Earth-Science Reviews 155 (2016) 49–59

Contents lists available at ScienceDirect

Earth-Science Reviews

journal homepage: www.elsevier.com/locate/earscirev

Invited review

The crisis of a paradigm. A methodological interpretation of Tohoku and Fukushima catastrophe

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ARTICLE INFO

Article history:
Received 15 April 2015
Received in revised form 8 January 2016
Accepted 25 January 2016
Available online 26 January 2016

Keywords:
Earthquake
Natural hazards
Epistemology
Paradigm
Research methodology
Intellectual virtues

ABSTRACT

The 2011 Japanese disaster often presented as a 'new Chernobyl' accumulated the effects of earthquake, tsunami and of the subsequent nuclear accident at Fukushima. In the light of this disaster, we review methodological reasons both from geophysical and philosophical perspectives that lead the scientific and technological communities to flawed conclusions, prime cause of the disaster. The origin of the scientific mistake lies in several factors that challenge a dominant paradigm of seismology: the shallower part of the subduction was considered as weak, unable to produce large earthquakes; a complete breakage of the fault up to the sea-floor was excluded. Actually, it appears that such complete rupture of the subduction interface did characterize megathrust ruptures, but also that hazard evaluations and technical implementation were in line with the flawed consensual paradigm. We give a philosophical interpretation to this mistake by weighing the opposition between a prescriptive account and a descriptive account of the dynamics of research. We finally emphasize that imagination, boldness, and openness (especially to alternatives to consensual paradigms) appear as core values for research. Those values may function as both epistemic and ethical standards and are so essential as rigor and precision. Ability to doubt and to consider all uncertainties indeed appears essential when dealing with rare extreme natural hazards that may potentially be catastrophic.

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1. Introduction

Earthquakes are natural physical events with important human, societal and economic consequences. The destructive character of an earthquake depends primarily on geological and physical parameters, such as location, magnitude and geometry of fault rupture. Anthropological studies offer another perspective. Oliver-Smith (1994) claims that "*disasters do not simply happen; they are caused*", adding that this is because "*disasters occur at the interface of society, technology, and environment and are the outcomes of the interactions of these features*" (Oliver-Smith, 1996). The main implication is that there is no disaster without a context of social-historical-political factors that will set up the vulnerability of human groups and settlements (Revet, 2012). In the aftermath of the Lisbon catastrophe of 1755 -accumulating the effects of the earthquake, fire and tsunami- the relative degree of responsibility of Nature and Humans was already subject of debate between Voltaire (1756) and Rousseau (1756). Dynes (2000) suggests that the "*first social scientific view on disaster*" – by Rousseau – clearly stated that the catastrophe was a social construction and that the urban pattern made a city located in a seismic risk area susceptible to damage. In our modern technocratic countries, the political or societal tasks designed to anticipate effects of natural hazards deserve a variety of studies, debates and controversies. In particular, the case of Nature versus Human responsibility is formalized by combining hazard with vulnerability to quantitatively rate the risk and to settle mitigation solutions. It

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http://dx.doi.org/10.1016/j.earscirev.2016.01.013

appears that several human and technical factors – including the way sensible infrastructures are structurally engineered – may impact the vulnerability, but forecasting the hazard itself chiefly rests on the scientific expertise which may be affected by large unknowns. Approaches to take into account the range of scientific ideas have been developed by the reinsurance and catastrophe modeling industry to eventually reach a consensus (e.g., Delphi method, Linstone and Turoff, 1975). In fact, social studies of science and technology (Callon et al., 2009) suggest that the process resulting in a dominant scientific perspective at a given moment – the paradigm on which the expertise is based – may adopt the form of a "social construction" (e.g., Tierney, 2007). With these thoughts in mind, we note that the geophysical community rarely questions its ability to deliver a correct expertise to the rest of the society, nor evaluate related epistemic and ethical issues.

After the 2011 Japanese magnitude 9 earthquake and tsunami, and the ensuing nuclear accident at the Fukushima Daiichi plant, an intense debate rose in the geophysical community (e.g., Avouac, 2011; Geller, 2011; Kerr, 2011; Normile, 2011; Sagiya et al., 2011; Stein and Okal, 2011; Kanamori, 2012; Lay, 2012; Stein et al., 2012; Geller et al., 2015), perhaps summed up by breaking titles in *Nature* magazine such as "*Shake-up time for Japanese seismology*" or "*Rebuilding seismology*" (Geller, 2011; Sagiya et al., 2011). That debate revealed community's unease considering what seems to be a failure to have correctly evaluated the earthquake and tsunami hazards before disaster's occurrence. In the light of the Japanese disaster, it appears crucial to re-evaluate theoretical and practical reasons and founding methodological principles, both from physical and philosophical points of view, that lead the scientific and technological communities to somewhat flawed conclusions and actions – or inaction – that should be considered as the prime cause of the disaster. We'll argue that it enlightens the processes leading scientific paradigms to survive and eventually collapse, and the ways scientific models and their uncertainties are implemented – or not – by the technical and political spheres and understood by the rest of the society.

We thus start with a review of the geophysical, technical and societal context to identify the different mistakes that lead to ravage of NE Japanese coastal settlements and to the Fukushima Daiichi nuclear disaster. We then give a philosophical interpretation of those mistakes, before exploring implications in term of epistemic and ethical values and norms that should be kept in mind while forecasting extreme natural hazards. To ensure readability by a large, geophysical and anthropological community, we use footnotes to explain basic seismological and philosophical lexicon, processes and concepts.

2. The geophysical, technical and societal context

The Mw[1] 9.0 2011 Tōhoku-oki earthquake broke a ~500 km long segment of the subduction megathrust[2] that marks the boundary between the Pacific and Okhotsk tectonic plates (Figs. 1, 2). The fault, which dips west beneath Japan, broke from depth ≥ 40 km to its emergence at the sea floor. Coseismic slip[3] was particularly strong on the shallower parts of the fault close to the Japan trench (several tens of meters, possibly more than 50 m, see Fig. 2c), causing large vertical displacements of the sea-bottom just above the fault and provoking huge tsunami waves (e.g., Lay et al., 2011; Simons et al., 2011; Ozawa et al., 2011; Kodaira et al., 2012; Satake et al., 2013; Tajima et al., 2013). On the coast facing the Japan trench, tsunami inundation reached heights typically larger than 15 m, locally 30–40 m, above average sea level (Mori et al., 2011), killing more than 15,000, drowning the Fukushima Daiichi nuclear plant (Fig. 3) and provoking the subsequent nuclear accident. In the past sixty years before that event, at least four Mw 9+ earthquakes – Kamtchatka 1952 Mw 9, Chile 1960 Mw 9.5, Alaska 1964 Mw 9.2, Sumatra 2004 Mw 9.1 to 9.3, and possibly Aleutians 1957 Mw 8.6 to 9.1[4] – broke various subduction megathrust segments worldwide (Fig. 1, see also Fig. 4). As a consequence, the risk of occurrence of such Mw 9+ events on any subduction zone in the World was correctly identified by few authors (e.g., McCaffrey, 2008), although dismissed or ignored by most of the geophysical community. Indeed, the scientific consensus before Tōhoku was that each subduction zone has its own, complex, segmentation and mechanical properties,[5] and that many subduction zones in the World will never produce a Mw 9+. This was admitted for the part of the Japan trench that eventually broke in 2011, where erroneous estimates of potential magnitudes and rupture segmentation resulted in bottom level estimates of the hazards (e.g., Fujiwara et al., 2006; Fujiwara and Morikawa, 2012). But, as noted a posteriori by Stein and Okal (2011), "*the size of the 2011 Tōhoku earthquake need not have been a surprise*". We identify several interwoven causes to what should be considered as a scientific mistake.

Hazard estimates were only based on the detailed analytical record of local past events, which were considered over a too short period of time. The Mw ~ 7.5 earthquakes of the past decades were taken as characteristic of the seismic potential of the subduction offshore Tōhoku. A model of segmented, patchy subduction interface was thus deduced (Fig. 2a) and used for earthquake and tsunami hazard calculations with the aim to produce the official hazard maps (Fujiwara et al., 2006; Yanagisawa et al., 2007; Fujiwara and Morikawa, 2012). It appears that the 2011 event largely overcame that segmentation (Fig. 2c). It is worth noting that those hazard estimates based on the short-term local analytical record were not put in perspective of the worldwide memory of giant megathrust events. Specifically, close to the N in Kamtchatka, the same subduction interface than in NE Japan hosted a very large magnitude (Mw ~ 9) earthquake in 1952 (Fig. 1). The fault segment facing the Tōhoku coast has the same first-order geometrical characters than the one that broke in 1952 offshore Kamtchatka. This should have hint for the potential of earthquakes with much larger rupture zones and magnitudes along the Japan trench.[6] Indeed, the millenary historical record implies that very large events broke the subduction offshore NE Japan in the past. The largest of these events appears to be the 869 AD Jōgan earthquake that gave rise to a tsunami with effects comparable to those of the 2011 Tōhoku-oki event (e.g., Sugawara et al., 2012a, 2012b). Other strong tsunami hit the NE Japan coast in the past centuries (e.g. 1611, 1793, 1896, 1933). Perhaps also akin to 2011's, the 1611 AD Keicho earthquake and tsunami, known from historical and geological records, inundated many places along the Japan coast

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[1] The moment magnitude, noted Mw, is a physical measure of the energy released by the earthquake. Its scale is logarithmic, not linear. A Mw 7 event has 30 times the energy of a Mw 6 (the same relation exists between Mw 8 and 7 or Mw 9 and 8 for example). Here we note Mw9+ for earthquakes with magnitudes ≥ 9.

[2] Subduction megathrusts are extremely large geological faults marking the interface of converging tectonic plates. They dip relatively gently (~10–30°) below the upper plate (Japan in our case) while the lower plate (here the Pacific plate) is sliding downward at a pluri-centimetric rate. The upper part of the megathrust, from depth ~40 km to its emergence at the oceanic trench, moves in a stick–slip way on century time-scale, a process called the seismic cycle. Between large earthquakes, the fault stays locked, and, on each side, upper and lower plates deform and store plate convergence in an elastic (reversible) way. Stresses thus accumulate and eventually reach a yield point generating a massive seismic slip on the fault – itself causing the earthquake – releasing part or totality of the stored elastic strain. Those processes – strain accumulation and catastrophic release – are now accurately measured by geodesy using GPS or other techniques.

[3] The "coseismic slip" represents the amount of slip on the fault that accumulated quasi-instantaneously (tens of seconds to minutes) during the earthquake. That slip generates destructive seismic waves and vertical motion of the sea floor responsible for the tsunami.

[4] Magnitude of the 1957 Aleutian earthquake varies significantly from one study to another.

[5] The magnitude of an earthquake depends on the size of the broken fault or fault segment, and on the coseismic slip. In addition these two parameters are linked by scale-laws. This implies that a small fault, or a very segmented fault will thus be unable to generate large earthquakes.

[6] The same subduction zone also caused large Mw 8+ earthquakes in 1963 and 2006 offshore the Kuril islands (Mw 8.5 and 8.3).

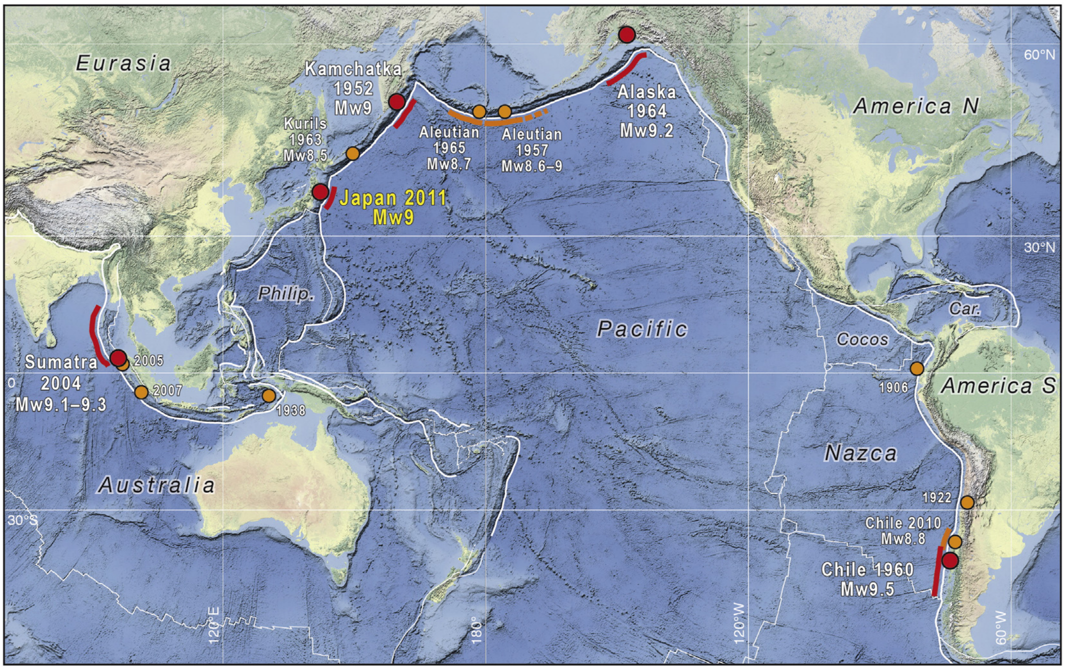

Fig. 1. Large magnitude (Mw ≥ 8.5) earthquakes at the origin of important tsunamis since 1900 (Mw 9 + larger dots, Mw 8.5–9 smaller dots). When known with an acceptable precision, the along-strike extension of coseismic ruptures are sketched by bold lines. Main fault traces are in white, names of main tectonic plates are in white italicized characters.

embracing the Sendai plain (e.g. Minoura et al., 2013). All those events – apparently too old to be considered – were not taken into account for hazard calculations, even after documenting the geological traces of huge tsunami inundations that occurred in the past with return times of ~ 1000 years (see for example Minoura et al., 2001; Sawai et al., 2007).

There was a wide consensus in the geophysical community on a paradigmatic physical model of along-dip segmentation of frictional properties and asperities on the subduction interface (e.g., Kanamori, 1986; Byrne et al., 1988; Hyndman et al., 1997, see recent discussion by Lay et al., 2012 and Hubbard et al., 2015). The shallower part of the subduction megathrust was considered as weak and mostly aseismic, thus unable to produce large earthquakes or only prone to perhaps slip alone during slow "tsunami earthquakes". A complete breakage of the fault from depth ≥ 30–40 km to its emergence at the trench sea-floor was excluded (while that sort of behavior is now accepted and documented for large active faults inland, see Hubbard et al., 2015 and Fig. 4). But actually, it appears that such complete rupture of the subduction interface did characterize the megathrust ruptures that happened during the past-decade (Sumatra, 2004, Chile 2010, Japan 2011, see for example Vigny et al., 2011; Lay et al., 2012; Hubbard et al., 2015). This was likely the case too for the Mw 9 + earthquakes from the 1950s and 1960s (Fig. 4) although the geophysical observations acquired at earthquakes' time are barely sufficient to confirm that inference. Yet, as already mentioned, each subduction zone in the world was considered to have its own, complex mechanical properties, and many subduction segments were thought to never produce very large ruptures (e.g., Kanamori, 1986). The idea of a patchy subduction interface offshore Tōhoku – on which hazard estimates were based (see above) – was in line with this consensual model. The short term seismic record – earthquakes of moderate magnitude – was taken as characteristic of the mode of rupture of complex fault segments and asperities (Fig. 2a), and the interface was viewed as poorly seismically coupled (i.e., little strain released by earthquakes compared to what is expected to occur from plate convergence rate; e.g., Yamanaka and Kikuchi, 2004). In contrast, modeling of the GPS measurements acquired in the past 15 years (Mazzotti et al., 2000; Nishimura et al., 2004; Suwa et al., 2006; Hashimoto et al., 2009; Loveless and Meade, 2010) suggested that a large part of the megathrust was indeed locked and efficiently storing plate convergence as elastic strain before its complete breakage in 2011. That this elastic strain was ready to be released by a future great earthquake was not clearly pointed out however – at best it was stated as one among several possible interpretations (e.g., Kanamori et al., 2006) – neither taken into account to reassess hazard evaluations.

There was an excess of confidence in sophisticated numerical models in line with the consensus described above, and in the soundness of the modeling results, as illustrated by the following example. In the past decade, geological studies of tsunami sand deposits in the Sendai bay tried to document the inundation distances and heights reached by the 869 AD Jōgan tsunami (Minoura et al., 2001; Sawai et al., 2007). Then, numerical modeling of the 869 AD earthquake was done to calculate predicted inland inundations and to compare these predicted values to the results of the geological investigations. From those comparisons, several possible magnitudes were considered for that medieval event depending on the size and location of the modeled earthquake source. A maximum magnitude of Mw ~ 8.3 was eventually retained corresponding to the rupture of a fault segment slightly larger than the patch labeled 4 on Fig. 2a (Minoura et al., 2001; Satake et al., 2008). However, the field observations acquired after the 2011 tsunami show that the inundation reached roughly the same level in 2011 than in 869 AD (Goto et al., 2011; Sugawara et al., 2012a, 2012b). This suggests that the previous modeling underestimated the size of the Jōgan earthquake source and thus the magnitude of the medieval earthquake, probably because it was dimensioned in conformity with the consensual idea of along-dip segmentation of the subduction, and because all uncertainties were not properly taken into account (see Goto et al., 2011). Yet, although the modeled magnitude for the 869 AD earthquake (Mw ~ 8.3) was much larger than the one (~7.5) kept to establish the official hazard maps, it has not been used to reevaluate those maps. It is also interesting to note that another class of sophisticated models was used for probabilistic calculations aiming to produce those hazard

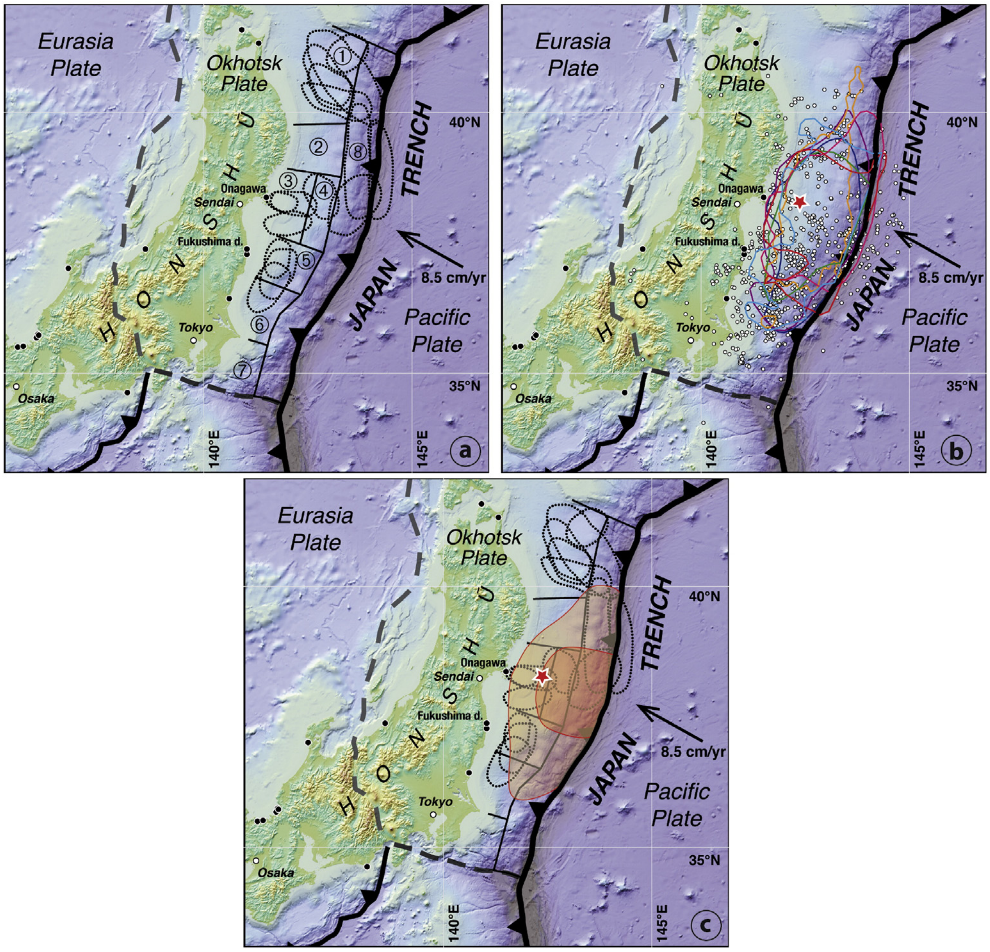

Fig. 2. Context and reality of the 2011 Tōhoku-oki earthquake. Upper left map (a) illustrates views prior to the 2011 event about characteristic seismicity and segmentation of the subduction interface offshore NE Japan. The past earthquake rupture zones since 1895 (of moderate-large magnitudes) are shown by dotted lines (after Tajima et al., 2013, based on Kanamori et al., 2006). The segmentation on which former hazard map calculations were based (Fujiwara et al., 2006; Yanagisawa et al., 2007; Fujiwara and Morikawa, 2012) is shown in black. The maximum magnitudes considered for segments indicated by circled numbers 1 to 8 are: [1] Mw ~ 8, [2] Mw ?, [3] Mw 7.5, [4] Mw 7.7, [5] Mw 7.4, [6] Mw 6.7–7.2, [7] Mw ?, and [8] Mw 8.2 (Fujiwara and Morikawa, 2012). Upper right map (b) illustrates the 2011 Mw 9 earthquake rupture. Several lines (in different colors) show the extension of the rupture zone as estimated by different source models (zones with coseismic slip ~ ≥5 m; Lay et al., 2011; Ozawa et al., 2011; Simons et al., 2011; Yue and Lay, 2011; Yagi and Fukahata, 2011; Hashimoto et al., 2012; Satake et al., 2013). Star locates main shock epicenter while open small dots are aftershock epicenters. Map on (c) shows a comparison of the previously admitted segmentation with the reality of the 2011 earthquake: the rough extension of the Mw 9 rupture is sketched by light shading from (b) and the zone with very large slip (~≥20 m) is shown by darker shading (from same source models). Both clearly overprint segmentation shown in black. On the three subset figures, black dots show nuclear sites with Onagawa and Fukushima daichi ones clearly identified. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

estimates and maps (e.g., Fujiwara et al., 2006; Annaka et al., 2007; Yanagisawa et al., 2007; Fujiwara and Morikawa, 2012). But it appears that those models were also scaled with respect to the flawed consensus mentioned above (see Stein et al., 2012), thus actually unable to give correct evaluations of the true hazards.

The technical, industrial and political spheres implemented protection measures in line with the scientific consensus and compatible with their economic interests (see for example Nöggerath et al., 2011; Funabashi, 2012; Funabashi and Kitazawa, 2012). At the TEPCO Fukushima Daiichi nuclear plant, the protection against tsunami was set up at plant's conception time (in the 1960–70s) from the height measured locally after the 1960 tsunami. However, that tsunami was due to a source on the opposite side of the Pacific Ocean (a massive Mw 9.5 earthquake that broke the subduction megathrust along the Chilean coast). Surprisingly, largest tsunamis due to past earthquakes with sources much closer than the Chilean subduction were not considered, although well-known from historical evidence. Until now, even after some warnings issued by few scientists in the past decade (Nöggerath et al., 2011; Hasegawa, 2012), these evidence were downplayed by most engineers (see Krolicki et al., 2011; Nöggerath et al., 2011; Aoki and Rothwell, 2013) and even by seismologists (see Stein et al., 2012). Tsunami protection at Fukushima Daiichi was thus never significantly reevaluated. It remained dimensioned for run-up heights ≤ 5.7 m (Nöggerath et al., 2011; IRSN, 2012) while the 2011 tsunami reached ~ 14–15 m at plant's site (Fig. 3; IRSN, 2012). In fact, likely to reduce technical issues and costs related to cold water supply, the Fukushima Daiichi plant was built close to sea level (≤10 m elevation, IRSN, 2012) on a platform artificially carved across the coastal escarpment limiting a small plateau at 30–40 m above sea level (Fig. 3). Clearly, most of destructions and troubles due to the tsunami would have been definitively avoided setting the plant on top of the coastal plateau. It's worth noting that the Onagawa nuclear plant – operated by another company and set even closer to the 2011 earthquake epicenter (Fig. 2) – was saved due to past personal determination of an engineer, Y. Hirai. At the end of the 1960s, Hirai fought to set Onagawa plant protection at ~ 15 m with respect to what he knew about the 869 AD tsunami (Reb et al., 2012; Yamada, 2012). The 2011 tsunami reached ~ 13 m at Onagawa, slightly less than the height of the wall. Thus, some warnings to the technical sphere, vain in the case of Fukushima Daiichi, were issued by few whistleblowers. However, a large part of the geophysical community remained tied to a paradigm (see above) and did not delivered unflawed models with a full

Fig. 3. Setting of the Fukushima Daiichi nuclear plant and 2011 tsunami inundation (based on Google Earth 3D view; background image is a satellite view taken on march 12th 2011, only one day after the tsunami). The plant is built on a small platform at less than 10 m above sea level (asl). That platform has been carved across the escarpment limiting a small plateau at >30 m asl (30 m elevation curve redrawn from Japanese topographic maps). Arrows on (a) point to trace of maximum tsunami run-up clearly visible on the satellite view. On (b) a flat plane at 14 m asl (in blue) has been added to simulate tsunami inundation. Its intersection with the topographic model provided by Google Earth fits remarkably well the maximum run-up trace identified on (a). (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

discussion of unknowns and uncertainties (Stein et al., 2012). This certainly hindered any serious reevaluation of hazard assessment and of protection measures. The existence of a nuclear "*myth of safety*" (Funabashi, 2012; Geller et al., 2013), anchored in the long-standing development of the Japanese "nuclear village" since the 1960s[7] and correlative conflicts of interest (Nakamura and Kikuchi, 2011; Onishi, 2011c; Funabashi, 2012; Funabashi and Kitazawa, 2012; Hasegawa, 2012; Reb et al., 2012), did the rest. After the disaster, the bad technocratic and political responses in the days and weeks that followed the Fukushima Daiichi accident worsened its direct consequences and societal impact (e.g., Onishi, 2011b; Funabashi and Kitazawa, 2012; The National Diet of Japan, 2012; Reb et al., 2012; Aoki and Rothwell, 2013; Hindmarsh, 2013).

Last, the society, confident in the scientific expertise and in the implemented technical protection measures, downplayed the ancestral memory of past-events. Along the Japan coast, huge concrete walls and breakwaters were built to protect coastal communities from tsunamis. In many places, those protections were overtopped and largely destroyed by the 2011 waves, and a debate then initiated on the way to design more efficient protections (Cyranoski, 2012; Normile, 2012; Stein and Stein, 2012; McNeill and McCurry, 2014). So far, it appeared that the existence of the walls set a false sense of security, leading people to stay in their house because they thought the wall would protect them (McNeill and McCurry, 2014). According to some reports, the presence of the walls even prompted people to rush toward them after the earthquake – thus toward the sea – sadly to be swept away by the tsunami (Onishi, 2011a). Noteworthy, Japan coast was dotted by hundreds of so-called tsunami stones, centuries old monuments indicating higher reaches of past inundations and carved with inscriptions telling people to seek higher grounds after a strong earthquake (Fackler, 2011). At the village of Aneyoshi the stone even stated to avoid building below it (Fackler, 2011; Pons, 2011). Except in few rural places like Aneyoshi, where everybody eventually survived, modern Japan people ignored those ancestral warnings likely because they were too confident in advanced technological protection (Fackler, 2011; Pons, 2011). We note that maintaining live memory is now clearly identified as a challenge for the future (Cyranoski, 2012; Shibata, 2012).

3. A philosophical perspective on the methodology of research

The preceding review of the geophysical–technical–societal context shows that it is crucial to correctly assess the seismic hazard and risk but also the methodological foundations on which this assessment is based. The science of earthquakes, seismology, attempts to explain and forecast these events with more or less accuracy, depending on the spatial

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[7] "Nuclear village" is the term used to describe the Japanese community of politicians, bureaucrats, engineers, business people and academics, involved in the development of the nuclear energy, and that developed a culture of being closed to outsiders, lacking mutual criticism and becoming overconfident about safety (see for example Nakamura and Kikuchi, 2011; Reb et al., 2012; Kingston, 2012).

and temporal scale considered. The major disaster in Fukushima, a new "Chernobyl" caused by the 2011 magnitude 9 earthquake, is often presented as a result of human error, from the initial choice of the location of nuclear sites to the management of the nuclear crisis by the authorities. At the regional level, the number of victims and the extent of damage due to the tsunami appear almost abnormal for the country which seemed a priori the world better prepared to cope with such disasters. It became customary to reduce the earthquake to its human consequences, both as environmental and social health, while it can be very enlightening to examine the physical causes. It is very informative, especially, to put into perspective the discourse of the international scientific community, anterior and posterior to the disaster, the competing hypotheses and their corresponding models.

Then it is a quite different landscape of the disaster that appears, in which the seismological science also bears some responsibility. Its failing to achieve correct explanations and predictions equals indeed disqualifying some of its far dominant models. The Tōhoku-oki earthquake and the subsequent Fukushima Daiichi nuclear accident raise some methodological problems that are rather classical in the philosophy of science. They can be suggestively presented by recalling the cornerstone opposition in philosophy between Popper and Kuhn on the dynamics of research.[8] It is then possible to give a methodological interpretation to this serious mistake by weighing the opposition between on the one hand a "*prescriptive*" logical–critical approach and on the other hand a "*descriptive*" pragmatic approach.

Karl Popper in his *Logic of Scientific Discovery* (Popper, 2002a; first published in 1934) provides a logical account of the dynamics of research through a critical method of trial, test and error. From a methodological point of view, this "logical negativism" based on falsification can be distinguished from a "logical positivism" based on verification. Popper states that no researcher can prove with certainty that one theory is true, but at least, he can prove with certainty that the theory is false — or at least provisionally non-false. The reason lies in the asymmetry between the true and the false, since the theory's statements must be true in all cases, while only one statement showing contradiction in one case is enough to make it false. If you say, for instance, that "There can be no mega-thrust earthquake in subduction zones", this theory can be shown false if you are able to exhibit only one exception to the rule, like "There has been one mega-thrust earthquake in one subduction zone". The logical criterion of science in Popper's is a negative one: it is not the possibility of confirmation, but the possibility of *refutation* of a theory (*refutability*) through a set of severe tests. A theory as a conjecture can be said "valid" or "corroborated" (but not "true" stricto sensu) once it has passed successfully (but maybe provisionally) the various observational or experimental tests aimed at showing that it is false. In this respect, the dynamics of research is basically that of a process of *conjectures and refutations* (Popper, 2002b; first published in 1963) in a context of radical uncertainty as to the truth of a theory. It is characterized by a collective stance of "*permanent critique*" that avoids the community members to rely satisfyingly on the current state of knowledge. This view essential to the stream of *Critical Rationalism* suggests that research is a demanding and daring task of intellectual subversion and is (or should be) always more or less in a state of "revolution" (Bouveresse, 1998). The evolutionary view on research and the rejection of certainty are correct, but this methodology faces some limits. First, the negative account based on refutation does not describe adequately the effective work of the researchers who do not seek only to prove things to be false. Second, it overestimates the rationality of research as grounded on the open and free discussion and the use of convincing arguments by the community members. Third, it underestimates the importance of the research framework that bounds the range of relevant and legitimate options to be examined.

Thomas Kuhn in *The Structure of Scientific Revolutions* (Kuhn, 2012; first published in 1962) provides a challenging pragmatic view to Popper's methodology by stressing also the "non-logical" (sociological, psychological) aspects of the research dynamics. This pragmatic approach underlines the importance of *paradigms* as research frameworks that structure the work of "puzzle-solving" achieved by the members of one research community. A *paradigm* (from the Greek *paradeigma*, example, model) is a theoretical, technical and practical framework that functions as a disciplinary matrix for the "normal science" — to be distinguished from the "revolutionary science".[9] In other words, the research led by a community of researchers in one academic speciality develops within a framework that indicates which puzzles and how the puzzles should be examined and eventually solved. In Kuhn's, the dynamics of research is a process of *normal science* in which the researchers dedicate their efforts to solve some ordinary puzzles. In practice, "normal" research activity builds a corpus of evidence while performing tests aiming to confirm the consensual ideas. But this process can evolve toward a state of *revolutionary science* if it happens indeed that some researchers identify a set of theoretical or empirical anomalies that questions the validity of the models. These anomalies are usually denied or rejected at the beginning by the majority of the community members who take dissonant phenomena to be some kind of meaningless abnormalities with no impact on the dominant model.[10] For instance, this is the case if, within a community of researchers believing that "There can be no mega-thrust earthquake in subduction zones", you pretend that "There has been one mega-thrust earthquake in one subduction zone", but this point is not interpreted as model-challenging. Nevertheless, the growing importance of those anomalies can lead to a *paradigm crisis* and eventually to a *paradigm shift* if it appears that the current framework sustainably fails to give satisfying explanations or interpretations of them. Then, the normal science turns into a period of revolutionary science in which the researchers dedicate their efforts to designing a new paradigm — that will be the framework for a new period of normal science…. This cycle of research provides seemingly a correct and relevant account of how researchers actually functions within a scientific community. But this view also faces some limits: first, the rationality of research is threatened by the nature of the changing factors which are less a matter of discussion or argumentation than a matter of sudden disclosure for the community members; second, the paradigms as conflicting "world versions" are not commensurable (the famous *Incommensurability Thesis*) and in that sense cannot be compared one another[11]; third, the weight of the community discipline can inflict a majority viewpoint or conformism that prevents some members from examining some deviant options and from developing an original creative research.

It seems that on a *descriptive* basis, the dynamics of research as conceived of by Kuhn is the most relevant to the case of Tōhoku-oki earthquake as it emphasizes the key-role of paradigms. It is now clear that the error made by the researchers (and not only by the engineers who built the nuclear plants) concerning the Tōhoku-oki earthquake had some fierce consequences on the shores of Fukushima. The dominant

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[8] These conflicting approaches remain topical for most contemporary methodological debates, even if one could also refer to some other parallel approaches located so to speak in between the two of them (for instance, Lakatos' *research programs* for Popper, or Hacking's *styles of reasoning* for Kuhn). However, one has to keep in mind some recent interpretations that temper the supposedly radical differences between Popper and Kuhn, especially on the rationality of criteria for choosing among two competing theories (see Lakatos and Musgrave, 1970; Fuller, 2004; Soler, 2007).

[9] In his "second thoughts" on paradigms, Kuhn suggested to re-define a paradigm, on the ground that its variety of uses could be confusing, as a "disciplinary matrix": "less confusion will result if I instead replace it with the phrase "disciplinary matrix"–"disciplinary" because it is the common possession of the practitioners of a professional discipline and "matrix" because it is composed of ordered elements of various sorts, each requiring further specification", in Kuhn, 1977.

[10] Alternative hypotheses of some of the members of the scientific community are held in the best case for some deviant fantasies and in the worst case for some malpractices.

[11] Even if Kuhn finally assumed that the incommensurability of paradigms is relative and that translation from one theory to another is closer to "learning of a new language" than to "living in another world": "If I were now rewriting *The Structure of Scientific Revolutions*, I would emphasize language change more and the normal/revolutionary distinction less", in "Commensurability, Comparability, Communicability", (Kuhn et al., 2000, p. 57).

paradigm entailed a series of errors, all tracing back to the following postulate: *it is impossible for giant thrust earthquakes to occur in some parts of the world including many subduction zones*, like that facing Fukushima. Many analytical data and sophisticated physical models came to support this dominant paradigm. On several occasions, scientists tried to propose an alternative design, including the possibility of giant thrust earthquakes in these prohibited areas. But this was seen as a deviant solution without any validity and legitimacy, and in that respect, the *inertia of the paradigm* shows the relevance of Kuhn's view.

Our hypothesis is that the mistake of the researchers can be illuminated by the paradigmatic focus on the analytical (or "*atomistic*") approach to the problem to the detriment of the synthetic (or "*holistic*") approach. Thus, the methodological tropism led to a kind of "*analytical drift*" of research polarized by the resolution of questions of detail. More precisely, this methodological bias concerned the local short-term record of relatively moderate earthquakes as a clue for the mechanics of the subduction mega-thrust and related hazards. This methodological tropism led to a subsequent denial of a more synthetic vision which, however, would have given a clearly different and more relevant meaning to the empirical data. The analysis of seismological articles from one or two decades shows the difficulty to formalize, or verbalize, questioning the established model, even in the presence of data showing clearly the opposite. The only difference from Kuhn's classical account as far as Tōhoku-oki earthquake is concerned lies in the nature of the consequences of explanations and predictions: a mere intellectual mistake in most cases, a large-scale human disaster in the case of Fukushima. The connection between one theoretical model and its practical consequences was already examined by Kuhn, but through the lens of applied science, not that of a human catastrophe. The catastrophe of Fukushima was sufficiently devastating to enforce the community of seismologists to achieve a paradigm shift, or at least to accept the crisis of the dominant model: this reveals (if need be) the power of paradigms in the framing of scientific thought.[12] One can reasonably wonder if, beyond the elementary rule of precaution, a philosophy of research more inspired by Popper's appeal to the bold open mind of the researchers as required by the "critical method" could not prevent this kind of catastrophe…. But this requires shifting from a descriptive to a *prescriptive* basis and as far as the dynamics of research is concerned, paying attention not only to the way research actually functions, but to the way it *should* function.

The role of the "catastrophic paradigm" in the Fukushima accident can be made more explicit by examining the methodological problem of *causality* that is at the core of the scientific mistake. This methodological problem of explanation can be examined by considering the difference between a *deductive-nomological model* based on laws and an *inductive-statistical model* based on cases (Hempel, 1962). In the formal reasoning, the explanation takes the form of an inductive or deductive argument, where the premises are called the "explanans" and the conclusion the "explanandum".[13]

In Hempel's "deductive-nomological" (D-N) model, the explanations make the occurrence of singular events intelligible by deriving their descriptions from premises that include at least one law:

$$\begin{array}{ll} \text{Premises :} & L_1, \quad L_2, \quad \ldots \quad L_k \\ \text{Conclusion :} & \underline{C_1, \quad C_2, \quad \ldots \quad C_r} \\ & \qquad\quad E \end{array}$$

where the explanans $C_1, C_2, \ldots, C_r$ describe specific conditions ("initial" or "antecedent") and $L_1, L_2, \ldots, L_k$ general laws, and the explanandum "E" describes the event to be explained.

In statistical explanations defined as "inductive-statistical" (I-S), a certain probability is attributed to the explanandum G (the event):

$$\begin{array}{c} P(G,F) = r \\ \underline{\quad F \quad} \\ G \end{array}$$

where the law $P(G,F) = r$ states that the probability of an event G, given the conditions F (i.e. a set of observations), is r.

The methodological interpretation of the Tōhoku–Fukushima case suggests that the dominant paradigm among the research community was based on a kind of reasoning that belongs to the inductive-statistical model (I-S). It happened indeed that the set of data collected from the subduction zones and used for hazard calculations gave no statistical support to the possibility – as expressed by a non-null probability (P) – of mega-thrust earthquakes in the area facing Fukushima (G). The problem is that the conditions (F) for the probability of an earthquake of this kind to occur were not correct, for the space–time coordinates and thus the dataset, were too limited. But as seen before, their too narrow scope was due to the analytical tropism of the seismological research that made it unthinkable, so to speak, to aggregate the data in order to get a broader (global rather than local) and deeper (longer term seismic record) insight on the overall seismic activity. Moreover, it might be that another shortcoming consisted in taking the statistical rule for granted, as a grounding premise shared by the majority of researchers in a common reasoning that was actually closer to the deductive-nomological model (D-N). Indeed, it appears that the paradigmatic model of a "segmented patchy subduction interface unable to break as a whole" justified the choice of a narrow set of data by a sort of retroaction. The interpretation thus suggests the following: (1) The methodological model of causality (probabilistic versus deterministic) was data-dependent; (2) but the selection of the data itself was *frame-dependent* (paradigmatic). To illustrate this, let's recall that the seismic hazard maps for Japan published by the Headquarters for Earthquake Research Promotion (HERP, 2015; Fujiwara et al., 2006) were calculated using a relatively standard probabilistic seismic hazard analysis (PSHA).[14] This analysis was based on a quite short-term earthquake catalog – thus *data-dependent* – and a patchy earthquake source model compatible with the consensual view of subduction earthquake mechanics — thus *frame-dependent*.

To be complete however, we must acknowledge the complexity of the physics of faulting, making earthquakes non-linear, largely unpredictable processes. It is now clear that megathrust earthquakes do not repeat in a strictly regular way on a given subduction fault. This means that concepts like those of characteristic earthquake or seismic gap should be used with caution or are perhaps even meaningless (e.g. Kagan et al., 2012; Geller et al., 2015). We may try to formalize

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[12] In that sense, the dominant seismological paradigm turned out to be a catastrophic one — all the more that, if one refers to the Greek etymology, "*catastrophe*" designates the "outcome" or the "conclusion" in a drama.

[13] One can recall briefly that an *induction* is a non-demonstrative inference from a set of particular cases to a general rule. For instance, for a set of particular cases: "I drop Stone 1, and it falls down"; "I drop Stone 2, and it falls down", and so on; hence the general rule: "If I drop a Stone X, it falls down". Conversely, a *deduction* is a demonstrative inference from a general rule to a set of particular cases, like in the following premise that functions as a basis for the reasoning: "If I drop any Stone X, it falls down". In terms of argument, if the author of the argument believes that the truth of the premises definitely establishes the truth of the conclusion, then the argument is *deductive*. If the author of the argument does not believe that the truth of the premises definitely establishes the truth of the conclusion (but believes that their truth provides good reason to believe the conclusion true), then the argument is *inductive*. However, one can mention as far as hypothetical-deductive reasoning is concerned that a general rule can be used as explanation of particular cases and that a set of particular cases can be used as a test for the general rule. Thus, the general rule can be examined in the light of some various experimental cases, such as: "If I drop a stone on Earth, it falls down", but "If I drop a stone inside a space shuttle out of the Earth, it does not fall down" (for an overview of the induction/deduction problem, see Rothchild (2006) and Lawson (2005)).

[14] Note that those seismic hazard maps does show the probability of occurrence of a given earthquake, but the probability of reaching a particular seismic intensity, or ground acceleration level, in the next 30 years, at a given site inland.

this observation as follows:

$$\frac{\begin{array}{c}L_{normal}\\C_1, C_2, \ldots, C_r\end{array}}{E_{normal}} \quad \text{or} \quad \frac{\begin{array}{c}L_{normal} + L_{catastrophic}\\C_1, C_2, \ldots, C_r\end{array}}{E_{catastrophic}}.$$

The normal case (E_{normal}) would explain the usual – but not so regular – repetition of moderately-large earthquakes (with average repeat times of several decades to century in Japan) and would fit quite well the established subduction earthquake paradigm. But, as these "normal" earthquakes do not relax all accumulated stresses, we hypothesize that the system needs very unfrequent catastrophic event ($E_{catastrophic}$) to return close to a zero state (potentially every thousand years or so in Japan). Such a mechanism may recall the concept of supercycles and superquakes proposed by some authors (Sieh et al., 2008; Goldfinger et al., 2013). Only the occurrence of $E_{catastrophic}$ (a rare "superquake", or "uncharacteristic" earthquake, Kagan et al., 2012) truly challenges the paradigm (it may be considered as a refutation in Popper's sense). An Inductive/Statistical model may safely explain the normal case provided that a representative set of data (not too narrow in time and space) is used, but will explain the catastrophic event only after its occurrence, thus a posteriori. Forecasting a priori the most catastrophic case bears deep uncertainties and is likely impossible with a statistical model (Stein and Stein, 2013). Yet, in a more deterministic way and with a paradigm shift, we may attempt to put more realistic bounds on the maximum credible earthquake (MCE). However, the deterministic problem remains imperfect and partly empirical because we still lack for a deep understanding of the physical laws (L_{normal} and more particularly $L_{catastrophic}$).

Finally, the interpretation of the methodological framework in the "Tōhoku-Fukushima" case raises the issue of the relationship between the scientific production of facts and models and the values and the norms of research. In the logical approach to research (typical of the philosophical stream of logical positivism supporting the "scientific world conception"), there is a clear divide inspired by the philosopher David Hume between the "*Is*" (description) and the "*Ought*" (prescription).[15] This means for the field of research that it is one thing to determine what research "*is*" – the way it really functions, but it is another thing to determine what research "*ought*" to be – the way it should ideally function. This basic "epistemic/ethical" divide implies in its radical version that science is based on facts, while morals (not science) as a separate domain is based on values and norms.[16] The problem with the separatist view comes from its blindness to the actual value/norm-dependence of science that Putnam as a critical heir to logical positivism referred to as "the collapse of the fact/value dichotomy" (Putnam, 2002). To some extent, the epistemic and the ethical merge if one takes for granted that research not only is based on facts but also is ruled by some values and some norms (see Dalibor, 2010). However, it is not clear whether all values and norms are of the same kind, and for some philosophers, indeed, there are actually several options as to the relationship between the epistemic and the ethical (Haack, 2001).[17]

One of the questions concerning the epistemic values and norms of science, as instances of the "virtues of the mind" (Zagzebski, 1996), is to identify what is a virtue and what is an obligation. For instance, are the boldness of conjectures and the openness of mind a value or a norm — or in other words, a virtue or an obligation? Is it legitimate to expect that the researchers are ruled by the obligation of being bold and open, almost in the same way as they are asked to be rigorous and honest? Can this be an obligation or should it remain a mere individual or collective virtue of the researchers that some do possess while some other do not? It seems legitimate to enforce researchers to be rigorous and honest, but it seems less easy to enforce them to be bold and open. In that sense, being bold and open for a researcher can be viewed as a value and a virtue; but if it is a norm that is called to regulate the functioning of research, the question remains open if it should be an obligation. The problem is that one really wonders what such a norm can actually mean if in no way it is linked to an obligation that requests some real people to adjust or modify their conduct. At least, it seems legitimate as a minimal request to ask them as a community not to ignore or not to prevent some of its members from being "bold" and "open" and to take them seriously when they support alternative or deviant options.[18] This applies to the non-evidence-based research (conjectures) and all the more to the evidence-based one (facts), for, as suggested in critical rationalism, the boldness of conjectures or the openness of minds must prevail at the prior stage of conjectures and not only at the posterior stage of tests.

Perhaps the notion of responsibility is likely to bridge the two sides of the epistemic and the ethical in a more satisfying way. The notion of epistemic responsibility tends to focus on the cognitive norms, obligations or duties that in some way warrant the justification of knowledge, i.e. the reasons why someone is justified in believing as true (or non-false) what he or she actually believes it is true (or non-false). However, beyond the stake of justification, it can also be conceived of, in respect to the usual meaning of the word "responsibility", as the ability to account for the quality (completeness, rightness, soundness or robustness) of the research work if asked to justify it by the rest of the community or the society. Then, the epistemic requirements seem to overlap with those involved by the notion of ethical responsibility insofar as they set up the conditions for a moral assessment in terms of imputation of the connections between knowledge, action and consequences. To some extent, even if the two notions do not merge completely, it seems that the epistemic responsibility of a research community covers a significant part of its ethical responsibility. The question is: what kinds of achievement or negligence researchers can be taken to be responsible for?

There are some examples of double-sided requests (epistemic and ethical), like in the Aquila's earthquake, that put in question the quality of the scientific expertise and of its communication to the public.[19] In that context, the rigor and honesty as well as the boldness and open-mindedness are not taken to be mere value-requirements but some genuine norm-requirements. In other words, the rule of open criticism functions as a basic norm of research while dogmatism in excess (and even skepticism in excess to some extent) appears as a transgression of this norm that the research community can be asked to account for. Of course, this epistemic/ethical responsibility of research, like for any form or any domain of responsibility, faces some limits: "Ought" implies "Can", the German philosopher Kant used to say, thus suggesting that one cannot require from someone that he or she achieve something that is impossible (like for example short term prediction of earthquakes). There is something new under the Sun., however: the

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[15] David Hume (1739, 2011), p.333.

[16] It also shed some light on what makes the difference between Kuhn and Popper, namely the hiatus between a descriptive and a more prescriptive approach to research.

[17] Susan Haack, for instance, suggests that there are at least five possibilities in which epistemic and ethical appraisal might be related: (1) epistemic appraisal is a subspecies of ethical appraisal (the special-case thesis); (2) positive/negative epistemic appraisal is distinct from, but invariably associated with, positive/negative ethical appraisal (correlation thesis); (3) there is, not invariable correlation, but partial overlap, where positive/negative epistemic appraisal is associated with positive/negative ethical appraisal (the overlap thesis); (4) ethical appraisal is inapplicable where epistemological appraisal is irrelevant (the independence thesis); (5) epistemic appraisal is distinct from, but analogous to, ethical appraisal (the analogy thesis).

[18] We are aware of research proposals submitted after the Sumatra 2004 earthquake. The aim was to dive at the trench and search for sea-bottom ruptures. Rejection was based on circular arguments such as: it is useless to search for surface ruptures because it is well known that seismic ruptures never reach the front of the accretionary prism. Assumptions definitively disproved by the 2011 Japan earthquake.

[19] In the case of l'Aquila earthquake, in Italy, the light-weight attitude of scientific experts and politicians resulted in the communication of incorrect scientific statements to the public (Jordan, 2013) and the eventual dramatic misunderstanding of the risks by local population (see Hall, 2011; Jordan, 2013; Yeo, 2014 for example).

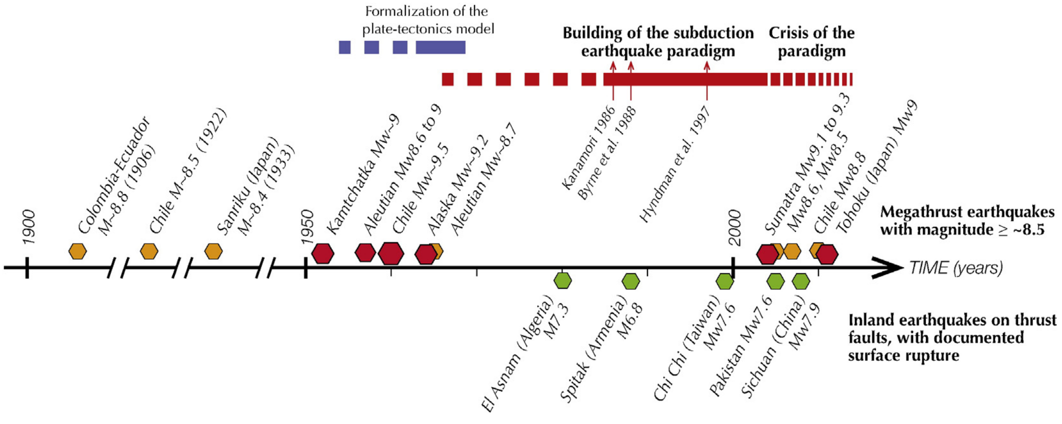

Fig. 4. Timeline of main subduction earthquakes since beginning of the 20th century. Megathrust earthquakes appear to have been clustered in time. Note that the 1950–60 earthquake cluster occurred before or during formalization of the unifying plate tectonic model. The subduction earthquake paradigm (see text) grew during the ~40 year. period that follows this mid-century cluster and which lacks Mw $\geq$ 8.5 megathrust earthquakes. However, the observation since 1970 that inland thrust earthquakes do frequently reach the surface, as well as the occurrence of the giant seismic rupture offshore Sumatra in 2004, should have been taken as possible refutations of the paradigm. But its crisis became tangible only after the 2011 NE Japan disaster.

research community is not the only one to determine what those limits of responsibility are, or should be, for the society is also concerned with the quality of its work. In this regard, if the co-production of knowledge (experts/citizens) is already a quite well-identified social trend, the possibility of a “methodological co-design” that would take into account the stake of epistemic and ethical responsibility is possibly one of the major issues for the philosophy of research in the future.

4. Discussion and conclusion

Our geophysical review and methodological analysis help to pinpoint interwoven causes to the scientific and technical failure that eventually lead to faulty implementation of coastal protection measures and to the Fukushima Daiichi nuclear disaster. Even though we may identify a full chain of responsibilities involving technical and political stakeholders, we may also point to a mistake on the scientific side. Indeed, scientists were unable to forecast a priori mega-earthquakes — and sometimes much more able to explain them a posteriori. Yet, many signals indicated that an earthquake of this magnitude offshore northeast Japan was possible, although the time of occurrence of such event was impossible to predict precisely. The origin of this scientific mistake with serious human consequences lies in several factors that challenge a dominant paradigm of seismology. Thus, the scientific community was long gained in its majority in a paradigm, in Thomas Kuhn's sense. Hazard evaluation in Japan was chiefly based on the concept of repetition of characteristic earthquakes, defined only from the short-term earthquake record; but what happened in 2011 was definitively an “uncharacteristic” earthquake (see Kagan et al., 2012). The Great East Japan Earthquake - as it is often referred in Japan - indeed crystallized the crisis of that paradigm: it's now “shake-up time for Japanese seismology” says R. Geller in *Nature* (Geller, 2011) partly using the title of an older *Nature*'s paper about characteristic earthquakes and prediction issues (Geller, 1991). We summarize on Fig. 4, the main events that lead to the crisis. In line with some other authors (e.g. McCaffrey, 2008), we favor the interpretation that most of the Mw > 8.5 events that occurred years and decades before the Tōhoku earthquake could actually be taken as refutations of the established model. However the paradigm crisis became clear only after the Tōhoku-Fukushima disaster and the need for a paradigm shift is now well identified (see Avouac, 2011; Stein et al., 2012; Geller et al., 2015; Hubbard et al., 2015, among many others).

The methodological confrontation between the logical-critical interpretation and the pragmatic interpretation can emphasize the importance of certain standards and conflict of standards in the dynamics of research. In this respect, it is clear that the *imagination*, *boldness* and *openness* as well as the *ability to doubt* and *consider uncertainties* appear for research as a set of core values (if not norms) that may function as both epistemic and ethical standards and be viewed as so essential as *rigor* and *precision*. Our interpretation is that blindness rather than openness was predominant in the analytical tropism (an Inductive/Statistical approach, too narrow in time and space) that led to biased hazard evaluation in Japan. We can postulate the same tropism to be inherent to nearly all probabilistic seismic hazard analyses (PSHA), which are generally based on limited datasets and unable to evaluate properly the maximum credible event (MCE).[20] This implies that the worst-case scenario (Sunstein, 2009) is in general not taken into account by such analyses, as it happened for NE Japan.[21]

When forecasting rare extreme natural events with potential catastrophic effects – and more specifically for extremely sensible industrial plants – a deterministic MCE evaluation may set up a much safer framework than PSHA, provided it is done with openness, considering all options and not only the laws compatible with the most consensual paradigm. This urges scientists and engineers to be aware not only of the *consensus* but also of the *dissensus* within their research community. And to promote real democratic and open debate and choices, they have the responsibility to communicate and properly explain all

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[20] Refer for example to Klügel (2008) for a critical review of methods used in seismic hazard analysis, to Castaños and Lomnitz (2002), Stein and Stein (2013) and Stein and Friedrich (2014) for a discussion on epistemic limitations of such methods.

[21] We must acknowledge here that the Tōhoku case was worse than what occurs elsewhere in Japan because the discrepancy between the characteristic earthquakes used for hazard evaluation and what really occurred was extremely large. Indeed, in central Japan closer to former imperial capitals, a better known seismic history makes the risk of several Mw 8+ along the Nankai trough (corresponding to ruptures of the Nankai, Tokai, Tonankai megathrust segments) clearly identified since a long time (e.g., Ando, 1975). A complete rupture of the three segments altogether was even envisaged, based on what likely occurred in 1707. The paradigm shift required after the Tōhoku disaster not only suggests that this catastrophic case is fully credible but that it perhaps represents only a minimum for the maximum credible earthquake (MCE) on the Nankai trough.

[22] Following Stein and Friedrich (2014), “*we should try to better assess hazards, recognizing and understanding the uncertainties involved, and communicate these uncertainties to the public and planners formulating mitigation policies.*” Similarly, as a conclusion of an opinion paper in Seismological Research Letter, Geller et al. (2013) point up that: “*In discussing natural hazards it is important to tell the public not only what we know, but also what we do not know, and how uncertain our knowledge is.*” They insist that “*It's time to change the terms of the debate from the oversimplified* “*safe/unsafe*” *dichotomy to an honest and open discussion of what the risks are and what is being done to mitigate them* […] *At the end of the discussion, the public and the leaders they have elected, rather than technical experts, should make the final call.*”

uncertainties and unknowns to the technical and political sphere as well as to the rest of the Society.[22] That could be one methodological lesson drawn by the research community from a paradigm crisis as an outcome of the Tōhoku-Fukushima catastrophe.

Acknowledgments

Authors acknowledge CNRS and IPGP (R.L.), ICAM Paris-Sénart and EHESS (S.L.) for funding their positions and providing research facilities. We benefited from fruitful discussions with many people, among which we wish to thank more particularly Rolando Armijo and Maud Devès. We thank Robert Geller for his useful suggestions on an earlier version of the paper, and two anonymous reviewers for their constructive comments. This is IPGP contribution 3711.

References

Ando, M., 1975. Source mechanisms and tectonic significance of historical earthquakes along the Nankai Trough, Japan. Tectonophysics 27 (2), 119–140.

Annaka, T., Satake, K., Sakakiyama, T., Yanagisawa, K., Shuto, N., 2007. Logic-tree approach for probabilistic tsunami hazard analysis and its applications to the Japanese coasts. Pure Appl. Geophys. 164 (2–3), 577–592.

Aoki, M., Rothwell, G., 2013. A comparative institutional analysis of the Fukushima nuclear disaster: lessons and policy implications. Energ Policy 53, 240–247.

Avouac, J.P., 2011. Earthquakes: the lessons of Tōhoku-Oki. Nature 475 (7356), 300.

Bouveresse, R., 1998. Popper ou le rationalisme critique. Vrin, Paris.

Byrne, D.E., Davis, D.M., Sykes, L.R., 1988. Loci and maximum size of thrust earthquakes and the mechanics of the shallow region of subduction zones. Tectonics 7 (4), 833–857.

Callon, M., Lascoumes, P., Barthe, Y., 2009. Acting in an Uncertain World. An Essay on Technical Democracy. MIT Press.

Castaños, H., Lomnitz, C., 2002. PSHA: is it science? Eng. Geol. 66 (3), 315–317.

Cyranoski, D., 2012. After the deluge, Japan is rebuilding coastal cities to protect people from the biggest tsunamis. Nature 483, 141–143.

Dalibor, R., 2010. The debate on epistemic and ethical normativity. Disputation Philosophica 12 (1).

David, Hume, 1739, 2011. A Treatise of Human Nature. John Noon.

Dynes, R.R., 2000. The dialogue between Voltaire and Rousseau on the Lisbon earthquake: the emergence of a social science view. Int. J. of Mass Emergencies and Disasters 18, 97–115.

Fackler, M. (2011) Tsunami warnings, written in stone. New York Times, April 20, 2011.

Fujiwara, H., Morikawa, N., 2012. Seismic Hazard Assessment for Japan after the 2011 Tōhoku-Oki Mega-Thrust Earthquake (Mw9.0). Proceedings of the International Symposium on Engineering Lessons Learned from the 2011 Great East Japan Earthquake, pp. 406–417.

Fujiwara, H., Kawai, S., Aoi, S., Morikawa, N., Senna, S., Kobayashi, K., Ishii, T., Okumura, T., Hayakawa, Y., 2006. National seismic hazard maps of Japan. Bull. Earthq. Res. Inst., Univ. Tokyo 81, 221–232.

Fuller, S., 2004. Popper vs Kuhn. The Struggle for the Soul of Science. Columbia University Press.

Funabashi, H., 2012. Why the Fukushima nuclear disaster is a man-made calamity. Int. J. Jpn. Sociol. 21 (1), 65–75.

Funabashi, Y., Kitazawa, K., 2012. Fukushima in review: a complex disaster, a disastrous response. Bull. At. Sci. 68 (2), 9–21.

Geller, R.J., 1991. Shake-up for earthquake prediction. Nature 352 (6333), 275–276.

Geller, R.J., 2011. Shake-up time for Japanese seismology. Nature 472 (7344), 407–409.

Geller, R.J., Epstein, W., Nöggerath, J., 2013. Fukushima — two years later. Seismol. Res. Lett. 84, 1–3.

Geller, R.J., Mulargia, F., Stark, P.B., 2015. Why we need a new paradigm of earthquake occurence. In: Morra, G., Yuen, D.A., King, S.D., Lee, S.-M., Stein, S. (Eds.), Subduction Dynamics: From Mantle Flow to Mega Disasters. John Wiley & Sons, Hoboken, NJ. http://dx.doi.org/10.1002/9781118888865.ch10.

Goldfinger, C., Ikeda, Y., Yeats, R.S., And, Ren J., 2013. Superquakes and supercycles. Seismol. Res. Lett. 84, 24–32.

Goto, K., et al., 2011. New insights of tsunami hazard from the 2011 Tōhoku-oki event. Mar. Geol. 290 (1), 46–50.

Haack, S., 2001. 'The ethics of belief' reconsidered. In: Steup, M. (Ed.), Knowledge, Truth, and Duty. Oxford University Press.

Hall, S.H., 2011. At fault? Nature 477, 264–269.

Hasegawa, K., 2012. Facing nuclear risks: lessons from the Fukushima nuclear disaster. Int. J. Jpn. Sociol. 21 (1), 84–91.

Hashimoto, C., Noda, A., Sagiya, T., Matsu'ura, M., 2009. Interplate seismogenic zones along the Kuril–Japan trench inferred from GPS data inversion. Nat. Geosci. 2 (2), 141–144. http://dx.doi.org/10.1038/ngeo421.

Hashimoto, C., Noda, A., Matsu'ura, M., 2012. The Mw 9.0 northeast Japan earthquake: total rupture of a basement asperity. Geophys. J. Int. 189 (1), 1–5.

Hempel, C.G., 1962. Deductive-nomological vs. statistical explanation. In: Feigl, H., Maxwell, G. (Eds.), Minnesota Studies in the Philosophy of Science vol. 3. University of Minnesota Press, Minneapolis.

HERP (2015). National seismic hazard maps for Japan (2015), Headquarters for earthquake research promotion, Report. http://www.jishin.go.jp/main/chousa/06mar_yosoku-e/NationalSeismicHazardMaps.pdf.

Hindmarsh, R., 2013. Nuclear disaster at Fukushima Daiichi, introducing the terrain. Nuclear Disaster at Fukushima Daiichi: Social, Political and Environmental Issues. Routledge, pp. 1–21.

Hubbard, J., Barbot, S., Hill, E.M., Tapponnier, P., 2015. Coseismic slip on shallow décollement megathrusts: implications for seismic and tsunami hazard. Earth Sci. Rev. 141, 45–55. http://dx.doi.org/10.1016/j.earscirev.2014.11.003.

Hyndman, R.D., Yamano, M., Oleskevich, D.A., 1997. The seismogenic zone of subduction thrust faults. Island Arc 6 (3), 244–260.

IRSN (2012). Fukushima, un an après. Premières analyses de l'accident et de ses conséquences. Rapport IRSN/DG/2012-001, March 12, 2012.

Jordan, T., 2013. Lessons of L'Aquila for operational earthquake forecasting. Seismol. Res. Lett. 84, 4–7.

Kagan, Y.Y., Jackson, D.D., Geller, R.J., 2012. Characteristic earthquake model, 1884–2011, RIP. Seismol. Res. Lett. 83 (6), 951–953.

Kanamori, H., 1986. Rupture process of suduction-zone earthquakes. Annu. Rev. Earth Planet. Sci. 14, 293.

Kanamori, H., 2012. Earthquake hazards: Putting seismic research to most effective use. Nature 483 (7388), 147–148.

Kanamori, H., Miyazawa, M., Mori, J., 2006. Investigation of the earthquake sequence off Miyagi prefecture with historical seismograms. Earth Planets and Space 58 (12), 1533.

Kerr, R.A., 2011. Seismic crystal ball proving mostly cloudy around the world. Science 332 (6032), 912–913.

Kingston, J., 2012. Japan's nuclear village. The Asia-Pacific Journal 10 (37), 1 (September 10, 2012).

Klügel, J.U., 2008. Seismic hazard analysis — quo vadis? Earth Sci. Rev. 88, 1–32. http://dx.doi.org/10.1016/j.earscirev.2008.01.003.

Kodaira, S., No, T., Nakamura, Y., Fujiwara, T., Kaiho, Y., Miura, S., ... Taira, A., 2012. Coseismic fault rupture at the trench axis during the 2011 Tōhoku-oki earthquake. Nat. Geosci. 5 (9), 646–650.

Krolicki, K., DiSavino, S. & Fuse, T. (2011). Special report: Japan engineers knew tsunami could overrun plant. Reuters, March 29, 2011.

Kuhn, T.S., 1977. Second thoughts on paradigms. In: Suppe, F. (Ed.), The Essential Tension. University of Chicago Press, pp. 293–319.

Kuhn, T.S., Conant, J., Haugeland, J., 2000. The road since structure: philosophical essays, 1970–1993, with an autobiographical interview. University of Chicago Press.

Kuhn, T.S., 2012. The Structure of Scientific Revolutions. fourth ed. University of Chicago Press.

Lakatos, I., Musgrave, A., 1970. Criticism and Growth of Knowledge. Cambridge University Press, Cambridge.

Lawson, A., 2005. What is the role of induction and deduction in scientific reasoning and inquiry? J. Res. Sci. Teach. 42 (6), 716–740.

Lay, T., 2012. Seismology: why giant earthquakes keep catching us out. Nature 483 (7388), 149–150.

Lay, T., Ammon, C.J., Kanamori, H., Xue, L., Kim, M.J., 2011. Possible large near-trench slip during the 2011 M (w) 9.0 off the Pacific coast of Tōhoku Earthquake. Earth, Planets and Space 63 (7), 687–692.

Lay, T., Kanamori, H., et al., 2012. Depth-varying rupture properties of subduction zone megathrust faults. Journal of Geophysical Research: Solid Earth (1978–2012) 117 (B4).

Linstone, H.A., Turoff, M., 1975. The Delphi Method: Techniques and Applications. Addison-Wesley Publishing (640 pp.).

Loveless, J.P., Meade, B.J., 2010. Geodetic imaging of plate motions, slip rates, and partitioning of deformation in Japan. Journal of Geophysical Research: Solid Earth (1978–2012) 115 (B2). http://dx.doi.org/10.1029/2008JB006248.

Mazzotti, S., Le Pichon, X., Henry, P., Miyazaki, S.I., 2000. Full interseismic locking of the Nankai and Japan-west Kurile subduction zones: an analysis of uniform elastic strain accumulation in Japan constrained by permanent GPS. Journal of Geophysical Research: Solid Earth (1978–2012) 105 (B6), 13159–13177.

McCaffrey, R., 2008. Global frequency of magnitude 9 earthquakes. Geology 36 (3), 263–266.

McNeill, D., McCurry, J., 2014. After the deluge: tsunami and the great wall of Japan. The Asia-Pacific Journal 12 (30), 1 (July 28, 2014).

Minoura, K., Imamura, F., Sugawara, D., Kono, Y., Iwashita, T., 2001. The 869 Jōgan tsunami deposit and recurrence interval of large-scale tsunami on the Pacific coast of northeast Japan. Journal of Natural Disaster Science 23 (2), 83–88.

Minoura, K., Hirano, S.I., Yamada, T., 2013. Identification and possible recurrence of an oversized tsunami on the Pacific coast of northern Japan. Nat. Hazards 68 (2), 631–643.

Mori, N., Takahashi, T., Yasuda, T., Yanagisawa, H., 2011. Survey of 2011 Tohoku earthquake tsunami inundation and run-up. Geophys. Res. Lett. 38 (7).

Nakamura, A., Kikuchi, M., 2011. What we know, and what we have not yet learned: triple disasters and the Fukushima nuclear fiasco in Japan. Public Adm. Rev. 71 (6), 893–899.

Nishimura, T., Hirasawa, T., Miyazaki, S.I., Sagiya, T., Tada, T., Miura, S., Tanaka, K., 2004. Temporal change of interplate coupling in northeastern Japan during 1995–2002 estimated from continuous GPS observations. Geophys. J. Int. 157 (2), 901–916.

Nöggerath, J., Geller, R.J., Gusiakov, V.K., 2011. Fukushima: the myth of safety, the reality of geoscience. Bull. At. Sci. 67 (5), 37–46.

Normile, D., 2011. Japan disaster. Scientific consensus on great quake came too late. Science 332 (1 April 2011), 22–23.

Normile, D., 2012. One year after the devastation, Tohoku designs its renewal. Science 9, 1164–1166.

Oliver-Smith, A., 1994. Peru's five hundred-year earthquake: vulnerability in historical context. Disasters, Development and Environment. J. Wiley and Sons, pp. 31–48.

Oliver-Smith, A., 1996. Anthropological research on hazards and disasters. Annu. Rev. Anthropol. 25, 303–328.

Onishi, N. (2011a). In Japan, seawall offered a false sense of security, New York Times, March 31, 2011.

Onishi, N. (2011b) In nuclear crisis, crippling mistrust, New York Times, June 12, 2011.

Onishi, N. (2011c) 'Safety myth' left japan ripe for nuclear crisis, New York Times, June 24, 2011.

Ozawa, S., Nishimura, T., Suito, H., Kobayashi, T., Tobita, M., Imakiire, T., 2011. Coseismic and postseismic slip of the 2011 magnitude-9 Tōhoku-Oki earthquake. Nature 475 (7356), 373–376.

Pons, P. (2011). Tsunami: les ancêtres savaient. Le Monde, May 7, 2011.

Popper, K., 2002a. The Logic of Scientific Discoveries. Routledge.

Popper, K., 2002b. Conjectures and Refutations. Routledge.

Putnam, H., 2002. The Collapse of the Fact/Value Dichotomy. Harvard University Press.

Reb, J., Iinuma, Y., Joshi, H., 2012. The Fukushima nuclear disaster: causes, consequences and implications. Case Collection. Singapore Management University (12 pp., available from http://ink.library.smu.edu.sg/cases_coll_all/28).

Revet, S., 2012. La rupture de l'événement. Une anthropologie des catastrophes. Bulletin Amades 84 (2011) (online 1 November 2012. http://amades.revues.org/1307).

Rothchild, I., 2006. Induction, Deduction and the Scientific Method. Society for the Study of Reproduction, Madison, Wisconsin (World Wide Web (URL: http://www.ssr.org/Induction.html)).

Rousseau, J-J. (1756). Lettre à Monsieur de Voltaire, le 18 août. http://fr.wikisource.org/wiki/Lettre_à_Voltaire_sur_la_Providence.

Sagiya, T., Kanamori, H., Yagi, Y., Yamada, M., Mori, J., 2011. Rebuilding seismology. Nature 473 (7346), 146–148.

Satake, K., Namegaya, Y., Yamaki, S., 2008. Numerical simulation of the AD 869 Jōgan tsunami in Ishinomaki and Sendai plains. Ann. Rep. Active Fault Paleoearthquake Res. 8, 71–89 (in Japanese).

Satake, K., Fujii, Y., Harada, T., Namegaya, Y., 2013. Time and space distribution of coseismic slip of the 2011 Tōhoku earthquake as inferred from tsunami waveform data. Bull. Seismol. Soc. Am. 103 (2B), 1473–1492.

Sawai, Y., Shishikura, M., Okamura, Y., Takada, K., Matsu'ura, T., Aung, T.T., ... Sato, N., 2007. A study on paleotsunami using handy geoslicer in Sendai Plain (Sendai, Natori, Iwanuma, Watari, and Yamamoto), Miyagi, Japan. Ann. Rep. Active Fault Paleoearthq. Res. 7, 47–80 (in Japanese).

Shibata, A., 2012. Importance of the inherited memories of great tsunami disasters in natural disaster reduction. Proceedings of the International Symposium on Engineering Lessons Learned from the 2011 Great East Japan Earthquake, March 1–4, 2012, Tokyo, Japan.

Sieh, K., Natawidjaja, D.H., Meltzner, A.J., Shen, C.-C., Cheng, H., Li, K.-S., Suwargadi, B.W., Galetzka, J., Philibosian, B., Edwards, R.L., 2008. Earthquake supercycles inferred from sea-level changes recorded in the corals of West Sumatra. Science 322 (5908), 1674–1678.

Simons, M., Minson, S.E., Sladen, A., Ortega, F., Jiang, J., Owen, S.E., ... Webb, F.H., 2011. The 2011 magnitude 9.0 Tōhoku-Oki earthquake: mosaicking the megathrust from seconds to centuries. Science 332 (6036), 1421–1425.

Soler, L., 2007. Popper et Kuhn sur les choix inter-théoriques. Philosophia Scientae 11 (1), 99–130.

Stein, S., Friedrich, A.M., 2014. How much can we clear the crystal ball? Astron. Geophys. 55 (2), 2–11.

Stein, S., Okal, E.A., 2011. The size of the 2011 Tōhoku earthquake need not have been a surprise. EOS Trans. Am. Geophys. Union 92 (27), 227–228.

Stein, J.L., Stein, S., 2012. Rebuilding Tohoku: a joint geophysical and economic framework for hazard mitigation. GSA Today 22 (10).

Stein, S., Stein, J.L., 2013. Shallow versus deep uncertainties in natural hazard assessments. EOS Trans. Am. Geophys. Union 94 (14), 133–134.

Stein, S., Geller, R.J., Liu, M., 2012. Why earthquake hazard maps often fail and what to do about it. Tectonophysics 562, 1–25.

Sugawara, D., Goto, K., Imamura, F., Matsumoto, H., Minoura, K., 2012a. Assessing the magnitude of the 869 Jogan tsunami using sedimentary deposits: Prediction and consequence of the 2011 Tohoku-oki tsunami. Sediment. Geol. 282, 14–26.

Sugawara, D., Imamura, F., Goto, K., Matsumoto, H., Minoura, K., 2012b. The 2011 Tōhoku-oki earthquake tsunami: similarities and differences to the 869 Jōgan tsunami on the Sendai plain. Pure Appl. Geophys. 1–13.

Sunstein, C.R., 2009. Worst-case Scenarios. Harvard University Press.

Suwa, Y., Miura, S., Hasegawa, A., Sato, T., Tachibana, K., 2006. Interplate coupling beneath NE Japan inferred from three-dimensional displacement field. Journal of Geophysical Research: Solid Earth (1978–2012) 111 (B4). http://dx.doi.org/10.1029/2004JB003203.

Tajima, F., Mori, J., Kennett, B.L., 2013. A review of the 2011 Tohoku-Oki earthquake (Mw 9.0): large-scale rupture across heterogeneous plate coupling. Tectonophysics 586, 15–34.

The National Diet of Japan, 2012. The Official Report of the Fukushima Nuclear Accident Independent Investigation Commission, Executive Summary. National Diet of Japan (85 pp.).

Tierney, K.J., 2007. From the margins to the mainstream? Disaster research at the crossroads. Annu. Rev. Sociol. 33, 503–525.

Vigny, C., et al., 2011. The 2010 Mw 8.8 Maule megathrust earthquake of Central Chile, monitored by GPS. Science 332 (6036), 1417–1421.

Voltaire (1756). Poème sur le désastre de Lisbonne, et sur la loi naturelle. http://gallica.bnf.fr/ark:/12148/bpt6k5727289v.

Yagi, Y., Fukahata, Y., 2011. Rupture process of the 2011 Tohoku-Oki earthquake and absolute elastic strain release (Mw 9.1). Geophys. Res. Lett. 39, L19307.

Yamada, T. (2012). Onagawa nuke plant saved from tsunami by one man's strength, determination. The Mainichi, initially published by http://mainichi.jp/english/ on Mar. 19, 2012. Retrieved from http://list.uvm.edu/cgi-bin/wa?A2=ind1204&L=SCIENCE-FOR-THE-PEOPLE&F&S&P=385.

Yamanaka, Y., Kikuchi, M., 2004. Asperity map along the subduction zone in northeastern Japan inferred from regional seismic data. J. Geophys. Res. 109, B07307. http://dx.doi.org/10.1029/2003JB002683.

Yanagisawa, K., Imamura, F., Sakakiyama, T., Annaka, T., Takeda, T., Shuto, N., 2007. Tsunami assessment for risk management at nuclear power facilities in Japan. Pure Appl. Geophys. 164 (2–3), 565–576.

Yeo, M., 2014. Fault lines at the interface of science and policy: Interpretative responses to the trial of scientists in L'Aquila. Earth Sci. Rev. 139, 406–419. http://dx.doi.org/10.1016/j.earscirev.2014.10.001.

Yue, H., Lay, T., 2011. Inversion of high-rate (1 sps) GPS data for rupture process of the 11 March 2011 Tohoku earthquake (Mw 9.1). Geophys. Res. Lett. 38, L03302.

Zagzebski, L.T., 1996. Virtues of the Mind. Cambridge University Press.