There’s a lot to commend about this MS – the sheer amount of anatomical description and figuring alone recommends it as a useful contribution. However, I do have some pretty deep concerns regarding some of the stuff touched on in the phylogenetic analysis portion of the paper, and I think that needs some heavy revision to make it publishable. The rest of my comments are generally pretty minor in terms of effort, but I think they do point out some areas where some additional reflection might be worthwhile.

Revised diagnosis

There are 7 autapomorphies here, 4 of which are problematic in some way for me (see line comments below). Also, Tschopp et al. (2015), where *Galeamopus* was named, suggested 13 autapomorphies were required for generic separation. I’m not a fan of that kind of hard-and-fast rule, even if restricted to a single analysis, so not having 13 here isn’t a problem for me, but there were 15 states described by Tschopp et al. (2015). Five of those are here in the revised generic diagnosis and some (three, I believe) are in the diagnosis for *G. hayi*, but I am left wondering where the other seven are. Also, and again, I’m not suggesting this should be the sole criterion, but it seems odd that the two species of *Galeamopus* should themselves be separated by more character states (14, combined) than would seem to diagnose the genus itself (currently 7), *particularly when the analysis upon which this paper is partially based describes 13 states as the minimum for generic separation*. It does not matter to me particularly how these points are addressed but given that the authors are the originators of the criteria, I think they need to discuss it.

On a more philosophical note, I have concerns that these sorts of character state change criteria can potentially tread too close to trying to interpret how “important” a change is – the authors have not done so themselves (nor do I suspect that they will), but I worry that this sort of thing becomes inevitable. I am also aware that phenetics is currently having a bit of a moment in neontology for workers trying to *identify* taxa at the species level and below, but it’s been repeatedly demonstrated that cladistics is a more reliable tool for *discrimination of lineage relationships at all levels*, which is what the authors are primarily doing here.

Lines 209-211: this seems like a really hard character to replicate – e.g. what does “strongly constricted” or “minimal” mean? Is there a proportion you can provide here?

Lines 212-217: these three characters are based on some pretty rare material (atlas/axis) – how widely are these distributed, really?

Material

Line 188: “11 cervical vertebrae” – this number doesn’t match up with either the material listed on the next line (either 10 or 13, depending on interpretation) or the number listed later in the paper (13).

Postaxial cervical vertebrae

Lines 758-759: I find this very interesting, given the distribution of pre-epipophyses in sauropods generally restricted to taxa with very elongate cervical centra, including the diplodocine *Barosaurus*. Do you have any thoughts on why this might be?

Line 782: There's only one species of *Australodocus* (and only two, presumably adjacent, vertebrae), so I'm unclear on how serial variation was used to delimit species in this case.

Dorsal vertebrae

Lines 886-888: Is this implying that the carnivores *bit a dorsal centrum in half* during feeding? Because that seems hard to imagine, for a variety of reasons – from purely a strength standpoint, the bite force there would seem to be staggering; there would seem to be no nutritive value in what little muscle and pulmonary tissue would surround the centrum; and from an anatomical perspective, the centrum itself would have been fairly inaccessible, would it not? Given the preservation of the element, wouldn't taphonomy seem to be the more parsimonious conclusion?

Discussion

*Phylogenetic position*

I find no issues with the phylogenetic position of *Galeamopus* remaining unchanged, and were this the only bit of information here I'd be equally ok with not including a cladogram. HOWEVER, the paper mentions a relatively minor computing error that returns what would seem to be a pretty important change in conclusion for some of the authors' recent work – that is, it would seem to overturn the authors' previous claim that the genera *Brontosaurus* and *Apatosaurus* can be used to describe discrete lineages. Correcting the error in the input file results in either a polytomy of "most" of the apatosaurines (that is, *Brontosaurus* and *Apatosaurus*) or a non-monophyletic arrangement mixing species assigned to both *Brontosaurus* and *Apatosaurus*.

The authors note that the results of pairwise dissimilarity analyses are not affected by this, but if the groups **cannot be distinguished by phylogenetic analysis, it is unreasonable to assume they are discrete lineages**. If nothing else, I feel this merits inclusion of at least two cladograms (the equal weights consensus and the implied weights consensus) – as worded it's not particularly clear exactly what's happening with the species arrangements and I think it's pretty important to divulge exactly what's going on with this group when the input error is corrected. I also would like some further explanation of why the authors place more weight on phenetics than cladistics, since as above that seems very problematic to me.

*Ontogenetic implications*

Re: the 'canal' connecting the aof and paof – I find little evidence that this could be a "seam" formed by late fusion of two processes of the maxilla. If anything, it most closely resembles the condition caused by a major nerve or blood vessel overlying dermal bone when closely appressed by other tissue (see the many clearly indicated cranial blood vessels on the inside of braincases in sauropods).

*Diplodocoid diversity*

I would use extreme caution in describing the Morrison ecosystem as a monolith – we often consider large swaths of it to be more or less equivalent, but there is a huge geographic and chronologic component to the Formation, with several notable variations in ecosystem type throughout.

I'm also not sold on the "rapid speciation" evidence – there's no diplodocids from the lower-most strata, but there's hardly anything at all from those lower-most strata and there's not a lot of rock there and even less terrestrial rock. As you grade toward the top of the sections, you are also moving inland as the Sundance Sea retreated, and the vast majority of what we know about Morrison dinosaurs comes from the two upper-most members. It's not unreasonable to assume that the progenitive taxa for the Morrison biota are simply not recorded in these rocks for that very reason – I'd say at least as likely as it is to assume that we're seeing a mass radiation event play out before our eyes.