

Comments on James E. King, Michael A. Woodley of Menie, Matthew A. Sarraf, & J. Richard Greenwell:

Eyewitness Reports Concerning a Putative North American Hominoid: Anomaly or Artifact?

HENRY BAUER¹

A Brief Commentary on the Paper by King, Woodley, Sarraf, & Greenwell

This article will be welcomed by many people for the amount of interesting data as well as discussions of such matters as the reliability of eyewitness descriptions and memories, and the minimum size of viable populations. The references are copious in number and usefully pertinent. There is much food for thought that could generate new ideas and further research, and the concluding plea for further research on the possible reality of Sasquatch is certainly justified.

However:

As to “reliability of eyewitness descriptions and memories”, no definite conclusion can be drawn that sheds light or guides probability estimations pertinent to the specific present issue. While the general discussions are surely of general interest, the various factors may well feature differently in each of the individual sighting reports. Dealing with generalities and statistical analyses it is sometimes necessary to disaggregate the data to avoid being misled. As to eyewitnesses, a single report from someone one knows and trusts carries more weight than any number of other reports. My own belief that “Loch Ness Monsters” was considerably supported by personal testimony from a friend who had served in the navy in WWII and was a lifelong fisherman on Loch Ness who told me of a quite close encounter.

Numbers given should not be excessively “accurate”: Surely not beyond 3 significant figures or one decimal place:

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Thus I disliked “40.78 cm ... 18.41 cm ... 26.67 cm and 9.91 cm... an average depth in the ground of only 1.28 cm ...average weight of the eyewitnesses in the dataset is 84.21 kgs.” “Mean track width: 7.247 inches (18.41 cm), Median track width: 7.000 inches (17.78 cm), SD: 1.695 inches (4.31 cm)”. This sort of meaningless “precision” is characteristic of scientific illiteracy and makes it harder to take the rest of the text seriously.

“These data indicate that whatever role there might be for a ‘Bigfoot subculture’ (Regal, 2011) in reporting sightings has been minimal.” (p. 33)

Not necessarily. Missing inevitably is data about “sightings” made but not reported because the witnesses believe they must have been mistaken or decide that they were misled by a deer, bear, etc. Reports are most at likely to become publicly known from those who take Bigfoot seriously, i. e. a sub-culture.

That species discoveries by year follows an approximately quadratic function (or any other curve) does not in itself justify predicting likely numbers of future discoveries. Moreover, the criteria used to decide a new species may change over time.

“Such an ape would solve a zoological enigma, i. e. the conspicuous absence of indigenous primates in the North American continent, despite the presence of such animals in South America, Europe, Africa, and Asia” (p. 36)

But is such an enigma noteworthy? Are there not other such apparent enigmas, say with marsupials? “Apparent” only until continental movements over geological time are fully understood?

STEPHAN KRALL²

Hominoids Include not only Hominids but also Gibbons

The article analyses the observations on ape-like creatures in North America from 1950 to 1994. Unfortunately no later data are included because, according to the authors, recent observations are not comparable for this kind of analysis. The analysis as such is serious and in the discussion the authors deal with various aspects of criticism. Special attention is paid to the extensive literature on the seriousness of eyewitness accounts in order to prove that eyewitness accounts cannot be dismissed as not reliable. A final assessment of whether there is a new species of hominid in North America is not given, but at least it is put into the realm of possibilities that

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this could be the case. For this purpose, among other things, a statistical extrapolation of the values of discovered hominoids to hominoids that might yet be discovered is used. But the curve shown in Figure 1 need not continue in the same way. Moreover, hominoids include not only hominids but also gibbons. Since the middle of the last century, however, only one hominid has been re-described, and was only assigned to another orang-utan species. Therefore, the conclusion that two new hominoids will be discovered in the next decades can only be understood if this also includes the gibbons. At least it is unlikely that a new species of hominid will be discovered and not only a known population will be described as a separate species. This could well have been discussed in the article.

Most hominids live in social groups. Only orang-utans have a solitary lifestyle. It would have been interesting to go into why only single specimens were ever seen and whether there were statements about the sex. If there were females, why is it that there were always no children reported? There were also no statements about artefacts, like tools. That speaks against hominini, i. e. the genus *Homo*. But all in all it is an interesting and well done work.

ULRICH MAGIN³

Three Major Mistakes for one Doubtful Bigfoot

There are several, and major, problems with this paper. I will only briefly outline the three biggest of them.

Pre-selected set of data

First, it must be obvious that the data-base used has been pre-selected at least by two agencies.

1. by the investigators – it contains only material that John Green and/or the authors of the paper thought belongs into that file, so that there has been a data bias from the start.
2. by the witnesses – only people who think they have seen something that resembles the traditional image of Bigfoot will report their encounter to a Bigfoot researcher.

No wonder, then, that the items chosen confirm to what both the audience at large and the researchers think Bigfoot looks like. If you exclude all reports of monkey-like or chimpanzee-like creatures that walk on all fours, all reports of dwarfs, werewolves, grey humanoids or 20 ft.

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giants, you will inevitably achieve the kind of uniformity you are out to prove in the first place. For example, the result that 0 % of the witnesses reported a tail (“Tail Presence” in the “full data set”) is to be expected because all sightings of animals with tails will not have been included in the data base anyway. (“I saw a mighty big Bigfoot with a ten foot tail” just won’t be added to the data base, as it will contain only sightings of Bigfoot, and Bigfoot has no tail.)

In the “Full Data Set”, there are items for “Interpreted aggressive behavior” or “Hair Color” but we do not find tables on Bigfoot dressed in coats of fur or carrying a club, although such stories were reported, and continue to be reported (cf. Shoemaker, 1990: 18, 22, 57, 58 [pants]; Green, 2006: 39, 233) Obviously, if these sightings had been included in the database, the result would have been that these are very uncommon traits, and that they can be ignored, and yet the data contained in these reports for hairlessness of face or wideness of shoulders would still be counted in the analysis. We also find no items on beards (but see Sanderson, 1968: 73; Shoemaker, 1990: 22, 23 [2 cases], 57 [2 cases]) or Bigfoot living in caves, although this was a common feature early in the story of this myth (Sanderson, 1968: 76, 112; Shoemaker, 1990: 21).

When the body of data has been selected according to an already existing belief, it is no wonder that this belief will be confirmed when you analyze that body of data.

Memory

The second and third major problems are what the author say, or claim others say, about eyewitness and recall reliability. They write, “Nevertheless, it should be understood that the case for the view that eyewitness memory is highly and inherently unreliable, and that individuals are very susceptible to false memory formation and to biases that distort perception, judgment, and memory, is quite poor” and yet the papers they quote do not confirm such a view – quite to the contrary.

Regarding memory, Wixted, one of the authors they quote, confirms that eye-witness memory is highly inaccurate, especially when it is retrieved by inexperienced interrogators. Wixted (2018: e14) stresses that questioning a witness several times will distort his or her memory:

Instead, well-intentioned investigators tested the memory of the eyewitness again and again (each time further contaminating it and increasing confidence) until, ultimately, in front of a jury, the contaminated memory evidence seemed conclusive because the eyewitness identification of the innocent defendant was made with high confidence.

That is, the act of interviewing a witness, even by well-intentioned investigators like Bigfoot researchers, will inevitably distort the memories. Memories only attain a high degree of reliability when certain protocols are closely followed which have just recently been established,

and which the Wixted calls “much improved eyewitness memory protocols” (Wixted et al., 2018: 333).

Generally, as Wixted (2018: e13) writes,

On the surface, the case in favor of the more research-based view – namely, that eyewitness memory is unreliable, regardless of confidence – seems strong. First, convincing research shows that memory is malleable, so much so that people can come to confidently remember traumatic events that never actually happened. Second, lab-based research was long interpreted to mean that the confidence an eyewitness expresses upon identifying someone from a lineup is not particularly indicative of accuracy, not even under “pristine” testing conditions. Third, and most compelling of all, eyewitness misidentifications made with high confidence in a court of law are known to have played a role in more than 70% of the 358 wrongful convictions that have been overturned based on DNA evidence since 1989. The verdict seems clear: contrary to what the lay public believes, eyewitness memory is unreliable no matter how confident the eyewitness might be.

Against this blanket indictment, we have argued that eyewitness memory is highly reliable on the first test conducted early in a police investigation. This is true of both recall (namely, a properly conducted police interview) and recognition (namely, a properly conducted police lineup). Critically, the very act of testing memory contaminates it, so the reliability of eyewitness memory is never higher than it is when first tested – and never lower than it is when ultimately tested in a court of law in front of a jury.

Wixted et al. (2018: 324) stress in their abstract:

Research convincingly shows that memory is malleable, and eyewitness misidentifications are known to have played a role in most of the DNA exonerations of the innocent. However, we argue here that, like DNA evidence and other kinds of scientifically validated forensic evidence, eyewitness memory is reliable *if it is not contaminated and if proper testing procedures are used.* (Italics mine)

And they start their paper like this: “In the view of many, if there is one fact that has been conclusively established by psychological science over the past 30 to 40 years, it is that eyewitness memory is unreliable. And in one important way, there is no doubt that it is” (ibid.).

Eyewitness stories can only be accurate (because they are not intrinsically accurate) only under certain and well-defined conditions.

Wixted, Mickes, Clark, Gronlund, and Roediger (2015) proposed that eyewitness identification evidence from a police lineup is highly reliable in the sense described above. That is, on an initial test of uncontaminated memory using proper procedures, low confidence implies low accuracy and high confidence implies high accuracy [...]. This is not to suggest that high-confidence eyewitness evidence can achieve the astronomically high

levels of accuracy [...], but we do suggest that high-confidence IDs can achieve levels of accuracy that are far more impressive than is generally believed to be the case. (Wixted et al., 2018: 327)

This refers to identifications of culprits in police line-ups, not to free-form, tales about encounters in the woods.

This is a far, indeed, a very far cry, from what James E. King, Michael A. Woodley of Menie, Matthew A. Sarraf, and J. Richard Greenwell say, “the view that eyewitness memory is highly and inherently unreliable [...] is quite poor” (p. 37). It was strong, and it remains strong. In other words – the certainty and reliability they claim for their Bigfoot data base is not given, as these data have not been calibrated in the way the new scientific research demands. The anecdotes they discuss and analyze were not taken in a controlled way and in “much improved eyewitness memory protocols”. Rather, they are from newspapers files and Bigfoot researchers’ interviews.

A consequence of King et al.’s biased view could be that readers who do not consult the original sources get a wrong impression. King et al. certainly have more confidence in the findings than I can read in the text they quote.

Perception

The authors claim, “[s]keptical views that would have us simply dismiss eyewitness data altogether on the basis of its ‘inherent unreliability’ clearly depend on either misunderstanding or ignorance (or both) of the pertinent science” (p. 42) This seems to me a clear misrepresentation of facts.

And talking about memory and recall does not even take into account how enormously people can misperceive what they see. In two studies I made (Magin, 2020c, 2020d) I have analyzed sightings of UFOs, sea-serpents, Bigfoot, and dragons when more than one witness was present and the witnesses were interviewed separately. The result is that in each and every case, the stories recounted do not agree, with descriptions as varied as can be imagined, say, one witness sees a serpent-like sea monster, the other one with a whale-like body with many fins, and yet both observed the same phenomenon, and both were experienced observers (captains of ships, in this case).

Then, and probably more relevant to the Bigfoot debate, in Germany alone, there are up to three incidents per month where a hunter, an experienced outdoor-man (an official exam is required before people can go hunting in Germany), mistakes a person for a boar or doe, even if the person injured was dressed in bright and unnatural colors. Given the number of three mistakes of such kind per month, or say: even per year, a similar amount of mistaken sightings of bears for Bigfoot in the individual states of the U.S. is not impossible (Magin, 2020a).

If you also take into account that Bigfoot is actually seen and allegedly filmed in all regions of Europe, even though the zoological probability of its existence in Scotland or Ireland, not to mention Germany, is depressingly low, the verdict that eyewitnesses are right about what they allegedly saw is even less credible in itself (Magin, 2020b).

A recently published French study presents 29 cases from 1976 alone (of a total of 138 UFO cases reported in that year) where people mistook the full moon for an alien spacecraft that had landed in their garden – surely, a misperception several times more massive than misperceiving a bear for a Bigfoot (Maillot et al., 2020).

The thesis about the reliability of eyewitness testimonies, which was presented with such enthusiasm, is based on clay feet.

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The Role of Eyewitness Testimony in the Search for Sasquatch

No matter what eyewitness testimony is in the court of law, it is the lowest form of evidence in the court of science. (Neil deGrasse Tyson)

Once during an interview about my research into the possibility of relict hominoids, a caller confronted me with the question of whether I was familiar with the work of Elizabeth Loftus, an American cognitive psychologist and expert on human memory. I replied affirmatively, upon which he pressed that I must therefore realize that all eyewitness testimony was unreliable as evidence and must be disregarded. We discussed some of the context and conclusions drawn from the studies of Loftus and her colleagues, and then their implications for a witness recalling an encounter with an alleged 8-foot, hair-covered, bipedal hominoid. I queried whether he saw a difference between for example, an elderly woman attempting to identify a suspected purse-snatcher from a line-up of subjects vs. an experienced outdoorsperson distinguishing a supposed sasquatch from an assemblage including a bear, moose, elk, wolf and hiker? The verbal sparring continued for a bit until somewhat exasperated, the caller exclaimed that if there were an 800-lb gorilla traipsing around in the woods, someone would have seen it! I replied that indeed people were seeing them, but that he was disallowing their testimony a priori.

The forgoing exchange, as well as Tyson's quip, exemplify the prevailing disregard for anecdotal accounts, as being without merit as scientific data, due to their assumed inherent unreliability. This is a central tenet of the skeptical community, which found recent expression in Darren Naish's, *Hunting Monsters: Cryptozoology and the Reality Behind the Myths* (2017) – "This conclusion is in keeping with copious evidence on how people perform as eyewitnesses. Studies have shown, time and time again, that people are essentially terrible 'data recallers' [...] If cryptozoological literalism has one erroneous belief at its core, it's the idea that eyewitness data is reliable" (p. 208). This view provides a common thread throughout his book and is held, Naish asserts, by the vast majority of zoologists and other scientists, and those well-informed in the science of perception.

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Certainly, the work of Loftus and others has focused attention on the constructive nature of memory and the problem of eyewitness testimony, particularly in the judicial context (Loftus et al., 1989; 1992). In many of the criminal exonerations resulting from subsequent DNA analyses, the initial erroneous conviction resulted from faulty eyewitness identification (Innocence Project, 2020). Bear in mind, research into eyewitness testimony in criminal judications, must contend with erroneous eyewitness reports due to deliberate untruthfulness (Gustafsson et. al., 2019). As alluded to in the opening paragraph, it is a different matter when dealing primarily with honestly reported correct vs. incorrect eyewitness memory. However, it must be acknowledged, especially given the increasing notoriety of the popular notion of man-like monsters, that ulterior motives or expectations may come into play with eyewitness accounts related to this particular phenomenon. On the other hand, the negative stigma associated with alleging an encounter with such a being has long been a deterrent to many potentially reputable witnesses divulging such an experience for fear of ridicule or professional repercussions (personal observation).

In this vein, King et al. (2020), which is the focus of this commentary, provide a timely treatment in light of recent challenges to the generalizations of Loftus and other academic psychologists. They cite and discuss research of the past couple decades, which contrary to the consensus painted by Naish and others, indicates that susceptibility to false memory formation likely has been seriously exaggerated (Andrews & Brewin, 2017; Brewin & Andrews, 2017; Wixted & Wells, 2017; Wixted et al., 2018). This position has elicited some cautionary acknowledgement, e.g.: “I agree with the thrust of Wixted et al.’s argument and welcome their invitation to confront the mass underselling of eyewitnesses’ potential reliability. Nevertheless, we argue that there is a comparable risk of overselling eyewitnesses’ reliability” (Wade et. al., 2018).

Even Tyson’s cautionary statement acknowledges that in science, eyewitness testimony is a form of evidence, at least in an absolute sense, if perhaps deemed of low merit on a relative scale. While on some level I would argue in defense of the qualified utility of eyewitness accounts, as would anyone familiar with the history of zoological discovery, I have always held reservations concerning a large portion of such reports due to their susceptibility to subjective perception and interpretation. The familiar adage “seeing is believing” implies that one must see something before accepting that it really exists. However, the implication of its inverse, “believing is seeing” is that our beliefs shape what we see by imposing bias on our perceptions. Perhaps recent evidence suggests that this caveat has been overgeneralized by the ideological or institutional skeptics, resulting in an unjustified universal rejection of all anecdotal reports a priori.

An example, perhaps by way of control for this expectation bias comes through my personal experience investigating footprint reports. Occasionally, the anecdotal discovery and description of an alleged footprint attributed to sasquatch, or other relict hominoid, is substantiated

through documentation – by photograph and/or by casting. Often, the witness' inferences drawn from the described evidence are not justified. In numerous cases the perceived “footprint” is little more than an isolated indeterminate depression, or sometimes a misidentified bear footprint. The later has proved particularly true in the case of the historical lore surrounding the yeti. In the extreme conditions of high elevation, melting and sublimation render the majority of footprints attributed to yeti entirely indeterminate. Furthermore, ethnomythology has seemingly conflated qualities of a man-like and a bear-like creature, leading to confused identification of those few examples of determinate spoor, most of which are clearly attributable to bear. Through all of this, it has become clear to me that rarely can I rely on the interpretive powers of the witness alone in recounting the nature of their discovery in the absence of corroborating documentation. On that basis, similar caution must be exercised when evaluating eyewitness memory and interpretation of unsubstantiated alleged visual encounters.

I have also harbored concerns over the malleability of memory and suggestibility of perception exposed to external sources of information. The proliferation of social media, the internet, and documentary and entertainment presentations on television have certainly influenced expectations, interpretations and recall of experiences in the field. However, King et al. (2020) draw upon data spanning 1950–1994, utilizing reports collected by John Green and Bobbie Short (database online). From this emerges a profile established prior to these external influences. Still, this profile seems to rest upon repetitive patterns and independent consistencies present among these reports – details of anatomy, behavior and evolutionary/ecological context, predating current scientific understanding. This to a degree that seems largely beyond the ken of a lay populace. Rather than converging on the literary image of a feral hirsute human, what emerges is the particular profile of a robust early hominin, an impression that accurately anticipates the scientific constructs of such, by decades.

A dramatic example of anticipation of scientific understanding comes in a seminal eyewitness encounter, that by Roger Patterson and Bob Gimlin, in 1967. The Patterson-Gimlin film provides not only a remarkable track record to substantiate the report, but a quality cinematographic record of the trackmaker herself. In spite of the noteworthy nature of the film, even perhaps the most open-minded adjudicator, Dr. John Napier, then director of the Smithsonian Institution's primate program, could not endorse it following its inaugural screening before American scientists. His rationale was: “The upper half of the body bears some resemblance to an ape and the lower half is typically human. It is almost impossible to conceive that such structural hybrids could exist in nature. One half of the animal must be artificial.” (Napier, 1973: 91). This conclusion was published in 1973. Shortly thereafter much more complete associated fossilized skeletal remains of our early bipedal forbears, namely *Australopithecus afarensis*, were discovered and analyzed. How were they described to an enthralled public? From the waist up, they look remarkably like a chimp, while from the waist down, they resemble a human – the

very combination of traits, deemed inconceivable, which served as the single sticking point in Napier's mind. One must wonder how his verdict might have read if publication of his book was delayed just a few years?

King et al. (2020) have provided an excellent service in summarizing and making these data available within a context that draws attention to the inherent value of this category of evidence. In establishing a baseline, it will spawn further discussions of the merits and pitfalls of eyewitness testimony. Most significantly, however, it argues convincingly for the inclusion of eyewitness testimony as a foundational tenet in the case for the investigation of the possible existence of a relict hominoid in North America.

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MICHEL MEURGER⁵

Sasquatches, Dragons and Mermen

Sasquatch investigators have so far concentrated on the object of experience rather than the experiencing subject (Halpin, 1980, 18).

The four contributors to *Zeitschrift für Anomalistik* and among them, the sadly missed Richard Greenwell (1942–2005), offer a quite substantial article about the vexing question of the putative North American humanoïd Sasquatch or Bigfoot.

King, Woodley of Menie, Sarraf and Greenwell use the quantitative approach, stating page 7, that they proceed from an eyewitness database assembled “by the late John Green during the period between 1950 and 1994”. This allegiance may be evaluated as problematic. Indeed, sharp methodological objections had been, in the past, raised against this authority. One of the most vocal critics, anthropologist Kenneth Wylie (1980: 169) writes that “he arbitrarily discards all data that doesn’t fit his conception of what the creature must be like or should be like”, and again, (ibid.: 170) that Green uses “a biased sample”, organizing his collection to prove that “his Sasquatch is a ‘gentle giant’, rather similar to a gorilla in its harmless, benign nature, and he simply discredits any reports contrary to this view”.

Strong words, but they are not contradicted by the reading of Green’s booklets. For example, in his first publication *On the Track of the Sasquatch*, the newspaperman comes straight to the point: “Another unanswered question is whether the Sasquatches are dangerous to humans. In the main, it is clear that they are not” (Green, 1968: 75). In his third booklet, Green gives us his interpretation of data: “I consider the Sasquatch to be an ape, probably the direct descendent of what science knows as *Gigantopithecus* [...]” (Green, 1973: 65).

Well, comparing John Green’s portrait of the putative resident of the Canadian woods with what King et al. have worked from his dataset, it seems to me that the four searchers follow mainly his ideological guideline, elaborating, sharpening his rough sketch, or sometimes pruning and correcting it. In 1973, John Green already proposed “Sasquatch Statistics” (ibid.:

5 Michel Meurger is an essayist. In particular, he has studied the rationalization processes of supernatural beings. His published works include: *Lake monster traditions: A cross cultural analysis* (1988) and *Histoire Naturelle des Dragons* (2006). Meurger is the French correspondent for the British magazine *Fortean Times*.

63–66). The present searchers had gone in the same direction. They – albeit cautiously – indirectly seem to endorse Green’s *Gigantopithecus* hypothesis (p. 36).

The four contributors also follow the Canadian newspaperman’s tracks. Rehashing the “gentle giant” image, they write that “a small minority of eyewitnesses reported aggressive behaviors” (p. 33). John Green had noted the Sasquatch elusiveness and his tendency “to avoid humans”, comparing it to cougars (1973: 65). More knowledgeable in recent primatology, King et al. assign such a behavior to great apes and take this coincidence as a proof of the ape nature of the hairy giant (p. 36). Against the prevalently nocturnal habits of the Sasquatch, the four scholars quote the evidence of the “full dataset” and write on p. 35 that a “significant majority of sightings offering relevant data occurring during the day”. Yet, the compiler of the dataset, John Green, had explicitly stated the “nocturnal” behavior of the Sasquatch (1973: 65). Therefore, on this point, the newspaperman was unfaithful to his own source material, vindicating Kenneth Wylie’s sturdy rebuttal.

Now, let us take a look at the simian portrait of the giant of the North woods promoted by the author of *On the Track of the Sasquatch* and apparently endorsed by King, Woodley of Menie, Sarraf and Greenwell. It does not take account of the discontinuities in the representation of the hairy colossus. Indeed, for native Americans, the giants living in the wilderness, were not huge subhumans, apes or apemen. One year ago, Canadian explorer and geographer Adam Shoalts, by his careful study of all the available historical sources, has convincingly demonstrated that what we are used to call today “Sasquatch” is a “mixtum compositum”, a cultural hybrid form, produced by the mingling of gorilla or ape attributes introduced by the Euro-settlers into various indigenous creatures of legend (Shoalts, 2019: 63). Exit *Gigantopithecus americanus*? But belief is one thing, experience another one. How may figures of fiction be encountered, interacted with humans and be accurately described? To understand this process, one has to reserve a place for the Sasquatch in a whole genealogy of monsters localized in the depths of untrodden nature, on snow-capped mountain or under unfathomable waters.

Since late Middle Ages to the eighteenth century, inhabitants of the European Alps have reported their meetings with a whole *Systema Naturae* of flying, swimming and crawling reptiles. Chroniclers of the 15th century mentioned sightings of crocodilian-like dragons on the waves of river floods. They focused on alleged discoveries of their remains. Collectors of wonders paid huge sums to purchase some bones of those creatures. Today, we know that so called “dragon’s skulls” belonged to cave bears (Meurger, 2006), but during centuries, such findings strenghtened the case for “*draco-europaeus*”. Pioneering naturalists like Conrad Gesner, Ulisse Aldrovandi, Johann Jakob Wagner and Johann Jakob Scheuchzer offered extremely precise descriptions of encounters of Swiss and Austrian rustics with dragons. Why are we, today, prone to admit the possibility of the Sasquatch, but to dismiss the dragon of yesterday? Are

Sasquatch witnesses more credible than dragon witnesses? Yet, dragons were as elusive as Sasquatches today. They could be experienced, interacted with witnesses, accurately and convincingly described, allegedly killed, but no authentic dragon's carcasses adorn in 2020 our scientific collections. Nor they are the only denizens of the "goblin universe".

A scholar of the Age of the Enlightenment, Norwegian Bishop Erik L. Pontoppidan stated in his *Natural History of Norway* (1752–53) that in his diocese of Bergen, "there are several hundreds of persons of credit and reputation, who affirm, with the strongest assurance, that they have seen this kind of creature [merman]" (Meurger, 1988: 18).

Again, have we the right to admit tentatively the seriousness of the Sasquatch's case, whereas we banish Norwegian mermen to the purgatory of superstitions? Well, we have to acknowledge the temporal side of the argument. There is a historical embedding of monsters. Mermen were fashionable for scientists in Scandinavia of 18th century, as Sasquatches fit in our contemporary landscape. Fish-tailed humanoids were thinkable in scientific terms in the Age of Rousseau. In 1752, there was still conceptual room for them and, therefore, one can find naturalists ready to collect popular sightings. The individual merman report was prepared, modelled and defined by a collective folk-engineering, to adjust the perceived object to iconic models available in the community (old man of the sea, bearded merman).

Such images unified plural experiences, giving to testimonies a collective coherency just as the representation of the Great Ape today solidifies quite diverse human life experiences. We no longer perceive mermen and dragons because cultural codes of the learned and the layman have changed, pushing such images beyond the boundaries of scientific admissibility. The authors of the present study, as literalists, would of course cringe for my group-induced perceptive model. But if we take account of the complete lack of physical evidence for their putative humanoid, their own naturalistic construction, notwithstanding its high degree of technical evaluation will remain largely speculative.

Nevertheless, I agree with King, Woodley of Menie, Sarraf and Greenwell that we must give all our care to a serious analysis of eyewitness reports. But their too strong an emphasis on natural sciences will be more a hindrance than a help, if we are trying to enlarge the deciphering of data, by submitting, for example, some of the high strangeness cases to a cultural approach. Such a method may indeed, help us to detect potential literary motifs dressed up as factual circumstances.

In my analysis of the alleged abduction of Albert Ostman by a Sasquatch, as reported by John Green (1968: 13–21), I have tried to show that the freeing device used by the logger – Ostman's snuff-box swallowed by one of the giants – corresponded to a prankish version of American and European narratives of offerings of tobacco to wood spirits. One may even find in the

French Alps a total correspondence with the pretended mishap of the Canadian woodcutter; here, a French logger sustained to have been captured by a family of giant apemen. The unlucky prisoner escaped when the sturdiest of the clan begun to scrunch his pipe. Other apemen tried to grab the morsel, a brawl followed and the captive took advantage of the chaos to slip away (Meurger, 1998: 172–175).

I am not implying that all Sasquatch encounters may be reduced to this jolly folklore model. Rather, I would like to suggest that cryptozoologists and hominologists would gain more accuracy and relevance by acknowledging the role of culture in their expertise. Ethnologists and folklorists would then undoubtedly be of a great help to track stereotyped narrative and folk motifs in oral documents.

Finally, even if I do not share their global conclusions, I extol nevertheless the epistemological value of the imposing scholarly study by King, Woodley of Menie, Sarraf and Greenwell. They have achieved a notable contribution to the trying field of Sasquatchiana.

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Identifying the Statistical Population of Relevance is Important

This is an interesting paper but, alas, the authors' conclusions are rendered moot by their rather idiosyncratic use of statistics.

It is important to identify the population under consideration when undertaking a statistical analysis and here it is essential. Are the authors making conclusions about bigfoots, bigfoot reports, John Green's reports or a subset of John Green's reports? If it is not the latter two (which would be uninteresting except to Green's biographer) then the authors really have to show that there are no systematic biases in their data analyses or collection that would frustrate their conclusions. Or more realistically/practically, justify an assumption that there are probably no such biases. This is especially the case here because the analyzed data set is a subset of the entire available data and authors are testing for consistency. Two thirds of the data has been removed using vague criteria. It is hardly surprising that the remainder is consistent. The authors now say that the rejected reports were not coded but an earlier draft stated that the rejected reports were "irrelevant and indirect" implying there was content based criteria for rejecting the reports. As they are asking how consistent the reports (or sightings or bigfoots?) actually are, they really have to demonstrate that the reports considered, are an unbiased sample of whatever unstated statistical population the authors are seeking to make conclusions about. How do they know, for example, that the coded reports were not the reports that were regarded as most interesting to John Green or someone else? Reports that might have been assembled using a criterion like "most fitted with John Green's view of what a bigfoot report should be" i.e. the reports would be consistent because they were chosen so to be! Such subtleties of data consideration are not mere pedantry but absolutely vital to the business of making the right conclusions from data here. In the case of Loch Ness, for example, we know data collectors were throwing away reports that did not fit their preconceptions. Therefore the analysis proposed here would be, if performed on Loch Ness data reports, worthless, unless the researcher was interested in the sociology of the monster reporting.

The use of χ^2 test is odd but not necessarily wrong if one accepts the argument that if everybody reported the same thing, it is, sort of, indicative of the underlying accuracy/reliability of the witnesses. I can accept that but a lot of statisticians would not. However for that argument to work, the authors need to show (in each case) that the fact there is a big modal class is not a function of the binning or the arbitrary selection of data by themselves, Green, the data coders or somebody else. They do not do this. For example, the significance of the first day-night result is down to how the authors classify the twilight periods.

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Chi-squared is, of course, often used as a goodness of fit test where the frequencies of the data are compared to some theoretical distribution. Indeed the “mode test” employed here, if it does what I think it does, tests a hypothesis of 50:50 comparison of the modal class to everything else. In this case, the obvious thing to do with regard to time of day of reports for example, is to compare the counts of bigfoot (reports/sightings) to the expected hours of daylight (for a given latitude, at a particular time of year) as surely the value of an analysis here is to find out if the reports are *disproportionately* nocturnal/crepuscular/diurnal. The “mode test” employed here is not a test of that. It is rather a test of whether there are more reports from day (from the unknown population of interest) compared to night (from the unknown population of interest). So the authors’ odd methodological approach blinds them to testing the most interesting hypotheses about their data.

The fitting of the cumulative curves is strange. The authors don’t use an asymptotic curve as is the norm for most ecologists instead fit a quadratic curve after a visual comparison to one class of the former. However I refitted both functions and the curves are pretty much identical over the range of the data, so I am utterly bamboozled as to how they can claim that the curves are readily distinguishable by eye.

The authors are a little opaque in their description of their curve fitting. They do not state the assumed error distribution, the range of data considered (1758–2017?, every year or just years when a change occurred as plotted in their figure?) which are rather critical in constructing the confidence interval. In trying to reproduce their results I assumed normal errors using data from every year (1758–2017), I found the inflection point to be 2107 so the point estimate of the cumulative total number of species is 26 with a 95% confidence interval of 25–28 (rounded to whole numbers) which does not indicate more species given the current known total is also 26. However, this is an underestimate of the uncertainty because years are not independent because of the autocorrelation in the residuals.

Estimating the uncertainty for these type of curves is not trivial given this autocorrelation in the residuals. A claim of species awaiting discovery requires the current number of species to be below the confidence interval associated with the point estimate of the total number of species. As far as I can tell, the quadratic analysis does not support this conclusion.

In summary, their major conclusions are based on assumptions which are not mentioned or are unjustified and some of their calculations are possibly wrong. I urge the authors to do a comprehensive in depth analysis of their data (assuming the quality is such, an analysis can be done) with explicitly and fully stated assumptions and caveats, and clearly identified statistical populations for generalization, otherwise, their work comes across as advocacy rather than science.