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Reply to "Comment on 'Late Holocene Rupture of the Northern San Andreas Fault and Possible Stress Linkage to the Cascadia Subduction Zone' by Chris Goldfinger, Kelly Grijalva, Roland Bürgmann, Ann E. Morey, Joel E. Johnson, C. Hans Nelson, Julia Gutiérrez-Pastor, Andrew Ericsson, Eugene Karabanov, Jason D. Chaytor, Jason Patton, and Eulàlia Gràcia" by Ganapathy Shanmugam

by C. Goldfinger, J. Patton, A. Morey, and C. Hans Nelson

Introduction

Goldfinger *et al.* (2008) primarily relate the paleoseismic histories of the Cascadia subduction zone and northern San Andreas fault (NSAF), which is why we chose to publish the work in *BSSA*. The evidence for paleoseismic triggering of turbidity currents in both Cascadia and along the NSAF has been published previously (Adams, 1990; Goldfinger *et al.*, 2003, 2007). Shanmugam (2009) has commented on a variety of sedimentological issues, many of which would be of interest to the sedimentology and sediment dynamics communities but are out of place in *BSSA*. We focus our reply only on those comments that have a bearing on the paleoseismic investigation in Goldfinger *et al.* (2008), specifically, (1) turbidite triggering mechanisms, (2) sedimentological concepts and criteria, and (3) correlation methodologies.

Testing Triggering Mechanisms

Shanmugam (2009) suggests that we selectively used particular datasets to establish seismicity as the principal triggering mechanism for the Cascadia turbidites, but then fails to mention what other datasets we missed. To the contrary, we have ferreted out every core taken on the Cascadia margin and other relevant data to the best of our ability. It was archive cores collected by Oregon State University (OSU) in the 1960s that led to the initial article attributing them to earthquakes (Adams, 1990). Many of these cores exist in the OSU Core Facility, along with the majority of cores collected in the Pacific Northwest, so this was straightforward. Not all of them are included in the article because our more recent larger diameter cores largely superseded the older data, but many more are included in an upcoming article (Goldfinger *et al.*, 2009). These additional data strengthen the Cascadia earthquake story. We know of no other cores or relevant data, but we are always looking for additional information and welcome suggestions. As far as the treatment of the various possible triggering mechanisms themselves, we distinguish between processes that decrease slope stability and processes that act as triggers that trigger turbidity currents. Tectonic oversteepening, sediment loading, and gas hydrate destabilization are examples of processes that decrease slope stability but are generally not responsible in and of themselves for initiating slope failures, nor are they likely to be regional. We focus then on triggers capable of initiating turbidity currents that can be both regional and synchronous.

With respect to a discussion of these triggers, Shanmugam (2009) has failed to familiarize himself with the available literature in Cascadia. Goldfinger *et al.* (2008) note that distinctions between triggering mechanisms for turbidity currents in Cascadia are discussed in Adams (1990), Goldfinger *et al.* (2003, 2007), and Nelson *et al.* (2000), as well as many other localities cited in the article from Japan to the Iberian margin. In these and in the recent article, the authors discuss two primary methods used to identify triggering mechanisms: (1) sedimentological evidence and (2) synchronous triggering, which requires an earthquake to produce a regional synchronous slope failure.

Sedimentologic Evidence

Although at present there are not unequivocal global, regional, or local criteria to distinguish between turbidite triggering processes, the combined evidence from sedimentology, tests of synchroneity, stratigraphic correlation, and analysis of nonearthquake triggers is compelling. The available sedimentological criteria, used lightly in the article, generally support earthquake triggering in Cascadia Basin systems because the turbidites have sharp bases and fining upward sequences; we find no evidence of the waxing then waning (coarsening upward then fining upward) sequences common from storm deposits (i.e., Mulder *et al.*, 2003; St-Onge *et al.*, 2004). We observe possible evidence of storm generated sediment transport to canyon heads from storms as a number of very small turbidites between the larger regional ones, particularly at Eel Canyon with a very narrow shelf of only 12 km (Puig *et al.*, 2004). Cross shelf transport to the Eel

Canyon head is well documented, and we calculated that wave resuspension from extreme storm waves and tsunamis may occur to depths of several hundred meters (space precluded inclusion in this article, but we include these calculations in Goldfinger *et al.* [2009]). However, given that major storms occur 2–3 times per year in Cascadia (600–900 of them in the last 300 yr), we do not see evidence of more than a few local events in southern Cascadia channels adjacent to very narrow shelves. Major tsunamis, storms, and floods impacting the Cascadia margin, the most likely triggers given the requirements for regional synchronous triggering, are apparently not recorded in most abyssal plain cores. The best evidence for this is the well-constrained age of the youngest turbidite (Eel and Trinidad Canyons excluded) being indistinguishable from that of the AD 1700 and 1906 earthquakes for Cascadia and the NSAF, respectively. If storms or other triggering mechanisms were operating, we should see evidence of them. We conclude that materials transported to canyon heads in Cascadia apparently settle in the upper to mid canyon until dislodged by something else such as an earthquake, based on the lack of post AD 1700 turbidites in all systems except Eel and Trinidad. Storm wave loading and resuspension are apparently just not enough in most cases to ignite a significant turbidity current on the Cascadia margin, an observation also documented in lower Monterey Canyon, where the youngest turbidite is also likely to be the linked to the AD 1906 earthquake (Johnson *et al.*, 2005). On Monterey Fan, the turbidite frequency is similar to the San Andreas earthquake frequency (Piper and Normark, 2001) despite a nearly continuous input of sediment into the canyon head with its nearly zero shelf width (Paull *et al.*, 2005). The disturbing force in the case of storm input and waves generally acts only in the uppermost canyon and dies quickly with depth whereas an earthquake can shake the entire canyon. Additionally, the flood tidal cycle during storm transport can stop or reverse the transport direction, resulting in deposition of material that has reached the upper canyons. A fuller discussion of these issues is included in Goldfinger *et al.* (2009).

Synchronous Triggering

The most powerful tool available to differentiate between triggering mechanisms and to establish regional continuity of turbidites is synchroneity, which can effectively discriminate between earthquake and nonearthquake sources in most cases. For this reason, we have used the spatial and temporal pattern of event correlations and the synchroneity test at the confluence of Willapa, Juan de Fuca, and Cascadia Channels to establish a regional correlation that cannot be the result of triggers other than earthquakes. This test is discussed in Adams (1990), Goldfinger *et al.* (2007, 2008, 2009). Using radiocarbon age control and detailed stratigraphic correlation (based on continuous physical property measurements) of the turbidites, we confirm Adams's (1990) hypothesis that the channels both above and below the confluence contain the same post-Mazama turbidite record and further extend the confluence test to 19 Holocene events (Goldfinger *et al.* [2008] focus on the most recent 3000 yr). Because turbidity currents deposit their coarse loads in at most a matter of hours, they are excellent relative dating horizons, with age resolution far superior to radiometric dating techniques. The synchroneity of turbidite records established at the confluence effectively eliminates nonearthquake triggers because other possible mechanisms are extremely unlikely to trigger slides in separate canyons less than a few hours apart 19 consecutive times during the Holocene. The synchroneity test is strengthened by extending the record of the original Adams (1990) test to the 19 correlative Holocene turbidites at all key locations in Juan de Fuca Channel, Cascadia Channel, and Rogue Apron, from T6 to T18 in Barkley Canyon and down to ~T9 in the Smith, Klamath, Trinidad, and Eel Channels, with the addition of local southern Cascadia events discussed in Goldfinger *et al.* (2008). The uncertainty as to whether these are actually the same turbidites has been greatly reduced by ^{14}C dating and detailed stratigraphic correlation, with an average standard deviation between correlative turbidites dated at multiple sites of less than 40 calendar years (Goldfinger *et al.*, 2009).

Alternative Triggering Mechanisms

A comparison of the recurrence intervals of regional and somewhat synchronous alternative triggers, such as tsunamis, storms, volcanic eruptions, and bolide impacts, suggests that these mechanisms are unlikely to be responsible for the observed record (Goldfinger *et al.*, 2009, table 6). Bolide impacts significant enough to generate tsunamis are quite rare; submarine and nearshore volcanoes are not present along the Cascadia margin. Historic crustal and slab earthquakes are both too frequent and too small to be responsible for the deep water record. Storms with significant wave heights exceeding 12 m occur 2–3 times per year in Cascadia (where we have extratropical cyclones, not hurricanes as Shanmugam [2009] commented). If only one of the 600–900 major storms since the AD 1700 great earthquake had ignited a turbidity current, we would likely have seen it in our numerous cores. The combined lines of evidence indicate that other nonearthquake mechanisms, as unlikely as it may seem, simply do not add significantly to the robust turbidite record of local paleoseismic events in Cascadia during sea level high stands.

While other mechanisms certainly exist, each is problematic in terms of triggering competency, frequency, synchroneity, or the sustainability of transport of sand-sized material to deep water (> 3000 m depth) and great distances across the abyssal plain. During great earthquakes, the entire canyon system is affected, a length that can exceed 100 km in Cascadia. The rupture zone also underlies the full length of all of the Cascadia canyons at a shallow depth, making a near ideal setting for causing slope failures. During a great earthquake, the hypocentral distance to the locked fault is never more than between 2 and 10 km from the canyons, which likely fail in nearly continuous wall failure during the severe ground shaking of a large earthquake. Peak ground accelerations at such short distances to a great subduction earthquake are 2*g*–3.5*g* (Atkinson and Boore, 1997; Youngs *et al.*, 1997; Atkinson and Boore, 2003), more than enough to destabilize the entire canyon and slope.

Another key piece of evidence is the data from Hydrate Ridge at 44.7° N. Hydrate Ridge Basin West (HRBW) is isolated from land sources of sedimentation. It is a lower slope basin at a depth of ~2275 m, and the only sediment source is a local one, the western flank of the ridge, a seaward vergent anticline (Johnson *et al.*, 2003; Johnson, 2004). The ridge rises 1800 m above the basin floor, and the basin is guarded on all sides by structural ridges that prevent downslope transport into the basin from any source other than the flanks of the ridge itself. The physiography and great depth of the basin eliminate input from storms, tsunamis, hyperpycnal flows, and other external sources, as evidenced by the absence of transported Mazama Ash. In any case, there are also no large rivers along the central Oregon coast and no canyon systems between Astoria Canyon (46° N) and Rogue Canyon (42.2° N) 150 and 280 km distant, respectively.

HRBW acts as a control site, reducing the number of potential triggers for turbidity currents that could operate in the basin to only three: (1) earthquakes (both regional and local), (2) gas hydrate destabilization, and (3) sediment self-failure. The turbidite record at HRBW, however, closely matches that of the nearest core sites at Rogue Apron and records all 19 margin wide turbidites based on stratigraphic correlation and ^{14}C evidence. Stratigraphic correlation between HRBW and the Rogue Apron is particularly good, with good ^{14}C age matches as well. Overall, the turbidite records at these two sites contain the same number of large events, 19 in total (T14 is very subdued at HRBW). We infer that the close stratigraphic correlation, ^{14}C data, and the nearly identical number of large events in the HRBW cores make nonregional earthquake sources unlikely, with the possible exception of one uncorrelated event.

Finally, the recurrence intervals of Cascadia turbidites (Trinidad and Eel Channels excepted) closely match that of the onshore paleoseismic record (Goldfinger *et al.*, 2003, 2007, 2008). Goldfinger *et al.* (2008) include the Cascadia land paleoseismic data in several forms for comparison to the temporal record of earthquakes along the NSAF (for which land paleoseismic data is also included). For both fault systems, the recurrence intervals and timing of individual events are statistically indistinguishable. Alternative triggering mechanisms must satisfy the following criteria: (1) must pass synchroneity tests, (2) must have good stratigraphic correlation, (3) must explain the identical records in the isolated Hydrate Ridge Basin, (4) must match the frequency of observed events, and (5) must coincidentally match the land paleoseismic records of two major fault systems. Earthquakes are the only triggers that meet all these criteria; the alternatives do not meet any of them.

Shanmugam (2009) suggests that several other alternative geologic and oceanographic processes might explain the stratigraphy in our cores (although none of them meet the preceding five criteria):

1. Mass movements. McAdoo and Watts (2004) mapped a number of slope failures in Cascadia as did Goldfinger *et al.* (1992, 1994, 1997, 2000). Numerous submarine slides large and small have been mapped; however, there is no information about triggering and little age control except that provided for four individual slides (Goldfinger *et al.*, 1992, 1997, 2000). These slides have no clear relation to the strata found in sediment cores from this study (the slides with known ages are Pleistocene [Goldfinger *et al.*, 2000]), though we suspect that they too were probably triggered by great earthquakes. Shanmugam (2009) cites Greene *et al.* (2006) for submarine failures from a study in southern California; we fail to see the relevance to Goldfinger *et al.* (2008).
2. Currents. The ability of bottom currents to erode, resuspend, and transport sand-sized material is affected by many things, including grain size, density, bottom roughness, grain shape, cohesion, and grain size distributions, among others. For simplicity, we refer the reader to a highly simplified approximation of particle size and current velocity regimes shown in Figure 1.

   The southward-flowing surface California Current has a flow velocity approaching 12 cm/sec in summer, which is insufficient for sand transport particularly

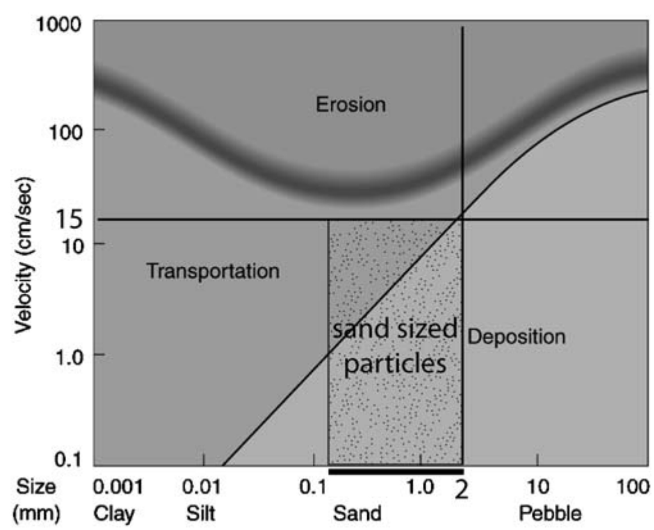

Figure 1. Hjulstrom's diagram showing approximate relations between current velocity and particle diameter. Plotted are the mean flow velocity required to initiate movement on a flat, uniform bed for flow depth of 1 m versus grain size. Flow velocity required to sustain movement is less. General regions of expected erosion, transportation, and deposition are shown. Sand-sized particles are shown in the sand-stippled box for a maximum velocity of 15 cm/sec, the minimum velocity for erosion of sand-sized particles. This type plot represents an empirical rule of thumb and does not consider flow depth dependency, turbulence, grain shape, or density and other factors. Adapted from Sundborg (1958).

considering that it is a surface current (Chereskin *et al.*, 2000; Strub and James, 2000). The counterclockwise California Counter Current is equally insufficient (4.2 and 10 cm/sec at 100 km and 100 m from the coast, respectively), barely sufficient to transport sand let alone erode and resuspend sand-sized material or trigger turbidity currents (Collins *et al.*, 2000). The implications of geostrophic velocity for the shelf offshore Oregon and northern California are not significant (with a maximum estimate of 30 cm/sec, Smith *et al.*, 2001) when considered as a trigger. Peak geostrophic velocity decreases with depth (Huyer *et al.*, 2002, figs. 5b and 9) as it does for any current as the bottom boundary layer is approached. Deep marine tidal currents in Eel Canyon such as those mentioned by Shanmugam (2009) have been documented by Puig *et al.* (2003). Puig *et al.* (2003) find that tidal currents are secondary to storm generated sediment flow in Eel River canyon and actually counter such flows every tidal cycle. Investigations of baroclinic (density instability driven) currents offshore Oregon (Torgrimson and Hickey, 1979) show that semidiurnal currents are baroclinic and are less than 10 cm/sec, similarly below the threshold to erode sand.

In support of his arguments, Shanmugam (2009) incorrectly characterizes the relevance of shallow water bottom flow near the Columbia River in his article (Shanmugam, 2008). For site M (Moritz, 2004), south of the Columbia River mouth, bottom flow is modeled at 29 cm/sec to the north-northwest, with instantaneous peaks to 150 cm/sec. If the current has the same sense of motion for the entire shelf offshore the mouth of the Columbia River (a simplified assumption that is not justified by Moritz [2004]), then sediment discharged from the Columbia River would migrate to the north carried by the Davidson Current, offshore Washington, not westward into the Astoria Canyon. This is well known and documented in several studies (Sternberg, 1986; Wolf *et al.*, 1999). However, nearshore storm transport is a well-known phenomenon observed worldwide as the nearshore bars move offshore in winter and back again in summer.

Another current type, upwelling induced flows (Hickey, 1997), vertically reach 50 m/day, far too weak to induce sediment gravity flows. While the aforementioned are all well-known processes, their sedimentary record on the abyssal plain, if any, is undocumented. In more than 100 Cascadia cores used in Goldfinger *et al.* (2008), we find no evidence of these continuous or very frequent events other than what contribution they may make to hemipelagic sedimentation. Most importantly, none of them meet the observational criteria for Cascadia turbidites and earthquakes previously described.

Sedimentological Concepts and Criteria

Shanmugam's (2009) comment suggests that Goldfinger *et al.* (2008) give "flawed sedimentological concepts and criteria that were applied for interpreting turbidites." In our view, Shanmugam's comment and recent article suffer from misconceptions and a poor understanding of oceanography and the Cascadia region. He states, "the entire Washington–Oregon–California margin was inundated by nothing but the omnipresent turbidity currents at the time of deposition." As we state in this and previous articles, the core sites were specifically targeted to recover turbidites in channel systems, many of which have been investigated since the 1960s, and their stratigraphy was well known for some channel systems. The cores were collected expressly for this purpose and were not random general purpose cores. They were designed to exclude other processes by taking advantage of physiography and the wide-shelf high-stand conditions that presently isolate Cascadia Basin and the NSAF margin from terrestrial sediment sources. While there are certainly numerous other sediment processes along the Washington–Oregon–California margin, the sedimentological records of them are mostly not found in the sediment cores collected for this study. This was established decades ago when OSU investigators and students (one of them, Nelson, is a principal investigator and co-author) studied the Astoria Fan and Cascadia Basin channels (Nelson, 1968; Nelson *et al.*, 1968; Griggs, 1969; Duncan, *et al.*, 1970; Nelson, 1976; Goldfinger *et al.*, 2003, 2007, 2008). We note that Shanmugam has never requested any information or data nor ever visited our cores at the National Science Foundation (NSF) core facility at OSU and that the cores are readily available to interested investigators.

Shanmugam (2009) states that we must adhere to his dogmatic view of how observations should be made, classifications that must be used, and terminology that he favors. There are, for example, many sediment grain size classification systems, and there is not a single system that is required to satisfy the scientific method, nor in fact is the absolute value of grain size of much relevance to our study. The data from our, and most, marine geological cruises include detailed lithostratigraphic descriptions of the sediment cores. These descriptions, indeed, have the components Shanmugam suggests are necessary, and we agree. These data alone would occupy ~300 printed pages but are available for the asking. We include examples in figures 3–5 of Goldfinger *et al.* (2008). References included do address this issue in greater detail (Goldfinger *et al.*, 2003, 2007). Most importantly though, the detailed turbidite stratigraphy, lithology, grain size analyses, chemistry, etc., for the region have been well documented over the past four decades, and there was no need to repeat that information (i.e., Nelson, 1968; Griggs, 1969; Duncan *et al.*, 1970; Nelson, 1976).

Shanmugam (2009) incorrectly states that Goldfinger *et al.* (2008) rely on sedimentological criteria for distinguishing seismoturbidites. We explicitly state in this and previous articles that we do not rely heavily on sedimentological criteria but use stratigraphic correlation, synchronous triggering, and confluence tests supported by ^{14}C ages as described previously to establish the triggering mechanism.

Shanmugam is correct regarding the important distinction between description and interpretation; however, detailed descriptions of the turbidites have already been published. Thus, we do not repeat these as new observations except where our observations diverge from or add to the previous literature. Further descriptions of the core stratigraphy can be found in the literature cited in the article and in previous publications (Nelson, 1968; Griggs, 1969; Duncan *et al.*, 1970) or by requesting the data for 1999 and 2002 cores from us.

Shanmugam (2009) then diverges into the terminology of sediment gravity flows, which is not the subject of Goldfinger *et al.* (2008). Bouma sequence classification for coarse grained turbidites is widely accepted by sedimentologists worldwide. Goldfinger *et al.* (2008), like others in the field, simply use Bouma classification as a widely familiar descriptive framework and do not wade into the sand trap of competing views of process sedimentology.

Goldfinger *et al.* (2008) also do not attempt to explain process sedimentology of sediment gravity flows; we use the synchronous deposition of turbidites to constrain the timing and lateral extent of strong ground shaking. The fact that details of process sedimentology, such as the transition from Ta to Tb to Tc, are or are not well explained by physical models (LeClair and Arnott, 2005) does not negate their observed deposition (Bouma, 1962). Rather, it simply illuminates model flaws; models must explain the observations, not the other way around.

Correlation Methodologies

Shanmugam (2009) notes that Goldfinger *et al.* (2008) did not explain the criteria for distinguishing the boundary between turbidite mud and hemipelagite mud, which is correct. The issue is dealt with to some extent in this article and in more detail in Goldfinger *et al.* (2007) and Gutiérrez-Pastor *et al.* (2009), which we cite. Goldfinger *et al.* (2008) is a follow-up to Goldfinger *et al.* (2007), though we perhaps could have cited that one more time. Further details are included in Goldfinger *et al.* (2009). This is an important issue and not always straightforward to resolve. The distinction between turbidite base, turbidite tail, bioturbated tail/hemipelagite, and hemipelagite (including how these units may or may not be well stratified) was clearly stated. Goldfinger *et al.* (2008) state that the hemipelagic sediment used for radiocarbon dating comes from the sediment directly underlying the base of the overlying turbidite, avoiding the tail boundary. To summarize, Goldfinger *et al.* (2008) used smear slides, grain size analysis, the multisensor track logs, visible and X-ray imagery, and visual observation to locate this boundary.

Dating from just under the turbidite avoids the tail boundary in most cases but does include the risk of basal erosion. In previous articles, we discuss this issue more fully (Goldfinger *et al.*, 2007; Gutiérrez-Pastor *et al.*, 2009) and do not repeat it in this article. The method uses multiple cores within a few kilometers to examine the hemipelagic thickness in all cores to identify those with eroded intervals. In addition, we also estimate erosion by comparing the total hemipelagic thickness in Cascadia Channel and interchannel cores (Duncan, 1968; Nelson, 1968; Griggs, 1969; Duncan *et al.*, 1970; Nelson, 1976). We find slightly more hemipelagic sediment in the channel cores and no evidence of significant erosion not captured in the analysis of differential erosion. This method is, indeed, important and is much more completely described in Gutiérrez-Pastor *et al.* (2009) and Goldfinger *et al.* (2009).

Shanmugam (2009) states that we cannot date earthquakes using turbidites. While it is true that dating events in the sedimentary record is commonly difficult, we have developed the technique over the past decade, and this article and Goldfinger *et al.* (2007) give straightforward explanations of our method for deriving event ages. This study uses ^{14}C from planktic forams to get age estimates for hemipelagic sediment immediately older than the overlying turbidite. This provides a maximum limiting age for the deposition of the turbidite. This method is used for paleoseismology quite extensively onshore (e.g., McCalpin, 1996; Atwater and Hemphill-Haley, 1997). Because we have continuous marine sedimentation, dating marine turbidites has significant advantages because we can correct for sample thickness and, with multiple cores, erosion as well. Thus, the reported ages are no longer maximum limiting ages but quite close to the event age. Examples of this are shown in Goldfinger *et al.* (2007), in which the age of the 1906 earthquake is recovered with ^{14}C and hemipelagic sediment data to within a few years. These tools are familiar to paleoseismologists, and abundant literature (e.g., McCalpin, 1996; Kelsey *et al.*, 2005) supports their use, though they are naturally unfamiliar to those who do not require precise age control in their work. When compared to known datums, the marine ages are as likely to be slightly young as slightly old based on comparisons with well-constrained land event ages and the Mazama ash datum. In addition to ^{14}C ages that are at least as certain as terrestrial counterparts, the time series is also tightly constrained by the hemipelagic sedimentation between events, which we use with OxCal to trim the tails of the age probability density functions. Finally, confluence tests can actually limit the relative timing to within minutes to hours (Adams, 1990; Goldfinger *et al.*, 2003, 2007, 2008). We are baffled by the comment that we cannot date cyclone triggered bottom flows. If there were any, we would date them in the same way.

Shanmugam in his comment and in his articles becomes entangled in semantic dogma, claiming that the Bouma divisions represent multiple events. Bouma divisions probably represent a sequence of transitions of flow regimes of a single depositional event (Bouma, 1962). Typically, sedimentologists ignore complex initial inputs because no information is available regarding the complexity of the initial source. Goldfinger *et al.* (2008) discuss multiple pulses that are composed of multiple Ta-Tb-Tc Bouma sequences followed by the tail Td-Te. These are the data that must be explained. We conclude that the earthquake source, inclusive of multiple rupture patches spread over some number of minutes, is the most likely source for multiple fining upward sequences capped by a fining upward tail. A good example of this is the 2004 Andaman Island earthquake (Chlieh *et al.*, 2007), where there were three main slip patches and some of the resulting turbidites we collected in 2007 have three fining upward sequences (Patton *et al.*, 2007, unpublished data, 2009). Nakajima and Kanai (2000) were in our view correct in observing the same phenomenon and attributing it to earthquakes in Japan.

Shanmugam (2009) states incorrectly that we use physical property logs as proxies for normal grading. As we stated, we use them as proxies for vertical grain size distribution; we demonstrate the validity of this usage specific to the region in question and do not assume normal or any grading scheme. The grading is revealed by the data. This is clearly shown in the figures in Goldfinger *et al.* (2007) and Goldfinger *et al.* (2008). Physical property logs simply allow rapid generation of grain size distributions, constrained by spot analyses and supported by visual observations and x-radiography. The use of magnetic susceptibility and gamma density logs as proxies for grain size is well established (Schlumberger, 1991; Goldfinger *et al.*, 2003; Morey *et al.* 2003; Wynn and Masson, 2003; Goldfinger *et al.*, 2007, 2008) and, because it is central to the article, is clearly illustrated in figures 3–5 of Goldfinger *et al.* (2008).

The success of physical property core logs in stratigraphic correlation has been demonstrated through its successful and widespread application to oil exploration. Because many other processes can create upward fining sequences, we do not use upward fining as an indicator of earthquakes, as noted previously, making the distinctions offered by Shanmugam (2009) irrelevant in this context. Shanmugam oddly states that without core one cannot use the physical property logs as we have, which is probably true. Fortunately though, we have all the cores and invite Shanmugam to come to Corvallis, Oregon, to see them. Shanmugam states that because physical property logs cannot be used to distinguish among a long list of subtly different gravity deposits, they cannot be used to correlate turbidite units. This is simply incorrect, and the hair splitting sedimentological differences between sandy debrites and sandy injectites (terms that no two sedimentologists would agree on) does not address the data in the article. Correlation is often done, perhaps almost always done, without necessarily having a complete understanding of all the details that affected the depositional sequence. Such information is virtually never available. Oddly, Shanmugam suggests model driven turbidite interpretation is obsolete, though he argues many of his points based on process models. Goldfinger *et al.* (2008) do not use any models outside of the general principle that sediment moves downhill. While one may endlessly classify and reclassify sedimentary deposits according to models and preconceptions, the correlations in Goldfinger *et al.* (2008) are based simply on their grain size distribution patterns and ^{14}C ages, supported by the Mazama Ash datum and correspondence with the terrestrial earthquake record. The hydrodynamics involved are well beyond the scope and purpose of the article. However, given that the core sites include plunge pools, slope basins, and narrow channels, some of the deposits might be classified differently if one wished to do so. The classification of transported deposits does not affect their correlation, and multiple types would be expected in the case of great earthquakes that undoubtedly generate the full range of mass transport deposit types in numerous locations simultaneously. Shanmugam's (2009) comments further miss this central concept of Goldfinger *et al.* (2008): that detailed stratigraphic correlation over large distances is most likely the result of synchronous triggering, which in turn is difficult to explain without regional earthquake rupture.

Finally, the confluence test is not used to validate earthquake triggering; that is why it is called a test. It is simply one of many observations that, when considered as a whole, lead us to conclude that earthquakes are the most likely explanation for the set of observations as a whole. Not only do the turbidites pass the original confluence test (Adams, 1990), which simply included the number of turbidites above the Mazama Ash, but the pulsed structure of each turbidite is preserved, requiring precise arrival timing of the coarse pulses at the confluence. In many ways, the events in the isolated Rogue Canyon and Hydrate Ridge Basin are as convincing and more difficult to explain, as these deposits correlate nearly as well and have no physical connection, aside from the subduction zone megathrust. None of the processes suggested by Shanmugam, nor any we can think of, can come close to passing these rigorous tests. Thus, we conclude that the Cascadia Basin Holocene turbidites described in Goldfinger *et al.* (2008) were likely generated by earthquakes, a trigger that easily explains the data with very few assumptions. We note that our original NSF proposal argued that the simple Cascadia turbidite–earthquake hypothesis proposed by Adams (1990) had to be more complicated, as Shanmugam suggests, but the data proved us wrong.

Data and Resources

All data used in this article came from published sources listed in the references.

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