance. I had thought that numerous sightings of a large, long-tailed cat in an area where *Felis concolor* historically occurred would indicate likely survival of the species. Bord and Bord have shown, however, that reports of the puma, whether valid or not and regardless of the causative factor, can come in abundance from an area where the species has never lived in the wild. Although their suggested explanations may not be correct, it may now be more difficult to support the presence of true *Felis concolor* based only on sightings. A more recent book, *Cat Country,* by Di Francis, is devoted entirely to reports of large cats in Britain. According to a critical review by Nigel Easterbee (*Wildlife,* London, July, 1983, p. 264), the book suggests that the reports refer to a real, but as yet undescribed, felid species.

Chapter 3, "Mysterious Black Dogs," delves deeply into legend and the supernatural. The creatures in question reportedly differ from ordinary dogs in usually being larger, having glowing eyes, and being able to pass through solid objects. They are traditionally associated with witches and the devil, and are said to haunt certain well defined places, especially in the British Isles. The authors suggest that the continued manifestation of black dogs in such places may be governed by the flow of energy along "leys," the supposed alignments of certain ancient ruins, religious structures, and other key sites.

Chapter 4, "Giant Birds and Birdmen," covers four basic kinds of winged things: "enormous birds, usually of unknown species; weird unidentifiable creatures with wings; weird creatures with some human features and wings; and apparently human figures with wings." The account is chronological, ranging from the alleged carrying off of a child by a large raptor in Switzerland in 1838, to the appearance of the "Cornish Owlman" in 1978.

Chapter 5, "Man or Manimal," deals with the Sasquatch, the Yeti, and other "big hairy monsters," or "BHMs," that seem to have some resemblance to humans. Most of the chapter is concerned with North America, where there have been reports from many localities besides the Northwest, but there is also emphasis on Australia, where the local term for the creature is Yowie. The authors give particular attention to supposed cases in which a BHM actually attacked or captured a human, or vice versa. They deviate from the general trend of the book in suggesting that some BHMs may be descendants of extinct Pleistocene hominoids, but they also discuss notions that these creatures are mutant humans or the products of electromagnetic forces.

In their concluding chapter, the authors contend that available evidence

indicates that alien animals are generally not physical creatures, but that they may arbitrarily take physical forms and exist temporarily therein by feeding on energy. Their initial appearance could have a psychic or extra-terrestrial instigation. The particular form that is taken may be created in the mind of the observer. While many students of cryptozoology will be skeptical of the authors' views on the nature of unexpected animals, there should be wide agreement that the workings of the human mind can be highly relevant. The following lines (p. 215) might be a reasonable guide for others in the field: "We are convinced that in most cases witnesses do see what they report. Whether they are seeing what is really there is another question."

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A Geo-Bibliography of Anomalies: Primary Access to Observations of UFOs, Ghosts, and Other Mysterious Phenomena. By George M. Eberhart (Comp.). Greenwood Press, Westport, Connecticut, 1980. 1,115 pp. $59.95.

Cryptozoologists should welcome this remarkable guide to the literature on general anomalies. This massive compendium surveys over 22,100 separate alleged anomalous events grouped under 10,500 geographic place names throughout North America. The book is a milestone in anomalistics research and, along with William Corliss' Sourcebook Project volumes, transforms the so-far largely unsystematic collections of eccentric and extraordinary phenomena into a serious data base for those committed to a progressive scientific research program into anomalies.

Mr. Eberhart, a professional library scientist currently with the American Library Association, states that the purpose of this monumental volume (which is dedicated to the late Ivan T. Sanderson) is "to provide a detailed index-bibliography for all scientific and some historical anomalies, and to make an attempt at standardization of terms for anomalous events." Despite its size, Eberhart modestly acknowledges that the book "is only a first step in the process of organizing and consolidating the wealth of literature on anomalies that has blossomed in the past twenty years." He presents us at the front of the book with a list of the journal sources indexed, and these include the major Fortean publications, from *Doubt* to the *Fortean Times.*

But many other journals are covered (presumably less systematically), ranging from the *Journal of Occult Studies* to *Scientific American* and *Science.*

As indicated by the title, the volume is arranged by geographical location of the alleged anomaly event. Eberhart gives as his reasons for this arrangement that (a) he wishes "to underscore the fact that anomalies of many different types may occur in the same area, often in a relatively short period of time," and (b) because "of all six journalistic questions (who, what, why, when, where and how), *where* is the easiest of all to determine." One advantage to most users is that they can immediately look up their own geographical area and locate nearby anomaly events that have been reported. Each general geographic region is first divided into states, and then subdivided into (a) Populated Places, (b) Physical Features, (c) Ethnic Groups, and (d) Unspecified Localities. Many users of the book will find this arrangement rarely useful, but Eberhart has also provided us with special indexes by subject, by observer, by ethnic group, and even a special index by name of any ship involved in the reported event.

There is, of course, no attempt by Eberhart to evaluate the evidence for any of these claimed events, and the sources of the reports vary tremendously in terms of credibility. Undoubtedly, a certain proportion of these alleged anomalies are likely to be hoaxes; thus, the listing by "observer" might better be termed "a listing by source of the report."

A major difficulty most users will encounter is trying to obtain a copy of the anomaly article after they locate the listing by Eberhart. Many of the journals are difficult to obtain (though addresses are given for many Fortean journals, and a few are available through regular library channels), and the older newspaper items from small newspapers around the country will present even greater problems. To that degree, this book will produce a great deal of frustration as well as enlightenment for many of us.

It seems likely that most users of this volume will approach it with a desire to look up a subject event rather than a geographical anomaly area. Using the subject index necessitates substantial flipping about in the book, and since the subjects are indexed by page number rather than by item number, the whole page must be examined in order to locate the exact item sought. In addition, the subject categories may present some problems for the novice. For example, nothing is listed at all under the expected subject headings "Bigfoot" or "Sasquatch," but relevant items can be found under "Humanoid" and "Humanoid Tracks." Thus, the interested cryptozoologist will need to carefully look over the subject index for key areas. Hopefully, the subject index can be somewhat expanded in the next edition. Other index subject headings of interest to cryptozoologists will be "Lake Monster," "River Monster," "Sea Monster," and "Mystery Animal."

Another highly useful feature that I would hope might be added to any future edition would be an index by publication source. This would prove

most useful, and not only for looking up anomalies: it would give some indication of report frequencies in such sources, and would also give users a better (and quick) idea of what sources have been included systematically.

These minor reservations aside, this monumental index is highly welcome, and should be in the library of all serious anomalists. Cryptozoologists can look forward to Eberhart's more specialized new compendium scheduled for 1983, and entitled *Monsters: Including Bigfoot, Many Water Monsters, and Other Irregular Animals.*

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Incredible Life: A Handbook of Biological Mysteries. By William R. Corliss (Comp.). The Sourcebook Project, P.O. Box 107, Glen Arm, Maryland, 1981. 1,018 pp. $22.50.

One often hears from students of scientific anomalies how difficult it is to publish papers on "anomalistics" in the established scientific journals. Perhaps this complaint has some validity. Nevertheless, it is amazing to observe how many scientific journals and popular science magazines *have* published reports on a wide range of biological curiosities, many of which are generally considered to exist, or subsist, outside the mainstream of science.

Incredible Life, an enormous volume of over 1,000 pages, has brought most of these published reports, at least in the English language, together under one cover. In a sense, the author has done our homework for us, and the researcher seeking literature on a particular topic can save hours, even days, of library work by simply consulting this volume.

The author did not undertake this compilation on a whim. Indeed, the book is merely part of a whole series of such "anomaly compilations" from scientific literature sponsored by the author's own Anomaly Data Research Center. The other volumes to date, all published as part of the Sourcebook Project, are *Unknown Earth: A Handbook of Geological Enigmas; Mysterious Universe: A Handbook of Astronomical Anomalies; Ancient Man: A Handbook of Puzzling Artifacts; The Unfathomed Mind: A Handbook of Unusual Mental Phenomena;* and *Handbook of Unusual Natural Phenomena* (the latter deals more with geophysical and meteorological phenomena). These volumes average about 700 pages each, the shortest one being 542

pages. Together with the volume being reviewed, they can provide the reader with an almost endless source of both bibliographic information and just plain enjoyment.

The present volume contains twelve massive chapters, each with its own table of contents, and each dealing with separate kinds of animals, from *Homo* and relatives to microorganisms and general life processes (yes, for some reason not made clear, the author follows a reverse phylogenetic order). Each chapter, in turn, is subdivided into sections: Morphological Phenomena, Unusual Physical Abilities, Curiosities of Behavior, Biochemical Anomalies, Anomalous Distribution in Time and Space, and Unrecognized Species. The latter two, of course, deal directly with cryptozoology, although the word is never used. Within each of these sections, we find specific anomaly headings, under which come the actual titles and texts taken (or abstracted) from scientific journals such as *Science, Nature, American Anthropologist, Evolution,* and *American Scientist,* and popular science magazines such as *Science News Letter, Scientific American,* and *Natural History.*

It is not possible here to list or even summarize the 309 anomaly headings (much less the almost 800 articles/abstracts) in the volume, so I will highlight just a few interesting ones from each chapter. In Chapter One (Humans), we find headings titled "Caudal Appendage in Man," "Reports of Human Luminosity," and "The Voluntary Erection of Hair." Chapter Two (Mammals) contains: "Unusual Use of Tools," "Sleep Curiosities," and "Mammal Behavior and Astronomy." Chapter Three (Birds): "Effects of Electricity and Magnetism" and "Imitative Powers of Birds." Chapter Four (Reptiles and Amphibians): "Extraretinal Light Detection" and "Pretense of Death." Chapter Five (Fish): "Fish With Unusual Appendages" and "Mouthless Fish." Chapter Six (Arthropods): "Artifacts and Engineering Feats" and "Flying Crustaceans." Chapter Seven (Animals Without Skeletons): "Unexplained Mass Deaths" and "Power of Flight in Molluscs." Chapter Eight (Plants): "Electrical Plants?" and "Plant Behavior and Astronomy." Chapter Nine (Microorganisms and Cells): "Unexpected Radiations From Cells" and "Learning in Microorganisms." Chapter Ten (Genetics and Heredity): "The Problem of Excess DNA." Chapter Eleven (Life Chemistry): "Biochemical Clocks." Chapter Twelve (General Life Processes): "Game Theory and Evolution."

Looking further under these anomaly headings (and almost 300 others!) we find the actual articles or abstracts. Under "Hedgehog Idiosyncrasies" (Mammals), we find several articles published in *New Scientist* in the 1960's on how these little European animals (*Erinaceus*) are sometimes seen running in circles. Under "Collective Action" (Mammals) we find a 1970 note in *Nature* on how rats are attacking *cats* in Sicily. Under "Animals in Rocks" (Reptiles and Amphibians), we find a number of articles from the 1800's on frogs and toads reportedly being found alive in rock or stone. (These appeared

in the *American Journal of Science,* the *Zoologist,* and *Scientific American.*) And under "Neuroses and Inhibitors" (Arthropods), we find a 1948 *Nature* article claiming neurotic-type behavior in ants during laboratory maze runs. And so it goes on, curiosity after curiosity.

Of particular interest to cryptozoologists will be the nine articles on the Sasquatch/Yeti, etc., most, surprisingly, from *Science* and *Nature*; the 37 articles on mammalian mysteries, such as the possible survival of mammoths, Nessie, and other assorted matters; the eight articles on birds, including reports of living moas; the 25 articles on "sea serpents," giant snakes, etc.; the mere two articles on fish; and a decreasing number as one continues to move down the phylogenetic scale.

It is not altogether clear why Corliss placed the articles on Nessie in the chapter on mammals, while the articles on "sea serpents" (including one on British Columbia's Ogopogo, which, like Nessie, is a "lake monster") are placed in the chapter on reptiles and amphibians. In the first place, there are those who think that both "lake monsters" and "sea serpents" represent essentially the same zoological animal. Secondly, there are those who might have done exactly the opposite of what the author did, by placing "sea serpents" under Mammals and Nessie under Reptiles and Amphibians.

Other, more serious problems with the book are 1) the lack of a definition of what exactly an anomaly is, and 2) the criteria used in the article selection process. There are many articles which, although interesting and highly relevant to their own fields of science, are not (or should not be) considered to deal with anomalies—at least they were not by the authors, or by the readers of the original journals in which they appeared. An article on rapid speciation by John Briggs in *Evolution,* for example, can hardly be considered as dealing with anomalies (unless one considers *all* aspects of evolutionary biology as anomalous!). Likewise, an article by my associate Jim King, titled "Evolutionary Changes in Primate Sensory Capacities," merely deals with the different visual and auditory capacities of monkeys and apes, as tested in laboratories, and King well wonders why his paper was included.

Corliss himself seems to foresee these kinds of criticisms when he writes (not altogether convincingly) in his Preface: "the . . . collection of phenomena and curiosities is based upon personal feelings and choices, and . . . some readers will doubtless find some items rather routine. My criteria for selecting 'anomalous' material were: (1) the information contradicted current biological theories, or (2) the article in question raised personal questions not answered adequately to my knowledge." Corliss also states that ". . . the secondary objectives of the book are the posing of challenges to establishment science and the stimulation of useful controversy." In these respects, the book is highly successful. Nowhere, of course, does Corliss claim to support all the findings and allegations in the articles he reproduces, although he

expresses the hope that hoaxes and errors have been minimized by the screening process of the editors of the original journals themselves.

One more point, which the author himself raises. *Incredible Life* contains a substantial number of published articles contradicting evolutionary theory, which many will find interesting. These are not necessarily pro-Creationist, however. They deal more with Lamarckian genetics, challenges to recapitulation theory, and what is today becoming accepted under the new name of punctuated equilibrium.

The book has a modest subject index, but unfortunately lacks an author index, making the locating of articles by specific authors difficult. Most readers, however, will want to look up the anomalies themselves to see what has been written about them, and by whom. And what a remarkable compendium of anomalies it is!

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Topsell's Histories of Beasts. By Malcolm South (Ed.). Nelson-Hall, Chicago, 1981. 185 pp. $18.95.

Topsell's "Histories of Beasts"—that is, the original *Historie of Foure-footed Beastes* (1607) and *Historie of Serpents* (1608)—are of particular interest in that they stand at the end of series of works of encyclopedic scope which endeavored to record all available knowledge of the animal kingdom and, unlike their predecessors, are in English. These compilations, in a tradition traceable back to Herodotus (Fifth Century B.C.), included not only a description of the physical appearance of each animal, as understood by the author, but also a discussion of its "nature" or temperament, and especially its economic (often medicinal) use to man.

After Aristotle (*d.* 322 B.C.), little science was added for centuries, but the socio-economic additions proliferated. The *Physiologus,* the origins of which seem to have been in Alexandria during the Second Century A.D., was the basis of a wealth of bestiaries, in which each animal became an emblem of some ethical or religious truth. These animal stereotypes need to be understood before the symbolism of medieval art can be fully appreciated, and Topsell is a good key to them. In the later Middle Ages, the wonders told by returning travellers gradually supplemented the Christian interpretation of the animal world, but, despite the addition of such exotica

as the su, the lamia, the camelopardal, and the phoenix, the fox remained forever cunning, the bee industrious, the lion strong and courageous, and the ass and the goose object lessons in stupidity.

The most assiduous animal encyclopedist of all was Conrad Gessner, whose massive and partly posthumous five-volume *Historia Animalium* (1551–1587) was an amalgam of Sixteenth-Century zoological knowledge and belief, whence the writers of the literature of western Europe drew their ideas about animals. Although Topsell's works, from their dates, are not the direct source from which the Elizabethan authors received their information, their importance to students of the history of zoological thought is that they were explicitly based on Gessner's work (indeed, almost all of Topsell's illustrations are either taken directly from Gessner or from the same sources as those used by Gessner), and therefore made them available to the English-speaking world. Moreover, Topsell endeavored to be even more exhaustive than Gessner, so he leaves us a very full record of the state of general knowledge of the animal world at the beginning of the Seventeenth Century.

Gessner and Topsell had both attempted to be "scientific," separating truth from fable, but much was included that might seem highly conjectural. We must remember, however, that Edward Topsell was a clergyman, for whom disbelief in the unicorn would have been an act of impiety, as Psalm 92 lends it a scriptural certainty, and as its horn (a sovereign protection from poisons of all kinds) was an accepted ingredient of the materia medica of the time, there was little reason for Topsell to doubt its existence. Even as late as 1801, the *Monthly Review* carried a discussion entitled "On the Probability of the Existence of a Unicorn"!

A second (virtually identical) edition of both *Histories* (together with Moffat's *Theatrum Insectorum*) was published in 1658, and as recently as 1967 an edition of those three volumes was published by F. Cass, of London, at £125, indicating the continuing demand for Topsell as a work of reference, the original copies of which may be difficult of access to students outside the main centers of learning. In 1926, an abridgement of Topsell's *Histories* (supplemented by a few additions from Holland's [1601] translation of Pliny's *Natural History*) was edited by Muriel St. Clare Byrne, and published by Etchells & MacDonald under the title *The Elizabethan Zoo.* As this has recently been republished in the U.S.A. (1979) by Godine, of Boston (Nonpareil Books, $17.50; paperback $7.95), it will serve as an interesting comparison with Malcolm South's new abridgement being reviewed here.

As Topsell's *Histories* comprise 1,073 folio pages, it is obvious that much must be lost in South's work of 185 pages (and Byrne's of 171 pages). Many animals are excised altogether; of 130 general headings in the original *Historie of Foure-footed Beastes,* only 18 survive the abridgement. There is no beaver, no bison or bull, all deer are absent, and there is neither goat nor gorgon (a mixture of gnu and basilisk with a death-dealing eye and a covering

of scales and wings). South omits the hare, so we miss the speculation whether male hares bear young like females, and we are spared Topsell's three pages of horrendous recipes for various hare-derived medicaments. For example, he informs the reader: ". . . whereas it was no small honour to virgins in ancient time, to have their brestes continually stand out, every one was prescribed to drink in wine or such other thinges, nine graines of hares dung: the same drunke in wine at the Evening staieth coughing in the night . . ." (p. 276). All 155 pages of the "horsse" are lost, but much of that, as noted by Byrne, is "frankly tedious, except perhaps to a horse doctor." South includes no mole or mouse, no porcupine casting its quills, no sheep, and no swine.

Of about 65 sorts of "serpents" noted by Topsell only four (the asp, the cockatrice, the crocodile, and the dragon) have been chosen for inclusion by South. To Topsell, anything that crawled was a serpent, and this part of the *Histories* contained not only snakes but such assorted fauna as frogs, sea-serpents, spiders and scorpions, bees, wasps, caterpillars and earthworms. Byrne's total of Topsell's animals in *The Elizabethan Zoo* is only two more than South's 22, and it is informative to compare the selections made by each author. Only the camel, cat, cockatrice, crocodile, dog, dragon, elephant, hyena, lamia, lion, mantichora, rhinoceros, and unicorn are common to both works, though Byrne includes only the "mimicke dogge" section of the long chapter on the dog. South has the ape, asp, bear, camelopardal, fox, gulon, panther, tiger and wolf, which are not in the *Zoo,* while the latter finds room for the antelope, beaver, bison, boas, vulgar bugill, gorgon, hydra, ibex, lynx, salamander and su, unmentioned by South.

Within each chapter, considerable boiling down has been done by each author, but South has taken more liberties with the text, paraphrasing certain passages, and modernizing the spelling. This has the unfortunate effect of actually making the text *less* easy to read, as the spelling is no longer harmonious with the style. For example, one may compare, in the cockatrice chapter, South's "they did hang up the skin stuffed" with Byrne's "they did hang uppe the skinne thereof stuffed." Byrne has seven pages of notes on the text, which not only give useful information about unfamiliar words or place names, but indicate what proportion of Topsell's text has been used, and the source of her illustrations. It is somewhat surprising that some of the well-known attributes of the included animals are omitted from her text, for example the crocodile's tears and the elephant's hatred of mice (included by South, although his chapter on the elephant is four pages shorter than Byrne's, which itself is only a quarter of the original).

Both authors illustrate their text with copies of the original woodcuts but, whereas those in *The Elizabethan Zoo* are effectively placed as appropriate chapter headings, those in South's edition are isolated on pages facing each chapter, often occupying less than half the available space, and are of uneven

quality according to their reduction or enlargement from the original. More space is wasted by the same illustration—either reduced, or a detail from it—being used as a tailpiece to the chapter, often on an otherwise blank page. Thus, of six pages available for the camel, only 3½ are actually used for text (whereas Byrne's text occupies 4¼ of her five pages). South expends four pages on "The Epistle Dedicatory" (greatly abridged, and with much of its flavor lost), and another on an Epilogue, neither really relevant to this edition. Surely these pages would have been better used for more of Topsell's animal lore, or for some scholarly notes on the origins and influence of Topsell's works. The typeface chosen by South's publishers is a pedestrian 11/13 point Garamond, in contrast to the 16 point Poliphilus type chosen for *The Elizabethan Zoo*—a typeface itself based on Fifteenth Century models—which gives the printed page a period charm totally in keeping with the text.

Shakespeare (3 *Henry VI* III.ii.161) used the simile "Like to a chaos, or an unlick'd bearwhelp." Topsell discussed this prevailing opinion that a new-born bear-cub was formless before being literally licked into shape (an expression still in daily use) by its parents, and dismissed the idea as fallacy. As an indication of South's method of adapting Topsell's text, the relevant section from the chapter on the bear is quoted, first in the modern version, and then in the original. The reader may then directly judge which he or she prefers.

> It has been written that the young ones are littered without all form and fashion and are nothing but a little congealed blood like a lump of flesh. It has also been written that the old one frames the young ones to her own likeness with her tongue. These beliefs are false. Yet it is true that the young are littered blind and without hair, that their hinder legs are not perfect, that the forefeet are folded up like a fist, and that other members are deformed by reason of the immoderate humor or moistness in them (South, pp. 22–23).

> And whereas it hath beene beleeved and received, that the whelpes of bears at their first littering are without all forme and fashion, and nothing but a little congealed blood like a lump of flesh, which afterwarde the old one frameth with her tongue to her owne likeness, as Pliny . . . and Ovid have reported, yet is the truth most evidently otherwise, as by the eye witnes of Joachimus Rhetichus, and other, is disproved: onlie it is littered blind without eies, naked without haire, and the hinder legs not perfect, the forefeet folded up like a fist, and other members deformed by reason of the immoderate humor or moystnes in them which also is one cause, why the womb of the beare cannot retaine the seed to the perfection of her young ones (Topsell, p. 37).

In conclusion, while South's new abridgement is to be welcomed for bringing more of Topsell's work to the attention of the modern reader, a feeling of frustration persists as so much opportunity to do justice to the subject has been lost.

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The Magic Zoo: The Natural History of Fabulous Animals. By Peter Costello. St. Martin's Press, New York, 1979. 222 pp. $8.95.

The Magic Zoo is a highly readable examination of possible real-world origins for twenty fabulous beasts, some well-known, such as the unicorn, the dragon, the mermaid and the phoenix, and many other lesser-known, such as the griffin, the manticora, the barnacle goose and the vegetable lamb. "Sirens, centaurs and satyrs" of the classical world, and the Biblical leviathan and behemoth are included. Most of the "monsters" are culled from those which commonly appeared in bestiaries—natural histories of animals—popular in the Middle Ages.

These twenty chapters, together with three short ones titled "Heraldic Creatures," "Literary Beasts," and "Modern Monsters," make up the body of the book as Part Two. Parts One and Three deal with "Animals and Man" and "Man and Animals" respectively: one chapter in Part One traces the sundering of urban man's experience with wild animals, and the importance of mystery and magic in man's relating to animals. Another chapter shows the rise of zoology as a modern science, and the growth of a skeptical view of monsters from the 1600's onward. With great admiration for fabulous beasts, wandering in the "Serengeti of the mind," Costello in passing offers a corrective to the wholesale dismissal of monsters by zoology. In the third chapter, he reviews the young science of cryptozoology by tracing the contributions of particular individuals. Using the example of the unicorn, the single chapter in Part Three investigates the potency of animals as symbols.

The book is a gem with several sparkling facets. For the investigative reader, it has a long and varied list of references for each chapter, and an index of authors and beasts. It is illustrated with line drawings in each chapter, and sixteen black-and-white photographic plates. More importantly, the author succeeds in his primary mission, which is to discover feasible and convincing explanations from the real world for most of the fabulous beasts he considers, and some of their salient and zoologically amazing characteristics (such as the unicorn's horn, and the salamander's ability to walk through fire). The author has come up with a startling set of explanations, ranging from the surgical practices of African tribes in the case of the unicorn, to avian ethology in the case of the phoenix, from details of Tibetan geology in the case of salamander wool, to simple misidentifications of now known, non-European animals in most cases.

However, even more arresting to this reviewer are the histories of the animals in Western thinking. These are drawn in some detail, with numerous fascinating annotations from writers of the last two thousand years. It becomes vividly apparent how credible were some of the monsters of the Middle Ages; and several arguments of the demythifiers are examined. (The author himself specifically denies that *The Magic Zoo* debunks the beasts, claiming that "even such efforts at reductionism as this book have not

dissipated [the monsters'] mystery.") The reader comes to realize how basic were non-European animals to many members of the bestiary—the tiger, the alligator, the hippopotamus, the cobra and the gnu. Thus, one of the main pleasures of the book is as much the deft tracing of ideas and reports through the ages as it is the knowledge of which real African animal lies behind the catoblepas, or how a report by Marco Polo was misrepresented, giving rise to the myth of the giant ants of India which could lead men to gold.

Although each chapter is satisfyingly organized, I would have liked something of an orientational overview, no matter how brief, because so many "monsters" pass in front of our eyes. One is left wondering how animals from the bestiary were chosen as the basis for this book; why were some, and not other, famous beasts chosen? Were they the most amenable to analysis, or the biggest, or the most bizarre, or the most famous? Similarly, one wonders why certain famous monsters, generally the most "modern," such as Nessie, the Yeti and "sea serpents," are given short shrift, and where the "philosophical" monsters, such as Pegasus, fit in. Although all of Costello's arguments and explanations are indeed possible, I had a vague feeling that some alternative arguments and views were left unexplored. I felt a little shortchanged, considering the author's patently wide knowledge of the subject, and wanted to know much more about the supposed Neanderthaloid *Homo pongoides,* for example, than the brief mention and tantalizing photographs given. I also thought he might have treated us to more speculation about animals for which he has less convincing real-world explanations.

Aristotle, Pliny, and Herodotus are much quoted. One of Aristotle's epigrammatic phrases, not quoted by Costello, could have acted as a leitmotif for the book. He said: "People marvel at what is far away," because, he suggests, "to marvel is pleasant."

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Comments and Responses

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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 International Society of Cryptozoology or the Editor of* Cryptozoology.

PARATAXA AND HYPOTHETICAL CONCEPTS—THEIR IRRELEVANCE TO CRYPTOZOOLOGY

(Comment on Heuvelmans, *Cryptozoology,* Vol. 1: 1–12)

In his introductory article "What Is Cryptozoology?" Bernard Heuvelmans draws a number of interesting and valid parallels between cryptozoology and paleontology. His argument for the recognition of cryptozoological data as a basis for a tentative classification of unknown animals is, however, seriously flawed by his confusion between taxa, parataxa, and species. He implies criticism of the International Code of Zoological Nomenclature for failing to provide for the scientific description of animals the existence of which is conjectural, but this criticism is largely based on a number of misunderstandings. As Heuvelmans' information appears to be derived only from secondary sources, such as George Gaylord Simpson's 1961 *Principles of Animal Taxonomy*—and even that is misleadingly quoted out of context—I should like to correct his inaccuracies concerning the Zoological Code and its application to cryptozoological taxa.

Under the heading "The Necessity of Parataxa," Heuvelmans refers to the proposals by Moore and Sylvester-Bradley for a separate system of classification to be created for parataxa. This matter has recently been reconsidered by the International Commission on Zoological Nomenclature in the course of a thorough revision of the Code, and new decisions have been made which clarify the status of animal tracks and traces. It may now be demonstrated that, far from there being a "necessity for parataxa" in a tentative classification of cryptozoological entities, the concept is, in fact, irrelevant to problems in that field, and no special provisions of the Code

are necessary for the scientific study of such species. If there is sufficient evidence to permit the description of a cryptozoological species, a named taxon can legitimately be established on that evidence, and a classification of that taxon into the scheme of known animals can be attempted. The systematic position of any animal species is liable to change with the discovery of additional information, but this does not negate the original evaluation of the data then available.

The publication of the general proposals of Moore and Sylvester-Bradley in 1957 (see Raymond C. Moore and Peter C. Sylvester-Bradley, 1957, Suggested New Article: Proposed Recognition of the Concept "Parataxa" and the Provision of Rules for the Nomenclature of Units of this Category, *Bulletin of Zoological Nomenclature,* Vol. 15: 5–13; 1957, First Supplemental Application: Application for a Ruling of the International Commission Directing that the Classification and Nomenclature of Discrete Conodonts be in Terms of "Parataxa," *Bulletin of Zoological Nomenclature,* Vol. 15: 14–34; 1957, Second Supplemental Application: Application for a Ruling by the International Commission Directing that the Classification and Nomenclature of Ammonoid Aptychi (Class Cephalopoda) be in Terms of "Parataxa," *Bulletin of Zoological Nomenclature,* Vol. 15: 35–70), together with detailed supplementary proposals for the special cases of conodonts and ammonoid aptychi, gave rise to a prompt and massive response both for and against the concept. An excellent summary of the numerous comments and additional proposals is given by Sylvester-Bradley (see Peter C. Sylvester-Bradley, 1957, Summary of Proposals and Comments Concerning Parataxa, Form Taxa, Organ Genera and Collective Groups, *Bulletin of Zoological Nomenclature,* Vol. 15: 841–50).

Among those groups which some paleontologists felt could be effectively dealt with under the provisions for parataxa were trace fossils, i.e., various tracks, borings, and other "works" of animals which, in general, could not be correlated positively with their causative organisms. The advantages of extending the parataxa system to the names of trace fossils is evident, for taxonomic stability is not upset by the rare, and often subjective, matching of an animal and its trace. However, although it was obvious that some form of provision had to be considered to meet the needs of taxonomists in various special groups, there was considerable opposition to parataxonomy proliferating without control, and it was feared that the use of parataxa for sharks' teeth, fish otoliths, and other vertebrate fragments would hinder rather than help the progress of paleontology. This is the proper context for the comment by Simpson, misleadingly quoted by Heuvelmans, that "it would be a crippling blow to evolutionary biology if regular biological classification were not attempted, at least, in all cases."

After much debate at the zoological Congress in London in 1958, the matter was referred to a committee, with instructions to report to the Washington Congress of 1963. The report recommended that no further consideration be given to parataxa, and the concept was accordingly dropped. Thus, no solution whatever had been reached for the original problems recognized in the taxonomy of conodonts and ammonoid aptychi.

Heuvelmans is certainly not alone in being confused about the concept of parataxa, which was proposed originally for use in the classification of taxa composed exclusively of fragments of fossil animals, which are of great stratigraphical importance but which are unidentifiable in terms of the whole animals to which they belonged. Within their own system of classification, or parataxonomy, they would have a value independent of the names of taxa represented by whole animals.

The parataxonomy concept can perhaps be understood by analogy with units of monetary currency, which may be classified by using such "taxa" as *dollar, peseta* or *franc.* Let us suppose that the various 5-cent, 10-cent and 25-cent "fragments" of these had been given standardized names such as *nickel, dime* and *quarter.* Any 10-cent coin of unknown origin could be assigned to the taxon (or parataxon) *dime* regardless of which "whole" unit it formed a part. Under the normal rules of zoological nomenclature, if the "type species" of *dime* were found to be, in fact, a fragment of a *dollar,* then *dime* and *dollar* would become synonyms, and generally the older of the two names would take precedence. Obviously, this could result in a well-known name, such as *dollar,* being lost if *dime* had happened to be named first. Conversely, the general term *dime* could no longer be applied to any 10-cent coin of unknown association.

Under a system of parataxonomy, on the other hand, *dime* and *dollar* could never compete as they would belong to quite separate classifications. In the *dime* classification, Canadian and British examples might be placed more closely together because of the similarity of the head, while in the *dollar* classification the Canadian and American might be grouped together on the basis of the inscription. Neither is necessarily a more "natural" classification than the other, and each system is "parataxonomic" in relation to the other as the relationship between the systems cannot be shown by normal synonymy.

The "*nickel, dime, quarter*" classification can convey important data concerning similarities of size, shape and color, which allow interpretation in terms of function and distribution, even though it cuts across a conventional phylogeny in which only *dollar, peseta,* and *franc* have their place. From this analogy, it should be clear not only that parataxonomy could provide a useful basis for the classification of such fragmentary fossil entities as holothurian spicules, aptychi, or dinosaur footprints, but also that such a system is entirely inappropriate for the classification of animals "known only by traditions, reports, or ambiguous physical evidence."

With hindsight, it is now obvious that much of the controversy surround-

ing parataxa was due to two fundamental misconceptions: that certain categories of organic remains would all be classified as parataxa, and that fossil tracks and other "works" of animals could not themselves be taxa, but were merely the basis for hypothetical whole-animal taxa. The error in the first premise is that there are no objective criteria for the recognition of a parataxon: parataxa are simply taxa that exist in independent taxonomic systems, and consequently do not mutually compete for priority. As I have already pointed out:

> The concept of parataxa must be independent of any actual classification of assemblages of fossil fragments as no one system of taxonomy is necessarily more "artificial" than another. It seems to have been a failure to appreciate this that has led to most of the argument against parataxa being recognized in the Code. It is thus evident that parataxa are not absolute but relative. Therefore, as all parataxa are taxa within their own taxonomic systems, the Code already recognizes them . . . . But in order to avoid competition between taxonomies (in practice a very rare event but in theory . . . liable to cause nomenclatural instability of considerable magnitude) it is necessary for the Code to be amended so that a taxon and its corresponding parataxon do not compete for priority (see David Heppell, 1981, Request Under Bylaw 27 to Defer Publication of the Results of the Votes on Parataxa, *Bulletin of Zoological Nomenclature,* Vol. 38: 47–48).

The fallacy embodied in the second premise arose from the decision of the Paris Congress in 1948 "to make it clear that the description of the work of an animal constitutes an 'indication' . . . even if unaccompanied by a description of the animal itself and that the name so given is not to be rejected on the grounds that it is based upon a hypothetical form." A recommendation was added, urging authors to avoid giving names, as far as possible, to new taxa based solely upon the work of an animal. This ruling was unfortunately highly ambiguous, as the Budapest Congress had already ruled in 1927 that no name published after 1930 was available unless it was published with a "summary of characters . . . which differentiate or distinguish the genus or species from other genera or species . . . ." Before 1931, only "an indication, or a definition, or a description" was necessary.

Did the Paris decision intend to rule out names based on the work of an animal after 1930? Its recommendation would definitely suggest that it did not. More likely, it intended to make the "work" (rather than the animal supposed to have produced the work in question) equivalent to "the genus or species" of the Budapest ruling. In the draft of the new Code, however, Bradley stated: "I cannot read intent into it that is not expressed, nor contrary to what is expressed" (see J. Chester Bradley, 1957, Draft of the English Text of the International Code of Zoological Nomenclature as Amended by the Paris (1948) and Copenhagen (1953) Congresses. Part One. Names, Nomenclatural Regulations, and Their Interrelations With Taxonomy, *Bulletin of Zoological Nomenclature,* Vol. 14: 11–189), and this view has prevailed. Accordingly, it has been stated many times, without contradiction,

that names founded on the work of an animal after 1930 are not available in zoological nomenclature.

This interpretation would not have been of much practical consequence as long as only names applied to galls (which, in truth, are "the work of a plant") and suchlike were involved, as these were generally no longer named before the causative insect had been discovered. In practice, however, all types of fossil tracks, whether footprints or feeding trails (such as leafmines of insects), burrows and other bioturbations, and even coprolites, had been lumped together as the "work of animals," and injected into the discussions of parataxa. When the parataxa proposals failed, they could not be simply extricated and treated as normal taxa so long as it was believed that such names, because they represented the work of animals, could no longer be accepted at all.

The failure of the Zoological Code to provide for the nomenclature of trace fossils, at a time when many new and significant discoveries were being made, led to the publication of a draft Code by Sarjeant and Kennedy, which intended to extract trace fossils from the domain of zoological nomenclature altogether (see William Antony Sarjeant and W. J. Kennedy, 1973, Proposal of a Code for the Nomenclature of Trace Fossils, *Canadian Journal of Earth Sciences,* Vol. 10: 460–75). This did not find general acceptance among ichnologists, but did encourage the Commission to look into the whole question of parataxonomy once again. Melville reviewed the various aspects of the problem, and presented a number of proposed amendments to the Code (see Richard V. Melville, 1979, Further Proposed Amendments to the International Code of Zoological Nomenclature. 2. Paranomenclature, *Bulletin of Zoological Nomenclature,* Vol. 36: 11–14). It was decided to consider separately those entities such as trace fossils (ichnotaxa) whose genera do not require type species, and those such as fossil fragments and detached organs (parataxa) which do. As parataxa in this restricted sense are not relevant to cryptozoology, we need not consider them further.

Proposals concerning ichnotaxa were presented to the Helsinki Congress in 1979, and were subsequently adopted. These provided, firstly, that zoological nomenclature applies to the names of fossils of the work of animals or their traces, even though they may not have been correlated with any organism in the animal kingdom that caused them, and, secondly, that names given specially to such ichnotaxa do not compete in priority at genus level with names given to taxa of recognized organisms in the animal kingdom, nor at any level with names given to taxa of the animals that produced the work or traces.

Bromley and Fürsich commented that the restriction of ichnotaxa to fossil material seemed a needless hindrance, as it may be difficult to define the fossilization threshold in the case of the work of an animal, but nevertheless concluded that new ichnospecies should not be based on unequivocally

unfossilized material (see Richard G. Bromley and Franz T. Fürsich, 1980, Comments on the Proposed Amendments to the International Code of Zoological Nomenclature Regarding Ichnotaxa, *Bulletin of Zoological Nomenclature,* Vol. 37: 6–10). In effect, trace fossils are now to be treated as taxa and not as the work of an animal, thus assuring their availability even if published after 1930.

Now that the concept of parataxa has been explained and the meaning of the term further clarified by the Commission's recent division of such entities into parataxa proper, for the fossil fragments, and ichnotaxa, for the tracks and other trace fossils, we can reexamine its relevance to cryptozoology. Obviously, the true parataxa do not come at all into its concern, but tracks and traces do. As recent ichnotaxa are still excluded from the Code, we have to consider whether in fact cryptozoology, dealing with putative extant animals, needs to embrace ichnotaxa, especially as Heuvelmans seems to be arguing for their recognition as an essential aid to the study of the causative animal.

As far as I know, there has been no suggestion that Yeti and Sasquatch (Bigfoot) tracks should be named and classified in a system including only other tracks unrelated to whole animals. On the contrary, all the studies have attempted to correlate the tracks, the hairs, the feces and any other material evidence, as well as the descriptions obtained from eyewitnesses, with the corresponding material evidence and morphological appearance and behavior of known human or animal identity. In all cases, whether "hairy giants" or "lake monsters," an attempt is made to bring them into the recognized system of known animals. Heuvelmans' plea for the recognition of parataxa (or ichnotaxa) in cryptozoology is thus based entirely on a misunderstanding of their nature.

Another popular fallacy reiterated by Heuvelmans is "the requirement of a type specimen for the legitimation of the existence of an animal species." This is stated to be "an arbitrary rule, founded solely on a recently established convention." The fact is that there is no such requirement in the Code, nor has there ever been, though, of course, the existence of a type specimen is both normal and desirable. If a type specimen is present, it does not have to be a whole animal; if the description of a species can be made from a single diagnostic bone, a tooth or a hair, or the cast of a footprint, that is a matter of taxonomy, not nomenclature. There is a requirement in the Code that any new generic name established after 1930 must be accompanied by a definite indication of the type species, but this is unlikely to be a hindrance to the study of unknown animals.

Heuvelmans' additional remark that "since 1930 . . . contemporary zoology no longer accepts those species which do not fulfill certain very strict conditions" is also alarmist, for reference to the Code shows that, in the case of names of species, the only significant change is that, after 1930, new

names are not accepted if accompanied only by an illustration and no description "purporting to state the characters that differentiate the taxon from others," and that names established solely for the "work of an animal" were no longer available. The first of these limitations is certainly no bar to any cryptozoological description, and the second is irrelevant to cryptozoology—as has been shown above. Furthermore, it has now been made explicit in the Code that names given to tracks and other trace fossils are to be treated as taxa (ichnotaxa), and not as the work of an animal.

A last point to consider is whether animals known only from cryptozoological evidence are excluded from zoological nomenclature as "names given to hypothetical concepts." A hypothetical concept may be defined as a taxonomic concept that, when published, contained no animal, past or present, known to exist outside the mind of the author. The hypothetical "missing link" between ape and man, named "Pithecanthropus" by Haeckel in 1866, is an example which is not an available name, and so does not preoccupy the *Pithecanthropus* named by Dubois in 1894 based on actual specimens. In other words, Haeckel's "Pithecanthropus" was only an idea, an extrapolation for which the description could be derived only by inference or speculation from the characters of its supposed descendants. This is essentially different from a concept based on such evidence as consistent reports, photographs, casts of tracks, or other "ambiguous physical evidence."

It is important to realize that it is the actual evidence embodied in the description that is the foundation of any scientific name, not the construct postulated from it, and this physical evidence, of whatever nature, functions as the type. Thus, *Eoanthropus dawsoni,* the so-called "Piltdown Man," was described by Woodward in 1913 from material subsequently discovered to be fraudulently associated. The scientific name is nevertheless not rejected as being given to a hypothetical concept, but adheres to the material, the actual bones, to which it was given.

A further example is the 2.5-inch-long "fang" from the Gran Chaco, described by Kerr as "*Bothrodon pridii,* an extinct serpent of gigantic dimensions" (see John Graham Kerr, 1926, *Bothrodon pridii,* an Extinct Serpent of Gigantic Dimensions, *Proceedings of the Royal Society of Edinburgh,* Vol. 46: 314–15). Assuming this to be the poison fang of a huge snake nearly 60 feet long, Ditmars suggested that it probably fed on peccaries and various rodents "if such a monster might deign to note the latter," and went on to imagine the awesome picture this first poisonous monster to come to scientific attention must have presented if it were quick and savage like the boomslang (see Raymond L. Ditmars, 1931, *Snakes of the World,* Macmillan, New York). Quenstedt eventually dispelled such fantasies when he deduced that the fang was, in reality, one of the finger-like processes from around the aperture of the Indo-Pacific scorpion shell *Lambis chiragra* (see *Fossilium Catalogus I, Animalia,* part 86: footnote to p. 25 of Ophidia,

1939). The name *Bothrodon pridii,* although proposed for a supposed monstrous snake, thus became an unexpected synonym for a marine gastropod!

The description of a new animal from a single feature is undoubtedly a risky procedure, but Heuvelmans' assumption that a complete specimen is required before a taxon can be named is incorrect, and, consequently, his criticism of the International Commission on Zoological Nomenclature is unjustified. Scott and Rines point out that a description from an illustration is permitted by the Code, and propose the name *Nessiteras rhombopteryx* for the object(s) appearing in a series of underwater photographs taken in Loch Ness (see Peter Scott and Robert Rines, 1975, Naming the Loch Ness Monster, *Nature,* Vol. 258: 466–68), justifying their action by "the urgency of comprehensive conservation measures." They assume that, only by scientifically naming the Loch Ness Monster, could "it" be given legal protection, but if some large, unknown animal should eventually be captured or found in the loch, *their new name could be associated with it only if the animal were to conform with the details observable in their photographs, as only they are the material basis for the name.*

If there is any criticism to be made of the publication of scientific names for cryptozoological species, it should not be laid at the door of the Commission. Cryptozoologists should take heart in noting that the abundant material believed to be attributable to an unknown North American hominid—casts of tracks, numerous eyewitness accounts, hairs, feces, and even photographs—is certainly no less a sound basis for a scientific name than the Rines photographs of the rhomboidal fins of Nessie, or even the type specimen of Heuvelmans' own *Homo pongoides* (see Bernard Heuvelmans, 1969, Note Préliminaire sur un Spécimen Conservé dans la Glace, d'une Forme Encore Inconnue d'Hominide Vivant *Homo pongoides* (sp. seu subsp. nov.), *Bulletin de l'Institut Royal des Sciences Naturelles de Belgique,* Vol. 45[4]: 1–24), observed by him and Ivan T. Sanderson through a casing of ice, and the property of a traveling showman who displayed it at fairground exhibits.

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CRYPTOZOOLOGY, PALEONTOLOGY, AND EVIDENCE

(Comment on Heuvelmans, *Cryptozoology,* Vol. 1: 1–12)

First, there may be a problem concerning just what cryptozoology is, or should be. I will try to explain my hesitation.

The name "cryptozoology" itself is perhaps unfortunate because it is ambiguous. The word "cryptozoa" has long been used in ecology (see Lamont C. Cole, 1946, A Study of the Cryptozoa of an Illinois Woodland, *Ecological Monographs,* Vol. 16: 49–86) for animals which are hidden in places like the soil or under bark. Probably neither usage will go away, but I will follow Heuvelmans.

There are, as Heuvelmans notes, some similarities between cryptozoology and paleontology, but there are also relevant differences. It is effectively necessary that many extinct organisms are unknown, because there are many large gaps in most phylogenies that are not bridgeable by simple developmental changes. There is no comparable necessity that any large *recent* animals are undescribed; here, the likelihood rests on the acceptability of available evidence, and (as in a subsidiary way for paleontology) on the still not negligible rate of discovery. For small recent animals like mites, the accuracy of the latter method is much better because the rate of discovery in relation to effort is so much higher. The analogy in the nature of evidence between the Mesozoic tracks called *Chirotherium* and the Himalayan Yeti is apt, but perhaps should not be pushed very far. Some traces of *Chirotherium* may have been made by species of pseudosuchian reptiles now known from skeletal material, the likes of which Heuvelmans does not accept for the Yeti.

I agree on the usefulness of parataxa in some cases (although not for scientifically undiscovered organisms), but the quote from George Gaylord Simpson which Heuvelmans used in support of parataxa was actually in opposition to non-Linnean nomenclature. A third alternative, which I mention without advocacy, would be to expand the scope of standard classification. For organisms which lack an adequate specimen, what is relevant is the restriction coming from the need for a type specimen. Such a restriction may seem unrealistic, in any case, where there is adequate knowledge not based on a physically available specimen. (Where there is inadequate knowledge, of course, as for mermaids or jinn, scientific nomenclature is inappropriate.)

There are, however, reasons for a type specimen. It permits repeatable studies, and it provides an unquestionable demonstration of the reality of the organism, which may even (as with misclassifications or fakes) prove to be very different from what was originally thought. There are exceptions to these advantages. Subspecies may have types which cannot be distinguished

from each other, and type bacterial cultures can evolve in the laboratory. But if we abandon a strict requirement for a type specimen, where should we draw the line? Different people will disagree on how good the evidence is. Perhaps someone would even try to incorporate mermaids into science. It is probably best not to use Linnean nomenclature even informally, as is done for tracks in paleontology, unless required for the protection of the animal. Someone might still describe mermaids, but such names then would not be confused with others.

One cannot use the availability of identification guides as a measure of how well we know a fauna. People are not afraid to make such guides for insects and other taxa less well-known than mammals.

As a general practice, one needs better evidence for phenomena that are important, surprising, or susceptible to conflicts of motives than for ordinary expectations. Opinions differ on the importance of undiscovered large animals, but there is less disagreement with respect to the other criteria. This is not something at which to be offended or defensive; it is a real aspect of scientific justification. Part of it, in general, involves the coherence of existing theoretical structure, which has a variety of unrelated support, and rightly requires some degree of force in order to be shaken. And then there are cases like the one when I was an undergraduate: some students from the next dormitory claimed to see, on several nights, a figure which jumped 10 feet high across a nearby golf course, and which escaped down a convenient ravine when chased. This received gullible coverage in newspapers and, I understand, was reprinted nationwide. It was, of course, a lie. In a different context, my identification (see Leigh M. Van Valen and Robert E. Sloan, 1965, The Earliest Primates, *Science,* Vol. 150[1796]: 743–45) of primates from the Cretaceous and early Paleocene, earlier than they had previously been known, has been questioned by some people who have not seen the original specimens, although not, I think, by anyone who has. If all these specimens were lost, the evidence would immediately become less clear-cut until more could be collected.

The existing cryptozoological emphasis on large animals, even to the virtual exclusion of small ones (not to mention various nonzoological kinds of organisms) seems odd to a paleontologist and an ecologist. Most animals, extinct or not, are small. That is, *we* are unusually large. And there are many more small animals than large ones (Leigh M. Van Valen, 1973, Body Size and the Numbers of Plants and Animals, *Evolution,* Vol. 27: 27–35).

In practice, cryptozoology seems restricted to cases where unusually good evidence is needed for adequate demonstration. Perhaps it could even be redefined as something like the study of the basis of the animals of folklore, "folklore" having its technical meaning of oral tradition. This would not cover new cases, but Heuvelmans' definition (the study of "animals undescribed by science") is worse. The latter includes *all* unknown species, and

the best way to discover most of them is to collect specimens of poorly known taxa from inadequately collected habitats or regions. There are surely a large number of undescribed nematodes. Indeed, there are thousands of undescribed species already in museums. Ants are well known for insects, but I am told there are dozens or even hundreds of still undescribed species residing in Harvard's Museum of Comparative Zoology alone.

My comments may seem hypocritical, but a statement containing only points of agreement with Heuvelmans would be repetitious. There is much that is not known in the zoological world, and I support competent efforts like Heuvelmans' to investigate and evaluate evidence.

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GOLDEN MONKEYS, MACAQUES, AND WILDMAN

(Comment on Zhou, *Cryptozoology,* Vol. 1: 13–23)

Zhou provides an interesting discussion of Wildman reports in the People's Republic of China. Such reports have circulated for at least 2,000 years, and have lately received attention in some American magazines, such as Topping's uncritical report in *Science Digest,* published in 1981. My comments on Zhou's article are based on information gathered in 1982 in Hubei Province, a region of recent reports and extensive research into the Wildman problem.

(While working in China, I had the good fortune of interviewing three Wildman investigators in Hubei Province. I also obtained a hair sample, and viewed footprint casts attributed to the Hubei representative of Wildman. While the interviews and specimens were intriguing, they were far from convincing. In Hubei, the group supporting the existence of Wildman consisted basically of non-scientists. The skeptics, by and large, were scientists. A 1980 conference held in Wuhan, Hubei, to discuss the evidence apparently solved nothing; each side maligned the other. Wildman's existence in Hubei was supported by the then governor of the province, and that surely helped supporters of Wildman's existence.)

Zhou adequately reviewed evidence for Wildman's existence. I will comment on a few of his points.

1. Although Zhou recounts a 200-year-old report from Fang County, Hubei, in which Wildman is said to eat humans, we heard nothing to that effect.

The reports we gathered suggest an omnivorous, frugivorous or herbivorous diet. In fact, we were repeatedly told that Wildman avoids humans whenever possible—resulting in the fact that there are no *substantiated* sightings in Hubei.

2. Zhou reports that Wildman hair has been gathered in Hubei. The hair sample I obtained proved to be from the elusive golden monkey, *Rhinopithecus roxellanae roxellanae.* The various coloring and lengths of the hair also overlap our observations made on golden monkeys. Some of the hair coloring reported for Wildman can also be duplicated among macaques, which may show reddish-brown, grey, brownish-yellow, or occasionally white hair.

3. With reference to the so-called "monkey children," Zhou mentions the higher than usual incidence of genetic deformities in the Shennongjia area. A relatively high number of albino animals (including bears, deer, and monkeys) is also reported from Shennongjia. The cause(s) of this albinism are unknown. Having seen one so-called "monkey baby," I concur with Zhou's opinion that such individuals manifest genetic abnormalities.

4. Zhou might have discussed in more depth the lack of uniformity in eyewitness accounts of Wildman. The diversity of descriptions in eyewitness accounts weakens Zhou's contention that ". . . it can be inferred that these unknown animals are not mere creatures of fiction." Too many different phenomena are apparently being called Wildman, contradicting Zhou's statement that ". . . morphological aspects of Wildman are consistently reported."

5. Zhou discusses reports of bipedalism among Wildman, basing his remarks on eyewitness accounts and footprint casts. Such casts, however, are often made under inadequate conditions. Furthermore, a number of these casts were touched up before and/or after casting, and a good deal of personal judgment is involved in their analysis.

6. Those who reject the existence of Wildman, especially any possibility of an evolutionary relationship to either *Ramapithecus* or *Gigantopithecus,* often rely upon evidence that extensive ecological change has occurred over the millenia. Zhou is correct to note, however, that the giant panda has survived, and was very much a component of the fauna associated with *Gigantopithecus.* Nevertheless, it is very doubtful that a relationship exists between *Gigantopithecus* and Wildman, and any relationship between Wildman and *Ramapithecus* can probably be forthrightly rejected. If 1) neither *Gigantopithecus* nor *Ramapithecus* existed, or 2) if there was no dispute concerning the evolutionary relationships of either, would reports of Wildman continue? We seem to have a case where disputed phylogenies are helping to support the existence of mysterious creatures. This is mysterious scientific methodology.

I agree with Zhou that we do not yet know what, if anything, we are dealing with. I also agree with Zhou that at least some (many?) of the reported sightings of Wildman are, in fact, reports of monkeys. Zhou leans toward

the macaques. Our research tilts us toward the golden monkey. During our research on macaques in Xhin Xhan County, Hubei, we received reports of two different sizes of macaques—one much larger than the other. Although unable to confirm these reports, they do lend some credence to Zhou's contention that a large, unidentified macaque may account for many Wildman sightings.

Whether or not scientists accept reports of Wildman and related phenomena (the Yeti and Sasquatch), it is clearly within the domain of science to investigate such reports. I am impressed by the similarity of such reports from far-flung world regions, and unimpressed by the argument that the similarity of such reports is attributable to the dissemination of information from one world region to another. That explanation seems highly implausible, at least in some cases.

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RI-ALITY: A MERMAID IN THE HAND?

(Comment on Wagner, *Cryptozoology,* Vol. 1: 33–39)

Roy Wagner has presented us with extremely interesting reports of mermaid-like creatures off a remote island in Papua New Guinea. These reports pose challenging questions on two levels. In this respect, they are not unique. Many such reports pose the same two rather different sorts of questions, and therein lies the fascination of cryptozoological study. The first question concerns the objective reality of the phenomenon which stimulates the reports. That is, what object did a given witness actually see? The second question concerns what the witnesses *think* about what they have seen. That is, what does a witness believe has been seen? These same two questions can, of course, be posed in respect to any observation, but their importance is magnified in cryptozoological reports. The two are inseparable, and to get a satisfactory answer to either often requires a satisfactory answer to both.

Wagner's narrative concerns an animal said to resemble a "mermaid," and known to the native inhabitants of New Ireland, an island in northeastern Papua New Guinea, as Ri. The narrative begins in typical fashion. An animal is reported and claimed to be well-known to local inhabitants. When more details are sought, the animal recedes tantalizingly to a safely vague distance.

In this case, Wagner himself actually saw "something large," "a long, dark body swimming . . . horizontally" some distance from shore. Unfortunately, it "submerged and did not reappear." Subjects were interviewed, and many claimed extensive knowledge of the Ri. One witness said he had even captured one in a net, but it had been "allowed to creep back to the water and escape" before other witnesses could be summoned. Another witness said he had attempted to spear a Ri, but deliberately threw his weapon crookedly. He did say, however, that its face resembled "a monkey." This sort of report is common in cryptozoological literature. People see something dark moving in the forest, and knowing that it is not a bear, conclude that it must be a Sasquatch. Or, again, something large emerges from the water, but is not a whale, and therefore must be Caddy, or Chessie, or Champ. When one looks closer, it is gone. For the cryptozoologist, these reports are like a dream, where one is chasing something glimpsed but never seen. The chase goes on and on, is always exciting, but never ends.

The unusual feature of Wagner's report is the apparent widespread knowledge of Ri. They are sometimes seen and even caught by fishermen, and their flesh is used for food. Some details of the anatomy are known, such as the color of the fat (yellow), the shape of its axial skeleton (straight), and the volume of blood ("a great deal"). Its cry of pain is even reported to be human-like. Such reports suggest an animal of considerably more substance than a dream.

It is at this point that the cryptozoologist fetches up on the shoals of the second category of question mentioned above. What do the witnesses *believe* they are seeing? It is as difficult for me as it probably is for Wagner to reconcile the objective reality of Ri flesh eaten by the natives with the elusive creature receding just too soon to be seen clearly. Are they the same creatures? Do the people who make the different reports share the same beliefs? Wagner, a cultural anthropologist, touches on these questions. According to the New Irelanders (New Irish?), the Ri are either the transmuted forms of the first people, or are the embodied spirits of a recently extinct clan of people. Some people evidently respect the origin and spiritual nature of the Ri by not causing them harm. Others harbor no such prohibition. Therefore, one can conclude that different groups of people hold different beliefs about the Ri. The question remains, however, whether these different groups have the same objective reality in mind when they discuss the Ri.

At the risk of stepping a long way out of my field and into Roy Wagner's, there seems to me good reason to be very cautious when attempting to interpret anecdotes which may impinge on the Melanesian spirit world. Many of these people are a few generations (at most) from a Stone Age cultural environment. In their ancestral *milieu,* the world abounded with spirits and shades which had tangible reality. These beliefs die hard. In New Caledonia, I know of a man who firmly believes, without any doubt, that

his father was murdered by magic. I have also heard tales of malignant little bearded men who inhabit the bush near certain hills. Some Melanesian people shun such places today, but it would be erroneous to assume that these creatures would be as "real" for non-Melanesians as for Melanesians. I have yet to hear of any modern Irish (Old Ireland) cryptozoologists searching for leprechauns! Similarly, what are we to make of a dark object seen some distance from shore, and given the appellation of one of the denizens of the Melanesian spirit world?

Since I am supposed to be a zoologist, not an anthropologist or epistemologist, I should be discussing the possible zoological interpretations of the Ri. From the information presented by Wagner, it is possible to indulge in some speculation about the identity of the Ri, and perhaps even of mermaids. Ri flesh is bloody and has yellow fat, and the animal has no vestige of pectoral appendages. It is reported to breathe air, and to vocalize in whistles, whispers, and groans. The upper torso is humanoid, with long dark hair on the head. They have humanoid genitals, and females have mammalian breasts. It is obviously some sort of mammal. The whistles and lack of pectoral appendages suggest some sort of cetacean. The hair and the humanoid face, conversely, suggest a sirenian or an otter-like creature. Wagner points out that the locals are well acquainted with dugongs, and that he himself knows a coastal porpoise when he sees one. So the Ri could be some sort of unusual marine mammal—one which might easily be identified from a specimen.

One hopes that the new (July, 1983) expedition to New Ireland to seek the Ri had the opportunity to obtain further evidence. Meanwhile, we should thank Roy Wagner for bringing these intriguing reports to our attention.

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RI OR DUGONG?

(Comment on Wagner, *Cryptozoology,* Vol. 1: 33–39)

I found Roy Wagner's article "The *Ri*—Unidentified Aquatic Animals of New Ireland, Papua New Guinea" interesting. It is an account, however, that relies entirely on second-hand information. The description of the author's actually seeing a Ri must be discounted; he couldn't discern enough

about the animal to be confident that it wasn't a dugong or a large fish. As I see it, the author has to convince his readers that there is sufficient information available to make it likely that this is not a native tale that has its basis in the dugong. The author gives enough information about the habits and appearance of the Ri to satisfy me that it is not a porpoise.

The author says the natives know the dugong (*bo narasi*), which is rare in the area, and can easily distinguish it from the Ri. It would bolster his case a lot if the author had some evidence of that, like natives recognizing dugong or porpoise photographs correctly. Also, in discussing the origin myths (p. 37), it would be significant if the natives also had myths that explained the origin of the dugong separately. That is, do the natives make the Ri-dugong distinction in their mythology?

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THE PRESS AND ANOMALY REPORTS: DISTORTIONS OF TONE, TIME, AND PLACE

(Comment on Bauer, *Cryptozoology,* Vol. 1: 40–45)

In Walter Lippmann's *Public Opinion,* there is a chapter on "The World Outside and the Pictures in Our Heads" (see Walter Lippmann, 1922, *Public Opinion,* Harcourt, Brace, and Co., New York). This contrast, between what is actually happening in the world outside and our representations of it, is of more than casual importance in considering what we hear or read about anomalous events.

Bauer suggests that newspaper coverage of the Loch Ness Monster distorts the opinions of the scientific community. Expert opinion is presented by the newspapers as being more negative on the creatures' existence than is actually the case. But there are certainly other comments that one might make about this "social intelligence" process and its defects (see Ron Westrum, 1979, Knowledge About Sea Serpents, *Sociological Review Monographs,* Vol. 27: 293–314 [Special Number edited by Roy Wallis, "On the Margins of Science"]). In doing so, one can call upon several decades of sociological research on the press and its processes. Bauer calls attention to *distortions of tone*; I would also call attention to *distortions of time and space.*

When a newspaper person writes a report or an article about an odd event such as a Nessie sighting, scientific accuracy is seldom of concern. What is of concern is that 1) the article deal with something that is newsworthy, and 2) that its contents be credible to other members of the media community. Thus, invoking stereotypes of "the" monster and its observers seems perfectly legitimate because such an approach will be readily accepted by one's editor and colleagues. Such copy is unlikely to be blue-penciled. Nor is there likely to be outside dissent by persons whom the reporter has to take seriously, such as well-known scientific authorities. Using what is essentially a media reflex or convention, then, is a relatively safe bet, and one that requires little time, reflection, or research. The time saved is an important consideration; the reporter has other articles to write, and sea or lake monsters are seldom priority news items. Thus, the superficial nature of most newspaper articles on Nessie, and their adherence to press stereotypes, should not be surprising. They are merely a reflection of the newsperson's need to get on with the job.

The negative tone in most stories on Nessie is thus an effect of the need for routine production of newspaper copy. In many cases, in fact, the humorous tone of the article supplies the "angle" so necessary to give direction to news stories (see David Altheide, 1974, *Creating Reality: How TV News Distorts Events,* Sage Publications, Beverly Hills, California). (I personally discovered the importance of "angle" when I gave a talk at the Manlike Monsters Conference at the University of British Columbia in 1978. The only remarks of mine which turned up in a magazine account of the conference [titled "Bigfoot Follies"] were some off-the-cuff and semi-accurate comments I made *after* the formal talk. The substance of my prepared remarks, over which I had labored carefully for several days, was completely ignored.) One could get a different angle, of course, by calling up scientific authorities and finding out that their opinions are not unanimous, but there are two reasons why this action is generally not taken. First, finding real experts on the Loch Ness Monster is not easy, and will almost certainly involve making expensive, long-distance telephone calls. It is much easier simply to call up the local museum or college and talk to a zoologist selected at random. Since Nessie is seldom discussed in the scientific literature, this person is likely to rely on his or her own *impressions* of the scientific community's attitudes, which are often derived from the press itself. The newspaper person's own attitudes are often formed in the same way, and further work, therefore, appears unnecessary because scientific opinion appears one-sided. Thus, the attitudes of reporters are shaped by what other reporters have previously written on the topic; it is no accident that one student of the press has described the situation as the "solipsism of the newspaper office" (see Paul Rock, 1978, News as Eternal Recurrence, *In* Stanley Cohen

and Jock Young [eds.], *The Manufacture of News,* Arnold Constable, London). This situation is made worse by the general tendency of scientists to act more conservatively when interviewed by the press than they actually are in person. (And who can blame them? Many an irresponsible reporter has changed the cautious and well-hedged *hypothesis* of a scientist-interviewee into a bold statement of fact, to the later dismay of the scientist.) As a result of all these factors, the general negative tone of Nessie articles is largely a result of routine press procedures. If challenged on this, of course, the reporter will simply invoke common sense, and ask how he or she could have been expected to behave differently. This is not an unfair response. After all, reporters cannot be expected to act like scholars. It is the scientific community's responsibility to inform the press about its own opinions. Since it has not done this concerning Nessie, the press is hardly to blame.

So much for distortions of tone. There are also other, more dramatic ways in which the press distorts news about "lake monsters." Consider, for instance, that until a few years ago, most Americans knew only about one lake monster site: Loch Ness. However, not only does Scotland possess other lakes where similar sightings take place, but reputedly Ireland does also (see Elizabeth Montgomery Campbell and David Solomon, 1973, *The Search for Morag,* Walker and Company, New York; see also F. W. Holiday, 1973, *The Dragon and the Disc,* Sidgwick and Jackson, London).

Not only have such animals been seen up and down the Scottish coasts (see Bernard Heuvelmans, 1968, *In the Wake of the Sea Serpents,* Hill and Wang, New York), but the United States and Canada possess a number of lakes with their own reputed monsters (see Peter Costello, 1974, *In Search of Lake Monsters,* Coward, McCann and Geohegan, New York; see also Roy P. Mackal, 1980, *Searching for Hidden Animals: An Inquiry into Zoological Mysteries,* Doubleday, Garden City, New York; see also Gary Mangiacopra, 1980, Sharlie: A Preliminary Report of Possible Large Animals in the Payette Lakes of Idaho, *Of Sea and Shore,* Vol. 12[1]: 43–46; 1981, The Two Monsters of Flathead Lake, Montana, *Of Sea and Shore,* Vol. 12[2]: 93–96, 114; 1982, Canada's La Bete du Lac: The Beast of Lake Pohenegamook, *Of Sea and Shore,* Vol. 12[3]: 138–40, 181; see also Mary Moon, 1977, *Ogopogo,* J. J. Douglas, Vancouver).

Nor are the latter a new discovery. Sightings have been taking place at many of these sites for decades. In fact, a national contest was held in 1954 to name an Idaho "monster" (see A. Boon McCallum, 1954, "Sharlie" Is Name Selected for Famous McCall Serpent by Group of Judges, *The Payette Lakes Star,* Vol. 36[3], January 21). So why the concentration of the press upon Loch Ness?

The answer lies with the tendency of the press to "give the public what it wants." Pierre Van Paassen, a "stringer" in Paris for the New York *Evening World* in the 1920's, used to write stories about the "apaches," those colorful

figures of the French underworld from whom we got the "apache dance" (see Pierre Van Paassen, 1939, *Days of Our Years,* Hillman-Curl, New York). These stories were quite popular with the *World's* American readers, who enjoyed reading about the latest apache battles with the police and each other. Yet Van Paassen's reportage on the apaches faced a significant problem: the apaches no longer existed. They had all been killed or arrested by the police. So Van Paassen simply made the stories up.

Similarly, the press writes stories about Nessie because this is what the public expects. I would not be surprised to find that the average reporter does not even know about the supposed existence of other lake monsters. Yet, even if the reporter did know, what story is likely to get more instant recognition (and therefore is likely to be more newsworthy), one on British Columbia's Ogopogo or on the Loch Ness Monster? Reporter and reader exist in a world where they must communicate with each other in terms that they mutually recognize and understand. Thus, the narrowing of the reporter's vision in space is the result of what the reporter expects the public to be able to understand. Yet, each such communicative act keeps the discourse in the narrow rut of the already explored. Unhappily, the Loch Ness monsters are not the only subject on which this narrowing takes place. The stereotype thrives in press writing because it is an essential tool in the survival kit of the reporter. Thus, like the distortions of tone, the narrowing of spatial horizons is a product of the routine processes of everyday news writing and editing.

Distortions of time are equally significant. It is often assumed that newspapers print more articles on "sea serpents" during the summer because they are looking for copy to fill their pages. Yet, as I have shown in a previous paper (see Westrum 1979, above), sightings are more likely to *take place* in summer than they are to be *reported* then. While 44 percent of newspaper articles about "sea serpents" relate to summer sightings, 53 percent of all such reports involve summer sightings, not terribly surprising since the observers are most likely to be around in warm weather. Summer is thus not the "silly season" for the press regarding "sea serpents," whatever the convictions of reporters or the public. The press does not distort the *season* of sightings. What it does distort is the year-to-year constancy.

It is often assumed that waves of anomaly reports in the press betoken massive surges of sightings, and that, contrariwise, a lack of reports signifies a lack of sightings. There is, however, little evidence that this is so. Furthermore, what we know of the internal dynamics of the press suggests that changes in coverage are more related to internal processes in the media than to external events. What is newsworthy is what the press defines as news. During a certain period, an anomaly such as "sea serpent" sightings becomes newsworthy. This period may be initiated by a spectacular or well-vouched sighting, typically reported in a large-city daily or a national newspaper.

When anomalies become news, they are well- or even over-reported (see Herbert Strentz, 1970, A Survey of Press Coverage of Unidentified Flying Objects, 1947–1966, doctoral dissertation, Northwestern University Department of Journalism). When they are not so defined, sightings are simply not covered. Thus, there *are* periods equivalent to a "silly season," but not at a definite time of the year.

The media, part of the "social intelligence" process, thus changes its parameters in regard to anomalies for a short period of time. The parameters then revert to the situation *ex ante.*

Upton Sinclair, in his book *The Brass Check,* related how the American press could suddenly change from a concrete wall to a channel for news (see Upton Sinclair, 1919, *The Brass Check,* published by the author, Pasadena, California). This is exactly what happens with "crime waves," and also with anomaly reports. I regret that the complete documentation for this claim must wait for another occasion; I am at present working on a book in which the problems of defining the anomaly "wave" are more fully addressed.

One phenomenon associated with a new burst of reports has already been examined, however, and that is the "report release effect" (see Westrum 1979, above). When a prominent anomaly report is published, several older reports are frequently revealed by the observers, who now feel that it is safe to "go public" with their experiences. The press, in turn, finding these older reports newsworthy, is quite willing to publish them. A not inconsiderable number of case reports is added to our knowledge of cryptozoology in this manner. The impression one gets from the media, however, is that there is suddenly a rash of *sightings,* not reports, which is usually incorrect. Thus, one produces a "wave" of anomaly reports: a distortion of the occurrence of the events in time.

There is certainly much more to say about anomalies and the press, but I will have to wait until another occasion to say it. In the meantime, one can consult with profit the excellent thesis of Strentz on UFOs and the press (see Strentz 1970, above), and perhaps also Curtis MacDougall's *Superstition and the Press,* to be published soon by Prometheus Press. I have not seen the latter, and cannot vouch for its treatment of the anomaly problem, but its author is a recognized authority on the press, and is also the author of a worthwhile book on hoaxes (see Curtis D. MacDougall, 1968, *Hoaxes,* Dover Publications, New York).

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Sanitized Science versus Anomalies

(Comment on Bauer, *Cryptozoology,* Vol. 1: 40–45)

Bauer's findings infer that newspaper science writers strongly mold public attitudes toward science. Although I cannot quote any figures, I suspect that one should also include those who write and edit popular science magazines (*Discover,* etc.) and TV documentaries (NOVA, etc.). The latter group, in particular, presents a pat, sanitized picture of the scientific endeavor.

Having been a freelance writer myself for almost 20 years, I can testify that, to keep the checks rolling in, a writer must not appear to be "too far out" to either editors or the scientists who must be interviewed. No scientist wants his work discussed in "tainted" publications, or by someone with questionable credentials. Can one imagine a Nobel Prize winner in particle physics granting an interview to a writer who has written extensively on "sea monsters" or UFOs, no matter how responsibly? What science writers write is largely what scientists want to hear, and this, in turn, molds public opinion.

Scientists, then, must be the culprits, insisting upon a rosy, nonanomalous world. Not so! Scientists are funded largely by universities, foundations, and government agencies. These institutions rarely wish to tackle scientific anomalies—despite frequent claims to the contrary. They believe it would be bad for their public images, especially in the eyes of the taxpayers. Thus, we come full circle back to the public, and its attitude toward science. My contention is that the public *wants* to have the cosmos portrayed as a neat, well-explained place, with the human intellect in full charge. After all, there are enough uncertainties in life as it is! It's all right to slip in a few mysteries here and there (a Sasquatch here, a "lake monster" there), particularly during the summer "silly season." Science writers—and scientists—only tell the public what they want most to hear: "Everything's O.K."

Of course, everything is not O.K. The world teems with thousands of scientific anomalies. My conclusion is that progress in solving scientific anomalies will be made primarily by individuals who are not accountable to the public, or institutions dependent on public image. Historically, many scientific breakthroughs have come from just such people.

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Nessie Statistics

(Comment on Bauer, *Cryptozoology,* Vol. 1: 40–45)

Bauer's first sentence comparing Nessie's public image to mermaids or unicorns is far too strong and overstates the case. Almost nobody, in the last hundred years, has seriously suggested that the classical mermaid or unicorn concepts represent real animals. Of course, there has been speculation as to what animals may have given rise to these ideas.

Although Bauer's findings are very interesting, there is no statistical analysis one can evaluate. The data were presented, but not analyzed statistically, making comparisons quite difficult.

This is especially relevant when trying to compare percentages of different sample sizes. There are standard statistical methods, i.e., adjusted R^2, which make allowances for comparing a sample of 20 items with one, say, of 100 items. Achieving 100 percent with a one-item sample cannot be compared with achieving 100 percent with a 50-item sample. A case in point is Table 4. Only 3 percent of 97 "strong or utter belief" articles were labeled as "totally erroneous presentations." This is far more spectacular and significant when we consider that 16 percent of only 58 articles were "totally erroneous presentations" in the category of "strong and utter disbelief."

A simple statistical analysis would have improved the paper greatly, and would have made it far more convincing. Otherwise, the article was well-organized and clear.

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The Wish to Believe

(Comment on Bauer, *Cryptozoology,* Vol. 1: 40–45)

As far as I can judge, Bauer is aiming to conclude that the press has lost interest in Nessie, and that the subject is kept alive by books such as those of Dinsdale and Witchell. With this I would agree. He also makes the case, at least by inference, that people who believe in Nessie as a solid-flesh entity do so because they wish to do so, even in the absence of any solid evidence, and in the face of overwhelming circumstantial evidence to the contrary. Again I would agree with him. I have encountered this again, and again,

and again over the past 30 years. This attitude is epitomized in an incident which occurred 20 years ago. I had delivered a lecture to a local natural history society in which I had set forth, as rationally as my competence would allow, and as impartially as it is humanly possible, the history of the search for Nessie. At the end, and as the meeting broke up, I was approached by a pleasant and intelligent member of the audience, who said: "I congratulate you on having presented a most convincing case against the Loch Ness Monster. But I shall go on believing in it—because I wish to." We smiled at each other, shook hands, and parted.

Since publishing my trilogy in *New Scientist* (see Maurice Burton, 1982, The Loch Ness Saga, *New Scientist,* Vol. 94[1311]: 872; Vol. 95[1312]: 41–42; Vol. 95[1313]: 112–13), more evidence has come my way about Kenneth Wilson and his infamous photograph of an alleged plesiosaur. I have no doubt that, in converting a local legend into a global mystery, Wilson was laughing up his sleeve. Moreover, the otter, whose tail he photographed, was laughing in the only way a carnivore is able—with its tail. The coincidence is remarkable. Perhaps one day I shall write-up the full story!

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Hominology: A Realistic Response to a Realist

(Comment on Bayanov, *Cryptozoology,* Vol. 1: 46–48)

Dmitri Bayanov calls for a dialogue between "realists" and "folklorists" in the study of beliefs and reports regarding such phenomena as the supposed Sasquatch of northwestern North America (which he refers to as hominology). I certainly agree with Bayanov on the need for dialogue, and believe that a good foundation has been laid in this region by the editor of *Northwest Anthropological Research Notes,* and by the organizers of the 1978 Manlike Monsters Conference at the University of British Columbia, to which he refers. In a co-operative spirit, then, I would like to comment on Bayanov's distinction between "realists" and "folklorists." It seems to me that he may be joining together two distinctions that are better kept separate.

First, there is an opposition of philosophical positions. "Realists," in this sense (as Bayanov uses the word), are those who suppose that beliefs and reports about Sasquatch (to take only one example in hominology) have

some real basis in the testable world of science, while "folklorists" are those who suppose that these beliefs and reports have no real basis, but are simply the expression of the human imagination and the myth-making propensities of the human mind. (A skeptic might say "realists" believe in the unreal, while "folklorists" do not.) An extreme case of "folklorism," Bayanov suggests, would be that of the philosopher who supposes that there is no reality outside of his own mind. An extreme case of "realism," I suggest, would be the person who insists that *all* traditions, beliefs, and even current rumors, must have some basis in fact, and who thus, in effect, denies the possibility that *any* statement can be utterly false. And just as there is probably no way of arguing with the extreme case of "folklorism," there is probably no way of arguing with the extreme case of "realism."

Fortunately, however, most people are not extremists in either direction. Bayanov himself is certainly not an extreme "realist." His statement, "the existence of mythological hominoids is a necessary, though not sufficient, condition of the existence of real hominoids," implies that, while there will be no real Sasquatches without mythological Sasquatches, there can be mythological ones without real ones. There can indeed be unfounded beliefs and false statements. The distinction between "realist" and "folklorist" in this sense is one of philosophical or scholarly inclination. It is a matter of degree. And surely it can vary with the issue.

In the last two paragraphs of Bayanov's paper, however, he seems to shift from this distinction, based on a philosophical position, to one based on academic training and professional expertise, using "folklorist" (I think) to refer to the student of "folklore." But surely a "folklorist" in this *academic* sense can be a "realist" in the *other* sense. And what is the "realist" in the academic sense? The human biologist? It seems that most of these are "folklorists" in the philosophical sense. In the dialogue that is needed, there ought to be representatives of various disciplines with varying degrees of "realism" and "folklorism"; that is, 1) of willingness to consider traditional beliefs as possibly based on reality, and 2) of skepticism.

Bayanov urges representatives of different disciplines not to trespass on each other's territories, and suggests that I have done so. My reaction to the general point is to suggest that hypotheses ought to be welcomed from any quarter. But I agree that it would be useful to formulate questions so that they are answerable by one discipline or another. When I (an ethnologist) said that "a large non-human primate would not really steal women," what I had in mind was the 19th Century legend, perpetuated in 20th Century cinema and cartoons, that male gorillas have abducted human females. It is my impression that such things have never happened, but I might be wrong. At the ideal conference, one might ask the biologists and primatologists present just how closely related do two separate species have to be

for sexual attraction to occur, and whether any hypothetical hominoid/hominid could be that genetically close to *Homo sapiens.*

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(Wayne Suttles is a cultural anthropologist. See the article by John Colarusso elsewhere in this issue which addresses the same questions raised by Bayanov.)

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