A riposte to an unbalanced thrust by an unskilled poseur

"Ah yes", to use the referee's parlance, cue in the clownage. The commentary completely misses primary points of the manuscript, and instead stabs at fantastical phantoms. It is only with an explosive echo of irony that a blind referee makes baseless remarks, and then suggests that we have some sort of agenda, well, other than presenting important observations correctly. In Italy, we call this "la lingua batte dove il dente duole" (the tongue beats where the tooth hurts).
Perplexed by the jaded commentary but being fairly open-minded individuals, who happen to believe in edification and opportunities of open review, we respond thusly:
(1) This is a good example of a bad review, where bad does not mean negative, but simply bad. Almost all criticism seemingly derives from a pre-conceived and false framework to a cool problem that mostly lies outside the domain of the manuscript. Amusingly, some of the commentary, which includes personal barbs, displays a high level of ignorance;
(2) This is a good example of where even a bad review can be used for educational purposes and manuscript improvement.
To the editor, referee, and any reader, we elaborate on these aspects below.

Sincerely,
Claudia Agnini, Gerald Dickens and Jan Backman

Comments and Responses by all authors:

Legend=>
Regular: Reviewer #2's comments
Italic: Comments and Responses

This paper characterizes the PETM carbon isotope excursion at the Cicogna section and puts the calcareous nannofossil changes observed there into this context. The title makes claim to explain the detail of P/E global carbon cycling, but this is not accomplished in anything more than a descriptive way.

This is not an accurate summary of our manuscript. We characterize a multi-million year time interval at Cicogna that happens to include the PETM, and show that the carbon isotope and nannofossil records at this location correlate with those at the two existing locations where such comparable records exist. The title makes no claim on the PETM or to modeling the carbon cycle. One might infer correctly, as indicated in the text, that we are very much trying to set up the appropriate detailed template for understanding carbon cycle changes in the early Paleogene, including the PETM. This is because such a template is sorely missing from the literature, an aspect under-appreciated in many works, where either the PETM is separated from surrounding time, or the stratigraphy is incorrect.
We particularly recommend that any reader carefully examines papers by Cramer et al. (2003) and Kirtland-Turner et al. (2014), as one can realize the issue. In both these papers, early Paleogene carbon isotope records at multiple sites have been aligned with each

other, but without recognition that the records are incomplete (because of core gaps, such as at Site 690), disturbed (because of coring, especially at Site 1258), or both (such as at Site 577). Unsurprisingly, given the incomplete records, both papers arrive at an interpretation that somehow carbon injection during the PETM was special and unrelated to surrounding time. Indeed, this seems to be the false framework from which the referee makes commentary.

Essentially, the authors only identify the PETM CIE at Cicogna and look at the nanno assemblage changes and compare those changes to other sites.

This is not correct. We show that a much longer interval at Cicogna, one that includes the PETM but also other carbon cycle variations, correlates with locations elsewhere.

There is no modeling of the carbon cycle, etc. I suppose an observational template is useful but hardly seems novel considering how the PETM has been beat to death and we still don't understand what caused it at a very fundamental level. There is a great deal of folderol in this paper on other CIE's with close proximity in time, and speculation on what they may or may not mean and why they do or do not correlate to this section, etc etc etc. I would suggest scrapping all this and moving the discussion of those other CIE's to another paper as they are very distracting.
The PETM is complicated enough without these other (probably mechanistically unrelated) CIE's muddying the waters. I recognize Dickens' prose on those sections (I review a great deal of his work) and so the motivation becomes fairly transparent as an agenda driven distraction rather than constructive comparison.

This comment, and others below, highlight ignorance in the referee's general understanding of the early Paleogene, carbon cycling, or both. The referee seems to adhere to a bizarre view that the CIE across the PETM (almost universally ascribed to massive input of organic carbon) was somehow separated from surrounding time (when storage and release of such carbon occurred). We mostly agree that the PETM has been beaten to death and remains problematic in details, in part because of some truly dreadful papers published over the last few years. We totally disagree that this, therefore, means that the surrounding time is not important. In fact, we argue that the PETM can only be understood by considering surrounding time, but such discussion lies beyond the scope of the paper, which is mostly about getting records aligned correctly on the multi-million year time scale.
There is no agenda in our manuscript in regards to past carbon cycling, although we do emphasize a crucial fact: there are no significant carbon isotope excursions for 1.6 Myr following the PETM in any stable isotope record generated to date, including at Cicogna. This finding is consistent with some views as to how carbon cycling occurs in the time domain, but not others. We suspect that the referee is fixated on some view for past carbon cycling that is incompatible with our findings.

My other major comment is that if the authors want to compare the CIE at this site to any other site in the deep ocean, they must also compare it to site 690. That location has by far the most detailed deep-ocean isotope record (e.g., see Thomas>et al 2002), which is quite a bit discordant with the bulk $\delta^{13}C$ shown here. At 690 the surface, intermediate and deep-water dwelling forams all show the excursion at a different time and with different magnitudes, beautifully recording the surface-to-deep perturbation. I cannot for the life of me understand why scrappy 1262 and (groan) 577 are used here instead of far superior 690.

We were not sure how to respond to this commentary initially, because it shows a stunning lack of knowledge. With some thought, we decided to educate the referee, including by following the recommendation absolutely.

At the root level are two issues. First, the main point of our manuscript is not to compare records across the short-term PETM; rather, we are trying to align and to understand comparable records over a much longer period of time surrounding the event. Second, it is mostly well known that the CIE across the PETM (and presumably other similar excursions) manifests differently in different carbon-bearing phases [e.g., Sluijs and Dickens, 2012]. (And yes, we are acutely aware of the interesting offsets that arise through comparisons of various foraminifer and calcareous nannoplankton stable isotope records at Site 690 and elsewhere, and have offered multiple explanations for such offsets beyond the overly simplistic idea of a surface-to-deep water penetration of ^{13}C depleted carbon).

At a medium level, we can answer the referee's bewilderment directly (although this is already stressed in the manuscript). Sites 577 and 1262 are, to date, the only sedimentary sequences with both detailed stable isotope measurements and calcareous nannofossil counts for the time interval spanning from about 58 to 53 Ma. The correlative lower Paleogene sedimentary record at Site 690 is not complete, because it lies in a single hole (690B) with core gaps, and nobody has yet generated detailed calcareous nannofossil counts, although a good low resolution qualitative effort was made (Pospichal and Wise, 1990). The late Paleocene and early Eocene paleomagnetic record at this site is also problematic (Ali et al., 2000).

At a higher level, we take this completely misguided commentary, and compare similar data at Site 690 to that at Cicogna, 1262 and 577. In contrast to referee's assertion, we suggest that available information at Site 690 adheres to the early Paleogene template that we describe in this paper (attached Figure), albeit this is through a "frosty glass" view, because of stratigraphic problems and sampling resolution issues at Site 690.

The primary discordance in $\delta^{13}C$ records does not lie between locations (e.g., Cicogna, Site 577, Site 1262 and Site 690); rather, it lies between different carbon-bearing phases at a single location (e.g., Site 690). We strongly suggest that this is NOT telling us about changes in how carbon cycles works (as inferred by the referee), but instead how changes in carbon cycling are being recorded (Sluijs and Dickens, 2012).

Is it the magnetics and nannos? It almost appears as a deflection – can this record not be explained based on our understanding from 690?

To summarize, at Site 690, the stratigraphic record is not complete, the paleomagnetic record is a mess, and the calcareous nannofossils have not been examined at sufficient detail.
We will add the attached figure and supplementary text that explains these issues in regards to Site 690 and comparisons to Cicogna and other locations.

The comparison to the New Zealand records is of marginal usefulness and also distracting. No one uses that New Zealand slope work as a benchmark for anything. For CIE in robust shallow water or shelf locals, the Atlantic margin has a phenomenal record that is basically not acknowledged here.

At this point, we honestly wondered whether the referee was trying to make a good joke with his/her review, or whether he/she was making inane commentary from some clouded perspective. Sadly, it appears to be the latter.
Again, this manuscript is not focused on the CIE across the PETM! More to the point, there are numerous lower Paleogene sequences now exposed in New Zealand, but the ones discussed in our manuscript were originally emplaced in a compatible depositional environment – the middle to upper continental slope. This is unlike any well-studied section along the western Atlantic margin, excepting perhaps ODP Site 1051, which has some major stratigraphic problems.
Within the context of broader time, there are huge problems with the Atlantic shelf sections, most notably that they span time with numerous intervals of non-deposition separated by rapid deposition. In fact, the end of the PETM is not recorded in any available section from the western Atlantic near-shore margin that we know of (although we are certainly open to learning otherwise). The beauty of examining lower Paleogene sections in Spain, Italy, and, yes, New Zealand, is that, in these regions, there exist continuous sections from the slope, as well as complicated discontinuous sections from the shelf (aka analogs to those from the western Atlantic margin). A good understanding of the early Paleogene world cannot be constrained using sections from the western Atlantic margin. However, we will acknowledge that if one focuses on the western Atlantic sections in isolation from those elsewhere, some truly terrible interpretations can be concocted.

I am not able to fully evaluate all the work on nanno assemblages. They look reasonable to me, but it should be no surprise to anyone that the timing does not line up with other locals. It is almost a guarantee that the absolute first/last appearance of taxa XYZ were not captured or recorded at this one site. The nature of biostratigraphy makes it ill suited for 'high-resolution' work in that way.

This poorly worded comment, if we follow correctly, not only misses primary points of the manuscript, but further demonstrates faulty misconceptions. There is no a priori reason as to why the abundances of certain calcareous nannofossils should not correlate across multiple sites in response to global changes during the Paleogene at the <100 kyr scale. In fact, one might suggest this to be true, given that many deep-sea records for the time interval have been aligned using calcareous nannofossil biozones. The difference here is that in our manuscript we are trying to constrain and understand this issue rigorously.

Overall, this paper tries to extend a great deal out of this data set but I think falls somewhat short, so the authors are left to pontificate on global trends and comparisons instead of interpret them explicitly. Stylistically I was somewhat irritated by the tone of the prose as quite preachy.

We do not understand the first part of this comment. We are happy to amend the writing, but without specifics, have no response other than the following. Clearly, we have not preached enough here or in other papers, otherwise it is impossible to explain how a colleague could write such a completely clueless review.

4332: similarity between the PETM and other hyperthermals is really not much more than an observational comparison – they are quite distracting here.

This is an odd and incorrect comment. The PETM and other hyperthermals exhibit similar characteristics in multiple parameters at multiple locations, and there are theoretical reasons for why this should be the case. We cannot help it if some people choose to ignore basic observations.

4335: This laundry list of why nannos are useful is missing one key assertion. Nannoplankton live demonstrably within the photic zone and as such should be most senative to the air-sea gas exchange disequilibrium presumed to be present at events like this. It sure would be nice to see what the $\delta^{13}C$ change looked like in nannos.... If authors want to be serious about discussing a “Carbon cycle template”, I think the real story is in the fine-fraction vs. foram isotope records from sites like this. With bulk measurements it is impossible to distinguish the two and that could be the reason for the discordance between magnitude of $\delta^{13}C$ change at this site vs. others.

This comment is mostly incorrect, although we will revise things to explain better. As noted above, the “discordance” is not between sites, but between phases at various sites. We also know that, at least in more recent sediment, there are offsets between the $\delta^{13}C$ of various planktonic foraminifera and calcareous nannoplankton at the same location, but that the $\delta^{13}C$ of bulk sediment mostly tracks that of calcareous nannofossils, because they dominate the total carbonate mass (e.g., Reghellin et al., 2015).

4337-25: The Atlantic margin, particularly IODP Leg 174AX has been far more useful than these New Zealand records. The Rutgers group and the USGS have produced some really key shelf/shallow water records of the PETM – why special note to these New Zealand sections while the bulk isotopes from e.g. Millville are completely ignored?

As noted above, the Atlantic margin records do not give complete sections over the interval of interest. Records from this area, while definitely containing important information, give "bits and pieces" of a complete record, a fact that becomes very obvious from examining slope sections now exposed in Italy and New Zealand.

4338-24: Those are really large samples, particularly when we can regularly measure 15-20 ug of carbonate.

These are the size of samples collected. We will clarify.

4340: Why is this not compared to site 690? This site should be a benchmark for all deep-sea discussion of the PETM.

We have now done so, and one can see the problems.

4341-29: I suppose for oceanographers this level of resolution is "high" but really isn't to the rest of us.

Agree. High-resolution is a relative term. We will change.

4346: Ignoring the Atlantic margin record seems a glaring omission.

This is addressed in the above commentary. Basically, a long-term collective template for carbon isotopes and calcareous nannosfossils cannot be constructed using discontinuous sequences. We would think this is fairly self-evident.

4347: It's become abundantly clear that we cannot comment on the magnitude or speculate on carbon cycling from bulk isotope records. As the authors do note, it has become abundantly clear that the bulk records are discordant with e.g., single foram records, etc. and may or may not show the full $\delta^{13}C$. In fact, without knowing exactly who/what comprises this record it is not terribly useful for this type of work.

This is not correct at multiple levels. The first thing the referee should realize is that planktonic foraminifera, especially those that likely had photosymbionts (e.g., morozovellids, acarininids), probably did not passively record ambient δ^{13}C *at a fixed water depth during changes in the surrounding environment. More importantly, the very fact that bulk carbonate records align in detail at multiple widespread locations invalidates the referee's assumption.*

4347-18-21: Ah yes, cue the mysterious tipping point.

… and the better explanation is … hmmm … treat the PETM as some strange anomaly, divorced from surrounding time, and invoke an explanation that conflicts with available data? Apparently, the referee does not like non-linear, coupled systems with thresholds. While well beyond the scope of this manuscript, we encourage the referee to read the IPCC reports, as these works discuss multiple examples of such behavior in a warming world.

4348-5-10: Or the opposite! See 2014.

We are unsure of the point and the paper. May be the reviewer #2 refers to Self-Trail et al. (2012). If this is the case, the authors did not mention anything about the issue in this paper. Instead, they did such a good work in highlighting the possible differences in the response of calcareous nannofossils to the PETM in a shelf depositional setting but this has been already included in the text when we discussed about the possible regional/local effect on the perturbation affecting the calcareous nonnoplankton communities during the PETM.

4349-7-15: And this has happened in a uniform manner? I really doubt it given the change in %CaCO3...

We are not exactly sure what this comment means, as we did not state that shifting of $\delta^{18}O$ *would be uniform. In fact, it should not be uniform, as clear from the references provided.*

4349-21: What happens if there are changes much faster than this?? Uh oh.

This is a rather strange comment. We know very much how things work when massive amounts of ^{13}C*-depleted carbon enter the exogenic carbon cycle really fast (<<2000 yrs), particularly into a single reservoir such as the atmosphere. It is called today. The most basic consequences are short-term differences in the* $\delta^{13}C$ *composition of different carbon reservoirs and the extent of carbonate dissolution. The has known this for a long time, including in regards to examining and to modeling records across the PETM (e.g., Dickens,*

2000). On a much longer time frame, however, one that actually pertains to our manuscript, the cycling of carbon mixes the composition within different reservoirs.
We acknowledge here (but not in the manuscript, as it is irrelevant) that some people have written some papers suggesting very rapid carbon input during the onset of the PETM (e.g., Wright and Schaller, 2013). However, there is zero good evidence to support such an assertion. In fact, once one recognizes how a rapid carbon input perturbs reservoirs differently on the short-time scale, one can generally exclude such an occurrence at the start of the PETM (Zeebe et al.,2014).

4350: It is almost a certainty that the absolute first/last appearance has been missed (through no fault of the authors). I am not at all surprised that there are differences in timing and I think the authors are trying to do a little too much with this data.

This is a rather unsophisticated comment. Biostratigraphy, especially when one refers to microfossils, is more complicated than this. If one refers to the discussions on this issue included in the most recent Paleogene and Neogene calcareous nannofossil biozonations (pag. 223 in Backman et al., 2012; pag. 132 in Agnini et al., 2014), this would immediately become clear.

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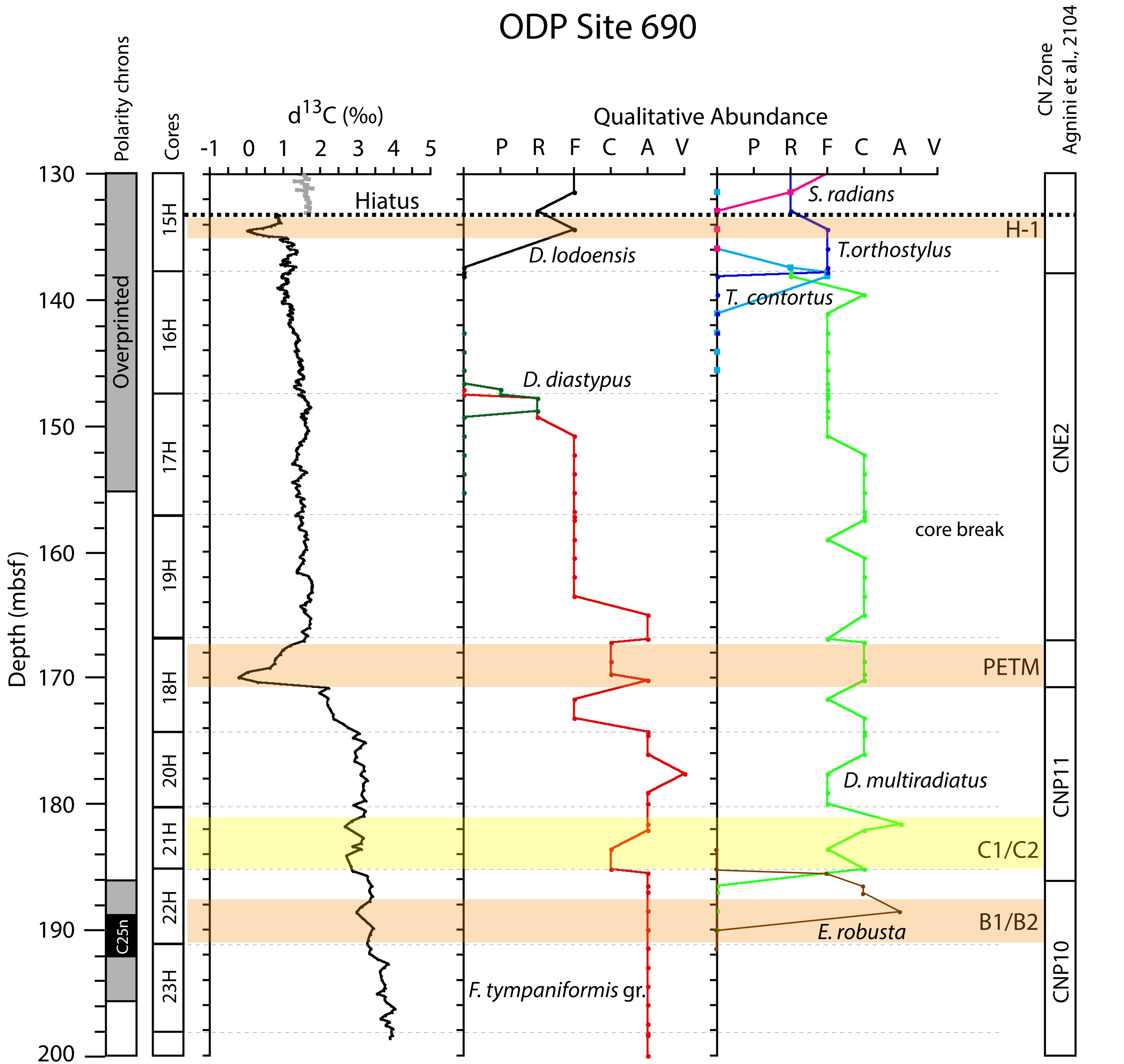

Supplementary Figure. Carbon isotope data from ODP Site 690 plotted agaist semi-quantitative abundance patterns of selected calcareous nannofossil taxa.