Release the Kraken? Well-Controlled and Dangerous Speculation in Geohistory

Abstract

This paper is about the wilder side of speculation in geohistory. Specifically, it is about what I call “dangerous speculation” and the circumstances under which it is likely to be well-received. Dangerous speculation is speculation that departs from an ideal of “well-controlled speculation” in one or more of several ways. These departures correspond to familiar epistemic sins and are individually sufficient to render a hypothesis epistemically suspect. But an epistemically suspect hypothesis may still be regarded as viable under certain circumstances. This paper explores these circumstances using three examples from geohistory. These are the “Triassic kraken hypothesis,” the “vendobiont hypothesis” and the “Nemesis (or Death Star) hypothesis.” It asks two major questions. First, what accounts for the relatively enthusiastic reception of the vendobiont hypothesis, the more complicated reception of Nemesis, and the heckling dismissal of the Triassic kraken? And second, what epistemic lessons (if any) can be extracted from this comparison?

**Monday, October 10, 2011. Minneapolis, MN**

The halls of the Geological Society of America meeting buzzed with anticipation.

“Have you heard?”

“About the kraken guy?”

“Mark McMenamin. Apparently he teaches at some little school out east.”

“I heard he’s a kook.”

“He’d have to be, wouldn’t he? Anyways, I’m going to head over early to make sure I get a seat.”

“Let me know how it goes. I’ll be doing, like, anything else.”

The talk on everyone’s lips began at two in the afternoon. Half an hour before, the medium-sized conference room had reached capacity. Latecomers were forced to huddle outside its open doors and crane their necks at the spectacle. Later they would leave, snickering and shaking their heads. Serious minded geologists cried foul. Others cracked jokes. The press, who had caught wind of the talk in advance of the meeting, ate it up. Major outlets do not usually cover meetings of the GSA; the time when the general public cared about meetings of geologists had drawn to a close in the United States about 150 years earlier. Still, even the most hardened philistine had to admit that giant sea monsters make great copy.

The talk bore the B-movie title “Triassic Kraken,” and the rather more academic subtitle “The Berlin Ichthyosaur Death Assemblage Interpreted as a Giant Cephalopod Midden [refuse heap].” Its abstract, co-authored by the husband and wife team of Mark McMenamin and Diane Schulte McMenamin, makes for good reading.

The Luning Formation at Berlin-Ichthyosaur State Park, Nevada, hosts a puzzling assemblage of at least 9 huge (≤14 m[eter]) juxtaposed ichthyosaurs…comparable to sperm whales…Adjacent skeletons display different taphonomic histories and degrees of disarticulation, ruling out catastrophic mass death, but allowing a scenario in which dead ichthyosaurs were sequentially transported to a sea floor midden. *We hypothesize that the [ichthyosaurs] were killed and carried to this site by an enormous Triassic cephalopod, a “kraken,” with [an] estimated length of approximately 30 m[eters], twice that of the modern Colossal Squid Mesonychoteuthis. In this scenario, [the ichthyosaurs] were ambushed by a Triassic kraken, drowned, and dumped on a midden like that of a modern octopus.*

This is perhaps excitement enough for one talk, but the authors go on to note that the ichthyosaur vertebrae “are arranged in curious linear patterns with almost geometric regularity.” What follows would become an infamous claim:

The proposed Triassic kraken, *which could have been the most intelligent invertebrate ever*, arranged the vertebral discs in biserial patterns, with individual pieces nesting in a fitted fashion as if they were part of a puzzle. The arranged vertebrae resemble the pattern of sucker discs on a cephalopod tentacle…*Thus the tessellated vertebral disc pavement may represent the earliest known self-portrait.*

Media reports of the talented cephalopod began to trickle out before the talk was even over. All relied heavily on the press release from the GSA and presented the case for the kraken in a generally positive light. However, with the entry of the science blogs into the picture, the tone of coverage took a turn. Developmental biologist PZ Meyers (*Pharyngula*)called the whole thing “rather pathetic,” and complained that the evidence for self-portraiture was particularly thin. “[D]ump a pile of Necco wafers on a table, and I’ll see a picture of squid suckers.” Freelance science writer Riley Black (then writing under the name Brian Switek) added a piece for *WIRED* titled “The Giant Prehistoric Squid That Ate Common Sense.” She was also critical of the evidence, but reserved her harshest criticism for science journalists: “Whether you think the ‘kraken’ story should have been reported or ignored due to lack of evidence, the fact remains that journalists should have actually done their jobs rather than act as facilitators of hype.” Finally, the vertebrate paleontologist and textbook author Donald Prothero weighed in on the blog of the Skeptic Society. His piece, titled “Octopus’ Garden in the Shale,” is perhaps most notable for its assessment of McMenamin himself:

The minute I saw the press release, I suspected something like this was going to happen. McMenamin is well known for his, shall we say, bizarre ideas or unorthodox ways of thinking…Those of us with a history in the profession know better than to take his wild claims too seriously.

Major news outlets, alas, were not so wary. And so the Triassic kraken was released on a rapt but ingenuous public.

The kraken was not long for the news cycle, but this was hardly the end of the line for the prehistoric beast. It first resurfaced in a magazine article summarizing evidence for the hypothesis (McMenamin 2012), before showing up for a second romp at the GSA (McMenamin and Schulte McMenamin 2013). Another round of media attention followed, centered on McMenamin’s claim to have found independent evidence for the kraken in the form of a beak. Eager to forestall the hype, the Paleontological Society issued a response by David Fastovsky, which appeared in most news stories discussing the new abstract. Then, things settled down. The kraken was last seen in the ninth chapter of McMenamin’s book, *Dynamic Paleontology*, where it is claimed that the Triassic kraken hypothesis “has survived all tests to date, and currently stands alone as the best explanation for the strange collection of large ichthyosaur bones at Berlin-Ichthyosaur State Park” (McMenamin 2016, p. 131). Few outside McMenamin’s household seem to share this assessment.

**Plan of the Paper, or What's the Matter with a Giant Kraken?**

The Triassic kraken hypothesis is almost certainly wrong. As critics observed, evidence for a kraken is wholly circumstantial (*pace* laterreports of a beak), and the idea that the bonebed is a midden flouts the principle of parsimony.[[1]](#footnote-1) Claims of hyper-intelligence are particularly extravagant. Writes Black, “I guess a giant, ichthyosaur-eating ‘kraken’ wasn’t enough. A squid with a stroke of artistic genius was *clearly* the simplest explanation for the formation of the bonebeds.”

But so what? It is no sin for a scientific hypothesis to be wrong. Nor is it necessarily wicked to sin against parsimony. It is true that simplicity provides a convenient standard for evaluating explanatory claims. But it is hardly unheard of for more complicated hypotheses to win out over simpler ones. Even a seemingly implausible hypothesis may have its virtues. In a famous article, the American geologist William Morris Davis defended the value of “outrageous” hypotheses on the grounds that judgements of outrageousness are calibrated to contemporary knowledge, which is fallible (Davis 1926). As he observes, many things that geologists now take for granted were once regarded as outrageous. But this just means that many advances in geology were “made by outraging in one way or another a body of preconceived opinions” (p. 464). It follows that scientists should speculate—wildly, even—assuming their speculations do not controvert robustly established results (and sometimes even then). Adrian Currie has recently made a similar suggestion: “Especially when the going gets tough…historical science should be wild, messy and creative [i.e., speculative]” (Currie 2018, p. 291).

This paper is about wild, messy, and creative speculation in geohistory. Specifically, it is about what I call *dangerous speculation* and the circumstances under which it is likely to be well-received.[[2]](#footnote-2) Dangerous speculation is speculation that departs from an ideal of *well-controlled speculation* in one or more of several ways. These departures correspond to familiar epistemic sins and are individually sufficient to render a hypothesis suspect. But an epistemically suspect hypothesis may still be regarded as viable under certain circumstances. This paper explores this phenomenon using case studies from geohistory. All are outrageous and almost certainly wrong; in addition to the Triassic kraken, I consider the “Nemesis” (or “Death Star”) hypothesis, as well as Adolf Seilacher’s interpretation of the Ediacaran biota (the “vendobiont hypothesis”). But these outrageous hypotheses elicited very different reactions from the paleontological community. Why? What accounts for the relatively enthusiastic reception of the vendobiont hypothesis, the more complicated reception of Nemesis, and the heckling dismissal of the Triassic kraken? And what epistemic lessons (if any) can be extracted from the comparison?

The remainder of this paper is organized in three sections. In “Death Star and the Quilted Pneu,” I introduce the remaining outrageous hypotheses, paying special attention to their motivation and reception. Then, in “Well-Controlled and Dangerous Speculation,” I review recent philosophical work on speculation in geohistory and develop my account of well-controlled and dangerous speculation. Finally, in “Dangerous Speculation in the Balance,” I use the resources developed earlier in the paper to analyze the reception of the hypotheses and to extract some provisional epistemic conclusions.

**Death Star and the Quilted Pneu**

*1. The Nemesis Hypothesis*

Few paleontological phenomena are as dramatic as mass extinction. Although the category is inherently fuzzy, paleontologists recognize five major extinction events in the past half billion years, with species losses ranging from sixty to ninety percent per event (Erwin 2015). There are also a host of smaller extinctions in the fossil record, including some that may warrant the epithet “mass extinction.” It has long been suspected that these extinctions occur more or less randomly throughout the geological column. However, in the mid-1980s, two paleontologists produced an analysis suggesting that major extinctions have occurred with an approximately 26-million-year periodicity over the past quarter billion years. This is the source of what has been called “the Nemesis affair” (Raup 1999); but before coming to the Nemesis hypothesis, some historical context remains to be filled in.

The study of mass extinction was forever changed in 1980, when Walter Alvarez and colleagues proposed that a large body impact polished off the dinosaurs (and many other groups) at the end of the Cretaceous Period (Alvarez et al. 1980). Prior to this time, most geologists favored earthbound causes for major extinctions: shifts in climate, worldwide mountain-building episodes, volcanism—pretty much anything you can think of. However, as evidence mounted for an impact at the end of the Cretaceous, a vogue set in for extraterrestrial causes of the Hollywood variety (Sepkoski 2020). Not a few paleontologists wondered if large body impact might explain allthe major extinctions in the fossil record: a possibility Alvarez and colleagues suggested in observing that the number of major Phanerozoic extinctions (five) “matches well the probable interval of about 100 million years between collisions with 10-km-diameter objects” (Alvarez et al. 1980, p. 1107).

One of the people who wondered about this was David Raup. A leading analytical paleontologist, Raup was greatly exercised by the excitement surrounding the Alvarez hypothesis. In addition, he was interested in the waxing and waning of taxonomic diversity over time: an interest he shared with his junior colleague J. John (“Jack”) Sepkoski, Jr. (Sepkoski 2012). Like Raup, Sepkoski was an important analytical paleontologist, who had become well known for compiling data on the stratigraphic ranges of all known marine fossil families.[[3]](#footnote-3) Using these data, Raup and Sepkoski published an analysis in 1982 purporting to show that “five mass extinctions are clearly defined in the familial data” (Raup and Sepkoski 1982, p. 1502). This is the origin of the idea that there have been five major extinction events in the past half-billion years. But this was only an appetizer, and two years later, the duo returned with a more sensational conclusion. Based on a qualitative examination of patterns in Sepkoski’s data, Raup and Sepkoski noticed that “[major] extinctions seemed to be regularly spaced in time, or at least far more regularly spaced than if they had been placed at random” (Raup 1999, p. 115). Intrigued, they used “every standard and non-standard mathematical technique [they] could find or devise” to refute the hypothesis, but to no avail (p. 122). This led them to conclude that “a 26-[million-year] periodicity” is an “inescapable” feature of the post-Permian extinction record (Raup and Sepkoski 1984, p. 804). Somehow, like clockwork, the earth had tipped into calamity at regular intervals over a quarter billion-year period, with only two “misses” against eight statistically significant “hits.”

There was a problem, however. Assuming extinction periodicity is real, what on earth could have caused it? Raup and Sepkoski could think of no earthbound process that operates regularly over so long an interval. So, *faute de mieux*, they favored a non-earthbound cause, like “the passage of our solar system through the spiral arms of the Milky Way” (Raup and Sepkoski 1984, p. 804). This seemed to operate on about the right time scale, and would be suitably periodic in its effects. However, the suggestion served mainly as an example of the *kind* of cause that might be implicated in producing an extinction pulse every 26 million years. In Raup and Sepkoski’s forgivable understatement, “much more information is needed before definitive statements about causes can be made.”

The publication of Raup and Sepkoski’s paper caused a minor scandal, in part because it appeared in *PNAS* and so evaded the full rigors of peer review (Raup 1999). Paleontologists were quick to criticize it, claiming, for example, that Sepkoski’s data were too incomplete to permit a demonstration of periodicity, or that uncertainties in geological dating scuttled the whole enterprise. Lurking behind these objections was the intuition that extinction cannot be periodic in a world governed by Darwinian processes. Yet scientists outside of paleontology evidently lacked these hang-ups, and seemed to take claims of periodicity at face value. Astronomers in particular were well disposed, and in the April 19, 1984 issue of *Nature*, five papers appeared that proposed extraterrestrial mechanisms for periodic extinction. Two of these proposed that extinctions were caused by the movement of the sun relative to the galactic plane (Schwartz and James 1984; Rampino and Stothers 1984); but given that the sun is now close to the galactic plane, and we are not due for another extinction for 14 million years, these were dead on arrival. Two others summoned a companion star to do the killing (Davis et al. 1984; Whitmire and Jackson 1984).[[4]](#footnote-4) One even gave the star a name: Nemesis.[[5]](#footnote-5) Perhaps inevitably, it was later vulgarized to the “Death Star” hypothesis.

The Death Star hypothesis imagines a dim companion star to our sun traveling on a highly eccentric (non-circular) orbit. Once per revolution—that is, once every 26 million years—the star passes through the Oort cloud: a hypothetical envelope of comets lying beyond the edge of our solar system. This produces a gravitational disturbance that sends a shower of comets hurtling into the space of the inner planets. It is the impact of one or more of these comets with the earth that triggers mass extinction: although the comets may also miss the earth, or fail to trigger an extinction, thus accounting for anomalies in the 26-million-year pattern.

Now, this must rank as an outrageous hypothesis, not only because it accepts the evidence for periodicity at face value, but also because it presses into service *two* hypothetical entities, Nemesis and the Oort cloud, one of which is known only from the extinction data. For some, this was all it took to rule the hypothesis out of court. So, an anonymous *New York Times* editorialist ended their letter by scoffing that “Astronomers should leave to astrologers the task of seeking the cause of earthly events in the stars” (Anonymous 1980). The jeer was not representative of the generally positive treatment Nemesis received in the popular press, yet it probably captured the dominant attitude among paleontologists, many of whom had yet to warm to the Alvarez hypothesis, to say nothing of extinction periodicity (Raup 1999). Anthony Hallam stated the obvious when he wrote that “[in] assessing the value of these speculative [astronomical hypotheses] it is clearly necessary first to scrutinize the Raup and Sepkoski analysis on which it is based” (Hallam 1984, p. 686). Suffice it to say Hallam was unimpressed. His skepticism seemed to be confirmed when paleontologist Anthoni Hoffman published an analysis purporting to show that “evidence for [periodicity] is strongly contingent on arbitrary decisions concerning the absolute dating of stratigraphic boundaries, the culling of the database and the definition of what is mass extinction as opposed to background extinction” (Hoffman 1985, p. 659). At least this suggested that it was overzealous to claim, as Raup and Sepkoski had, that periodicity is an “inescapable” feature of the post-Permian fossil record.

Periodicity had at least one prominent advocate, however, and it just so happened to be the paleontologist with the biggest bully pulpit of all. This was Sepkoski’s PhD supervisor Stephen Jay Gould, who threw his considerable weight behind the Nemesis hypothesis in a *Natural History* essay of 1984. Gould stopped short of saying that he expected Nemesis to be found; he only said that he hoped it would be found, and that he regarded the prospect as a respectable one. He pleaded, however, that if astronomers should discover the star, they would not name it after the “personification of righteous anger” (Gould 1985, p. 447). This would only reinforce the old view of mass extinction in which the vanquished literally get what they deserve. But in the wake of the Alvarez hypothesis, a new view of extinction had begun to assert itself that demanded a more apt name. Gould recommended “Siva,” the Hindu god of destruction, whose “placid face represents the absolute tranquility and serenity of a neutral process, directed toward no one but responsible for maintaining the order of the world” (p. 450). He added, blushingly, that “I can only hope that I will not be remembered as the man who campaigned with a new name for [a] nonexistent [star].”

Alas, it has been almost forty years, and no sign of Nemesis has been found. This is not for want of effort; multiple astronomical surveys since the 1980s have looked for a companion star or a similar celestial object, and all have come up empty. It is therefore reasonable to conclude that Nemesis does not exist. The hypothesis of extinction periodicity, on the other hand, refuses to go away. Most recently, Adrian Melott (an astrophysicist) and Richard Bambach (a paleontologist) argued for a 27 million year periodicity in extinction spikes, although not necessarily for periodic mass extinctions (Melott and Bambach 2010). Their preferred explanation is that “some periodic stress…at a 27 [million year] period, adds a background on which a variety of pulse events from a variety of causes [may be] promoted to major extinctions events by the additional stress” (Melott and Bambach 2017, p. 919). They place no bets on the identity of the stressor, but it is notable that the Nemesis hypothesis receives only a single mention in their most recent paper, in a sentence stating that any mechanism for periodicity should “require no new physics and avoid the irregularities that beset the Nemesis and galactic oscillation models” (see also Melott and Bambach 2010). The Nemesis affair is accordingly over.

*2. The Vendobiont Hypothesis*

The soft-bodied organisms of the Ediacaran biota are an enduring puzzle in evolutionary paleontology (Runnegar 2021). Dating from just before the Cambrian Explosion, they are preserved mainly as a collection of shallow casts and molds resembling no living animals in particular. They came to prominence through the efforts of Martin Glaessner, an Austrian paleontologist who assigned most taxa to living groups on the basis of supposed structural homologies. Pancake-like *Dickinsonia* he described as a segmented worm (Glaessner 1961). Foliate *Rangea* he described as a “sea pen” (Glaessner 1959). Slug-like *Kimberella* (neé *Kimberia*)he assigned to the medusoids (Glaessner and Wade 1966). And the enigmatic *Parvancorina* and *Praecambridium* he assigned to the arthropods (Glaessner and Wade 1971). Glaessner was under no illusion that these organisms were modern animals in disguise. Still, his decision to assign them to living groups was so successful that he could claim, near the end of his life, that while “questions have been raised and doubts [expressed] about the placing of some species…no major changes in the assessment and composition of the fauna have been made in the last 25 years” (Glaessner 1984, p. 51).

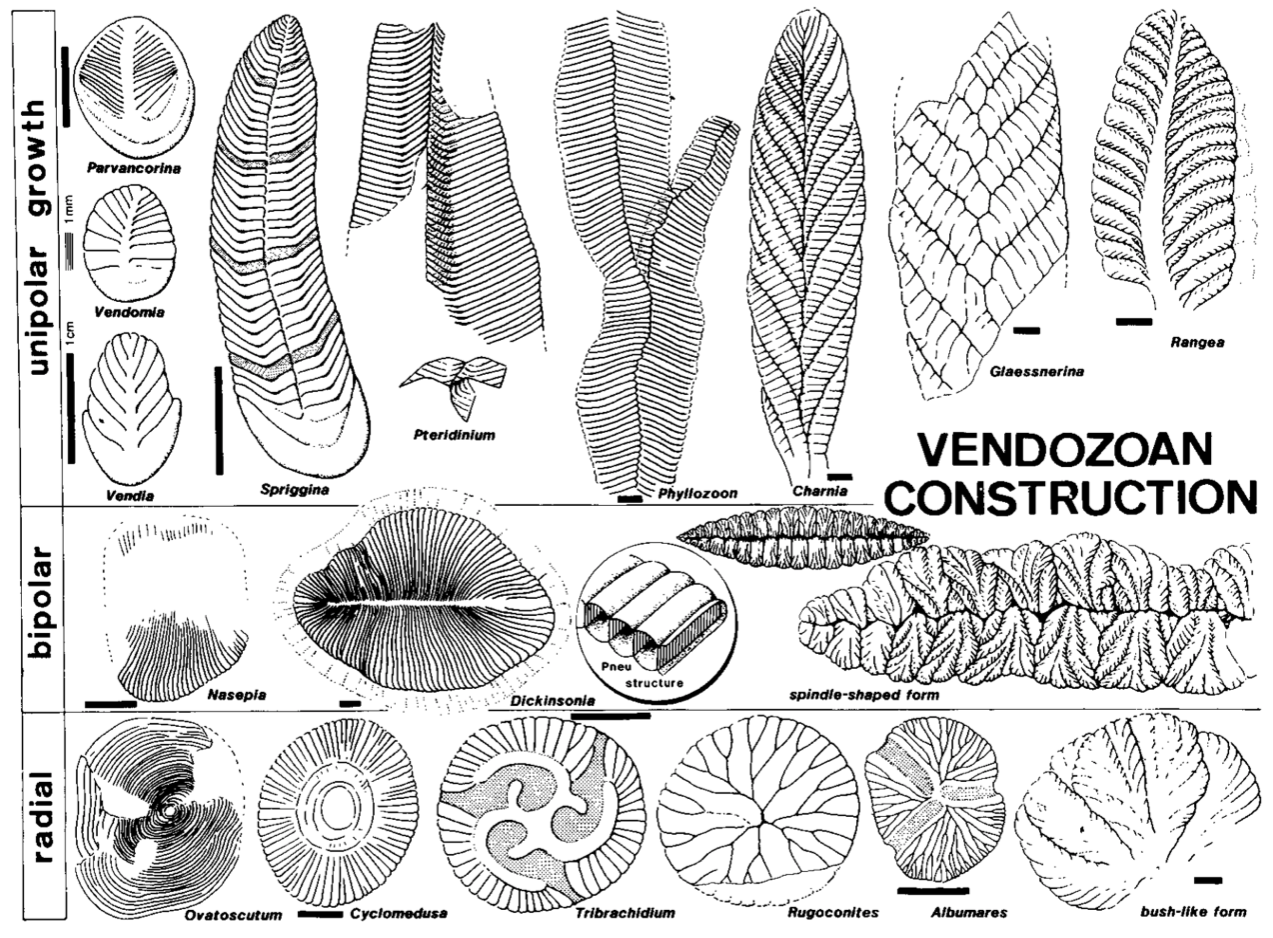
Glaessner wrote these words in 1984. At the time, the basics of his interpretation seemed well nigh unassailable. Over the course of three decades he had shown that the earliest large animals in the fossil record could be compared with some of the most primitive animals living today. A steady stream of new fossil discoveries did little to shake this conviction. Ediacaran paleobiology came to seem a matter of filling in the details of the picture drawn by Glaessner and his associates.

Then came Adolf Seilacher. Invariably described by his colleagues as “a marvelous observer” (Fortey 1997, p. 81) or even as “the finest paleontological observer now active” (Gould 1989, p. 312), Seilacher was not a specialist in Precambrian paleontology.[[6]](#footnote-6) Still, he had the gall to propose that the specialists had it all wrong—not just the details of particular analyses, but the most basic features of their interpretation. (Possibly this is related to the other thing colleagues said about Seilacher: that he possessed a strong didactic streak “which comes from a conviction that he is right about every issue” (Fortey 1997, p. 81).)

Seilacher’s interpretation of the Ediacaran biota is based on what he terms “constructional morphology” (Seilacher 1989). Regarded as a method, it involves the analysis of form in relation to three factors: history (“phylogenetic tradition”), adaptation (“biological function”), and *Bautechnik* (“morphogenetic fabrication”). The details of the analysis are not always so clear (cf. Reif et al. 1985). Yet it is evident that the purpose of the analysis is to avoid the pitfall of treating any one factor as the key to understanding. Presumably this is what Glaessner did when he focused on structural homologies (phylogenetic tradition) to the neglect of functional and fabricational considerations. Seilacher, by contrast, set out to interpret the impressions on “purely constructional principles”: an “uncomfortable” strategy, he admits, but one that is presumably necessary since “modeling the Ediacaran body impressions in terms of modern phyla appears to be in conflict with basic functional necessities” (Seilacher 1989, p. 232).[[7]](#footnote-7)

It is important to be clear about what it means to interpret fossils on “purely constructional principles.” Basically, it means to reconstruct the morphological design of extinct taxa *without the help of metazoan analogies*, and then to interpret this design in functional terms, again, *without the help of metazoan analogies*. This is indeed an “uncomfortable” strategy, since it practically guarantees that interpretations will be highly speculative. But it is also just plain difficult, since in eschewing comparisons with living organisms, it declines the assistance of a powerful method for constraining interpretations. It is a bit like fighting with an arm tied behind one’s back—something that is generally a bad idea, unless one is in the habit of using that arm to punch oneself in the nose.

In any event, Seilacher’s constructional analysis is a particularly outrageous bit of paleontological speculation. It holds that the iconic Ediacaran fossils, from the “worm” *Dickinsonia* to the “sea pen” *Rangea*, are representatives of an “exotic principle of organismic construction” unique to the Ediacaran interval (Seilacher 1989, p. 230). This principle is based on a shared constructional element called a “pneu structure”: basically, a cylindrical tube filled with fluid. Ediacaran organisms were compounded of many such structures, quilted together to produce “carpets” reminiscent of air mattresses (Figure 1). How they lived is a mystery. They evidently lacked mouths and anuses, as well as sensory and locomotive organs. Perhaps they fed upon the microbial mats that lined the Precambrian seafloors, but Seilacher’s hunch was that they harbored microbes capable of extracting energy from seawater.[[8]](#footnote-8) Whatever the story, it is at this juncture that “the disadvantage of [the purely constructional] approach” is most evident, since all the investigator has to go on are the “basic [non-taxon-specific] principles of physiology,” and these provide little guidance for interpreting particular specimens (pp. 236–7).



**Figure 1.** Seilacher’s drawing of vendozoan morphology, based on the shared constructional element of the “pneu structure” (middle row, center). According to Seilacher, vendozoans had three modes of growth—unipolar, bipolar and radial—which enabled them to achieve a range of distinctive morphologies, limited by the need for pneu “quilts” to retain the same order of cross-sectional size. Notice that this illustration depicts several of the iconic Ediacaran forms, including *Dickinsonia* (formerly interpreted as a worm)and *Rangea* (formerly a sea pen)*.*

Seilacher (1989) abstains from speculating about the taxonomic affinities of his carpet organisms. However, in his (1992) he makes the bold suggestion that they constitute a new *kingdom* of life (the Vendobionta) that became extinct at the end of the Precambrian.[[9]](#footnote-9) Possibly this is because the lifestyle permitted by their “quilted hydrostatic construction” became obsolete with the advent of mobile predators. Such predators would have found these biological carpets an easy mark, and as Seilacher speculates, “even small bites would probably have led to fatal leakage from the living bags” (Seilacher 1992, p. 607). Unfortunately for the vendobionts, their capacity to mount an evolutionary response was constrained by another feature of their construction. This is that vendobionts became large by “[s]ubdividing a syncytial protoplasm by quilting.” Vendobionts were accordingly unicellular, and this served to limit the possibilities for tissue differentiation and specialization. Altogether, vendobionts could not hack it in a world of eyes, muscles, and jaws, and for this reason succumbed to mass extinction around the time of the Cambrian radiation (Seilacher 1984).

How were these conjectures received? Certainly they raised some hackles. Emblematic is the response of Jim Gehling, an Ediacaran paleontologist whose affability is as fabled as Seilacher’s powers of observation. In a festschrift honoring Martin Glaessner, Gehling subjected Seilacher’s conjectures to a searching examination and critique. His basic claim is that attempts to assess “[the] construction and life style of Ediacaran organisms should be preceded by a close study of the preservational characteristics of particular taxa, from as many different settings as possible” (Gehling 1991, p. 185). In Australia, for example, many Ediacaran impressions are found in trace-fossil bearing sandstones: an unlikely repository for soft-bodied fossils. This suggests that one of two things must be the case. Either “something was different about the Ediacaran organisms” (Seilacher’s view) or “the process of preservation was different.” Gehling favors the latter alternative, in part because “sandstones of Ediacaran age bear textures rarely described in both older and newer sediments.” Particularly noteworthy are the elephant skin textures indicative of the “prolific development of cyanobacterial mats” (p. 218). These would have served “both to erosion proof the substrates occupied by the organisms [after burial]…and to initiate immediate mineral encrusting of organic surfaces.” It follows that there is no need to resort to exotic conjectures to explain the fossilization of Ediacaran organisms: unusual taphonomic conditions can do the trick.

This is probably Gehling’s most damning criticism, since Seilacher was a leading expert in the science of fossil preservation. But no less serious are his allegations that Seilacher misinterpreted the fossils themselves, perhaps because he was not sufficiently acquainted with them. Writing of *Dickinsonia*, Gehling observes that “*[e]xamination of several hundred specimens* *of this taxon* reveals the presence of several characteristics that the ‘air mattress’ model cannot accommodate” (Gehling 1991, p. 195, emphasis added). For example, most specimens show a clear distinction between their front and back ends, contrary to Seilacher’s claims. Some specimens also exhibit a crinking of individual segments suggestive of muscular contraction and the possibility of locomotion. In Gehling’s view, these features “point to *Dickinsonia* as a functioning coelomate-grate [animal], able to react to sensory input” (p. 198). Similar difficulties attach to other of Seilacher’s interpretations, leading Gehling to conclude that it is “[o]nly with a very broad brush [that] all Ediacaran organisms [can] be represented as fractal growth variations based on the same units of construction” (p. 202).

Despite these apparently serious criticisms, the vendobiont hypothesis “initially attracted considerable support from paleontologists (Dunn and Liu 2019, p. 513). Jan Berström observed in 1991 that “many of the [Ediacaran] organisms…probably belong to a group of ‘quilted organisms’ which may not be animals and exhibit no close similarity to bilaterians” (Bergström 1991, p. 32). Several years later, Guy Narbonne followed Seilacher in arguing that some Ediacarans probably represented “a failed experiment in Precambrian evolution” (Narbonne 1998, p. 1).[[10]](#footnote-10) These remarks show a direct uptake of at least certain components of Seilacher’s interpretation. Yet arguably a more significant development was that it “turn[ed] the discussion of Ediacaran affinities from a monolith [into] a free-for-all” (Narbonne 2003, p. 431). In the fifteen years following Seilacher (1989), members of the Ediacaran biota were interpreted as protists, lichens, fungi, colonial prokaryotes, and extinct photosynthetic “metacellulars” (the last suggestion owed to McMenamin). At the same time, the vendobiont hypothesis “stimulated research in comparative biology, taphonomy, and ecology in an attempt to deduce the affinities of these pivotal fossils.” This research has undermined Seilacher’s interpretation in anything resembling its original form (Dunn and Liu 2019; Runnegar 2021). Still, what is more significant is that research on diverse aspects of Ediacaran biology remains a going concern. Today there are more researchers studying the Ediacaran biota than at any point in the past, and this seems unlikely to change any time soon.

**Well-Controlled and Dangerous Speculation**

So much for my examples of outrageous speculation in geohistory. The remainder of this paper attempts to make sense of them by developing and then applying the concepts of “well-controlled” and “dangerous speculation.” The former task is partly a matter of extending some recent discussions of speculation in the historical sciences, so I begin this section with a brief review.

The literature on speculation in the historical sciences is mostly a response to the account put forward in (Currie 2018). According to this account, historical scientists are warranted in practicing “empirically-grounded speculation” whenever they find themselves in “unlucky” circumstances. Circumstances count as “unlucky” when direct (trace) evidence is scarce or highly decayed, rendering hypotheses about the past massively underdetermined. Speculation is “empirically grounded” when it outruns the empirical evidence and “generates epistemic or empirical goods that increase epistemic traction” (Currie 2018, p. 289). So, Currie’s insight is that speculation that outruns the evidence can scaffold inquiry in situations of evidential scarcity, guiding the search for evidence and facilitating the development of new techniques and approaches that might not otherwise be developed. And the unluckier the circumstances, the longer the leash for speculative adventures: “Especially when the going gets tough…historical science should be wild, messy and creative” (Currie 2018, p. 291).

The first thing to say about this account is that it is normative. Its aim is to recommend speculation as an appropriate response to certain epistemic situations, which Currie takes to be typical of the historical sciences: “[in] historical investigation…empirically grounded speculation is *the* way forward” (Currie 2018, p. 290, emphasis added). Apropos of this aim, Currie is more concerned with the benefits speculation can bring than with its drawbacks and pitfalls. However, as Derek Turner (2019) observes, the drawbacks of speculation require scrutiny as well. Two drawbacks, in particular, counteract the epistemic benefits highlighted by Currie. The first involves failures of reflexivity: some speculations manifestly fail “to scramble existing assumptions or nudge us out of our cognitive ruts” (Turner 2019, p. 4). Especially when speculation reinforces an idea that lacks evidential support, this bit of speculation is likely to be epistemically detrimental.[[11]](#footnote-11) The second involves misinformation. Some speculation conveys a positively misleading impression of the subject matter, by ignoring relevant facts or presenting them in a misleading way. This too is epistemically harmful. Responding to Turner, Currie (2019) groups together failures of reflexivity and fidelity-to-data as “egregious speculation.” This refers to speculation that not only fails to advance research towards its epistemic goal, but actively undermines it. In contrast, “idle speculation” fails to advance research towards its epistemic goal without actively undermining it. (Consider an otherwise inoffensive hypothesis that provides no pointers for investigation, if such a hypothesis can be imagined.)

Putting all this together gives us a threefold taxonomy of speculation:

1. “Empirically grounded” or *productive* *speculation*: speculation that helps a community reach its epistemic goals by opening new routes to empirical knowledge
2. *Idle speculation*: speculation that neither promotes nor thwarts a community in reaching its epistemic goals
3. *Egregious speculation*: speculation that actively thwarts a community in reaching its epistemic goals

Notably, this taxonomy traffics in results, and is therefore mute about what makes speculations productive, idle, or deleterious (“egregious”) in the first place. However, Turner’s remarks indicate that we are not entirely in the dark here. A hypothesis that misrepresents the facts is unlikely to be a productive one. Likewise a hypothesis that reinforces an existing idea lacking evidential support. To investigate the reception of speculative hypotheses, we need more constraints like these: constraints that illuminate why some speculative hypotheses gain traction while others are derided or ignored (Turner 2019, p. 4).

This is what the ideal of “well-controlled speculation”aims to provide. It aims to provide a set of constraints corresponding to (what are often taken to be) minimal epistemic requirements on successful scientific hypotheses. Well-controlled speculation does not provide a standard for judging speculations as productive or unproductive (although extremely uncontrolled forms of speculation are unlikely to yield true claims about the world). Instead, it provides a framework for analyzing bits of speculation in relation to the putative good- and bad-making qualities of hypotheses in general. It will be useful to the extent that it helps us parse different kinds of risky speculation, including speculation that fails to measure up to the ideal while nonetheless finding a favorable reception in a scientific community.

Well-controlled speculation can be characterized in terms of four constraints or requirements. The first is *fidelity-to-data:* speculation should not omit relevant facts or otherwise misrepresent the data it appeals to. The second is *reflexivity:* speculation should not reinforce ideas lacking evidential support or other biases that lead to faulty inferences. The third is *coherence:* speculation should cohere with robustly established results in areas germane to the speculation (if a hypothesis fails to agree with the best knowledge in an area, that is generally points against it). And the fourth is *parsimony:* speculation should not multiply entities beyond necessity, or frame more complicated accounts when simpler ones fulfill the relevant adequacy criteria. Of these, the first and second constraints cannot be relaxed without harm. To relax either the fidelity-to-data or reflexivity requirements is to engage in egregious, or at the very least idle, speculation (Turner 2019; Currie 2019). The third and fourth constraints, by contrast, *are* relaxable under certain circumstances. Relaxing either or both leads to what I call “dangerous speculation.” This is speculation that, while it need not be egregious or idle, is perhaps more likely to represent a false start or dead-end than more well-controlled speculation. Put differently, dangerous speculation is a risky epistemic game, although unlike egregious speculation, it is not self-defeating.[[12]](#footnote-12)

It is clear that dangerous speculation is warranted under certain circumstances. As Davis observed in 1926, many advances in the history of geology were “made by outraging in one way or another a body of preconceived opinions” (p. 464). He might as well have said that many advances in geology resulted from dangerous speculation; and this was before the acceptance of continental drift furnished the best example in the recent history of science of dangerous speculation triumphant (Oreskes 1999). But issues of warrant are not my interest here. Rather, I am concerned with those factors that influence the reception of dangerous speculation, and whether any of them suggest epistemic lessons. To get a handle on this, I turn in the final section to an analysis of the cases presented above.

**Dangerous Speculation in the Balance**

This paper has discussed three examples of outrageous (I would now say “dangerous”) speculation. These met with very different receptions. The Triassic kraken hypothesis was dismissed with either a huff or a snicker, depending on the person. The Nemesis hypothesis, by contrast, garnered a decidedly mixed response. Many paleontologists were skeptical, bordering on dismissive, but others were more responsive, and astronomers seem to have been positively enthusiastic about it. Finally, Seilacher’s vendobiont hypothesis was for the most part well received, even though cogent criticisms prevented its widespread acceptance. What are we to make of this pattern?

The first thing to say is that the pattern clearly has a non-epistemic texture. Raup was a member of the National Academy of Sciences when he speculated about extinction periodicity, and Sepkoski was fast becoming the most influential paleontologist of his generation. Seilacher too was a scientific star: in Gould’s estimate, “the finest paleontological observer now active” (Gould 1989, p. 312). This charged their speculations with a credibility that McMenamin could not access, and in Raup and Sepkoski’s case, enabled them to publish in the widely-read (and lightly edited) journal *PNAS*. Yet McMenamin was far from a nobody in paleontology. His work on the Ediacaran biota was widely known and well cited (e.g., McMenamin 1986), and when Seilacher was awarded the Paleontological Society Medal in 1994, it was McMenamin who presented him the award. This indicates that McMenamin occupied a non-marginal (if non-elite) standing in the paleontological community, at least before he released the kraken at the 2011 GSA. Yet the disparity between his reputation and those of Raup, Sepkoski and Seilacher was considerable, as Prothero’s remarks strongly suggest.

There is no reason to shrink from the implication. Were it possible to perform a factor analysis on the results I have presented, the first factor would correspond to a non-epistemic influence, and would probably explain a fair amount of the data. I grant this and am interested in exploring whether any of the remaining factors have epistemic dimensions. It is my contention that they *do*, but to appreciate this it will first be useful to introduce a few additional terms corresponding to types of dangerous speculation.

Dangerous speculation comes in three flavors. Speculation that relaxes the coherence requirement while maintaining fidelity-to-data, reflexivity, and parsimony may be termed

“revisionary speculation.” Speculation that relaxes parsimony while maintaining fidelity-to-data, reflexivity, and coherence can be termed “extravagant speculation.” Finally, speculation that relaxes both coherence and parsimony while maintaining fidelity-to-data and reflexivity can be termed “wild speculation.” These terms are summarized in Table 1. Notice that the several flavors of dangerous speculation stand in contrast to the two flavors of egregious speculation discussed by Turner (2019) and Currie (2019).

|  |  |  |
| --- | --- | --- |
| **Constraint** | **Relaxable?** | **Kind of speculation when relaxed** |
| Fidelity-to-data | No | Egregious speculation |
| Reflexivity | No | Egregious speculation |
| Coherence | Yes | Revisionary speculation |
| Parsimony | Yes | Extravagant speculation |

**Table 1.** The four constraints associated with well-controlled speculation, and the kind of speculation that results when they are ignored. Not represented is “wild speculation,” which involves the simultaneous relaxation of coherence and parsimony requirements.

I regard the Nemesis hypothesis as an example of extravagant speculation shading into wild speculation. The reason is that, while its assault on parsimony was severe (making it extravagant), it also represented a challenge to existing beliefs about extinction that was mitigated, without being removed, by the earlier revelations of the Alvarez hypothesis. It follows that in considering the reception of Nemesis, we must keep track of both its challenge to coherence (owing to the postulate of extinction periodicity) and its flouting of parsimony (in the form of two hypothetical objects: Nemesis and the Oort cloud). The latter feature makes it an extravagant hypothesis *par excellence*, especially considering that the only evidence for the existence of Nemesis was patterns in extinction data.

The vendobiont hypothesis, by contrast, is an instance of revisionary speculation. It involves no general loosening of the parsimony requirement.[[13]](#footnote-13) Indeed, what was outrageous about Seilacher’s suggestion was just how parsimonious it was: how it managed to render nearly all the characteristic Ediacaran forms as variations on a single unique body plan. Yet from a coherence standpoint, the hypothesis was a scandal, casting aside nearly the entire framework of interpretation developed by Glaessner and his associates. Arguably it was this radical challenge to the traditional framework that stimulated the burst of interest in the Ediacaran biota that began in the 1990s and continues to the present day.

What about the kraken? To begin, it is deeply unparsimonious, both because it postulates the existence of a “kraken” on the strength of no evidence besides that which it purports to explain, and because nothing in the evidence seems to demand so exotic an explanation. But it is also seemingly at variance with existing knowledge of cephalopod evolution, since the limited fossil record suggests that a Triassic coeloid would have just one row of suckers, as opposed to the two required by the postulate of self-portraiture (Greenfield).[[14]](#footnote-14) A giant cephalopod would also have massively exceeded its contemporaries in size (no Triassic forms are thought to have exceeded one meter in length) and, at 30 meters, would represent a >90% increase over the next largest member of the taxon (including all members, past and present). In short, hypotheses don’t get much wilder than this, notwithstanding that none of the above reasoning invalidates the hypothesis in the strictest sense.

The kraken is an extreme case of wild speculation, and functions here mainly as an outgroup comparison for more acceptable forms of speculation. What it illustrates is that there are limits to the “wild, messy and creative” postulates that historical scientists will put up with, even under conditions of evidential scarcity. (There is such a thing as “too wild,” *pace* Davis’s claims about outrageous hypotheses and Currie’s tenable optimism about speculative hypotheses.) To be clear, the problem is not that the Triassic kraken hypothesis furnishes no scaffolds for open-ended inquiry; before McMenamin came along, no one dreamed of looking for evidence of giant cephalopods in the vicinity of the Berlin Ichthyosaur Death Assemblage. It is rather that the scaffolds it provides are so rickety that no one is willing to mount them. Speculation that is *this* wild is unlikely to be productive speculation.

But how different is this from Nemesis? After all, the Nemesis hypothesis implicates two hypothetical objects related to each other in a very specific way; and all this to produce a disputed effect (extinction periodicity). By contrast, the kraken hypothesis manages with one hypothetical object to produce its disputed effect (a midden/portrait). Yes, there are independent reasons for thinking that a giant coeloid equipped with biserial suckers is implausible. But there were independent reasons for disbelieving in Nemesis as well, chief among them worries about the stability of its orbit over so long a period. In addition, the Nemesis hypothesis constituted a challenge to prevailing beliefs about extinction, mostly owing to its endorsement of extinction periodicity. This is why I called it an instance of extravagant speculation *shading into* wild speculation.

Here, three remarks are in order. The first is that the Nemesis hypothesis, while it sought to explain a disputed phenomenon, was nonetheless based on an analysis of the most extensive dataset that had ever been compiled for the purposes of analyzing trends in marine diversity over time.[[15]](#footnote-15) This alone lent it some credibility, even if it did not remove all worries about the quality of the data (recall Hoffman’s criticisms about the importance of arbitrary decisions concerning the dates of stratigraphic boundaries). The second is that Raup and Sepkoski’s analysis of extinction periodicity, for all its uncertainty, was highly suggestive—tantalizing, even. Everyone agreed that *if* claims of periodicity could be confirmed, then the phenomenon would require a special explanation, and probably an extraterrestrial one. This is because the pattern was unlikely to have arisen by chance (the most parsimonious explanation), and no earthbound process was known to operate periodically over a 26-million-year interval. The contrast with the kraken hypothesis is sharp, since the Berlin Ichthyosaur Death Assemblage strikes most observers as just another bonebed, and thus not as the sort of thing that requires a special explanation.

Third, claims of extinction periodicity (and by extension, the hypotheses marshaled to explain them), were part of a developing research program that had gained serious momentum by 1984. This was the research program variously called “evolutionary” or “nomothetic paleobiology,” and that included Sepkoski’s early work on Phanerozoic marine diversity trends, as well as Raup’s work on extinction and clade dynamics (Sepkoski 2012). It also included Stephen Jay Gould’s work with Raup and Sepkoski, which largely accounts for Gould’s enthusiasm for the Nemesis hypothesis. As Gould saw it (and others doubtless agreed), discussions of periodic extinction were a good thing for paleontology. They highlighted the utility of a particular way of working, which involved the use of computers to analyze large-scale patterns in life’s history. And they illustrated the potential of paleontology to contribute new insights about the workings of evolution: “The cyclical theory of catastrophic extinction leaves paleontologists in the driver’s seat with a decade of exciting work in front of us” (Gould 1985, p. 446). Here is another contrast with the kraken. Unlike McMenamin’s hypothesis, Raup and Sepkoski’s work was part of an emerging project that Lakatos might have called a “progressive research programme.” This gave it both an audience and a claim to relevance that the kraken lacked.

Turning to the vendobiont hypothesis, things were different. While Seilacher was the leader of a thriving group of paleontologists centered at Tübingen, his constructional analysis of the Ediacaran biota was too idiosyncratic to assimilate to any tradition in Precambrian paleontology. This left Seilacher as a lone voice in the wilderness—or at least as close to a lone voice in the wilderness as the most famous paleontologist in Germany can be. Yet the vendobiont hypothesis had other things going for it, including things the Nemesis hypothesis did not. Arguably the most important was that, in disrupting nearly every feature of the traditional interpretation, it cleared space for a renewed look at the affinities, taphonomy, and paleoecology of Ediacaran organisms. In the years following Seilacher (1989), every aspect of Ediacaran paleobiology came in for renewed interest and scrutiny. No better evidence exists than Gehling’s (1991) article, which uses Seilacher’s proposal to motivate a far-reaching reassessment of the life, death, and classification of Ediacaran organisms. Nemesis, by contrast, set a narrow research agenda mostly focused on locating the Nemesis object itself—a task for which paleontologists were singularly ill-equipped.

This potential for productivity may be a general feature of revisionary speculation. At least when it is successful, such speculation seems likely to stimulate diverse lines of research aimed at reconciling old observations with new frameworks, resolving ambiguities, and tidying up conceptual and taxonomic categories.[[16]](#footnote-16) Even if a speculative hypothesis is ultimately rejected, it may stimulate a significant amount of research while under discussion, especially if it is sufficiently compelling to undermine confidence in prevailing categories and approaches. This is basically what happened with Seilacher’s interpretation of the Ediacaran biota. Few paleontologists accepted it whole hog, but fewer still dismissed it as obviously wrong or absurd. This created the conditions for a “free-for-all” in Ediacaran paleontology, with important consequences for ongoing research on the biota (Narbonne 2003, p. 431).

This suggests that *the tolerance a scientific community exhibits for revisionary speculation depends, in part, on how fruitful a hypothesis is in stimulating diverse lines of research*. Even outrageous hypotheses like Seilacher’s may find a relatively warm reception if they suggest diverse research questions, as the vendobiont hypothesis manifestly did. By contrast, hypotheses that fail to suggest new questions—or that suggest only a few questions of narrow scope—are likely to get the brush-off.[[17]](#footnote-17) The Triassic kraken hypothesis has many problems, but one of them is that it doesn’t open up new lines of research apart from a needle-in-the-haystack search for the remains of a kraken. Had the kraken been shown to exist from independent evidence, then a range of questions would have arisen about its morphology, phylogenetic affinities, and ecological relationships. But in the absence of such evidence, these questions remain in the realm of cryptozoology as opposed to serious, professional paleontology. Simply put, there isn’t all that much you can *do* with a giant kraken.

So communities may be willing to relax coherence requirements when revisionary speculation suggests diverse lines of research. Does the same thing hold for parsimony requirements and extravagant speculation? Perhaps. But in the case of the Nemesis hypothesis (my main example of extravagant speculation), something else seems to be going on. To the extent that paleontologists were amenable to Nemesis, it was because the hypothesis sought to explain a potentially important phenomenon known from copious and (apparently) reliable data. Extinction periodicity had yet to be confirmed, and this cast a pall of suspicion over the entire affair. But few would have denied that extinction periodicity was a major finding if true, and this created a demand for explanation even in the absence of a knock-down demonstration of periodicity. It was the recognition of this demand, paired with the absence of a workable explanation based on known factors, that prompted some paleontologists to entertain the possibility that otherwise unevidenced objects might participate in the relevant causal mechanism. Generalizing, we may hazard that *the tolerance a scientific community exhibits for extravagant speculation depends, in part, on whether there exists an independently characterized phenomenon that generates formidable demands on explanation*. Where such demands exist, and the rewards of success are high, extravagant speculation is likely to be more tolerated than where they are absent. (This logic should make sense to anyone who has ever purchased a lottery ticket.)

I began this paper by asking what accounts for the relatively warm reception of the vendobiont hypothesis, the mixed reception of Nemesis, and the heckling dismissal of the Triassic kraken. Part of my answer has been to suggest that these hypotheses, while all “dangerous,” stretch credulity in different ways. The vendobiont hypothesis is an example of revisionary speculation; Nemesis represents extravagant speculation (shading into wild speculation); and the kraken is almost indecently wild. This helps to account for the brusque dismissal of the Triassic kraken, since wild speculation flies in the face of multiple norms of responsible inference. But it fails to differentiate Nemesis and the Vendobionta, since there is little in these norms that favor extravagant speculation over revisionary speculation or vice versa. Here thinking in terms of tradeoffs helps. Scientific communities, I have suggested, are willing to relax coherence requirements on speculative hypotheses to the extent that a hypothesis is fruitful in generating new lines of research. The vendobiont hypothesis is fruitful in exactly this way; hence it received a relatively warm reception, despite its apparent difficulties. I have also suggested that communities are willing to relax parsimony requirements to the extent that a hypothesis addresses the demands generated by an independently characterized phenomenon: especially if these demands are recognized to be sufficiently onerous. This accounts for the positive aspects of Nemesis’s reception, since the hypothesis attempts to explain the independently characterized (and challenging) phenomenon of extinction periodicity. But claims of periodicity had yet to be vetted by the paleontological community when Nemesis was proposed, and many paleontologists found them too incredible to believe. They were accordingly unwilling to entertain this bit of extravagant speculation, at least until the 26-million-year extinction cycle had been validated.

Rightly understood, this paper makes only modest claims. I am not saying that communities only tolerate revisionary speculation when it opens up diverse avenues of open-ended research. Nor am I saying that communities only tolerate extravagant speculation when it addresses the demands generated by an independently characterized phenomenon. Rather, I am arguing that weaker associations obtain between these variables, which can nonetheless help us make sense of the reception of dangerous speculation in geohistory and beyond. Perhaps the most useful contribution this paper makes is to identify different forms of dangerous speculation as loci for philosophical engagement. This allows us to pose new research questions, like how much do communities differ in their tolerance for dangerous speculation, and (how) does this relate to features of their epistemic situations?[[18]](#footnote-18) We can also ask whether there are situations in which dangerous speculation is especially likely to be productive as opposed to idle or deleterious. These issues are complex, but they are worthy of serious philosophical attention. Perhaps they even call for some speculation: empirically-grounded, of course, but not averse to a bit of danger.

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1. Philosophers distinguish two kinds of parsimony, one of them syntactic (having to do with the number and complexity of hypotheses), and the other ontological (having to do with the number and complexity of entities postulated in a hypothesis). In this paper, when I speak of parsimony, it is the ontological sense I have in mind. [↑](#footnote-ref-1)
2. By “well-received” what I mean is treated as live options, actively discussed, and tested. [↑](#footnote-ref-2)
3. This risks underselling Sepkoski’s accomplishment, which was not just to *compile* this database, but to *analyze* it using multivariate statistics and models adapted from population ecology (Sepkoski 2012). [↑](#footnote-ref-3)
4. The fifth paper was co-authored by Walter Alvarez, and noted an apparent periodicity in the record of large impact craters (Alvarez and Muller 1984). [↑](#footnote-ref-4)
5. This was the suggestion of Marc Davis’s team, who also suggested Kali (“the black”), Indra (the vedic god of storms and war) and George (as in “Saint George and the Dragon”) as possible names. [↑](#footnote-ref-5)
6. While Seilacher ranged widely, his primary areas of study were ichnology (the study of trace fossils) and taphonomy (the study of fossil preservation). His first work on the Ediacaran biota was Seilacher (1984), although Runngar (2021) dates his interest in the biota to the 1970s. [↑](#footnote-ref-6)
7. For example, while Glaessner interpreted some Ediacarans as sea pens, living sea pens have polyp-lined branches that permit water, and suspended food particles, to flow around them. But in Ediacaran organisms, the “polyps” seem to have been joined in quilt-like structures that would have precluded this mode of feeding (Seilacher 1984). [↑](#footnote-ref-7)
8. The idea that Ediacaran organisms gained sustenance through microbial symbiosis was first proposed by none other than Mark McMenamin (McMenamin 1986). [↑](#footnote-ref-8)
9. He would later revise this interpretation, first reinterpreting the Vendobionta as an extinct animal phylum (Buss and Seilacher 1994), and then as an extinct class of giant rhizopod protists (Seilacher et al. 2003). [↑](#footnote-ref-9)
10. Particularly salient in this connection are the frond-like organisms of Mistaken Point, which consist of “inflated, self-similar branches that are indistinguishable from the ‘fractal pneus’ defined by Seilacher” (Narbonne 2004, p. 1141). [↑](#footnote-ref-10)
11. Similar remarks have been made about modeling. So, for example, Oreskes et al. (1994, p. 644) observe that “[models] may confirm our biases and support incorrect intuitions.” This leads them to suggest that “models are most useful when they are used to challenge existing formulations, rather than to validate or verify them.” [↑](#footnote-ref-11)
12. In the remainder of this paper, I will have little use for the notions of productiveand idlespeculation*.* I mention this here to avoid confusion, since dangerous speculation can be either productive or idle (or detrimental/“egregious”). [↑](#footnote-ref-12)
13. I say “general” here because one aspect of Seilacher’s hypothesis was not so parsimonious. This was the postulate of a cuticle with “rather incompatible properties: flexible, but able to become creased and fractured; watertight…yet permeable enough to metabolically interact with the environment; cuticular, but expandable during growth” (Seilacher 1989, p. 237). Gehling’s “death mask” hypothesis was an attempt to save the same phenomena without postulating such an improbable structure. [↑](#footnote-ref-13)
14. Coleoidea is the group of cephalopods including octopuses, squid and cuttlefish. [↑](#footnote-ref-14)
15. Sepkoski’s *Compendium of Fossil Marine Families*, published in 1982, contained almost 3,500 families of marine animals and animal-like protists, along with times of apparent origination and extinction. [↑](#footnote-ref-15)
16. One is reminded of Kuhn’s useful term “mopping-up operations,” with the Kuhnian caveat that nothing derogatory is here implied. [↑](#footnote-ref-16)
17. This is very much in the spirit of Currie’s remarks about speculation, which tend to assume that the most important thing a speculative hypothesis can do is generate diverse “epistemic goods” (Currie 2018, 2019, 2021). [↑](#footnote-ref-17)
18. This is a variant of a question posed by Turner (2019). [↑](#footnote-ref-18)