Clim. Past Discuss., 11, C1383–C1392, 2015
www.clim-past-discuss.net/11/C1383/2015/

Climate of the Past
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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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Received and published: 4 September 2015

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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

Climate of the Past, summer 2015

Among the abundant literature regarding the Paleocene Eocene Thermal Maximum (PETM, 55.8 Ma ago), its carbon isotope excursion (CIE) and its impact on various

compartments of environment, I'm always pleased to read good papers. That said I'm delighted to be the referee of such a paper, namely the contribution from V. Pujalte, J.I. Baceta and B. Schmitz which I consider appropriate for publication in the journal Climate of the Past.

This paper is following a series of well documented and argued ones from the same authors which were devoted to the latest Paleocene to earliest Eocene stratigraphic interval and southern Pyrenees as a whole, but, in each case, to distinct paleogeographic areas and/or particular processes occurring in the terrestrial realm to the deep basin, including often the correlation from one to the other. Below I comment very quickly each step of their research in the Pyrenees before commenting the present paper submitted to Climate of the Past (CP), and I underline, here and later, most important features by bold characters.

In addition to the necessary regional integrated studies performed to establish the detailed stratigraphic frame of the Paleocene and Eocene of northern Spain and to reconstruct its paleogeographic, tectonic and climatic evolutions, some of their more focused previous papers first deal with the now well-known low-calcareous siliciclastic unit (SU, typical exposure in the Zumaia section, see Schmitz et al., 1997 and Storme et al., 2012). They show how it is widely represented in western Pyrenees, demonstrate its coevalness with the PETM, and then they decipher and deeply discuss the role of the climate change in its formation (Schmitz et al, 2001).

They also emphasize the role of a shrinking vegetation cover in the Pyrenees during that event and its "similar origin as detrital units previously assigned to the early Palaeogene so-called 'Siderolithic' discharge in other parts of western Europe", i.e. the Paris Basin and the North Sea Basin, where the PETM related kaolinite influx is also present but very diverse and sometimes absent (Knox, 1998; Thiry Dupuis, 1998; Quesnel et al., 2011, among other references). In relation with this part of their discussion, please note that 1) in the Paris Basin, this concerns only the Sparnacian facies (with smectite much more abundant than kaolinite) among other post-hercynian

‘Siderolithic’ discharges, and 2) the main Siderolithic paleoweathering episode and associated production of supergene kaolinite in Belgium and France has been recently dated by various methods and is Early Cretaceous in age, and even Late Jurassic in a few places of the Massif central (Yans, 2003; Thiry et al., 2006; Ricordel-Prognon et al., 2010).

Then B. Schmitz and V. Pujalte study other Pyrenean outcrops eastwards, in the terrestrial realm in the Tremp Basin, where they are the first ones to show the PETM record in the Claret Fm, enabling its tight correlation with the SU of the Basque region. Moreover, they describe the expression of its impact on land, with a conglomeratic braid plain (or megafan) replacing at the beginning of the event uppermost Paleocene semiarid coastal plains with a few river channels, and they highlight “both increased topographic relief associated with a prominent sea-level lowstand and enhanced seasonal precipitation over a dry landscape with sparse vegetation” (Schmitz Pujalte, 2003 and 2007).

Finally in a more recent publication (Pujalte et al., 2014) based on outcrop and borehole information from the Tremp-Graus Basin, they focus on the detailed chronology of the sea-level evolution across the P/E boundary. They show 1) a “sea-level fall of at least 20 m less than 75 kyr prior to the PETM”, accompanied by “a seaward displacement of the shoreline of ca. 20 km and a widespread incision of valleys in the alluvial plains and the subaerial exposure and excavation of the adjacent marine carbonate platform", 2) a subsequent sea-level rise (begun before the onset of the CIE) leading to the infilling of the incised valleys by aggradation of the alluvial plain and eventually to the (Ilerdian) transgression of the whole Tremp-Graus Basin.

Comparing with other similar sea-level evolutions around the P/E boundary from the North Sea to the Tethys, they discuss the possible mechanisms involved, notably the tectono-magmatic activity in the North Atlantic and a large change in ridge length in the Norwegian-Greenland Sea. Here the role of the climate change is not the main purpose of the paper, but the PETM record within all successions studied enables the

accurate reconstruction of the sedimentary architecture and of the landscape evolution across the study area, almost 100 km long. This is clearly depicted within their remarkable Fig. 10 thanks to which we immediately understand the major fingerprint of the climate hyperthermal event 55.8 Ma old within the sedimentation from land to sea: the PETM related megafan, fluvial sediments and their paleosols and top gypsum horizon are situated within the story (after the valleys incision and beginning of their filling by previous fluvial sands) and the strong perturbation of the hydrological regime is also shown, notably its abrupt onset through the sharp facies shift and its wide distribution along the fossil piedmont.

I have written this short review of their past contributions in order to 1) strengthen the relevance of their work in relation with the PETM global study (regional case studies are necessary), 2) highlight their previous main imputs which are not detailed in the present paper and about which some readers could ask questions or would like to know a little bit more. Although the authors have clearly summarized their previous imputs, I suppose the details were not repeated here because of the expected length limits of the CP articles and/or they were already well demonstrated by the same authors and they consider readers can find alone the references. Nevertheless, the authors could have added a little bit more citations of their own references, particularly at the end of the introduction of the present paper (about the climatic conditions and the vegetation barren landscape during the PETM, this was detailed in Schmitz et al, 2001).

Now in that new contribution of V. Pujalte and co-authors for Climate of the Past, the attention is again focused on the western Pyrenees, mainly on new outcrops and on an interval which was previously poorly studied, in order to look for “additional evidence for dramatic and abrupt changes in the hydrological cycle”. The coarse grained siliciclastics here studied (rediscovered?) are present in restricted settings in the basin (incised valleys in the inner platform and long lived deep-sea channel). They are a major component of the story indeed, and I consider that 1) the demonstration is particularly well conducted and relevant; 2) the title perfectly condenses the purpose. Moreover methods are correctly used and appropriate to achieve the demonstration; there is nothing useless, all data (observations and measurements) presented are useful and in appropriate amount and quality. Figures and photos provided are also of great quality and clarity, this is a kind of "brand" of those authors' papers, and I would like to see such pedagogic illustrations in many other authors' articles.

Furthermore, the authors can produce such a high standard study because they have a perfect knowledge of the field and available outcrops, after repeated field surveys over years, and they use and overcome the most appropriate methods. Evidences provided come from sedimentological, lithostratigraphic, isotopic and XRD new data, supported by biostratigraphy, enabling robust correlation between sections across more than 100 km long and time model to be established. Proofs of the coarse grained siliciclastics belonging to the PETM are thus conclusive, and it is also well established that they are the missing link between the SU in the deep basin and the mega fan and braid plain on land, through the inner platform. The authors are now very close to the reconstruction of the whole picture of what happened during the PETM in northern Spain.

If the paper can be slightly longer, in the part of the discussion related to the sedimentological expression of the amplitude of the hydrological change associated to the PETM, i.e. increase in stream power, flow strength, capacity of turbidite currents and volume of fine grained siliciclastics also delivered to the deep basin, it would be worth comparing those Pyrenean phenomena with the ones observed elsewhere in the world. I think here to the changes in the fluvial regimes reported for example in recent publications by Foreman et al. (2012) in Colorado and Abdul-Aziz et al. (2008), Foreman (2014) and Kraus et al. (2015) in Wyoming. I recommend also to the authors to read the review of depositional style and forcing factors of morphodynamics of rivers strongly affected by seasonal floods (Plink-Björklund, 2015) in order to check if such diagnostic facies and criteria of highly peaked discharge pattern exist in Pyrenean terrestrial successions or not.

Moreover, PETM related hydrological changes have also been shown by using other

methods than sedimentology and by performing analyses of the flora or organic content of terrestrial and marine sediments (leaf analyses, pollen and spore, δD of n-alkanes, ...) of sections studied in various parts of the world (Arctic, North Sea, UK, France, Italy, Tanzania, India, New Zealand, Colombia-Venezuela, US Gulf Coast, Wyoming etc.). If the expected size of the papers in CP enables it, they could be mentioned (I can send the references concerned if needed) to reinforce the idea that it is not an isolated phenomenon, but very likely a global one.

Bearing in mind the analogy with the current global warming, its relation with massive liberation of greenhouse gases and its impact in the environment, a next step could be to quantify the fluxes of all the erosion, transport and clastic sediments redeposition during the PETM, but this may need further studies of course, and seems to be beyond the scope of the present article.

Regarding the discussion about climatic conditions, I would have enjoyed to see also palynological or paleobotanical data to support the reconstruction of the dry landscapes and vegetation evolution, but I understand by reading their previous papers that plant debris or palynomorphs are very rare if not absent (or likely weathered in the paleosols?) in the fluvial sediments to the east and that the palynologic signal in the laminated clay beds within turbidites to the west is not very diverse and (in my opinion) likely biased, so they cannot help more.

It may be further suggested that one of the global expression of the hydrological change associated with the PETM, which is clearly shown here in the Pyrenean Gulf, is the large fraction of fine particles in suspension delivered to the marine realm, bringing also oxides and organic particles, so nutrients, and enhancing the development of some plancton such as dinoflagellates. This has already been suggested as a probable cause for the almost global Apectodinium acme during the PETM (see Sluijs Brinkhuis, 2009 and references herein); it could be mentioned here also. Another issue concerns the sediment accumulation rate's enhancement: it has also been documented on some continental margins, notably on both sides of North America (John et al., 2008). Those shelves became therefore carbon sinks during the PETM, this implies a significant negative feedback role of carbon burial and much greater than previously recognized.

A Fe oxides influx during the PETM has already been noticed in Italy also (Dallanave et al., 2010) and interpreted as resulting from “production, transport, and sedimentation of detrital hematite grains”. The detrital character of the hematite grains seems clear, but I will consider its coeval terrestrial production only when this supergene process will be dated on land at 55.8 Ma (with its proper age uncertainty, of course). However, given the present state of knowledge and available paleoweathering dating methods, this remains a tremendous scientific challenge. . .

Finally and in relation with the Fe oxide influx, a lot has already been written about the kaolinite influx during the PETM, here in the Pyrenees, and elsewhere, for example in the North Sea Basin (Knox, 1998) or the New Jersey (John et al, 2012). I think that the demonstration of their inherited (detrital) character is well established in the Pyrenees here again: first the kaolinite influx is not the rule during the PETM in the studied sections, second it may be rather variable and third in the Orio section, illite is much more abundant than kaolinite. In the North Sea Basin the kaolinite influx begins before the onset of the CIE (Kender et al, 2012) as well as in New Jersey (John et al., 2012). Moreover, in the Paris, London and Dieppe-Hampshire Basins, i.e. directly on land or on the coast, the terrestrial to lagoonal units and paleosols of the Sparnacian facies do not record any generalized increase in kaolinite production during the PETM (Huggett Knox, 2006; Quesnel et al, 2011; Dupuis and Quesnel, unpublished data). Instead smectite and illite are individually always much more abundant than kaolinite, which remains in average < 10

We cannot furthermore consider clay assemblages of marine sediments as direct evidence of coeval climatic conditions prevailing on land on timescales of 10 ky to 170 ky, which is the duration of the PETM. This has been clearly discussed by Thiry Dupuis (2000) and explained in more details by Thiry (2000) who shows that kaolinite needs much more time to be produced in weathering profiles established on hercynian or

older basements and Mesozoic rocks (> 1 My). Those Spanish, French, English and Belgian lines of evidence are all in agreement with the study of John et al. (2012) in New Jersey, which states that “viewing the increase in kaolinite as a signal for enhanced physical rather than chemical weathering would represent a paradigm shift for some Paleocene-Eocene thermal maximum clay studies”. Nevertheless, for specialists of (terrestrial) regolith and “spiritual grandchildren” of J.-J. Ebelmen and G. Millot, all this does not represent a novelty (if a little bit of wittiness is accepted).

To conclude, I recommend the publication of the manuscript of V. Pujalte and coauthors in the journal Climate of the Past. I consider the scientific question addressed as relevant, the data and methods of high standard quality and the approach and demonstration as properly valid in the Pyrenees (and also elsewhere). I suggest only to widen the scene in the discussion, if the Journal’s recommendations to authors permit a longer article.

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Please also note the supplement to this comment:
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Interactive comment on Clim. Past Discuss., 11, 2889, 2015.