

# ***Interactive comment on “A massive input of coarse-grained siliciclastics in the Pyrenean Basin during the PETM: the missing ingredient of a coeval abrupt change in hydrological regime” by V. Pujalte et al.***

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Journal editors often complain about the increasing difficulties to find specialists willing to act as referees. We therefore thank Benjamin S. Slotnick for having accepted the job of reviewing our paper, and for his extensive comments. We are disappointed to find, however, that he has not found one single positive aspect of our contribution, and taken aback by the fact that he has failed to grasp key aspects of our paper and by the somewhat disrespectful way of expressing his view. Our study builds on many years of field work, and we believe it presents new and intriguing aspects of the PETM in

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the Pyrenees, one of the most important areas for research on the hydrological cycle during the PETM.

The main four points criticized by Slotnick are: 1, age model poorly constrained (meaning the identification of the PETM interval); 2, introduction weak, as it does not include “the extensive literature now existing on the subject”; 3, claim of a pre-PETM sea-level fall is ambiguous; 4, quality of the writing sub-par. We address separately each of these points below, and conclude our reply with a short summary of our view.

1.- Age model. Slotnick argues that the age model “is poorly constrained, with just carbon isotope data, and therefore nannofossil data would be needed since carbon isotopes alone do not necessitate an age nor confirm stratigraphic position, due to circular logic”. He later adds that “The authors have not done their due diligence to confirm the age of the sequence. . . one cannot date sequences from isotopes! Chronostratigraphy is a method applied to sequences with independent age constraints such as magneto and biostratigraphy [. . .] Fig. 6 does not include any form of bio/magnetostratigraphy to confirm this sequence is coeval to the PETM [. . .] Fig.11 shows NP10 just above CIE, there is no confirmation of NP9 during CIE which is nanno biozone PETM occurred during. . .” To alleviate our supposedly poor age model Slotnick recommends to draw an “overall encompassing figure in which authors correlate Laminoria and Korres sections to another section with good chronostratigraphic control such as Zumaia so that age model can be strengthened”.

Reply.- In our study we have dealt with sections of two different settings, shallow and deep marine. Shallow marine sections (Korres, Laminoria, Villalain) are mainly made up of shallow to very shallow marine carbonates, but also contain variable proportions of non-marine siliciclastic intervals. The latter are obviously devoid of marine fossils, but the carbonates include abundant benthic fossils (Fig. 3 of the paper), notably larger foraminifera. However, calcareous nannofossils are conspicuously absent and thus cannot be used for dating or correlation. Instead, larger foraminifera are employed, using the shallow benthic zonation (SBZ) of Serra-Kiel et al. (1998). It is now widely

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accepted that the PETM lies between SBZ 4 (late Thanetian) and SBZ5 (early Ilerdian = earliest Ypresian) (e.g., Pujalte et al., 2003, 2009; Scheibner et al., 2005; Vandenberghe et al., 2012). The referee correctly points out that “the late Paleocene and early Eocene was comprised of at least 16 separate CIEs”. But only one of them, the PETM CIE, occurs between the SBZ 4 and SBZ5. Our Fig. 3 shows that the non-marine units C, D, E and F are intercalated between SBZ4 and SBZ5 marine carbonates. Therefore, the CIE recorded in terrestrial unit D can only correspond to the PETM, as corroborated by the kaolinite influx. In the deep marine setting we have to deal with two different types of sections, some accumulated in the basin floor (Zumaia, Trabakua Pass East), the others in the axis of a deep-sea channel (Orio, Gonzugaraia; Figs. 7 and 9). Zumaia and Trabakua Pass East are mainly composed of hemipelagic marls and limestones rich in foraminifera and calcareous nannofossils. It is possible therefore to establish a high-resolution biozonation and magnetostratigraphy and, thus, to unambiguously constrain the PETM (e.g., Schmitz et al., 1997; Alegret et al., 2009; Storme et al., 2012). However, the succession of the deep-sea channel sections is quite different (Figs. 7 and 9), as it mainly consists of coarse-grained calciclastic and siliciclastic turbidites, which are directly and concordantly overlain by alternating thin-bedded turbidites and marlstones (i.e., mixed flysch in Figs. 7 and 9). Marine fossils are absent in the coarse-grained siliciclastics, probably due to abrasion and/or dissolution, which therefore cannot be directly dated through calcareous plankton biostratigraphy. Yet, as shown in our supplementary Fig. 1\*, the calciclastic turbidites underlying the main siliciclastic body belong to NP7/8, and the mixed flysch above it to NP10. Therefore, the main siliciclastic body must necessarily be comprised between NP7/8 and the lower part of NP10. To our knowledge, the only major CIE occurring during that interval is the one related to the PETM. Certainly, this is the case in the Zumaia and in the Mead Stream sections, to quote but two well-studied reference sections (Orue-Etxebarria et al., 2004; Bernaola et al., 2007; Slotnick et al., 2012). Based on that, attributing to the PETM the CIE occurring at the upper part of the coarse-grained siliciclastic body (Fig. 11B) is fully justified. \*We agree with Slotnick that our supplementary Fig. 1 should be

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incorporated into the main text. We did not do so originally because the nannofossil data are from someone else (van Vliet, 1982).

2.- Introduction. According to Slotnick the introduction is weak because we “only cite a few regional publications for background purposes even though there is already an extensive literature on this particular subject” (meaning the notion of an intensified hydrological cycle). He later adds that this “line of thought and interpretation is not new. As such, this paragraph should be moved to the introduction as background. Other authors have already integrated the idea that an intensified hydrological cycle, particularly enhanced seasonality as a result of warming, may have induced a flux of clastics into shelf/slope settings”. To support his line of reasoning Slotnick lists a number of papers\*\* “that have already dissected this point in great detail”, including Murphy et al., 2004; Randall et al., 2007; Meehl et al., 2007a, 2007b; Christensen et al., 2007; Slotnick et al., 2012), and eventually points out that “the authors need to cite the works before this study to give credit where it is due. This is not the first study to consider lower vegetation and enhanced seasonality in relation to a flux in clastics during the PETM.” (Emphasis added). \*\* References of these papers were not provided

Reply.- From the above quoted paragraph it seems that the referee has obtained the erroneous impression that in our paper we are presenting the case of an intensified hydrological cycle during the PETM as an original concept. Far from it. Between other reasons, because we already did so back in 2001! (Schmitz et al., 2001). In that paper we based our conclusions mainly on the occurrence of fine-grained siliciclastic units during the PETM that occur at Zumaia and other sections of the western Pyrenees. One purpose of our new contribution is to demonstrate that, in the western Pyrenees, enhanced seasonality during the PETM resulted in the influx of BOTH coarse-grained and fine-grained terrestrial deposits into the marine Pyrenean Gulf, and not only of fine-grained ones. We do appreciate the references on the topic provided by the referee, and will give them proper consideration. But it is not the purpose of our contribution to make an extensive review of the state-of-the-art on the topic. A final note on this

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point: Slotnick points out that Climate of the Past (CoP) “has global distribution and significance so [...] it is absolutely crucial the authors tie this work of theirs to other sections globally [...]. If authors choose not to do this, then this is not the correct journal of choice for this paper.” Considering that we have published papers focused on the PETM of the Pyrenees in *Geology*, *EPSL*, *Palaeo3*, and in a special volume of the *GSA*, all of them widely cited, this is a surprising statement. Unless, of course, the referee rates these journals as of local distribution and significance. Incidentally, it was the conveners of the Ferrara Meeting, not us, who choose CoP for a post-meeting volume and, as two of us are conveners of past CBEP meetings (Goteborg 1999, Bilbao 2006), we obliged. Robert Speijer cautioned against that option, and we hope we will not have to regret that we did not follow his advice.

3.- The pre-PETM sea-level fall. The referee addresses this topic in two different paragraphs, which we comment separately. (A) “ Not sure there is enough evidence to support this claim of a sea-level fall, which could be due to the difficulty of interpreting this poorly written and badly organized paper”. Reply.- Our writing might be poor, but our figures 3 to 6 clearly illustrate the occurrence of terrestrial deposits overlying upper Thanetian marine carbonates, and proof of subaerial exposure at the top of these marine carbonates at the Korres section and in many other similar sections elsewhere in the southern Pyrenees (Pujalte et al., 2014). Most sedimentologists would consider any of these facts sufficient evidence of a sea-level fall. But perhaps Slotnick knows better! (B)... “not sure a lower sea level would necessarily help delivery of bedloads to marine environments. If authors want to relate coarse siliciclastic interval during the PETM body, then they have to think about a potential rise in sea level, not fall beforehand (during the actual thermal event). Reason: thermal expansion of sea water in warmer environments. A fall in sea level, as stated by the authors, would have been before the PETM and not contemporaneous to this deposits, if in fact they are correct with their poorly constrained age model. Regardless, I am not sure they have enough supporting evidence to make this case (not during the event). Conclusion written in an unclear way to tease out whether authors discussing their pre-PETM sea level fall

interpretation of contemporaneous PETM coarse siliciclastic accumulation”.

Reply.- It is our turn to find the referee’s remarks confusing. In any case, we take the opportunity to try to clarify our interpretation with the following arguments: Inspection of Fig. 3 reveals that: - Stratigraphic correlation and reconstruction through detailed geological mapping demonstrate that, in the Laminoria area, terrestrial units C, D and E at Laminoria infill an incised valley. - Unit C is pre-PETM, which entails that the excavation of the valley was prior to the thermal event. - Most models of evolution of incised valleys agree that they are excavated during periods of sea-level lowering, and filled during subsequent sea-level rises (e.g., Boyd et al., 2006; Strong and Paola, 2008) - According to these models, unit D (that we maintain is coeval to the PETM) was accumulated while the sea level was rising. - Re-flooding of the valleys by the sea (recorded by unit G, the Alveolina limestone) did not occur until after the PETM. - Consequently, although the sea level was rising during the PETM, it was still comparatively low, lower than during the late Thanetian and early Ilerdian, when the whole study area was submerged under a shallow sea. - Delivery of fluvial bedloads to marine environments is of course independent of the position of the sea level. However, a lower sea level facilitates the accumulation of these bedloads farther within the marine platform and closer to the platform edge, making it easier for them to reach the deep basin.

4.- Quality of the writing. The referee values the quality of our writing as sub-par, backing his opinion by evaluating the quality of a number of sentences of our text. Fourteen of them are considered “overly verbose”, eleven “awkward”, four “poorly written” and others simply “badly worded”.

Reply.- Slotnick might be right in this point. But we would have appreciated some advice how to improve at least some of these defective expressions, instead than simply being commanded “Re-write!”. The referee also points out some typos, kindly indicating their correct spelling, and we thank him for that. English is not our native language, but it is not the first time some of us have had to face this kind of challenges. We have never before, however, received this kind of affective negative comments. Fortu-

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nately, more often constructive help is provided by referees and editors. Commercial proofreading services may be of some help, but more often not. They just correct misspelled words and the most obvious misconstructions. Sometimes journal editors have advised us to ask a native colleague or friend to revise our text. Unfortunately, helpful colleagues are difficult to find, and the request may result in the end of a friendship.

To end this point, we explain below the purpose of some of our paragraphs disliked by the referee:

Referee: Page 2894 lines 9: “Stratigraphical” not a word. Authors: No problem avoiding it. But there are papers and text books on “Stratigraphical Procedure” (e.g., Whittaker et al., 1991, Journal of the Geological Society; Rawson et al., 2002, Geological Society, London)

Referee: page 2898 line 9: Remove all comments about ‘economic interest’ since this is not a journal about economic geology. Just state not quarried, no need to explain why. Authors: Inland exposures of non-carbonate rocks are extremely rare or not existing in the heavily vegetated north Spain, except in quarries. We wanted to make clear why observations of unit C are so limited - because it has no economic interest and therefore is not quarried. Should the Editor find this explanation superfluous, we have no problem removing it.

Referee: page 2898 lines 19-21 . . . This referee does not see the need to incorporate economic points of the quarry, just complicates paper for readers. Quality glass not at all related to this paper, except to state the presence of quartz (that’s all). Authors: We wanted to make clear that, with the exception of heavy minerals and clay matrix, quartz is the only mineral of the sand fraction. We fail to see why this “complicates paper for readers” but, again, no problem removing the statement, at the Editor’s advice.

Referee: page 2898 lines 22-23: Remove methodology point about separation by sieving and wet spiral concentrators entirely or move to methods section. Out of place at best, unnecessary at worst. Authors: The purpose of the point was the same as

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above: once heavy minerals, pebbles and clay matrix are removed all that remain is quartz grains. This composition is a distinctive feature of unit G (PETM sands).

Referee: page 2898 line 25: Awkward. “Pebbles occur randomly dispersed in the sands. . .” Authors: The pebbles are neither concentrated at discrete horizons nor in pockets, but randomly dispersed in the sands. Why is this sentence awkward?

Referee: page 2903 line 2: delete following unnecessary fragment: “. . .the papers resulting from these studies being too numerous to list here. . .” Instead, authors should just choose three-four papers like they have and cite. Authors: With this paragraph we try to emphasize the huge difference in the number of papers focused in basin floor sections of the Basque Basin, especially in Zumaia, and the papers dealing with deep sea channel sections (Orio, Gonzugarai). Citing just three-four papers of Zumaia may not convey this fact. We do fully acknowledge the great interest of the Zumaia section, but wanted to draw attention to the fact that the deposits of this section are but an end-member of a larger spectrum of deposits in the Basque Basin.

Referee: page 2903 line 18: Authors cannot state “. . . sometimes misinterpreted.” If they are not going to cite specific examples of who did what and what was actually misinterpreted and add for reference to readers. Either add citation(s) or remove. Authors: Examples are cited: Hanisch and Pflug, 1974; van Vliet, 1982, and their misinterpretations explained.

Referee: 2904 lines 4-25: This is not scientific writing! It is simply a list full of typos, misspelled words, and other issues I have brought up in previous comments. All of this has to be rewritten into a paragraph form suitable for scientific publication. Referee: page 2906 lines 8-17: Another list! Not only is writing sub-par, this is another list. Authors need to remove all lists, as mentioned on numerous occasions and write what it is they want to say to produce scientifically valid writing. Authors: Lists may be a literary resource when the authors’ writing skill is limited. If their message is clear we see no real harm using them. But should the Editor consider otherwise, we will remove

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them.

6.- Summary. The two scientific objections raised by B. S. Slotnick (poorly constrained age model and ambiguous claim of a pre-PETM sea-level fall) are, in our view, unsupported. As per the quality of our writing, and the clarity of our figures, we will do our best to improve them. Any suggestion from the Editor to help us in this task will be most welcome.

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