

STATEMENT OF CONCERNS AND FACTS BY THE SENIOR AUTHOR IN RESPONSE TO THE MANY UNSCIENTIFIC CRITICISMS IN THE MEDIA OF EVOLUTIONARY BIOLOGY PAPER ON *TYRANNOSAURUS* SPECIES

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As the lead author of the *Evolutionary Biology* (EB) paper that named the two new species of *Tyrannosaurus*, I wish to express numerous concerns regarding the excessive criticisms of the study by a number of paleontologists and paleopundits in popular venues. I did not expect to need to do so, but I am doing this for a number of reasons. This is intended for both those involved in paleozoological and evolutionary sciences, and those in the new media who cover these topics. While doing so, I take the opportunity to lay out the scientific requirements that future technical papers addressing whether *Tyrannosaurus* was monospecific or multispecific need to meet in order to be scientifically valid, those doing so are urged to consider these items.

First, some of the claims made by paleontologists to the science news media were not accurate and were clearly such when made. This includes the assertion that prior papers had included comprehensive analyses of the species question and shown that only one was present. This is not correct because those papers simply do not do so, and the idea should not have been presented. Also in perplexing and obvious error have been statements about the actual contents of the paper, when the authors in anticipation of such misreadings went to lengths in the text to warn off such mistakes that have caused many to think the paper is not as well founded as it actually is. Scientists are supposed to be, above all else, accurate, and they have often been way off in this case.

It is not just us. Some researchers who have conducted advanced work on determining tetrapod paleo species support the EB paper –

The splitting into different species for a genus, like *Tyrannosaurus*, that probably lasted around 2 million years, is not surprising at all. We know that this happened in several ceratopsians of the same period of time, and when the fossil data set is large, like the abundant fossils that come from more recent sediments like those of Plio-Pleistocene, the species splitting is confirmed to be the general rule for warm-blooded vertebrates that lasted for more than 1 million years. Also, it should be noted that recent ancient DNA analyses are telling us that the species splitting (as well as the hybridization) happened much more often than we previously thought. To test the possible splitting events in the very similar and old taxa that came from Mesozoic beds is more complicated, however. This is because, usually, the specimen amount that we have to deal with is much reduced, but in the case of *Tyrannosaurus* the number is relatively high, in fact much larger than most of the genera within Dinosauria and enables to trace possible speciation. The efforts and observations made by Paul et al. (2022) in their latest study shed light on this specific topic in *Tyrannosaurus*. Their work should be welcome and should receive proper consideration since it opens a new

line of research on the possible speciation of *Tyrannosaurus*, and no hardline rejection should be made until more research is done.

The new paper has been like a nuclear bomb, especially in social networks that has devastated everything. It has aroused a torment of disqualifications to the study, most of the time unjustified by people who have not even read or understood the article, and some of the more rational criticisms have been unfair in many cases, probably because of FOMO. The worst thing is that the number of ad hominem attacks has been countless, something that has never happened in the field of paleontology, at least in recent decades. This shouldn't need to be said, but we all of us have to remember that *Tyrannosaurus* must not be considered anything special or receive a particular or differentiated scientific treatment compared to other past vertebrates, it is just another dinosaur, so the science of paleontology cannot be influenced or coerced by disqualifying opinions given in the public sphere.

Asier Larramendi – Research Director
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(Work on mammoth speciation cited in EB
paper, has collaborated with Paul on
mass estimation in extinct animals)

Much of the criticism of the paper was casual and ad-hoc, without substantiation, and sometimes laced with egregious and nonscientific hard edged-rhetoric. These polemics included outrageous and very misleading statements to the public that the evidence in our paper was “vanishingly small” and that the hypothesis is “poorly justified” when our paper studied the proportions of 7 elements of about three dozen specimens of which about two and a half dozen were stratigraphically correlated, a sample of a size never before examined for the genus, and larger than for most dinosaurs. And femur variation in the one genus is greater than all other tyrannosaurids. By excluding the above points commentators failed to let the public know about how the paper is in reality innovative and contributes lots of new data to dinosaurology. Serious misleading myths about the paper have also already locked in, including that there are only two elements that help distinguish the species, when the robusticity pattern applies to other elements as well as is clearly explained and plotted in the paper. And as is extensively documented in the paper, one or two features is indeed enough to distinguish paleospecies, such being commonly done in the modern paleotetrapod literature. And there is – as we predicted and went to great lengths in the study to show is not correct to do – the claim that because the EB work on *Tyrannosaurus* does not meet the atypical standards of the *Triceratops* results that it is invalid, when there is no other dinosaur genus that can be done for. It did not appear that critics had read, in the supplement, the extensive comparison of the methods we used to distinguish paleospecies to those used in a variety of recent peer reviewed publications showing that our techniques were at least broadly comparable to current norms, while pushing dinosaurologists to pay more attention to the paleospecies issue they have been neglecting for decades.

Although reviewers unfortunately required us to cut back on our expressions on the issue, one commenter did exactly what I feared, and in a news article stated that publicly popular fossil animals like *Tyrannosaurus* should receive a special level of scientific

analysis. and proof compared to less well known taxa. Science standards should not be impacted by popular interest, they should always be as consistent as possible. *Tyrannosaurus* is just another dinosaur.

Another concern has broader implications. The sometimes harsh tone of the criticisms as cited above. Had the paper been published in a minor journal under dubious circumstances that would have been one thing. Or if it had big and obvious data errors. A reason that the paper was published by going through the usual and high quality vetting process in a leading, mainline journal on bioevolution was to hopefully cause colleagues to react by considering how our paper was processed as a cause to take it seriously. That did not work with many critics. There was little in the way of generosity and due deference. Scientific routine when addressing the public in news stories and the like should of course be as follows. Unless a paper contains really obvious errors, the way to properly scientifically handle this sort of thing is to without hard rhetoric tell the reporter or interviewer that the results are interesting but they have doubts, and that new analysis may show different results, with some reasons offered. And also best to actually read the paper in full to be sure one is not making outright misstatements about it (admittedly supplements in many papers do not contain information as vital as in this one). That did not happen. To a degree that was beyond acceptable bounds.

And matters got out of ethical hand, as the host of a popular podcast (~100K views) that auto repeats much of the misinformation about the study that causes him, without qualifications, to tell his big audience that the paper is probably false, then goes on to slanderously imply that the paper (that could not have appeared without the approval of reviewers personally indifferent to its appearance) was published for publicity reasons. That is as unprecedented as it is out of ethical bounds. This is the sort of invidious criticism that if not called out can contaminate science going into the future.

The result of long hard work in an effort to find out what is going down with *Tyrannosaurus* species, the properly vetted and innovative paper that should have received considerable positive reception for presenting novel and informative data on subjects that have received too little attention – most of all for taking on the torpid issue of tyrant lizard species – even if one does not fully agree with the taxonomic conclusions. Instead, off the cuff reaction to the paper has been so dysfunctional that the paleontological community needs to decry the poorly founded criticism in news venues and public videos of what is actually a normal, rigorously peer reviewed paper on yet another set of dinosaur species names, both in this instance, and to prevent it from becoming the norm in future instances – we don't want to become like the general society do we? Some of those who made inaccurate statements about the paper should give serious consideration to withdrawing or modifying them.

THE NEED FOR THIS RESPONSE

Normally criticisms of a seriously flawed technical paper occur in the technical literature itself sometime later (Paul did this in *Science* – <https://www.science.org/doi/full/10.1126/science.1192963>) and our team would do so if that happened eventually. But the errant criticism in the news media was so swift and extensive that they cannot be allowed to fester in the public consciousness. It is common for what pops up in the earliest versions of a news story to quickly become the perceived truth of the matter even when it is not, entrenching an anti-scientific mythos that can be as hard to kill off as a vampire. This is especially true in news accounts because the 2nd generation stories in the days and weeks after the initial original articles often auto repeat what was said in the first wave. The necessary way to minimize such distortions is correcting the record as early and forcefully as possible. Note that is not a recounting of the basic arguments found in the paper and that have been covered elsewhere, sometimes well in otherwise overly critical news accounts and videos, much of it is new items.

So, for those who wish to – or need to if they are going to research or comment on the issue – delve deeper in the details of the controversy via the eyes of those who started by doing peer reviewed science, here goes.

JUST THE PALEOSCIENCE FACTS, MA'AM: Criticizing the Critics – Myths Versus Facts About the *Evolutionary Biology* Paper – the Scientific Reality

The below set of issues is extensive because a tsunami of critical comments, most of which are problematic if not outright wrong and not vetted in peer review, suddenly flooded popular venues. So don't blame us.

Fact v Myth – The EB Study Uses Only Two, That's Just Two, Features to Separate the Three Species, It's More Than That

This is a major falsehood that if not quickly shot down risks becoming entrenched as a perpetual myth about the study. That the features used to distinguish the three species are limited to two, femoral robustness and the anterior dentary teeth. That this has been repeatedly stated is seriously troubling it being clearly otherwise. The text just before the diagnoses explicitly states that “Note that the species diagnoses incorporate the cumulative proportions of six elements in addition to the femur.” That was deliberately put there directly to preempt people from saying only two characters were used. Yet people are going right ahead and doing that because they have been happy to criticize the paper before carefully reading it seems. And the diagnoses specifically state that it is a matter of general robustness or gracility, which includes the maxilla, humerus, ilium, femur, and metatarsals, only the dentary does not show a plain trend towards gracility with later time, although there is no example of any low set strongly gracile element. The three holotypes possess all or nearly all of the 7 pertinent elements. Also observe that all those elements show a clear pattern of little variation low in the TT-zone to more variable higher up. This is documented in the data tables and visually in Fig. 6. It needs to be

asked why have none of the videos that repeat the only two characters claim have not yet presented the Fig. 6 t that is a core of our paper and makes clear all the characters we actually use, so their viewers can see that. That when the same videos include all the other main text figures (as per <https://www.youtube.com/watch?v=sPniAlccInE>). Also note that metatarsals as well as femora are illustrated in Fig. 2. The paper uses only one specific robusticity ratio for defining the species, 2.4 for the femur, because that is the only practical way to produce a value that can be readily applied, the individuals all having some internal variation in robustness (although our thesis that individuals tend to be broadly consistent in their proportions does not seem to be being challenged) and the massive thighbone being commonly preserved intact. The inaccurate claim that the study works with just two characters should not be repeated and should be withdrawn by scientists and video hosts who make it – literally, publically withdraw the claim or at least acknowledge the disagreement with it. It is actually currently 7 characters, with 6 showing shifts from one condition to another proceeding stratigraphically through time, and all 7 robusticity measures show an increase in variation.

Myth v Fact – Prior Work Demonstrated One Species. No, It Did Not –

A big issue that suddenly appeared in the wake of the EB work is the myth that a host of papers over the decades, most of all Tom Carr's 2020 PeerJ paper, just happens to have precluded our study by having shown there is just one species, that our paper it seems ignores. Which is surprising. Because had the 2020 study and earlier works firmly established that *Tyrannosaurus* is monospecific, our team obviously would not have produced the new paper, because it would have been useless if the 2020 paper had accomplished what many claimed it did in news accounts on and shortly after 2/28/22. The claim is false. The paper's intro cites no prior papers as having actively demonstrated that *T. rex* is the only species because no such thing exists – many papers mentioned in that regard do not actually address the question, only papers done in recent years have potential access to the needed data, and none utilize it. So it was not possible to cite such and there was not thought of doing it (we do cite the 2020 study, but in the context of what it actually contains).

Please note that when Carr's paper was published news accounts did not note that it showed that only *T. rex* is valid because it was not a major topic of the paper. In particular, Carr's paper is titled "A high-resolution growth series of *Tyrannosaurus rex* obtained from multiple lines of evidence." There is no mention of the species issue. Or in the introductory sections of the paper. Nor in the conclusions except for a brief mention that the *Tyrannosaurus* "x" hypothesis is not viable which the EB paper agrees with (AMNH 5027 being nondiagnostic). The paper focuses on the status of the small tyrannosaurid specimens from the TT-zone vis-à-vis the adults, and barely addresses the systematic status of the big boys.

The first mention of *Tyrannosaurus* species in Carr 2020 is in a section titled "Assumptions" – that's *assumptions*. The following is stated –

“For the purposes of this study, it was *assumed* [italics added] that the assemblage of *T. rex*, which spans Laramidia for a duration of less than 1.0 million years (Fowler, 2017 – our note: was more likely 1.5 million but this is iffy) was a single non-anagenetic population.” Got that? Carr is basically *assuming* that there is only *T. rex* from the get-go, not conducting a serious test of the issue in a paper mainly looking at the proposed growth of small specimens into the same one species. Later in the same section there is a small effort to look at the all critical stratigraphic issue, but that includes only 7 adult specimens (Table 18), just a quarter of the specimens we take a look at (our Table 1 and Fig 6). Not only is the Carr stratigraphic sample much too small (in part because he excludes all private specimens, see discussion on BHI fossils below) to be definitive, it does show all graciles are high up as we affirm with the bigger sample.

The EB paper could hardly be more different. When Paul began the effort he did not know what would pop up, and wanted to find out. The EB paper is an exploratory effort to see what would turn up and base any whatever conclusions arose on the preponderance of evidence. The team went where the data took us, no assumptions up front. And although not surprised to discover good evidence for multiple species such already having been shown for *Triceratops*, I was WAY surprised by the pattern that was found.

Delving deeper into the 2020 study finds that it is even more numerically problematic. It does not examine the robustness, as is critical to our study, of the maxilla, dentary, ilium, and humerus. Without this data species analysis is not practical. Femur stoutness is included, but for only 3 adults – we have over 7 times more stratigraphically placed femurs – which other than affirming no low lying graciles is useless. Same for just two tibias (problematic to use because the strength of the fibula is not taken into account), and there are no adult metatarsals. Nor does Carr look at the fine gradation of robustness as we do, he only scores whether it the femur ratio is above or below 2.27 (a value that is too low because the BHI specimens are excluded). So the claim of the title that the study is high resolution is exaggerated, ours is more refined and sophisticated in critical regards.

As for the dentary teeth the 2020 study does consider them, but in a different non-quantitative way that is statistically inferior, and with a smaller sample both in total numbers and those that can be stratigraphically assessed. Again it is the 2020 work that does not meet the higher standards of the 2022 paper when it comes to the speciation question.

Carr told *The Guardian* about his paper that “I found no evidence of more than one species. And if that signal was in the data, I would have picked it up.” No. He had no ability to pick up such a signal with just 7 strato specimens, with a mere 3 femora tied to the geology, and robustness measured for just a few elements of a few specimens, in a paper that pretty much assumed one species from the start. The 1850 characters may sound convincing, but that is misleading because they are of little use when they are observed from a set of specimens that is too small to be geologically correlated. The 2020 paper simply comes nowhere close to scientifically testing the number of species recorded by large *Tyrannosaurus* remains which it was not designed to do. Far from having

demonstrated *Tyrannosaurus* monospecificity, the 2020 study shows the opposite, that little effort had been conducted to directly challenge the *T. rex* issue due to the long casual assumption there was only one species.

That leaves our 2022 work as the first and so far only to directly take on the issue seriously with a sufficient data set. What Carr could have said was that he was surprised by the EB results because his 2020 paper had not shown extra species, but the new paper's much more extensive stratigraphic data was interesting and he would give the situation consideration. The 2020 study has its positives, but it should not have been abruptly presented to the press as a refutation of the 2022 EB paper that was based in part on data contained in the 2020 paper as cited in our work. To cite this paper in the future as a refutation of the EB paper that had not yet been published and contains a many times larger sample of strato specimens whose robustness is much more extensively examined, as well as cross comparisons of variation in *Tyrannosaurus* relative to other theropods is entirely nonscientific and inappropriate, it barely having addressed the subject and lacking the data to do so. To test and affirm or refute (both of which may be difficult because of inherent issues concerning paleo/species discussed in the supplement) the EB paper will require future studies.

Fact v Myth – It's Always Easy to Tell Apart Species with Lots of Characters, Often it is Not –

Carr said with an edge that “Perhaps most damning is the fact that the authors [Paul et al.] were unable to refer several excellent skulls to any of the three species. If their species are valid, then more than just two features should identify them: Nearly every detail, especially in the head, should be different” (Reuters). And that four unassignable specimens “have perfect skulls, they should be able to tell species apart if they have perfect skulls, and can't do it” (NYT). This statement is well out of accord with modern taxonomic principles.

First. Of course no *Tyrannosaurus* skull is perfect – Hone makes the same hyperbolic nonscientific “perfect” skull mistake (TL vid). As the EB paper notes the 5027 skull is distorted, hindering measurements, and the level of the specimen is not available. Latter is same for MOR 008 the proportional data for which is inadequate and borderline. LACM 23844 measures out as borderline between *T. rex* and *T. regina*. I am not sure what the 4th skull is.

Anyhow, Carr's statement is wrong at a higher level. He appears to presume that species are easily told apart from one other by looking at the skulls and skeletons. It is not just him to varying degrees. In the press accounts and videos David Hone, Jingmai O'Connor, Leonard Finkelman expressed similar thinking. Steve Brusatte said “I understand the temptation to divide *T. rex* into different species, because there is some variation in the fossil bones that we have. But ultimately, to me, this variation is very minor and not indicative of meaningful biological separation of distinct species that can be defined based on clear, explicit, consistent differences” (Reuters) and "Dividing *T. rex* into three

species based on measurements from 38 bones just isn't a strong enough case for me" (CNN). Now, in early reviews of the EB submission there were claims we were not meeting the standards for designating paleo species (which is a reason we brought in Van Raalte to add to the statistical analysis, be sure to see her vid on her work). But that got us wondering what exactly are those standards, partly because they had gone pretty much not discussed in the literature. So the literature of all dinosaur genera with more than one species since the 90s was examined, and a lot of non-dinosaur examples. It turns out they are no firm standards. Which is because there is no set definition of species as is discussed at great length in the supplement which is the first major look at the problem from a dinosaur perspective in years.

Fact v Myth – The Burden of Evidence is On the Multispecies Hypothesis –

The fictitious premise that Carr 2020 etc. had already firmly established just one species has spun off another myth, that mono specificity is the null hypothesis and showing otherwise requires extraordinary evidence. As the paper discusses, *T. rex* has been so poorly supported over all the decades it is weak hypothesis, and lacking strong supporting evidence as this time is if anything inferior to multiple species, and that is the situation going into the future. This is all the more true because the probability is high that there should be more than one big theropod species in the TT-zone for the reasons Persons further illuminates in his video. Showing there is just one species therefore requires as much positive supporting evidence as does demonstrating a set of species, so the latter cannot be knocked down just by questioning the data for them.

Myth v Fact – To Tell Species Apart Always Requires Lots of Characters –

What is true is that it is common for skulls and skeletons of different species to be barely different. That of course is what happens when there are very closely related sibling species within a genus that have barely diverged from one another. When Hone says that “For *T. rex*, we have multiple complete skeletons in good condition with about as much anatomical information as you could reasonably ask for as a taxonomist, so to base these new species on just a couple of traits [see above refutation of that inaccuracy], both of which have already been suggested to vary within populations, is probably not a strong basis for naming new species” (NHM) he is well off base. The critics make broad seemingly sensible statements like this, but they are avoiding looking at the many current examples in which just a couple of traits or even less are indeed being used to define species of extinct creatures known from excellent fossils, in some cases substantially better than those of *Tyrannosaurus*.

Examples. The American lion is widely considered a distinct species from its probable ancestor the Eurasian/Alaskan cave lion, but only a few minor characters do so, as figured in the supplement. It can be hard to tell apart the skeletons of extant lions and tigers –

both also in genus *Panthera*. Species of brontotheres well documented by abundant remains have of late been distinguished by as few as one, that's *one*, skull character as cited and figured in the supplement, sometimes as chronospecies. *Chasmosaurus russelli* and *C. belli* which were not stratigraphically separated in a 2016 paper were distinguished by one character without contention. Ergo, Carr's claim that every detail should differ in the skulls of sibling species is entirely wrong and nonscientific. There are endless examples of very minor differences in skull and skeleton being solely used to distinguish species of fossil vertebrates in the current literature a number of which are cited in the supplement which many seem to not have read – and it is quite possible for species to be distinguished by postcranial features, say the presence or absence of tall dorsal spines, and/or very different fore/hindlimb ratios, while the skulls are not distinguishable. This is pretty basic stuff. And no one has made a fuss about it. Until now. Which is very interesting.

Because species that have just split initially are not going to be all that different, will exhibit inconsistencies in the most consistent differences, and the hybridization common among sibling species can make matters all the more murky – biology is sloppy – sibling species can sport an enormous number of characters that vary randomly between them, it is when at least a few of them show significant trends that species are indicated. That was a point of our paper which including in the title notes that this is an example of subtle evolution (unlike *Triceratops* which exhibits more visually apparent changes because it was evolving at a faster rate, which is an important evolutionary comparative result of the EB paper – animals often do not evolve at the same rate, as our paper shows. Reviewers of the paper who work on paleospecies did not have a problem with our data and statistics in the final manuscript. That some dinosaurologists seem to have a problem with it suggests they are not aware of the standards now being applied to Cenozoic mammals, which is not surprising in view of there being hardly any discussion of these issues in dinosaurology. One reviewer was especially useful because aside for pushing for more statistical analysis that he ended up very happy with (that is why we brought in Van Raalte), he studies discrimination of paleospecies in Pleistocene mammals that exhibit hybridization (his work is cited, Larramendi's and others cited does the same), and he agreed our results were sound. That's rigorous peer review.

With a half dozen characters helping distinguish *T. rex*, *T. regina* and *T. imperator*, our team's study is actually well within current norms if not ahead of the curve.

Myth v Fact – The Paul et al. Paper is Not Up to Systematic Snuff Because it Does Not Meet the Stratigraphic Standards of the Scannella et al. *Triceratops* Work, or, the Sample Size in the EB Paper is Too Small

A number of critics do something else explicitly warned against in the paper – that they do not note the caution is one of many reasons how closely they had read the paper must be questioned. They cite the Scannella et al. work on *Triceratops* as the comparative analysis by which ours need to be judged, and because our work does not meet that

standard ours is not sufficiently definitive to be taken as seriously as has been past work on dinospecies. Same thing happened in the initial reviews even though we keep pointing out in paper that no other study on dinosaur species can meet the *Triceratops* standards because none other has such a massive set of specimens and stratigraphic data base, so if our study is not valid because it does not meet three horned face standards, then no other work does or while until decades into the future. That is why a deep dive into the actual standards used with other dinosaurs and paleotetrapods in general was conducted to see what the actual standards are by wading through a lot of papers. They are actually well in accord with our study, and that is extensively documented via numerous citations in the supplement. After the reviewers (some of who study paleomammal species) were advised to carefully read those sections their citation of the Scannella et al. work ceased.

This brings us to the stratigraphy. About how the placement of the specimens is not as exact as in the Scannella et al. work on *Triceratops*. That work is restricted to a limited vertical column in a limited geographical area, and does not contain sufficient *Tyrannosaurus* specimens to test the species situation – but there is no evidence of a robust example low in that column. Critics opined as how Paul et al. should have done the work to better geologically assay the specimens. That was not pertinent in the practical sense because that is a major project that will take decades if it can be done, and is not necessary. Any effort to tightly expand stratocorrelations lateral into other regions will may fail because of the lack of sufficient radiometric layering. Here are the critical stratigraphic facts: To date not a single robust specimen out of the two and a half dozen that can be placed stratigraphically has been found low in the TT-zone. Not a single two incisor toothed specimen has been found high up. That is enough to indicate that substantial Darwinian speciation was underway.

And some critics are being hypocritical. They criticize us for the quality of the stratolevels our team used. Yet the same people laud Carr 2020 for its supposed high quality. When that paper too uses the same form of stratigraphic data ours does – not at the level of Scannella et al either – just a lot less of it (we cite Carr’s stratodata as verification of ours). That our critics are not being fair and balanced about this sort of thing is another reason they come across as overly tied to the *T. rex* tradition.

Getting adequate stratigraphic data for *Tyrannosaurus* is actually not all that hard, which is why the study was doable. That is unlike TT-zone *Edmontosaurus* which very badly needs systematic work, but the stratigraphic data for many specimens is not available, and will take it seems major effort to produce. It may not possible to assess that species question in the near future.

On issue of the sample size the only dinosaur genus to be split at the species level with a really large data base is *Triceratops*. So if Brusatte is consistent he will reject all other examples of multiple species in fossil vertebrate genera below the sample level seen in *Triceratops*.

Myth v Fact – Never mind that the EB sample size is too small, drop the BHI specimens and make it smaller --

The Black Hills Institute (BHI) and other private specimens are a significant problem. But let's get real. The generally conservative rancher lobby is not going to be OK with fossils on their largely red state lands not being their property. If it somehow becomes law that ranchers and tribes cannot sell off their fossils, many owners, a good number of whom are creationists of various sorts, will bar prospecting and excavation on their land by what they see with justification as evolutionists. Carr has presented what he calls a scientific analysis showing all the specimens that would be in proper institutions if American laws were like those in Canada (<http://www.thefossilforum.com/index.php?/topic/118819-controversial-abstract-by-tom-carr-tyrannosaurus-rex-an-endangered-species>). Although many of his concerns are legitimate, his analysis is not scientific because it is not possible to determine how many of the privately collected specimens would have been collected otherwise, or still be in the ground, or perhaps more likely scoured away considering how fast badlands erode. Holtz (TL vid) seems to express less concern about use of the BHI specimens. In any case, being in an accredited museum does not ensure their accessibility for further research, some *Tyrannosaurus* specimens are in display in ways that make them unavailable (such as the classic American Museum skull 5027 which further hindered its assessment), and some museums burn or their contents are otherwise destroyed or looted.

For better or worse, excluding the BHI etc. specimens rendered Carr's 2020 non-definitive, and with 11 specimens gone doing such would have decreased our samples by a crippling over a quarter, rendering it too of little practical use. BHI and other private specimens are documented to a significant extent by casts in a number of institutions, and photographs in print, online, and in paleo files.

Fact v Myth – Variation in *Tyrannosaurus* is Not Too Minor and/or Random to Be Taxonomically Telling

Paul Barrett says that “The differences in tooth anatomy are relatively minor, and while the differences in proportions are less subtle, they have been known for some time among those who work closely on *T. rex*” (NHM). Yes, the differences have been known for a long time. And no one correlated them with the stratigraphy. We did that. And it turns out

there is a distinct pattern with no low placed graciles yet known. Hone emphasizes that tooth form is highly variable in reptiles (TL vid). Again, the correlation with stratigraphy shows a pattern that is not in line with random variation – since one incisor being the typical count is apparently absent from tyrannosaurids over the prior 10 million years, that is looking like speciation, not willy-nilly intraspecific variation.

As for the variation in *Tyrannosaurus* we document that it being minor is far from reality. That is why the variation within the genus has been noted and discussed since the 1990s –

when has any one talked about all the variation in *Allosaurus*, or *Gorgosaurus*, because there isn't much -- it is not a secret, that is why the study was done. The paper leaves no doubt the variation is exceptional in Fig. 4, there being more divergence in the *Tyrannosaurus* sample than all other tyrannosaurid taxa from Asia and N Amer over 10 million years combined. That is all the truer when the smallest juveniles are dropped – I realized that late in prep of the paper (Fig. 4C) and doing a statistical run on that was not possible at that time. What should have happened when the paper came out is that commentators should have said something along the lines that this and others facts had not been documented before, and may have significant implications and is notable evidence of speciation, and needs further consideration. Not Carr's purely rhetorical assertion that this potentially ground breaking data "represent(s) meaningless variation, not biological signal" (CNN).

Which is exactly what our peer reviewed data *does* signal. The current information indicates that the early and basal *Tyrannosaurus*, *T. imperator*, retained the ancestral tyrannosaurid condition of robust femora and two dentary incisiform teeth. The later then shifted to an atypical derived condition of just one, while part of the sample also shows a way atypical gracile femur (exactly the opposite pattern I expected; Persons gets full credit for seeing that in his statistical work). That is classic Darwinian speciation for the many reasons are carefully detailed in the paper, with the two derived species *T. rex* and *T. regina* living alongside one another like earlier examples of same sized robust and gracile tyrannosaurids sharing the same ecospace. It does not fit variation within a species, that not being yet seen in other dinosaurs. Such should not be particularly controversial.

That none of our critics bothered to note remarkable items first discovered in the EB paper, such as femoral variation in *Tyrannosaurus* is greater than the rest of the family it was a member of. is a tell they are being biased, rather than objective critics ready with a fair balance of praise and doubt.

More Myth v Fact – How Strong Skeletons Are IS a Way to Tell Species Apart --

Hone says that species should not be based on items like robusticity. Such is often done, proportional differences being a classic way – it being a reflection of potentially important functional adaptations -- to distinguish fossil taxa especially at the species level. Asier Larramendi, who supports our paper, used varying skeletal robusticity to help distinguish mammoth species.

Fact v Myth – The Problem is Not Really With the EB Study, it is With the Current Sloppy Standards for Determining Paleospecies –

As we note above, our paper more than meets the usual standards for paleospecies. Finkelman (NYT) says the real problem is that dinosaurologists need to develop a better

set of consistent standards for tagging species. Seems like a good idea, but is very unlikely to ever happen because there is no standard for species and that is not likely to arise for a host of intrinsic biological reasons. The supplement discusses this at length. Plus, circumstances differ radically for different fossil samples so what works in one case is not going to in another. Flexibility is required.

A Particularly Disturbing Myth v Fact – Popular *Tyrannosaurus* Deserves Special Treatment

Hone makes the very unsettling assertion that if “you are going to shoot for the King, don’t miss” (NYT). That our team’s level of analysis is generally OK for paleospecies which is entirely true as per above comments, but that splitting up the best known dinosaur requires a higher standard. That is a really bad idea. Interesting fact: in the submitted version of the paper I laced it with comments about how *Tyrannosaurus* is just another dinosaur and it should be treated like such and our results not be subjected to greater criticism because it is special. Reviewers hated that and insisted that those comments were not scientifically proper and they be cut back. Just stick to the facts and all would be OK. Quite naïve. I knew I would regret cutting the lines and I do. Because so many skeptics did exactly what was feared, went hardline over what is coming across as the taxonomic sanctity of *T. rex* being challenged on a study whose standards are if anything above the dinosaurian norm.

So it is said here plain and simple, as has Larramendi in his letter. *Tyrannosaurus* is just another dinosaur. *T. rex* is not scientifically special. The species taxonomy of the genus has been moribund for decades, our team has at long last shaken the dinosaurologists out of their systematic lethargy.

Hone is basically saying that popular opinion should impact the quality of scientific research. That if a set of fossils is not well known we can use one set of standards on that because the public won’t care, but if it is a pop icon that a rock band was named after we have to do better work. No. In what area of science is what the public may think supposed to impact the quality of research? I have been asking non-scientists what is wrong with the Hone statement and they say it is not scientific – they get that basic point. Always apply reasonably consistent standards, do the best that can be done at the moment. That is what we did and whether the public likes it or not is not we paid no attention to.

Juvenile Fact v Myth – *T. regina* and *T. imperator* are not junior synonyms of juvenile species *N. lancensis* and *G. megagracilis*.

Shifting gears some, Tom Holtz, who is not so uptight about multispecific *Tyrannosaurus* suggests that the holotype of the species from the lower TT-zone is actually the juvenile *T. (=Nanotyrannus) lancensis* which he notes is probably not from higher as previously thought. As we explain in the supplement the specimen cannot be

used for that purpose. Basically is it a nomen dubium – only a distorted skull lacking the diagnostic limbs material, and too juvenile to assess whether it would grow up to be a robust or gracile in any case. Adding to the problem is that it is looking like some of the small specimens did not grow up to be giant *Tyrannosaurus*. The arm of Bloody Mary from low in the TT-zone is longer than the femur which never happens in any other Campanian/Maastrichtian tyrannosaur except *Dryptosaurus* from earlier east coast sediments (condition not known in *Appalachiosaurus*), it always being shorter including in juveniles of Asian and western N Amer tyrannosaurids. And the BM hand is literally as long of that as its supposed grownup form Sue, while the similar hand of Jodi is even longer! That just does not happen in ontogeny. *N. lancensis* is not even a good holotype for its own taxon because it is nondiagnostic, much less for any grownup *Tyrannosaurus* species. Had the EB study used the *N. lancensis* skull as the foundation for the species of the lower TT-zone *Tyrannosaurus* (*T. imperator*) instead of big highly complete Sue it would have been severely and correctly criticized.

The holotype of *A. megagracilis* (named FYI by me in 1988 – fun fact in a book edited by Alice Mayhew who edited the Woodward and Bernstein Watergate books) is from high in the TT-zone, and could belong to *T. regina* as suggested in the paper. But like the *N. lancensis* specimen this one also is too juvenile and incomplete to be used as the holotype of the gracile species (*T. regina*). Although the incomplete femur looks like it was gracile, that is an estimate not a measurement, so the actual ratio is not known, and it is possible that some *T. rex* juveniles of this size had the same femur ratio. Placement of this holotype in *T. regina* is therefore automatically tentative, too much so to be taxonomically significant. *A. megagracilis* is another nomen dubium, and again we would have been severely and correctly criticized if we tried to use it as a holotype in place of the far better grownup Wankel.

Lindsay Zanno comments on the issue of doing bone histology to determine ontogenetic ages. Great idea. But not critical. Stan which is a heavy as *Tyrannosaurus* gets among known fossils is the most gracile adult, while the far smaller Smithsonian femur is markedly more robust. That the biggest specimens are so similar in mass indicates the genus did not grow much bigger. We agree with Carr who in 2020 also showed that ontogeny does not explain the robustness-gracility pattern.

Fact v Myth – I am a Taxonomic Lumper. No I'm a Splitter –

Some EB paper commentators have, for reasons not entirely clear – contents of scientific papers stand on their own – spent time critiquing this paleozoologist for his past taxonomic work, in ways that look ad hominem (as per Wikipedia discussion noted below). On the one hand, I am stereotyped as a lumper, which I *sometimes* am at the genus level. But that is not pertinent to this dispute which involves splitting at the species level. At other times, I am characterized as a splitter, which I have on occasion been at the genus level. Actually, as stated in the technical literature, I am trying to apply the

“Goldilocks Principle” in which reasonably consistent gradistic standards are applied to genera and species. Currently, dinosaur genera are all over the internal variability map, with some genera distinguished by minor display features that should be used for species, which is why a number of well-preserved dinosaur genera with very close relations are monospecific. Other genera have been having a wide diversity of species sometimes from around the globe tossed into them. So I have done what I can to straighten matters out, splitting when genera are over lumped and lumping when oversplit. A number of these changes have been accepted. To clear up an item, the NY Times report that my work on iguanodonts has not been accepted is not correct in that *Mantellisaurus* and *Dakotadon* are in universal use; and many of my higher level taxonomic names are in wide use as well. The splitting of *Tyrannosaurus* from mono to multispecific is driven entirely by the patterns revealed by the data, and does follow any split versus lump ideology or preference.

Not Really Fact v Myth But Needs Discussing – Although the Basic Contents of the EB Paper are Useful, Naming the Species Was Unnecessary --

As for not naming the species, as I told Phil Currie, who agrees with the analysis, that if not done now then there would be a high risk of someone else going ahead and doing it – if I received a manuscript to review doing so I would have to approve it while gnashing my teeth at not being proactive. Look how the Aussies, after not dealing with their *Kronosaurus* problem, ended up with the famed Harvard skeleton being renamed – not unreasonably – *Eiectus* by S. Americans. As it is, we now have a set *Tyrannosaurus* names to scientifically play around with. Holtz notes this advantage in the TL video.

Fact v Myth – Going Out of Bounds, the EB Paper Was Published For Publicity –

This really shows knee jerk bias against the study. In a video that begins by doing fairly good of describing the contents of the study (<https://www.youtube.com/watch?v=sPniAlccInE>) although it repeats the only two characters were examined myth without presenting Fig. 6 (when all the rest were shown) proving otherwise, the host then repeats all the above problematic criticisms. And then gets worse by slyly planting the notion, mainstream media pundit-style, that just maybe the study was published to garner publicity for those involved which would include the publishers. That is technically slander. Of course the paper could not have been published without the approval of the reviewers, who had no personal interest in its publication. And this nastiness after the reviewers were tough and demanding. The truth is that Springer/Nature who handles the PR did not initially show interest in doing outreach to the news media until alerted the a major paper was going to do an article. The host needs to retract this fake news falsehood on his program. And while doing so tell his viewers what the paper really says. That applies to all the visual media on the paper so far.

Facts v Realities – A Quick Wrap Up with a Final Criticism of the Criticism --

Planted in the paper were all sorts of cautions for professional readers as to what it actually contains and so forth because I know from long experience how these things work. Yet they have been pretty much ignored by a number of researchers. Over all, most of the commentary has not been in accord with what should be high standards of scientific discourse in popular venues. More on that below.

THE EB PAPER IS A SOUND AND VERY BADLY NEEDED PEER REVIEWED ANALYSIS THAT HAS SHAKEN UP A COMPLACENT PALEO ESTABLISHMENT

Contrary to claims that 'the *Tyrannosaurus rex* is the only species question' have been well settled, the issue had never been close to being properly addressed, and the subject of the number of species was stagnant. As even some critics have said, the EB paper will be getting paleo behinds into gear to address the issue (see below concerns about how this will be done).

To be blunt, the paper should have been praised from the get go. The study presents a whole lot of data and analysis that no one else would have put out there, and has not been shown to be flawed. It is the first to show that *Tyrannosaurus* apparently suddenly went from all robusts to a much wider proportional variation previously seen only among different tyrannosaurid genera. If not for the study, paleos would still not know that femoral variation is greater in *Tyrannosaurus* than all other tyrannosaurids combined, and that the last gracile *Tyrannosaurus* are very peculiar beasts, etc. and so forth..

Think about it. What was our alternative? Our team had done all we could do with the specimens and data on hand including their stratigraphy and anatomical data we consider reliable. Where we supposed to just then hide it? We published the data in a leading bioevolutionary journal after passing it through demanding peer review. Knowing there was a real risk of others using the data to name the species, we named them, with approval of the reviewers.

So what is the real problem with the paper that has instead sparked such swift, autobacklash not directed towards other similar efforts? By critics who show signs of not having carefully read the full contents of the paper they went after with such intensity? The *Tyrannosaurus* speciation pattern is actually quite clear, and in the case of another dinosaur would not be controversial and receiving such over the top criticism?

As Larramendi in his letter notes, one can suspect some are reacting so angrily because we have shown that the dinosaur paleo community had been sitting on their duffs for too long on this one.

THE FUTURE

Media Coverage and Commentary

For paleontologist commentators –

Read the entire study closely, and then be sure to characterize the contents accurately. If one is uncertain about the contents, qualify one's observations, such as "it seems to me the paper uses two characters which if so could be on the low side" rather than "the paper is using just two characters and that is way too few." Even if skeptical, be generous as long as the contents are not screamingly out of line with reality, such as "I am skeptical after my initial read through, but it is a new study and perhaps will hold up with consideration, or with future work. Or maybe not." Outright predictions that the paper will not prove correct are often problematic at best. Do not make exaggerated claims about past work that supports your position that is contradicted by the new analysis. Do not be snarky, all the more so if you cannot show that the paper is clearly and critically errant in its contents (as have been recent papers based on overestimates of giant Triassic sea reptile masses by a factor of four). How about contacting the author/s to see if they can clear up some of your concerns before you offer them out the public that lacks the means of judging them?

Perhaps think this is overreaction to the overreaction to our paper? First, you have probably never experienced something like this. And what if you did? How would you feel if you put honest effort into a peer reviewed analysis and got bashed like the EB did? Would you wave away the charge that your properly vetted paper was published for reasons of publicity not science? Very probably not and you should demand a withdrawal of such a charge. How about if the contents were being extensively misstated and your ability to respond was limited? Anyone in that case should mount an effort to correct the record.

Some of you good people (scientists and dinobuff video hosts) need to – after having really closely read the paper including the supplement – need to give serious consideration to withdrawing or modifying the seemingly off the cuff statements that have been made. Regarding the science in and around the paper – and that unethical smear about the motivations behind the publication. Aside from wanting to see the record set straight on the paper, we all do not want over the top criticism of new papers to become the future norm. Right?

For the science news media –

When a set of researchers commenting on an inherently controversial topic pile on the criticism of an analysis, it may be best to be skeptical. If the commentators say too few characters were used then wonder how the paper could have gotten through peer review, and check the paper and/or ask the authors if that is correct. It might be a good idea to

reach out further in the science community to see if there are others more supportive of the work. As for those doing voice discussions an author of the paper, or at least someone who favors it, always needs to be included in the discussion. The former has the advantage of best know what is actually in the paper and counter criticisms in real time.

Any future coverage needs to include the authors to give us the opportunity to explain what the paper really says and why, and address the criticisms.

Pod/videocasts –

Many video hosts seem to think it OK to critically comment at length on a study they may not be qualified to do so on without first doing the courtesy of obtaining the views of the authors. Never ever do that.

Future Technical Research –

In the end it will be the coming technical work that will address the issue. Whether it can actually settle the issue is another matter as we discuss in the supplement. About that, what Hone (TL vid) noted on the subject was as unsettling as it is probably true. He goes on about how there are quite likely others itching to publish potential technical rebuttals of the EB paper and fast. Very possibly so. Of course our team looks forward to follow up studies that our work is intended to inspire, but they must meet basic scientific standards. Will quick replies be actual fair and balanced science?

The EB effort itself sets some standards. It was far from rushed. Or biased. I initiated the project almost a dozen years ago because I was wondering what was going on within the genus *Tyrannosaurus* – just one species, or more like in *Triceratops* as was being shown at the time? An obvious question to ask. I and others patiently accumulated the data and did the analysis with help of others and when the work produced solid interesting results we vetted them through peer review. Had the data indicated one species then new species would of course not been named. Any future academic replies need to be similarly prudent, and done over the time required even if is long, rather than potentially panicked efforts to overturn the tyrant lizard emperor and queen in defense of the beloved king. Think about it. If, as others desire, much improved stratigraphy is needed, then won't that take decades to produce if it can be from across a large span of the western northern plains in sediments with little in the way of radiometrically datable layers? If the BHI specimens are discounted, as Carr and some others wish, then will it not take decades to build up a database big enough to test the question? So how can a response that is both quick and sound going to be generated? And if such rapid response effort is mounted, does that not confirm that even paleontologists who are supposed to exhibit calm dispassionate rational analysis are indeed affected by *T. rex* fandom? The possible race to address the EB paper has the smell of an ideology in favor of *T. rex* traditionalism, over objective hypothesis testing, so there is reason to be skeptical.

For replies slow and all the faster to meet scientific standards they must meet the below criteria:

They must address what the EB paper actually contains, not what some may think it contains. One example: There are 7 features analyzed and used to demonstrate the species, not just 2.

New work in the form of a technical article should not be a fast project intended to refute the new species. It needs to be a project that takes the time to, without pre-interest in the results, properly test the contents of the 12 year EB study.

The correlations with stratigraphically placed specimens must be at least broadly comparable to that contained in the EB paper. If that is not done the paper is not able to test the number of species and cannot refute the new ones. This applies to use of the 1850 characters in the 2020 Carr study, if that cannot be correlated with a sufficient number of stratigraphically placed specimens, then they do not provide a test of the number and nature of species.

Do not simply cite that papers prior to the EB study provide serious evidence for one species because, for reasons discussed above, they do not.

If BHI specimens are excluded from new studies due to (plausible) concerns about their status, then the new study needs to acknowledge that it lacks the data base to test the number of species and cannot refute the new ones.

Authors of the new papers must thoroughly familiarize themselves with the EB supplement section on designating paleospecies, and in particular with the contents of all the papers that are cited as practical examples of doing such. It cannot without substantiation be presumed that numerous characters and bimodality are needed to sort out closely related species. Then, compare the procedures and results of the EB paper and either show they fail to meet the usual standards in which just a few or even one character/s are sometimes used to parse out sibling species, or meet or exceed them. If the latter, then acknowledge that our paper is valid, and the new species meet ongoing standards and need to be accepted. Note that this does not mean that taxonomic lumpers must use them, but that they cannot be considered solidly refuted. Or, if workers think the current standards are themselves defective and the bar for naming new species needs to be raised in paleozoology in general, then explain why, and acknowledge that a large number of other widely accepted paleospecies should be considered no more valid than ours.

If it cannot be shown that graciles are present low in the TT-zone in substantial numbers, rather than being common only high in the formations, and it cannot be shown that one incisor is at least fairly common low down, and two are the same high up, then the new species cannot be contradicted.

If the level of exactness of the stratigraphic placements in the EB paper are of concern, then the same concerns need to be cited regarding Carr (2020).

To refute the exceptionalism of the variation in femoral robustness in *Tyrannosaurus* it needs to be shown that it is not, after all, more extreme than all other tyrannosaurids combined. If that cannot be done, the extreme variation is major evidence for the existence of more than one species. This raises a particular point. We ran the stats on the entire sample of non-*Tyrannosaurus*. As noted in the paper, if the smallest juveniles are dropped, the variation within *Tyrannosaurus* is even greater relative to the rest of the family (Fig. 4C), but that came too late to run the stats on, so such is necessary to do, and will probably reinforce the multispecies hypothesis.

If many characters we did not examine are randomly distributed that probably does not refute the new species, as long as the characters they are defined by do show the trends presented in the EB paper.

Assertions that characters utilized in the EB study such as attributes of the teeth are randomly distributed within other taxa are not pertinent unless it can be shown that the same is actually true of *Tyrannosaurus*. As it is, the teeth and robustness exhibit clear patterns most compatible with speciation.

It is tempting to presume that skull display characters are a means of further assessing the species status of *Tyrannosaurus*, as are the horns and frills of *Triceratops*. On the other hand, these features are highly sensitive to sexually dimorphic and ontogenetic factors, and may prove more useful for sorting out sexes within species than in determining them (*Tyrannosaurus* sibling species distinctions may have been more focused on soft tissues and colors, as in lions and tigers, or species of giraffes). It is possible that being chronospecies that there was not sexual selective pressure for at least one of the descendants of *T. imperator* to change hard tissue display features. Hindering assessments is the more subtle form of these low lying ridges and bosses in the predator compared to its big horned prey, which makes them difficult to quantify for comparative purposes in terms of more precise numbers and ratios.

As Jay and Scott explain in their videos, the burden of proof is not automatically upon the multiple species hypothesis relative to the single species alternative, or the reverse. In the basic sense the alternatives are equal, with preponderance of evidence needed to favor one over the other. That one species was favored for decades by the public is entirely scientifically irrelevant, and that paleos have also accepted it is not important because there never had been a solidly based demonstration of one species, so that reinforces that monospecies is not the null hypothesis. That the evidence shows or suggests other dinosaurs in the TT-zone were undergoing speciation if anything indicates the more than just *T. rex* option is more probable, and if there is a pattern in accord with speciation then that is probably correct. That *Tyrannosaurus* was a high metabolic rate endotherm with a high reproduction rate is also strongly suggestive that it speciated in the ~1.5 million

years of the TT-zone. Note that the MSH remains viable whether the evidence favors two, three or more species. (Historical analog. In the 80s I was sharply criticized for applying feathers to small nonavian dinosaurs because it was argued the null hypothesis was no feathers present unless fossils showed otherwise. I countered that scales were not preserved with small dinosaurs either so in that regard the evidence was neutral, and the strong evidence for birds being dinosaurs, plus for high metabolic rates in nonavian group members, indicated feathers were at least as likely in nonavian dinosaurs if not more probable. In the 90s fossil finds began to prove that I was right, and feathered dinosaurs are now universally accepted.)

To put it another way, it is not just the multispecies hypothesis that needs to be positively supported, so does the monospecies theory to the same degree if not more since other dinosaurs at the time were speciating, so *Tyrannosaurus* not doing so would be quite atypical. If the data presented in the EB paper is reasonably accurate, it is well explained by more than one species. It is one species that presents a host of problems. If the observed correlations between robustness and teeth is good, then those who favor just *T. rex* need to explain the nonrandom pattern in the context of a single species – why no graciles in *T. rex* at first which retained ancestral stoutness, then a doubling or more of variation in the various elements in the same species driven by the appearance of derived gracility, when that would either indicate a degree of sexual dimorphism not seen before that is itself indicative of a new mode of reproduction that indicates a new species, or represents a split into species into different taxa replicating that seen in earlier contemporary robust and gracile tyrannosaurids. Are specimens showing such extreme variation really going to be placed in one species when the divergence exceeds that seen in the genera *Gorgosaurus* and *Daspletosaurus* combined, much less all tyrannosaurids put together? And how and why did just the one species *T. rex* start out with the long standing ancestral condition of two incisors and suddenly shift to the derived condition of just one and not become a new species when the graciles were also showing up? If the observed pattern cannot be positively explained by nonspeciation, then the multispecies hypothesis that explains it all at a time when other dinosaurs were exhibiting Darwinian speciation is, if anything, superior. In that case it becomes an issue of discerning what the species plural are, and naming them in best accord with the data on hand, which we did.

Exhibit curators –

Simply re-identifying display skeletons from *T. rex* to *T. regina* or *T. imperator* is not necessary. But curators on the opposite extreme ignoring the results of a peer reviewed study (as per O'Connor, NYT) is a disservice to your public. Many visitors who are checking out your *Tyrannosaurus* are going to know that there are now those who contend there are multiple species. Not letting them know the situation regarding your specimen/s if it is not now assigned to *T. rex*, all the more so if it is the holotype of *T. imperator* or *T. regina* – including casts of those – risks causing some visitors to further distrust those scientists who don't think they need to know about that subject. You can instead use the controversy as a teaching moment as outlined in the supplement.

Controversy in science such as disputes over names, is a normal and good thing, science not being dogma. We do have concerns that some may be reluctant to tell their attendees about the science lest they lose the visitation draw of having a *T. rex*.

Wikipedia –

Because Wikipedia is probably the primary information source for most people about fossil creatures including *Tyrannosaurus* they must maintain consistent standards. Until now when new species are named in a peer reviewed publication, in a day or two they are placed in the taxobox on the right side of the first page. Sometimes a question mark is used if there is some question about the name. Species may be removed from the prominent location when new peer vetted research makes that necessary. There have been no exceptions to this. Until now, *Tyrannosaurus* received special, discriminatory, nonscientific treatment. The new names are still not highlighted in the taxobox when this is posted, despite protest raised by the senior author. The Wikipedia editors made an arbitrary decision based on initial news accounts and podcasts featuring the problematic, nonpeer reviewed comments to limit mention of the new names to a brief section of text. Examination of the discussion on this decision appears to reveal a bias, driven in part by seeming ad hominem criticisms of me.

Wikipedia needs to immediately place the new species in the taxobox, with question marks if the editors prefer. They should both be removed only in the event that future peer reviewed research, following the requirements detailed above, firmly establishes that there could have been only the one species *T. rex*. If future work is not able to do that, but does not strongly verify the two species either, then the names should remain in place, perhaps with question marks. That situation may remain in force for an extended period. If the species are supported then the question marks need removing. It is possible that one of the new species will be sustained while another is shown to be substantially weaker or errant, in that case adjust the contents of the taxobox accordingly to reflect the scientific complexities, rather than the simplistic scheme that is currently misleading readers.

Links

Evolutionary Biology paper main text
<http://www.gspauldino.com/Trex2022.pdf>

Link to important supplement
<https://link.springer.com/article/10.1007/s11692-022-09561-5>

Terrible Lizards podcast with guests Tom Holtz and Dave Hone
<https://www.youtube.com/watch?v=bgZUE8tiLGs>

The New York Times
<https://www.nytimes.com/2022/02/28/science/tyrannosaurus-rex-species.html>

The Guardian
<https://www.theguardian.com/science/2022/mar/01/tyrannosaurus-rex-may-have-been-three-species-scientists-say>

Reuters
<https://www.reuters.com/business/environment/scientists-propose-tyrannosaurus-had-three-species-not-just-rex-2022-03-01>

CNN.com
<https://www.cnn.com/2022/02/28/world/t-rex-three-different-dinosaurs-scn/index.html>

Natural History Museum (London)
<https://www.nhm.ac.uk/discover/news/2022/march/controversial-paper-suggests-there-are-three-tyrannosaurus-species.html>

Daily Mail
https://www.dailymail.co.uk/sciencetech/article-10560927/T-Rex-actually-THREE-species-fossil-analysis-reveals.html?ns_mchannel=rss&ito=1490&ns_campaign=1490

National Geographic
<https://www.nationalgeographic.com/science/article/call-to-split-tyrannosaurus-rex-into-3-species-sparks-fierce-debate>

Telegraph
<https://www.telegraph.co.uk/news/2022/03/01/triple-threat-experts-say-speedy-queen-t-rex-one-three-separate>

Smithsonian
<https://www.smithsonianmag.com/smart-news/scientists-propose-that-the-t-rex-was-actually-three-different-species-180979663>

Times-Call

<https://www.timescall.com/2022/03/06/scott-rochat-rochat-can-you-see-triple-your-t-rex-triple-your-fun>