Discussion

Reply to Shanmugam, G., comment on "Internal waves, an underexplored source of turbulence events in the sedimentary record" by Pomar et al. [Earth-Science Reviews, 111 (2012), 56–81], Earth Science Reviews (2012)

L. Pomar a,⁎, M. Morsilli b, P. Hallock c, B. Bádenas d, D. Bourgault e

a Département de Ciencies de la Terra, Universitat de les Illes Balears, Ctra. Valldemossa km 7.5, E-07122 Palma de Mallorca, Spain
b Dipartimento di Fisica e Scienze della Terra, Università di Ferrara, Via G. Saragat 1, 44100 Ferrara, Italy
c College of Marine Science, University of South Florida, 140 Seventh Ave. S., St. Petersburg, FL 33701-5016, USA
d Departamento de Ciencias de la Tierra, Universidad de Zaragoza, 50.009 Zaragoza, Spain
e Institut des Sciences de la Mer de Rimouski, Université du Québec à Rimouski, 310 Allée des Ursulines, Rimouski, Québec, Canada, G5L 3A1

A R T I C L E   I N F O

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1. Introduction

Shanmugam (2012) just published a criticism about the study of Pomar et al. (2012) on the potential importance of internal waves in shaping the sedimentary record in lakes and oceans. We would like to acknowledge the effort done by G. Shanmugam for initiating such a discussion. However, while scientific debates and controversies are necessary for the advancement of science in general, and for sedimentology in particular, they should be carried out with coherence and rigor to be constructive. Unfortunately, Shanmugam’s comment appears to us not to follow this logic such that it is difficult for us to reply in a sensible manner.

It is challenging for us to formulate an instructive reply of general interest partly because Shanmugam’s criticism is based on misunderstanding of basic physical processes associated with internal waves. For example, it is difficult to discuss the impact of internal waves on sediments with someone who thinks that “baroclinic tidal currents tend to propagate along pycnoclines” or who questions the work of Shepard et al. (1979) on observations of internal tides because “velocity measurements were not made along density stratifications where internal tides propagate” but near the bottom of a canyon.

Dr. Shanmugam seems to think that currents induced by internal waves are confined within a narrow zone around the pycnocline (see for example his Figure 2 and arguments used in the text). In fact, the horizontal currents induced by an internal solitary wave is nil at the pycnocline (Fig. 1). Internal wave-induced currents can be felt within a good portion of the water column, if not over the entire water column for long internal waves (Fig. 1). The presence of internal waves could therefore fairly easily be determined from near bottom velocity measurements taken far below the pycnocline, as done routinely in physical oceanography.

It is also difficult to discuss with someone who is convinced that “we know nothing about the fluid mechanics of baroclinic currents and their ability to form traction and suspension structures in sediments”. Dr. Shanmugam is obviously not aware of the fundamental work of Bogucki and Redekopp (1999), Wang et al. (2001), Stastna and Lamb (2002), Diamesis and Redekopp (2006), Venayagamoorthy and Fringer (2007), Stastna and Lamb (2008), and Boegman and Ivey (2009), to cite a few.

Finally, it is difficult to discuss with someone who shows only approximate understanding of internal waves and seems mixed up with the terminology (internal waves, internal tides, internal solitary waves). For example, Dr. Shanmugam does not seem to understand that internal tides, or low-frequency internal waves, do not propagate purely horizontally in the continuously stratified deep ocean (see for example Fig. 1 in Cacchione et al., 2002). His Figure 4, which is intended to schematically represent internal tide generation, propagation and interaction with the shelf, is not meaningful. Firstly, the red and blue vectors presented on that figure have not much to do with baroclinic and barotropic currents (the same could be said for Figure 2). Secondly, internal tides generated at ridges or guyots in the deep ocean do not propagate horizontally as schematized, but rather at an angle relative to the horizontal (again see Cacchione et al., 2002). This figure and Shanmugam’s proposed “framework” completely lack the physics associated with critical slopes for the reflection of internal tides, which play an important role for sediment resuspension (e.g. Cacchione et al., 2002).

There are many similar misunderstandings regarding the physics of internal waves throughout Shanmugam’s comment. It would be simply too long to correct all of them. Below we reply to some of the most
near bottom velocity measurements can be adequate to measure the passage of inter-
prolific Sproul-Newman theory (Stastna and Lamb, 2002). Right: the horizontal current
principle of Uniformitarianism giving birth to the modern geological
operating hypotheses, but an excess of temperance may also be detri-
environments with internal waves and internal tides in modern marine
iments deposited from turbiditic
validate other approaches. First interpretations of graded beds as sed-
ments progressing by using and combining all possible observations
sediment dynamics. In this context, Sedimentology has been success-
the rock successions with modern phenomena and their inherent
ary successions has been accomplished from comparative analysis of
Shanmugam's comments.

2. Some points of reply

In summarizing his criticism, G. Shanmugam asserts in the ab-
stract that “the temptation to interpret ancient rock record as
internal-wave deposits must be tempered” as “there are absolutely
no core-based studies by sedimentologists on the origin of primary
sedimentary structures formed by baroclinic currents associated
with internal waves and internal tides in modern marine
environments”.

We agree, of course, that temperance is a great attitude in devel-
oping hypotheses, but an excess of temperance may also be detri-
mental. Since the late 1700s, when James Hutton established the
principle of Uniformitarianism giving birth to the modern geological
practice, progressive refinement in the interpretation of the sedimentary
successions has been accomplished from comparative analysis of
the rock successions with modern phenomena and their inherent
sediment dynamics. In this context, Sedimentology has been success-
fully progressing by using and combining all possible observations
from outcrops, from laboratory experiments, from observations
made in modern sedimentary environments and also, of course, on
core-based studies. But the absence of core-based studies does not
invalidate other approaches. First interpretations of graded beds as sed-
iments deposited from turbiditic flows (e.g. Kuenen and Migliorini,
1950; Bouma, 1962; and many others) were not based on the direct
observation of the “turbidity currents” or core-based studies. And,
as in other branches of the geological sciences, uncertainty is consub-
stantial with sedimentological interpretations. Pretending that no
interpretations can be proposed without core-based studies on the
origin of primary sedimentary structures is fallacious.

In his point 2 “Comparison of internal waves with turbidity currents
and storms” Shanmugam also includes several additional fallacies. Com-
parison (Table 2 of Pomar et al., 2012) between the 1) bathymetric ranges, 2) turbulence induction processes, 3) triggering mechanisms, 4) direction of propagation and 5) name of the resulting deposits, does not require, as G. Shanmugam contends, to “have a clear understanding
of fluid mechanics of these processes and their respective depositional
mechanisms”. Although Shanmugam's comment that (sic) “turbidity cur-
rent is a sediment-gravity flow with Newtonian rheology and turbulent
state in which sediment is supported by fluid turbulence and from
which deposition occurs through suspension settling” is correct, it has
nothing to do with the comparison made in Pomar et al. (2012) in their
Table 2.

Another fallacious reasoning is introduced about storms. Among
eventites (the sediments reflecting the effects of turbulence events;
Seilacher, 1982, 1991), tempestites and turbidites are the most fre-
dently recognized. And, although “hydrodynamic interpretation of
modern shelf storm deposits is difficult”, tempestites, the sediments
deposited during simple storms, have been widely recognized in the
geological record since the Ager’s (1974) seminal paper interpreting
storm deposits from outcrop studies, in the Jurassic of the Moroccan
High Atlas. G. Shanmugam, however, and without modesty, states the
fallacy: “in comparing storms with internal waves, Pomar et al. (2012, their Table 1) have used the term ‘storm’ casually without ref-
ence to velocity. Because the term storm represents a meteorologi-
cal phenomenon, it is crucial that sedimentologists closely adhere to
the meteorological definition of storms. According to the Atlantic
Oceanographic and Meteorological Laboratory, a tropical storm is a
type of tropical cyclone (broader category) with a maximum sustained
wind velocity of 62–119 km/h. Unlike tropical storms, in-
ternal waves are not defined on the basis of velocity”.

G. Shanmugam restrictively uses the American Meteorological
Society definition of a pycnocline (see basic concepts and nomenclature)
as “the interface between the mixed and the deep ocean layers where
the density gradient is the greatest (i.e., primary density stratification)
and, accordingly states that the ocean’s uppermost 100 m is commonly
well mixed by wind-driven surface currents, whereas the deep ocean is
vertically stratified”. This restricted usage of the concept of the
pycnocline is at the basis of many of the Shanmugam inconsistencies.
It drives G. Shanmugam to ignore that a pycnocline (from the Greek
pyknós “thick, dense” and clíene “to posses or exhibit gradient”) can
exist at any depth and in any fluid (e.g.: atmosphere, oceans, lakes,
etc.). In oceans and lakes, this depth variablity is fundamental for un-
derstanding the possible effects on the sediments induced by the turbu-
rence created by breaking internal waves. And, based on this misleading
usage, G. Shanmugam does not consider the possibility that internal
waves can break and cause turbulence on the shelf, despite the fact
that the phenomenon has been widely documented (see Pomar et al.,
2012, and references therein). Additionally, in Fig. 3 B, Pomar et al.
(2012) reproduce a figure from Bourgault et al. (2008) of echograms
showing internal waves propagating along a shallow pycnocline
(~15 m) and breaking at the depth where the pycnocline intersects
the seafloor in the St. Lawrence Estuary, Quebec.

G. Shanmugam also includes several fallacies in his comments about
the genetic nomenclature. Pomar et al. (2012) proposed the term
“internalite” for deposits originated by internal waves for simplifica-
purposes. G. Shanmugam rejects this term because it (sic) “implies that
internal waves are depositionnal processes”. Internal waves are not
depositionnal processes, as surface waves are not either; they can be sedi-
ment laden when propagating in the ocean or in lakes. But both
surface- and internal waves cause turbulence in the breaker zone and
can induce entrainment and deposition of sediments, “Internalite” is
the product of turbulence events induced by breaking IWs (see table 1
in Pomar et al., 2012).

Following G. Shanmugam reasoning, tidalite should also be rejected
because tides are not a depositional process, and similarly, the term
tempestite has to be rejected because surface storm waves creating
the turbulence on the sea floor are not depositional processes. According
to Bates and Jackson (1987) [Glossary of Geography: Alexandria, American
Geological Institute, 788 p.], a “tempestite is a storm deposit, showing ev-
eidence of violent disturbance of pre-existing sediments followed by their
rapid deposition in shallow-water environment (Ager, 1974, p. 86). For

Fig. 1. Left: Example of the horizontal current (color scale) induced by a
horizontally-propagating large-amplitude internal solitary waves in a two-layer sys-
tem (contour lines are isopycnals). We computed this field using the fully nonlinear
Dubreil-Jacotin-Long theory (Stastna and Lamb, 2002). Right: the horizontal current
profile through the wave trough. The figure shows that the baroclinic currents induced
by this type of waves do not propagate along the pycnocline. The figure also shows that
near bottom velocity measurements can be adequate to measure the passage of inter-

nal waves, as done for example by Shepard et al. (1979) for internal tides.
the relation to turbidites and inundates, see Einsele and Seilacher (1982, p. 334). It is obvious that if we follow G. Shanmugam reasoning all other terms, which are commonly used by the scientific community, such as tsunamiite, eolianite, tidalite, pelagite, hemipelagite, contourite, etc., must also be rejected. We agree with G. Shanmugam, however, that a more consistent term for internal-wave deposits might have been “internal wavite” but, as he states, is awkward sounding, and would not serve as a simplifying term with respect to internal-wave deposits.

As stated in our conclusions, internalites, like tempestites and turbidites, are the product of turbulence events and, as such, they share some common features: a first erosional phase – reflected in an erosion surface – and a subsequent depositional phase, during the waning of the episodic turbulence. Differences between tempestites, turbidites and internalites, however, derive from the different natures of the turbulence processes and the resulting association of sedimentary structures. And also stated in our conclusions, internalites may have many different expressions, depending on the depth of the pycnocline, the amplitude and frequency of the internal waves and the sediments available at the breaker zone, and new criteria for their recognition are still to be developed.

In his point 4 “Empirical data from modern seamounts and guyots”, G. Shanmugam includes the existence of bedforms that were not included in Pomar et al. (2012) review. The focus of our review was on the impact of internal waves breaking on sloping surfaces, particularly in relatively shallow-water environments. For this reason, we did not include either the analysis of other bedforms (e.g., subaqueous dunes) that have been attributed to internal waves (e.g., Stride and Tucker, 1966; Carruthers, 1963; Butterfield et al., 1979; Heathershaw and Cod, 1985; Karl et al., 1986; Heathershaw, 1985, 1987, 1988; Heathershaw et al., 1987), or the interaction between internal waves and turbidites (e.g., Knepler et al., 1991; Mutti et al., 2003; Tintner et al., 2003; Remacha et al., 2005; Edwards et al., 2006; Meiburg and Knepler, 2010; Tintner and Muzzi Magalhaes, 2011) or between internal waves and nepheloid layers (e.g., Cacchione and Drake, 1986; Dickson and McCave, 1986; McCave, 1986; Thorpe and White, 1988; Bogucki et al., 1997, 2005; Cacchione et al., 2002; Puig et al., 2004, 2007; Quaresma et al., 2007; Inall, 2009; Hernández-Molina et al., 2011). Surprisingly, when emphasizing the incompleteness of the Pomar et al. (2012) analysis by ignoring the modern seamounts and guyots, G. Shanmugam ignores the abundant literature existing in these other cases.

Continuing his biased analysis and using the information taken from the guyots as the source of empirical data on modern bedforms associated with internal waves and tides, G. Shanmugam proposes a preliminary depositional framework. In his model, and despite rejecting one of the models proposed by Pomar et al. (2012) which is based on the experiments carried out by Southard and Cacchione (1972) and on observations on modern shelves (Butman et al., 2006), G. Shanmugam proposes a similar succession of processes: 1) erosion and sediment entrainment when internal waves and tides encounter the seafloor, 2) downslope sediment transport by return flows and, 3) sediment deposition. The difference is that Shanmugam’s model only envisages the erosion to occur near the shelf edge and deposition either on the continental slope or in submarine canyons. But the inconsistency of his argument against the Pomar et al. (2012) model reaches the maximum expression when he states that similar transformation occurs when surface waves of all kinds (i.e., sea surface waves, storm waves, or tsunami waves) approach the shoreline. This is what the Pomar et al. (2012) model includes (similarity of some processes between surface- and internal waves) and that Shanmugam criticizes and rejects. The reason is because Shanmugam rejects the possibility that internal waves can break at depths shallower than the continental platform edge despite the fact that they have been widely documented to occur at many different depths, shallow-water settings included. And, at the acme of inconsistency, Shanmugam rejects all Pomar et al. (2012) interpretations due to the absence of core-based analysis, but he bases his model on examples from guyots without core-based data, and extrapolates this model to continental slopes and canyons.

Nevertheless, we fully agree with G. Shanmugam that (sic) some return flows could behave as mass-transport processes (i.e., slides, slumps, and debris flows), turbidity currents, or bottom currents at the time of deposition. Paradoxically, this is evidenced in the analysis of several outcrop examples provided by Pomar et al. (2012) that G. Shanmugam ignores in his comments. The Eocene Nummulitic banks in the Pyrenees (Mateu-Vicens et al., 2012) are interpreted as the product of mass-transport processes at the time of deposition, but triggered by breaking internal waves. Similarly, the rudstone and packstone wedges abutting the Eocene coral buildups (Morsilli et al., 2012) are unsorted and also reflect mass-transport processes. The flank facies around Jurassic coral buildups (Alnaghah et al., 2010, 2011) are also interpreted to derive from mass-flows to turbiditic flows. And, in the climax of its contradiction, after asserting that although the return flows (after breaking) are equated with baroclinic currents, Shanmugam rejects the use of backwash flow in the provided example of tractive return flow deposits in the Jurassic of Ríca (Bádenas et al., 2012). For G. Shanmugam, only continental slopes and submarine canyons are considered to be environments with high potential for deposition from return flows.

Under “Unresolved sedimentological issues; 6.1. Uniformitarianism” Shanmugam reaches the acme of distortion of Pomar et al. (2012) interpretations when stating: “in short, the principle of uniformitarianism has never been tested in this domain” (internal waves), and in “gently sloping ramp settings, without sudden bathymetric (topographic) variations, are unlikely environments for generating internal waves. This dilemma raises the following questions: 1) Are there modern carbonate ramp settings where internal waves are generated locally near the middle-ramp position? 2) If so, what is the cause of generation of internal waves on a gently sloping ramp setting?”. Here Shanmugam creates a great distortion because Pomar et al. (2012) have never stated that the internal waves were created in a ramp setting. The sedimentary processes interpreted in the Upper Jurassic succession in the Ríca section (Pomar et al., 2012; see also Bádenas et al., 2012) are related to breaking internal waves in a ramp setting dominated by two grain populations: mud and sand. So, the two questions raised by Shanmugam do not make sense. And even worse, with this non-scientific attitude, he seriously distorts the interpretations made by other authors and attempts to impede the advance of science.

Following Shanmugam’s strict use of Uniformitarianism, most the advances made in Geology over the last century to the present, and more specifically in Sedimentology, would be “unresolved sedimentological issues” because we cannot confirm the models from modern settings; for example, the “calcite” and “aragonite” seas at the scale of the Phanerozoic (e.g.: Sandberg, 1983; Stanley and Hardie, 1998, 1999), the appearance of anachronistic facies following mass extinctions and the oceanic anoxic events (i.e. “time specific facies”: Brett et al., 2012). Shanmugam also seems to ignore that from the mid-nineteenth century to the present the empirical approach to geology has included the building of models (i.e inductive science) and the use of models to guide further research (i.e., deductive approach: Mill, 2004). This “simplistic view” is running through most of Shanmugam’s comments, but especially in Section 6.1. (Internal waves in ramp settings; Upper Jurassic strata, Ríca section).

Under “6.3. Variable wave-propagation directions” G. Shanmugam raises another erratic fallacy when, after asserting “satellite images of modern internal waves reveal that the directions of propagation of internal waves are highly variable with respect to the shoreline and the shelf edge”, states “but there are no systematic documentation of wave-propagation directions seen as the sea-surface manifestations and their respective influence on internal sedimentary structures (i.e., dip directions) in the depositional bedforms on the modern seafloor”. Like surface waves, internal waves can propagate in all directions, but they only interact with the seafloor (the shoreline for surface waves) when approaching a sloping surface. Thorpe and Lemmin (1999) demonstrated that the internal-wave surf zone has some, although possibly not all, the characteristics of the conventional “surface-wave” surf zone, with waves
steepening as they approach the slope. And, like surface waves approaching the shoreline, refraction on the shelf strongly orients the packet crests along isobaths and retards their speed of advance, while development of a benthic boundary layer leads to re-suspension of bottom sediments and nutrients, often fertilizing the water column (Apel, 2002).

Also under “6.5. Outcrop-based vertical facies models” Shannmugam misunderstands and distorts what it is written in Pomar et al. (2012). Our paper has two parts. The first part summarizes the key ideas about what it is known about internal waves and, consequently, includes the models proposed by Chinese sedimentologists (e.g.: Gao et al., 1998; He and Gao, 1999; He et al., 2008) (our Figure 7; Shannmugam’s Fig. 8) and some other papers, Shannmugam included, for internal waves/tides in deep-water environment. In the second part, Pomar et al. (2012) analyze outcrop examples in which the impact of breaking internal waves becomes the most plausible interpretation. However, Shannmugam rejects the re-interpretation of the Upper Jurassic event of the Rcia section because “there is no conceivable reason for the original upward-coarsening and upward-thickening trends, apparently existed at the time of previous ‘storm’ interpretation (Bădenas and Aurell, 2001), to vanish at the time of ‘internal-wave’ reinterpretation (Pomar et al., 2012). After all, the inherent depositional attributes of the rocks do not change. Considering that the original models of internal-wave and internal-tide deposits do show upward-coarsening trends (Fig. 8), the distinction between storms and internal waves based on elusive vertical trends is moot”. Shannmugam is correct when saying “depositional attributes of the rocks do not change”, but he disregards what is common in Earth Sciences: interpretations do change, particularly when new concepts are learned. Or, should we reject the Plate Tectonics paradigm because the rock attributes on which the Geosynclinal theory was based did not change?

Also in the same section, Shannmugam states “Pomar et al. (2012) have emphasized that hummock cross-stratification (HCS) in the Upper Jurassic strata could be generated by internal waves”. This is absolutely untrue because we do not mention any hummock cross stratification in the Jurassic strata presented in our paper. Nevertheless, HCS structures occur in these Jurassic examples, on which the research work is ongoing. Pomar et al. (2012), based on several rationale, stated that (sic) “although the flow regimes on the seafloor associated to internal waves are not yet fully understood, we can postulate internal waves as the most plausible process under which HCS and HCS-like structures can form”, and suggested “HCS not to be linked to the storm-wave base but linked to the position of the pycnocline, although both surfaces can be concomitant, particularly during autumn and winter seasons”. Answers to many of the criticisms raised by Shannmugam about HCS can be found in the review on the origin of the hummock cross-stratification and the potential role of internal waves in their formation, recently published by Morsilli and Pomar (2012).

Under point “6.6. Evidence for paleo-pycnoclines” Shannmugam states: “the supreme evidence for interpreting deposits of internal waves and internal tides in the rock record is the physical evidence for paleo-pycnoclines”. This assertion equals: “the supreme evidence for interpreting foreshore deposits in the rock record is the physical evidence for paleo-sealevels”. Can it be considered correct? Density stratification is inherent to fluids, as anyone who has ever consumed an iced drink or witnessed fog has observed. Nevertheless, Shannmugam does not provide any clue for recognizing the existence of paleo-pycnoclines. In the rock record, the existence of a chemocline can be recognized if it has produced imprints in the sediments (e.g.: oxic-suboxic-anoxic) or in early diagenetic features (e.g.: Baird and Brett, 1986; Stanton et al., 2000; Immenhauser et al. (2002); Van der Kooi et al., 2007, 2009). Thermoclines and haloclines are not so prone to yield a record. Their occurrence, however, can be at times deduced from carbon and oxygen isotope signatures, for example, or from planktonic and nektoplanktic calcifiers living at different depths in the surface waters, or from the occurrence of benthic organisms (e.g. Hillaire-Marcel et al., 2001). Shannmugam, however, has no problem in asserting “that without that evidence for paleo-pycnoclines, there is no difference between a surface tidalite formed by surface (barotropic) tides on a shallow-marine shelf, and an internal tidalite formed by internal (baroclinic) tides in a deep-marine slope or canyon environment”. May it be because G. Shannmugam does not yet know that sedimentary rocks are commonly interpreted by integrative analysis of the sediment-grain types and sedimentary structures, fossil content and chemostratigraphic indicators within strata, along with analysis of the strata succession? Recognizing that circular reasoning must be minimized, interpreting the breaking of internal waves through sedimentary structures within a rock succession implies recognizing the occurrence of pycnocline.

Finally, in point “7. The Bottom Line” Shannmugam states: “Given the total lack of process-sedimentological knowledge of internal waves and tides, the complacent promotion of outcrop-based facies models, without confirmation from modern settings, can only promote circular reasoning and thus prolong our quest for the truth”. To this Shannmugam’s bottom line we can answer that despite the truth is difficult to reach in Earth Sciences, as we can only develop models that represent successive and progressive approaches to reality, ignoring the evidences and distorting interpretations certainly does not bring the way to find any truth.

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