

**Comments on  
"Estimating the impact of early diagenesis  
on isotope records in shallow-marine carbonates:  
A case study from the Urgonian platform in western Swiss Jura"  
by A. GODET *et al.*  
[*Palaeogeography Palaeoclimatology Palaeoecology* 454 (2016) 125-138]**

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**Highlights**

- In this study we reiterate that there is a reliable alternative, well-documented bio- and sequence-stratigraphic model in addition to the one used by GODET *et al.*
- Application of our model leads to different conclusions concerning stratal correlations.
- We question the sequence and timing of diagenetic events presented by GODET *et al.*
- We elaborate on the dissolution of calcitic ooid cortices.
- We discuss SANDBERG's and HARDIE's models of calcite and aragonite seas.

**Abstract:** A recent paper by GODET *et al.* on Urgonian carbonates from the Swiss Jura concluded that diagenesis may hinder chemostratigraphic correlations of deep- to shallow-water facies. Although we agree with this conclusion we question their arguments and interpretations. These authors correctly identified diagenesis as the key factor, but we question the timing of events in their paragenetic sequence. In particular, they reported the leaching of originally calcitic oolitic cortices but failed to discuss this puzzling feature properly. In addition, the backbone of their argument is the stratigraphic correlation of a specific sequence from three dominantly shallow-water sections together with a basinal reference section. However, a reliable alternative biostratigraphic framework shows that this attempt of long-distance correlation might be erroneous.

**Key Words**

- Paragenetic sequence;
- Calcite and aragonite seas;
- Chemostratigraphy;
- Carbon isotopes;
- Early Cretaceous.

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**Résumé :** *Commentaires sur "Estimating the impact of early diagenesis on isotope records in shallow-marine carbonates: A case study from the Urgonian platform in western Swiss Jura" par A. GODET et al. [Palaeogeography Palaeoclimatology Palaeoecology 454 (2016) 125-138].*- Une publication récente de GODET *et al.* sur les carbonates urgoniens du Jura suisse concluait que la diagenèse pouvait compromettre les corrélations chemostratigraphiques des faciès distaux vers les proximaux. Bien que nous soyons d'accord avec cette conclusion, nous nous interrogeons sur les arguments des auteurs et leurs interprétations. Ils ont bien identifié la diagénèse comme étant le facteur déterminant, mais nous mettons en doute la chronologie des événements de leur séquence paragenétique. Ils ont notamment signalé la dissolution de cortex d'ooïdes originellement calcitiques mais n'ont pas abordé sous un bon angle cet élément étonnant. De plus, leur argumentaire s'appuie sur la corrélation stratigraphique d'une séquence spécifique identifiée dans trois coupes aux faciès

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essentiellement peu profonds avec une coupe de bassin utilisée comme référence. Toutefois, un autre cadre biostratigraphique plus robuste que celui utilisé par GODET *et al.* suggère que leur tentative de corrélations sur de longues distances est erronée.

#### Mots-clefs

- Séquence paragéométrique ;
- Mers à calcite ou à aragonite ;
- Chimiostratigraphie ;
- Isotopes du carbone ;
- Crétacé inférieur.

## Editorial note

This 'comment manuscript' was submitted for publication in *Palaeogeography, Palaeoclimatology, Palaeoecology* and was rejected on the basis of the Editor's recommendation (personal communication to Bruno GRANIER, July 10, 2016) and the following anonymous review. We (the Editor-in-chief, Bruno GRANIER, and the Senior Editors, Christian EMIG and Michel MOULLADE), at *Carnets Geol.*, agreed to publish it because we think our readers should be informed and have the chance to reach their own opinions:

"After having carefully examined the Comment by Bruno Granier *et al.* on the paper by Alexis Godet *et al.* ("Estimating the impact of early diagenesis on isotope records in shallow-marine carbonates: A case study from the Urgonian platform in western Swiss Jura") as well as the Godet *et al.* paper, I came to the conclusion that the publication of this comment is not justified.

The motivations for my appraisal are the following:

### 1. Does the Comment raise important scientific issues?

The Comment of Granier *et al.* (hereafter GEA) is not focused on the core dataset, interpretations and conclusion raised by Godet *et al.* On the contrary, GEA acknowledge that the conclusion of this paper is correct. As such, this comment fails at carrying any substantial criticism of the Godet *et al.* study, but seems more related to another debate between the two groups of authors, namely the biostratigraphy of the Urgonian limestones in Eclepens.

### 2. Are the criticisms of the Godet *et al.* study contained in the Comment justified?

There are numerous criticisms raised by Granier *et al.* that are of little importance, some concerning vocabulary, that are not of primordial interest for the reader. Some are even useless and off-topic such as the first one in the text.

There are however two main criticisms raised by Granier *et al.*: Firstly, about the leaching of oolitic cortices and its interpretation, secondly about the correlation of the Eclepens section with basal reference section. For the first point, GEA bring forward original materials derived from an unpublished MSc study (Grundman, 2007) to discuss the interpretation of Godet *et al.* Although it might be interesting to hear further about this, it is certainly not appropriate to present and discuss these original results in a Comment of the Godet *et al.* paper, acknowledging that these authors couldn't be reasonably aware of this unpublished MSc thesis. Furthermore, the fact that Grundman (2007) hasn't seen some of the features observed by Godet *et al.* in the Urgonian Jaune doesn't prove anything. The absence of evidence is not the evidence for the absence!

Concerning the second point, this is absolutely not central to the discussion and conclusion of the Godet *et al.* paper which focuses on the Eclepens section, and for which the observation and interpretation with regards to the diagenetic history are independent of the biostratigraphic scheme used. The correlation with the basal reference section (namely the Angles section) made by Godet *et al.* aims at showing that the negative  $\delta^{13}\text{C}$  isotope shift observed in Eclepens has no equivalent in the Angles section, and is therefore related to diagenetic overprint. This conclusion doesn't change even if the alternative biostratigraphic scheme of GEA is applied.

### 3. To what extent the "alternative model" presented in the Comment represents a viable alternative stratigraphic interpretation of the study area?

The alternative model presented by GEA is not about diagenetic model, but an alternative age model, derived from a different biostratigraphic interpretation, mostly based on differential appreciation of orbitolinids taxonomy between GEA and the work of Godet *et al.* (2010, 2011) and references therein. This part of GEA's comment is therefore not related to the Godet *et al.* (2016) paper *s.s.*

The dating of the Urgonian limestone is a long-lived debate, which has so far not been settled and awaits further independent studies. GEA model however implies the absence of any major hiatus (time gap) within the Urgonian limestone, leading these authors (Clavel *et al.*, 2014; their Fig. 9) to attribute part of the proximal Urgonian sequence as Lowstand System Tract deposits, which is at odd with conventional sequence stratigraphic schemes. This is a major weakness of GEA's stratigraphic model, questioning its viability as a robust alternative scheme.

In summary, GEA comments are mostly off topic or of only minor importance, undermining any justification for their publication as a Comment in *Paleo3*."

## Introduction

Recently, while revisiting the correlations of the Shu'aiba and Bab lithostratigraphic units in Abu Dhabi (United Arab Emirates) based on peaks and trends observed in stable isotope curves (VAHRENKAMP, 1996, 2010; GRÖTSCH *et al.*, 1998), GRANIER (2012a, 2014a; GRANIER & BUSNARDO, 2013) was able to demonstrate by means of ammonite occurrences that VAHRENKAMP's (1996) seminal publication had reached erroneous conclusions regarding basin to platform correlations. Thus, it would have been gratifying to read another publication concluding that "chemostratigraphy can only be applied to shallow-marine successions with great caution" (GODET *et al.*, 2016). However, in this last case (GODET *et al.*, 2016), the application is not valid.

In their study, GODET *et al.* (2016, p. 127) use a sequence stratigraphic framework with age "controlled by orbitolinid and ammonite findings" that was developed by ARNAUD and ARNAUD-VANNEAU in the 1990s (ARNAUD *et al.*, 1998, *inter alia*). GODET *et al.* do not discuss or even mention the existence of an alternative stratigraphic framework developed by another research team (CLAVEL *et al.*, 2007, 2009, 2010, 2014; CONRAD *et al.*, 2012; CHAROLLAIS *et al.*, 2013; GRANIER *et al.*, 2013). Here we present a summary of the alternative biostratigraphic scheme suggesting that the B3 (Barremian no. 3) sequence(s) correlated from four sections by GODET *et al.* (2016: Fig. 7) might not be coeval, a point that would significantly undermine their conclusions regarding correlation and diagenesis. However, before that, we address several inconsistencies present in their publication.

## Secondary comments

First, using an irrelevant or imprecise vocabulary to describe features in sedimentary petrography can lead to confusion and ambiguities:

- the title of the paper refers to "early diagenesis" and the text to "eogenesis" but it is hard to get a precise idea of how long this "early" period lasted. From their paragenetic sequence (GODET *et al.*, 2016: Fig. 4), we understand it ends with the first phase of fracturing that is associated with a blocky calcite subzone C2. This is a smart idea as fracturing probably induced a renewal of the brine circulating within the porous network (as observed by GRANIER & STAFFELBACH, 2009, in the Paris Basin). However, the first fractures could be as early as Late Cretaceous (Eo-Alpine phase) or as late as Oligocene (Pyrenean-Provence phase), *i.e.*, at least the duration of the Aptian and Albian stages, which is at least 25 m.y. after the end of Urgonian times. This is a rather

long time-span for "early" diagenesis;

- on page 127, GODET *et al.* (2016) state that a specific "surface is (...) heavily bored by *Skolithos*-like perforations". *Skolithos* is a tubular burrow, not a boring, indicative of a soft or firm ground. A tubular boring would likely have been *Trypanites* (as mentioned elsewhere in their text), indicative of a hardground (see, for instance, the recent list of ichnogenera provided by TAYLOR & WILSON, 2003);
- on page 130, they state that "Former aragonitic bioclasts are systematically dissolved and replaced by calcite". Being metastable aragonite is commonly leached, leaving a mouldic cavity (commonly outlined by a micritic envelope) that might later be filled in part or in full by a drusy cement. These molds were cemented by calcite, but the bioclasts themselves were not "replaced by calcite". Replacement of aragonite by calcite (*i.e.*, calcitisation) commonly results in a mosaic of crystals of a similar size, with a brownish pseudopleochroism due to the preservation of the original organic matter of the bioclast. In the same paragraph GODET *et al.* (2016) point out that "centripetal subhedral growth bands indicate crystal growth in a cavity rather than progressive replacement of aragonite by calcite". In our opinion, the competitive growth of calcite crystals that results in a drusy fabric provides better primary evidence of cementation, as opposed to calcitisation;
- on page 132, GODET *et al.* (2016) write that "The micrite-rich part of 56 whole-rock samples were microdrilled and analysed for their carbon and oxygen stable isotope composition". In her recent "critical evaluation of carbon isotope stratigraphy", WENDLER (2013) described some limitations of the method. Regarding "micrite matrix", she suggested sampling mudstones or mud-supported fabrics only. However, in the current case, the log of GODET *et al.* (2016: Fig. 5) displays almost no mud-dominated or mud-supported fabrics, *i.e.*, no mudstone and wackestone textures with a significant amount of micrite. On the contrary, this log displays mostly grain-dominated and grain-supported fabrics, with a limited amount of micrite in intergranular position or no micrite at all, *i.e.*, respectively packstone and grainstone textures. Of course it is possible that their definition for packstone may cover some genuine wackestone. However, because the end results of their measurements do not differ significantly from SCHOOTBRUGGE's (2001; GODET *et al.*, 2016: Fig. 5) for the bulk rock volume of similar samples, one could legitimately question the authors' ability to pick microsamples

with micrite in such unfavorable facies. Another issue is the nature of the micrite, a word that results from the contraction of "micr"-ocrystalline and calc-"ite". In shallow-water sections the main contributors to calcareous mud are benthic "calcareous" Chlorophyta, Bryopsidales and Dasycladales, that produce aragonite needles, whereas in basinal facies the main producers are various planktonic Haptophyta (coccolithophorids, *Nannoconus* sp., etc.). It is hard to imagine that a diagenetically modified, originally aragonitic mud (the future micrite) will have a final geochemical signature similar to a coeval low-Mg calcite ooze;

- in the figure caption on page 129, GODET *et al.* (2016: Fig. 3.E) note "An intense microstylolitization (...) developed between ooids". Such features are usually referred to as intergranular pressure dissolution seams, typical of fitted fabrics with sutured grains. They represent an early stage of chemical compaction (as opposed to mechanical compaction) and they should not be called stylolites (which, by definition, are visible to the naked eye), nor even microstylolites.

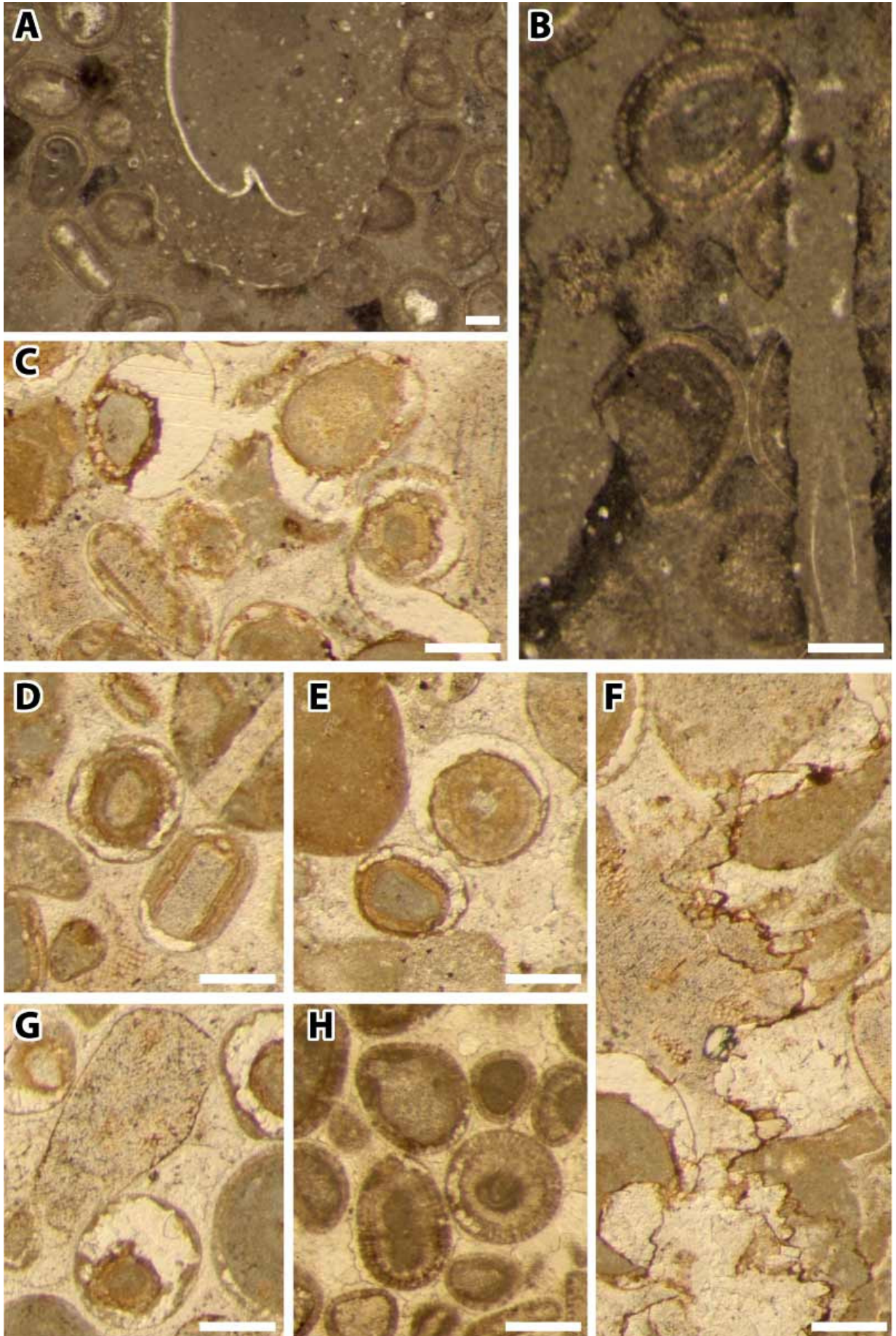
Second, we question parts of the diagenetic sequence of events:

- in the figure caption on page 129, GODET *et al.* (2016: Fig. 3.F-G) report "Dolomite rhombs with two successive subzones (D1a and D1b) developed in the intragranular [*sic*: the authors were probably thinking of intergranular] pore space, before precipitation of C1b and C2 blocky calcite subzones". However, in our opinion, the edges of the dolomite rhombs clearly cross cut the luminescent growth zones of their C2 calcite, suggesting that dolomitic replacement post-dates this calcite cement;
- on page 130, GODET *et al.* (2016) write that "Calcite dissolution first partially and locally affects oolitic cortices. Ooids are very common in this section, especially in the F6 and F5 microfacies associations". In an earlier publication GODET *et al.* (2005) stated that "F6 microfacies with oomoldic porosity were observed by BLANC-ALÉTRU, so these surfaces are interpreted as sequence boundaries". Actually BLANC-ALÉTRU (1995) reported it from three intervals only, all sited in the Urgonien Jaune. These intervals, which are two metres thick on average, are located below marker discontinuities labelled D, I and H from top to bottom, and that correspond roughly to 80 m, 66 m and 59 m on GODET *et al.*'s log (2016: Fig. 5). However, in the new interpretation by GODET *et al.* (2016), none of these surfaces coincide with a sequence boundary. Furthermore, D, the uppermost one, which marks the top of the Urgonien Jaune, is close to a maximum

flooding surface! GODET *et al.* (2016: Fig. 5) add new occurrences of intervals with dissolution of oolitic cortices, at the bottom of the Urgonien Jaune above a sequence boundary, and at the top of both the upper and the lower Pierre Jaune below sequence boundaries. FÖLLMI and GODET (2013: Fig. 7.4, bottom right corner) illustrate one ooid with a partly dissolved cortex from "Heterozoan [*sic*], oolitic carbonates" of the "Lower Pierre Jaune de Neuchâtel (early Hauterivian). Eclépens, western Switzerland". Thanks to a MSc student, Gaëlle GRUNDMAN (2007), we examined some one hundred thin-sections from the working Eclépens quarry. However, in this case, ooids with leached cortex are found only in the upper Pierre Jaune (Fig. 1). There are two options, either 1) the student failed to pick such oolitic samples in the Urgonien Jaune, or 2) because the quarry walls are regularly rejuvenated and material removed, leaching may be a local phenomenon, not related to stratiform discontinuities (including possible sequence boundaries) but to heterogeneous zones possibly determined by a fracture or karstic network (as interpreted by GRANIER *et al.*, 2014). In the upper Pierre Jaune at Montricher, the interval with partly leached ooids is almost 14 metres thick (GRANIER *et al.*, 2014). At Eclépens, this interval is roughly two metres thick. It is found near the top of the upper Pierre Jaune (Fig. 1), almost one metre below the discontinuity, a feature that pleads in favour of the second option;

- earlier records of oolitic cortices leached in full (WHERRY, 1915; SANDBERG, 1975, 1983; RICHTER, 1983; ZEN *et al.*, 1983; TUCKER, 1984; BATES & BRAND, 1990; ALGEO & WATSON, 1995) or in part, *i.e.*, only some cortical layers (TUCKER, 1984; ALGEO & WATSON, 1995) came to the conclusion that they are dealing respectively with either aragonitic or "bimineralic" (aragonite-calcite) cortices.

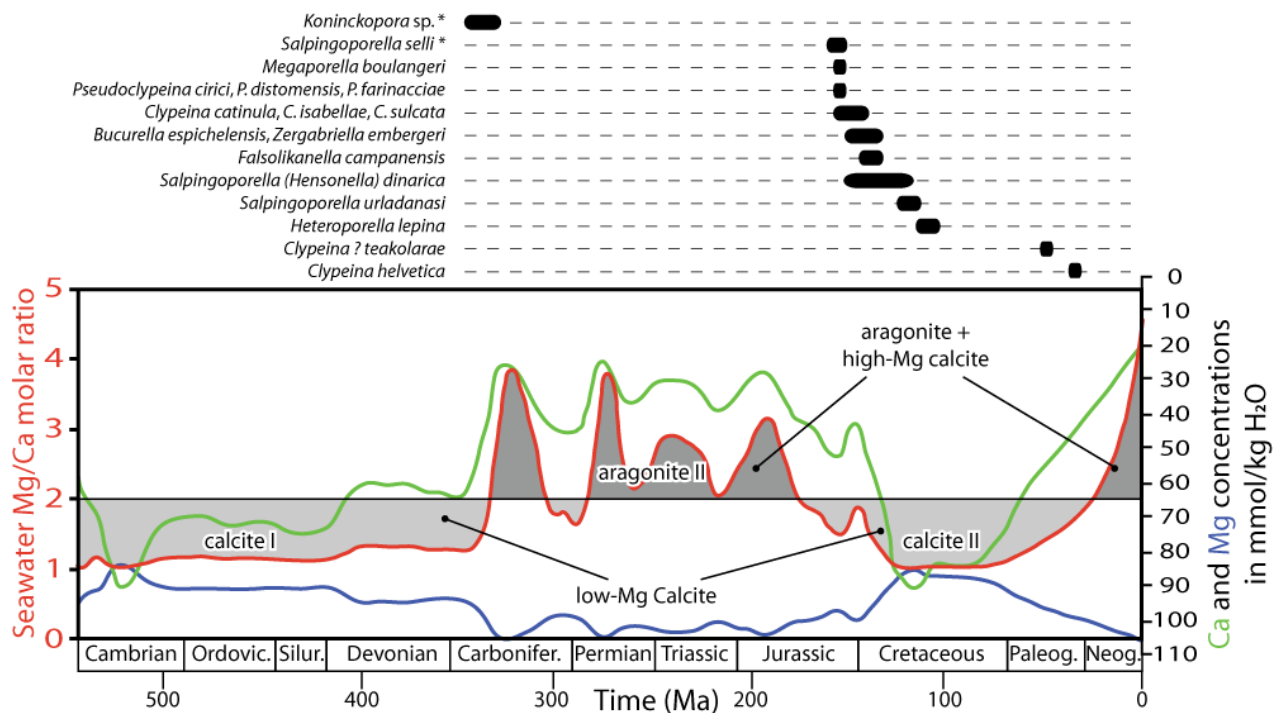
► **Figure 1:** A) *Gastrochaenolites* macroboring with the borer shell preserved; B) *Trypanites* mesoboring with the endolith foraminifer *Bullopore tuberculata* (SOLLAS, 1877). A-B) Thin-section EC55, hardground at the top of the Pierre Jaune; C) dissolved cortices and fractures cemented by white calcite whereas the intergranular porosity is cemented by a slightly yellowish calcite. Thin-section EC53; D-E & G) as above, but without fracture. Thin-section EC44; F) late tectonic (not stratiform) stylolite. Thin section EC53; A-G) Eclépens, upper Pierre Jaune, Hauterivian; H) dissolved cortices. Thin-section -127 m, Montricher, Urgonien Jaune, Upper Hauterivian. All scale bars = 250 µm.



- Ancient occurrences of aragonitic ooids may be subject to interpretation. STRASSER (1986) who was more cautious suggested that the metastable mineral could have been either aragonite or high-Mg calcite. However, he also stated that the latter "converts faster to low-Mg calcite than aragonite when exposed to freshwater diagenesis, without textural changes of the affected crystals", as it is the case with high-Mg calcite echinoderm remains accompanying our ooids. The cortices of some ooids from Eclépens and from the neighboring locality of Montricher were partly leached. However they correspond to a new category (GRANIER *et al.*, 2014), first because some cortical layers are not fully leached and second because the remaining parts display a typical radial fabric that corresponds to a primary calcite (Fig. 1), not to a primary aragonite. Leaching of the calcite from oolitic cortices is a challenging question that was not addressed by GODET *et al.* (2016). There are very few records in the literature that advocate either leaching by acidic hydrothermal fluids, but not eogenetic dissolution by meteoric waters, or the dual, organic and mineral nature of the ooids (GRANIER, 2014b; GRANIER *et al.*, 2014);
- on the same page (130), same paragraph, they state that "When fully or partially preserved, their cortex shows structures indicative of an initial precipitation of calcite, as it is expected in the context of a calcite sea period (WILKINSON *et al.*, 1985; MORSE *et al.*, 1997)". In writing this, GODET *et al.* ignore the starting point of this concept and its authors, namely SANDBERG (1975) and HARDIE (1996). While investigating marine ooids in order to identify their primary mineralogy SANDBERG (1975) was the first to recognise an oscillating trend of periods dominated by aragonitic ooids alternating with periods dominated by calcitic ooids (SANDBERG, 1983; WILKINSON *et al.*, 1985). HARDIE (1996) used a "simple quantitative model for calculation of the chemistry of ancient seawater based on the MOR" (mid-ocean ridge) / "RW" (river water) "flux ratio". Best predictions were obtained with a MOR/RW equal to 1.25 and a molar ratio  $Mg^{++}/Ca^{++}$  equal to 2 (Fig. 2). To test this model, he plotted the resulting curves for  $Ca^{++}$ ,  $Mg^{++}$ , and  $Mg^{++}/Ca^{++}$  ratio against the stratigraphic ranges of some marine non-skeletal precipitates, more specifically "aragonite cements" and "aragonite oolites". However, the stratigraphic inventory of both aragonitic ooids and aragonite cements is incomplete.

### Testing HARDIE's (1996) model

Ooids, which are by definition non-skeletal grains, are not entirely of "abiotic origin". There is evidence that microbial processes are actively involved at various stages in their mineralization (BREHM *et al.*, 2004; EDGCOMB *et al.*, 2013; DIAZ *et al.*, 2013, 2014, 2015; O'REILLY *et al.*, 2016). For instance, some microbes may promote mineralization of ooids that takes place in extracellular polymeric substances (EPS) whereas others may alter the original mineralization of ooid cortices (DUGUID *et al.*, 2010). A parallel may be drawn with the mineralization of the "calcareous" green algae that also takes place within an extracellular mucilage. Accordingly Bryopsidales and Dasycladales, which are giant unicells with a relatively fair stratigraphic record, have been used as an alternative to the marine ooids (and the marine cements) to test the validity of HARDIE'S (1996) model. Thus, looking for some supportive evidence, STANLEY and HARDIE (1998) drew attention to the "aragonitic dasycladaceans" which are "dominant rock formers" in the Aragonite II interval that spans the latest Carboniferous through to the Early Jurassic. However, that view does not match the data of palaeophycologists. For example, BARATTOLO (1991: Figs. 1 & 4) documented the scarcity of the Dasycladales at the transition from the Triassic to the Jurassic (end of Aragonite II) and noted their abundance during the Paleogene (end of Calcite II), in clear contradiction with STANLEY and HARDIE'S (1998) conclusion. Similarly WILKINSON (1979) provided a short summary of his views on "calcareous" green algae stating that "Modern marine chlorophytes are exclusively aragonite" and that "Paleozoic occurrences are sufficiently rare and petrologically non-descript that their original mineralogy cannot be determined with certainty". STANLEY and HARDIE (1998) noted that WILKINSON (1979) "failed to include data for (...) pre-Jurassic dasycladacean algae" and even that "some" of his data "were in error". As a matter of fact, although Bryopsidales never mineralized as calcite, some Dasycladalean species do, but there are very few calcitic species in comparison to the coeval aragonitic species (GRANIER, 2012b). Plotting their stratigraphic ranges against the predicted molar Mg/Ca ratio of seawater for the Phanerozoic (Fig. 2) shows that they occur both at the beginning and the end of Calcite II, but that they are surprisingly missing at the height of this interval, which was not "expected in the context of a calcite sea period". Revisiting the publication of STANLEY and HARDIE (1998), AGUIRRE and RIDING (2005) also reached a conclusion similar to ours, noting that there is no "straightforward relationship between dasycladalean diversity and calcite- and aragonite- sea



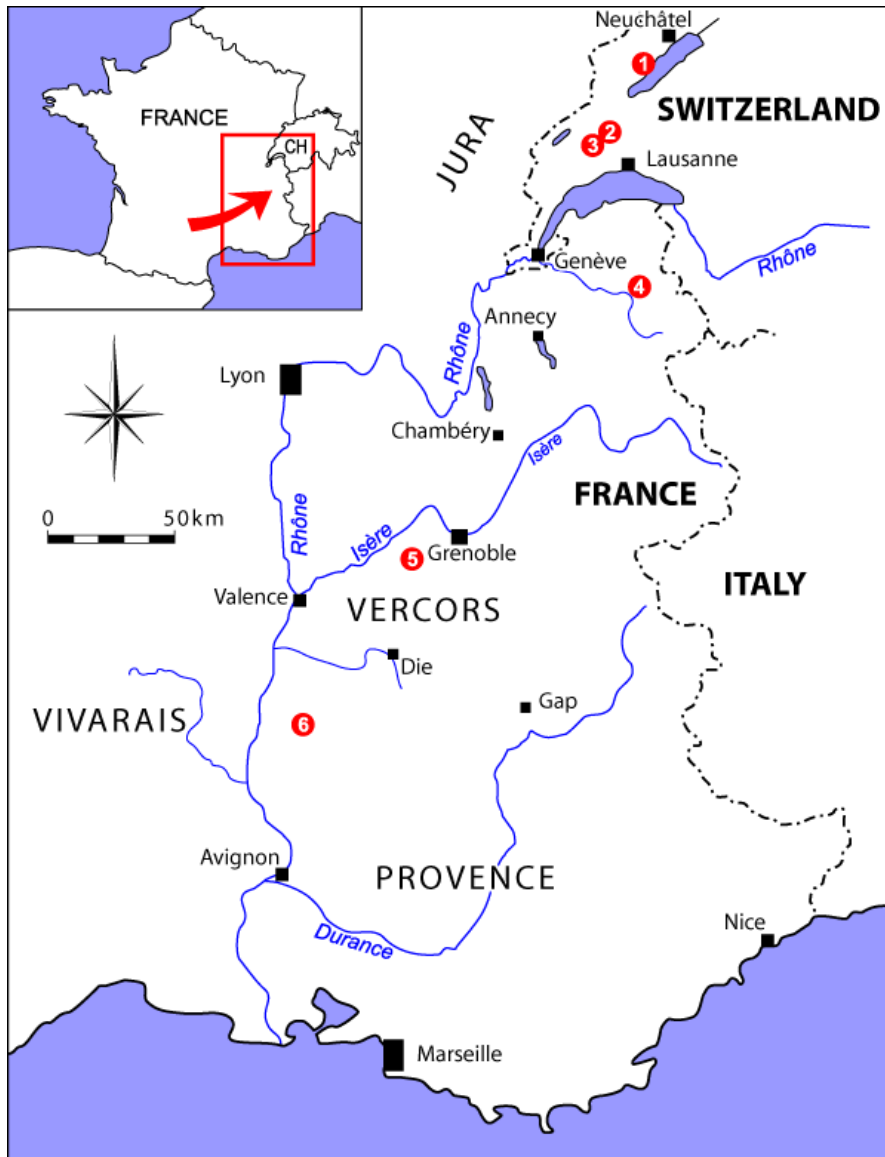
**Figure 2:** Stratigraphic distribution of the supposedly calcitic Dasycladales (\* starred) and of the originally calcitic Dasycladales (modified from GRANIER, 2012b) plotted against the predicted molar Mg/Ca ratio of seawater for the Phanerozoic (adapted from HARDIE, 1996). The horizontal line is the approximate divide between low-Mg calcite seas ( $Mg^{++}/Ca^{++} < 2$ ) and aragonite seas ( $Mg^{++}/Ca^{++} > 2$ ).

episodes". Does this invalidate the HARDIE (1996) model? The answer must be in the negative because, as documented above, biological processes involved in  $CaCO_3$  mineralization may mask or prevail over physicochemical (purely abiotic) processes.

### Alternative stratigraphic model and its implication

As mentioned at the beginning of this comment paper, this last section will present an alternative stratigraphic model that has implications for the relative ages and geometric arrangement of the various stratal units. On page 127, GODET *et al.* (2016) state that "Their age is controlled by orbitolinid and ammonite findings" with orbitolinid ranges from ARNAUD *et al.* (1998). The alternative biostratigraphic model (CLAVEL *et al.*, 2002, 2007, 2009, 2010, 2014, *inter alia*) is based on the same fossils but the interpreted ranges of the orbitolinids differ materially. For instance, let us consider *Palorbitolina lenticularis* (BLUMENBACH, 1805), which is an emblematic, cosmopolitan orbitolinid for the Barremian - Aptian interval (CLAVEL *et al.*, 2002). According to ARNAUD *et al.* (1998) it first appears in the Sartousiana Zone of the Late Barremian but according to CLAVEL *et al.* (2014, *inter alia*) this species is already known from the Pulchella or even the Nicklesi Zone of the Early Barremian.

GODET *et al.* (2016: Fig. 7) provide a correlation panel for their SB B3 (Figs. 3 - 4), which is "close to" the Lower-Upper Barremian boundary, as a reference horizon for four sections ranging from the basin, on the left, to the platform, on the right (Fig. 3). In the first column, *i.e.*, a composite section for the Vocontian basin, *Palorbitolina lenticularis* would theoretically first occur some 20 metres above their SB B3 according to ARNAUD's scale, but 25 metres below it according to CLAVEL's scale. Actually, in a reference section of the Vocontian basin, namely L'Estellon (Drôme), the first *P. lenticularis* occurs some 70 metres below the Lower-Upper Barremian boundary in turbidites that are directly dated by ammonites of the Pulchella Zone of the Early Barremian (GRANIER *et al.*, 2013). In the second and third columns, *i.e.*, in the Gorges du Nant section and in the Cluses section, this foraminifer first occurs some 190 and some 175 metres (CLAVEL *et al.*, 2014), respectively, above the reference horizon. Finally, in the last column, *i.e.*, at Eclépens, it occurs in karstic cavity fills of the Urgonien Blanc (GODET, 2006; CLAVEL *et al.*, 2014). Actually, in the Swiss Jura, this species is also found in a similar karstic cavity setting from La Lance (CHAROLLAIS *et al.*, 1994; BLANC-ALÉTRU, 1995) but it is not reported from the borehole at Montricher (GRANIER *et al.*, 2014; JAN du CHÊNE *et al.*, 2016).



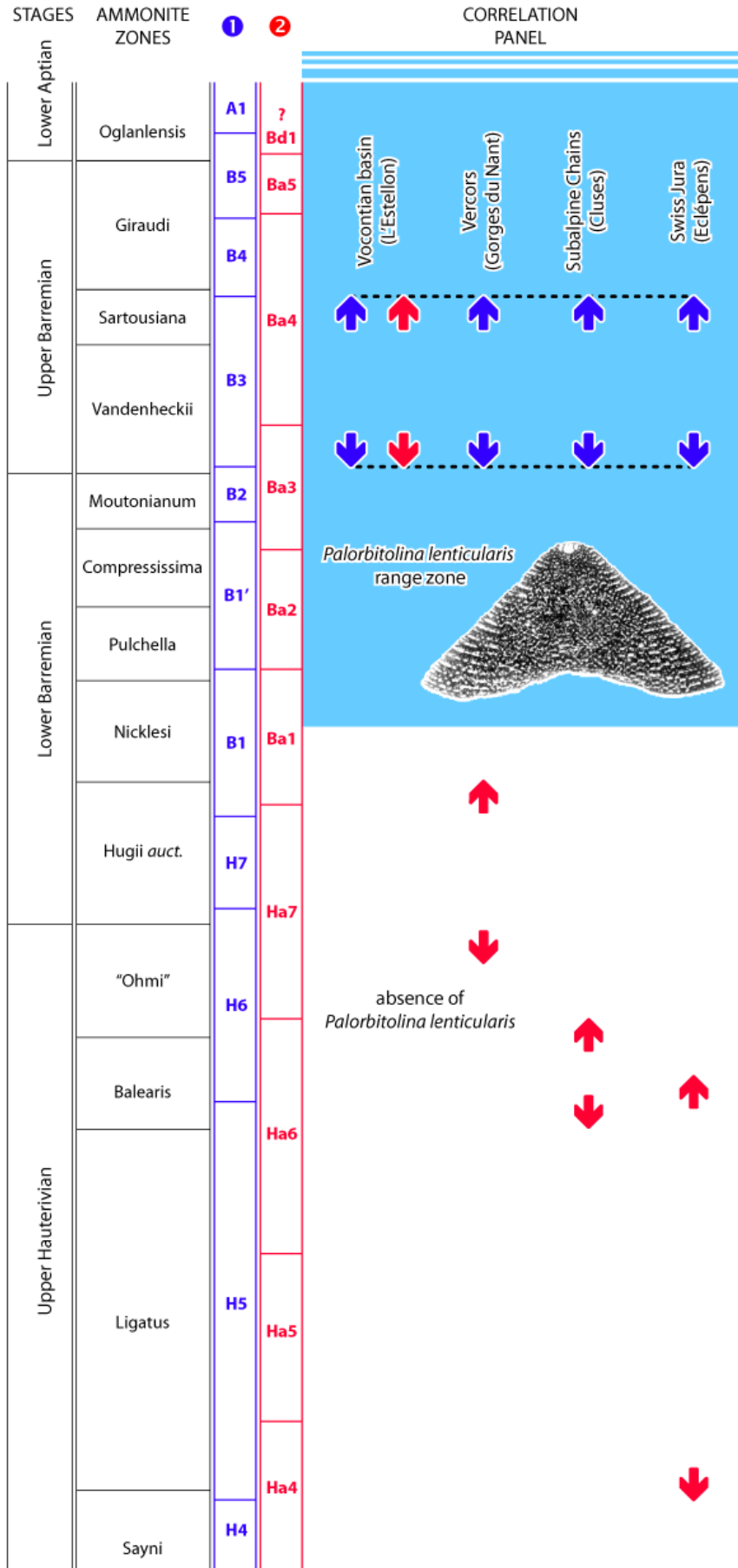
◀ **Figure 3:** Location map. Swiss Jura: 1) La Lance, 2) Eclépens, 3) Montricher; Subalpine Chains: 4) Cluses; Vercors: 5) Gorges du Nant; Vocontian Basin: 6) L'Estellon.

According to *GODET et al. (2016)*, their whole sequence B3, equivalent to the upper Urgonien Jaune and the Urgonien Blanc at Eclépens, is Late Barremian in age (Fig. 4, in blue colour). However, they never questioned the critical lack of the cosmopolitan *Palorbitolina lenticularis* in most of their supposed Upper Barremian shallow-water facies (although it is found reworked in coeval or even older deep-water facies, e.g., at L'Estellon in the Vocontian basin). On the contrary, according to *CLAVEL et al. (2014)*, this interval B3 is Late Hauterivian in age (Fig. 4, in red colour), older than the first record of *Palorbitolina lenticularis*, a simple and logical explanation for its absence. In the Table 1 below, we summarize the stratigraphic interpretations of the latter authors.

**Table 1:** Sequences and ages of the *GODET et al.*'s B3 sequence (supposedly Late Barremian in age) according to the alternative stratigraphic framework put forward herein (*CLAVEL et al., 2014*). Ha for Hauterivian, Ba for Barremian, LST for Lowstand Systems Tract, TST for Transgressive Systems Tract, HST for Highstand Systems Tract. See locations on Fig. 3 and ranges on Fig. 4.

Sections	Sequences equivalent to <i>GODET et al.</i> 's B3 (2016)	Corresponding ammonite zones	Ages
<b>Vocontian basin (L'Estellon)</b>	<b>Ba3</b> HST, and <b>Ba4</b> LST and TST pars	Vandenheckii - Giraudi	Late Barremian
<b>Vercors (Gorges du Nant)</b>	<b>Ha7</b> TST and HST, and <b>Ba1</b> LST pars	"Ohmi" pars - Hugii auct.	transition of the Hauterivian to the Barremian
<b>Subalpine Chains (Cluses)</b>	<b>Ha6</b> LST pars, TST and HST	Balearis - "Ohmi" pars	Late Hauterivian
<b>Swiss Jura (Eclépens) = Urgonien Jaune + Urgonien Blanc</b>	<b>Ha4</b> TST and HST, <b>Ha5</b> LST, TST and HST, and <b>Ha6</b> LST	Sayni pars - Balearis pars	Late Hauterivian





◀ **Figure 4:** Column 1) in blue colour, sequences according to ARNAUD *et al.* (1998) and correlation (arrows) according to GODET *et al.* (2016); column 2) in red colour, sequences and holostratigraphic (bio-, litho-, and sequence stratigraphy) correlations (arrows) according to CLAVEL *et al.* (2014, *inter alia*). The light blue background corresponds to the lower part of the Range Zone of *Palorbitolina lenticularis* (BLUMENBACH, 1805). See locations on Fig. 3.

GODET *et al.* (2016, p. 135) use not only orbitolinids and ammonites but nannofossils. We shall refer here to a "blind test" undertaken under the supervision of STRASSER (see GODET, 2006, p. 373-386). Three specialists: E. ERBA, Università degli Studi di Milano, S. GARDIN, Université Pierre-et-Marie-Curie, and E. de KAENEL, Consulting Geologist, examined the same set of samples from the lower part of the Urgonien Jaune, immediately below the SB B3, and their results are shown in Table 2 below.

The dating confidence is the narrower range common to the seven discrete samples: Hauterivian for ERBA, Late Hauterivian for GARDIN, possibly "Late Barremian" for KAENEL with two odd Late Albian dates (the studied interval cannot be younger than the Late Aptian!). Although both academic experts pointed to an Hauterivian age, GODET (2006) and GODET *et al.* (2016) still refer to this interval as (?) Upper Hauterivian-Lower Barremian, and the overlying interval as the Upper Barremian.

Over more than two decades our team has released a large amount of information regarding the dating of the Urgonian and coeval facies. For instance, we reported the discovery of the L'Estellon section, a "Rosetta stone" for Urgonian biostratigraphy because this section allows calibration of the first appearance of several orbitolinids directly on Barremian ammonite zones. However, against all odds (*e.g.*, the above discussion on *Palorbitolina lenticula*

*ris*; CONRAD *et al.*, 2012; CHAROLLAIS *et al.*, 2013; CLAVEL *et al.*, 2014; JAN du CHÊNE *et al.*, 2016), some authors, *e.g.*, GODET *et al.* (2016), keep on referring to the obsolete bio- and litho-stratigraphic framework of ARNAUD *et al.* (1998).

In conclusion, although we agree with GODET *et al.* (2016) that diagenesis "hampers long-distance chemostratigraphic correlation", we suggest that they have failed to illustrate this with their case study because, in our opinion, they have compared non-coeval intervals; this error undermines the credibility of their research.

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**Table 2:** Ages of a set of seven samples based on nannofossils according to E. ERBA, S. GARDIN and E. de KAENEL (GODET, 2006). There are no "Urgonian" platforms in the area above the red horizon.

Specialist		ERBA							GARDIN							KAENEL						
Sample no.		2	3	4	5	6	7	8	2	3	4	5	6	7	8	2	3	4	5	6	7	8
Albian	late																					
	middle																					
	early																					
Aptian	late																					
	early																					
Barremian	late																					
	early																					
Hauterivian	late																					
	early																					
Valanginian	late																					
	early																					
Berriasian																						

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