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FROM THE EDITOR

DIWA tectonics

I attended the 14th International Association on the Genesis of Ore Deposits (IAGOD) Symposium held in Kunming, South China, from 19 to 22 August 2014. This was a rare opportunity for me to gauge the state of Chinese geological science, and to establish personal ties and develop joint projects with Chinese scientists. My attendance was financially supported by the Chinese Academy of Sciences through the Guangzhou Institute of Geochemistry.

The Symposium was attended by about 1,000 scientists from all over the world. The vast majority were young Chinese scientists. The session I attended as one of the conveners was on “Tectono-magmatic activity and associated metallogeny”, which was the largest of the 19 sessions at the Symposium. A list of the papers presented either orally or in poster format can be found in the News section of this issue. The session was organized by the Diwa tectonics group run by students and followers of the late Guoda CHEN, father of Diwa geotectonics and metallogeny, and one of the most prominent geologists in China. This conference rekindled my interest in Chinese geology and tectonics, and helped rebuild personal contacts and make new friends as well.

Although general information was briefly introduced in *NCGT Newsletter* many years ago by Zhou (1999 and 2000), Diwa (*meaning geodepression in Chinese*) tectonics is relatively little known outside China and Russia. Tectono-magmatic activization or DIWA, originally proposed by Guoda CHEN in 1956 and 1959, represents the third tectonic element of continental crust, besides geosynclines and platforms – a new type of post-platform mobile region. It is characterized by significant magmatism, tectonic geomorphology, and complex structural layers, which differ from those in both geosynclines and platforms. The reactivated tectonics reveals tectonic transformation from stable to mobile regions. The Diwa region (or platform activation region) is an extraordinarily important tectono-metallogenic region, because it contains multiple and distinctive mineralization including many world-class polymetallic ore deposits.

Chen’s 1988 book states that Diwa tectonics considers the coacervated thermal energy generated by mantle creep flow to be the engine of tectonic movement. On the basis of an exhaustive study of earthquakes and their space-time distribution in relation to tectonic belts in China, he discussed transmigrating seismic energy along tectonic belts. Later, in his monumental 1996 book in Chinese, he referred to continental rocks in the ocean floors and adopted the basic tenets of surge tectonics. In this context, Diwa tectonics can be related to surge tectonics, the former with emphasis on continental tectonics and metallogeny.

I am glad I could associate with the Diwa group. They allowed my non-plate-tectonics paper on the Archean Geanticline to be delivered as a keynote paper at the start of their session. They gave me an additional opportunity to deliver a lecture on “structural culminations and energy flow” for graduate students and scientists from the Institute of Geochemistry in Guangzhou. Mineral exploration requires creative thinking with high-level geological knowledge, training and acumen. It must be guided by the right tectonic model too. Although many young geologists have uncritically adopted plate tectonics, as I found at the Symposium, China still has a glimmer of hope to put geology back on the right track.

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Photo taken at the office of Prof. Deru XU of Guangzhou Institute of Geochemistry. Left to right: Deru XU, Dong CHOI and Ge LIN. Inset, Guoda CHEN, 1912–2004.

LETTERS TO THE EDITOR

The 1977 Vladimir V. Beloussov letter to Victor E. Khain

Dear Editor,

The letter below is from the archives of Vladimir V. Beloussov (1907–1990) on the dramatic development of plate tectonics in Russia. As is known, the prominent Russian geologist Beloussov did not accept this concept from the very beginning. Until the middle of 1970s he remained rather alone among the remarkable Russian researchers who readily admitted the new concept. Of these was Victor Efimovich Khain (1914–2009) a prominent scientist for over half of a century and also the one to bring the new ideas from foreign investigation into our scientific literature. His article, "Whether the scientific revolution takes place in Geology?", published in 1970 in the Journal *Priroda* (Nature) was in essence a first in our country where the fundamental principles of plate tectonics were distinctly presented. This was a response to the discussion between Beloussov and Tuzo J. Wilson in the pages of *Geotimes*, 1968, which article was mostly inaccessible to the wide geological community in USSR.

The prominence of Khain, his fascination with plate tectonics, and this first discussion on the topic promoted the spreading of new ideas on this concept in USSR. Fierce disputes between Khain and Beloussov took place for many years after, on diverse occasions. It should be added – both men were leading scholars in the country, both lectured at Moscow State University, and for many years both were tied with common investigations.

It seems the content of Beloussov letter has not lost its glitter. The significance of continental geology for understanding the global Geodynamic is not exaggerated. All the mentioned crucial questions for plate tectonics are still unsolved; at best some ad hoc theoretical provisions were invented. This letter has never been publicized before.

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V.V. Beloussov (1907–1990)

V.E. Khain (1914–2009)

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Dear Victor Efimovich,

I was curious to read your article and express myself on the subject according to your request. I believed I would find something new and it would be interesting to discuss. However as I read it, my expectation vanished: I have seen the lines repeating once again: what I've read and heard about and what I repeatedly have spoken about, many times. At this point I decided that I have just nothing more to say since you, as it is well known, know everything that I think about all this.

My mood changed dramatically at the end of the article when I came across your reasoning. Patting me on the back, you patronizingly promised to provide a cubbyhole for some of my ideas (those which you will find “the most valuable”) in that stately edifice of “a more generalized theory of global tectogenesis” which you and your adherents would erect on the “plate tectonics” ground. Just these phrases gave me the right to speak about your article with more clearness than I expected to do.

Having behind us such remarkable scientists as Karpinsky, Pavlov, Archangelsky, Tetyaev, Shatsky, we, as the tectonists, were proud to belong to the school they founded and developed. This school has grown on the basis of the huge amount and diversity of factual data which our country provides for Geology. There is no other country in the world which would give such abundant material for the study of different types of continental structures, the comparison and investigation of their history and the regularity of their evolution. If we add the historical approach, the holistic principle, the ability to distinguish processes of different scales and the thorough familiarity with world literature, then it is quite natural that our school takes the leading position in the continental tectonics. The least we could be accused of is lack of independence, or what is commonly referred as the feeble imitation.

However the new school of thought has appeared with us and it is until now a helpless imitation. We line upon the back of the western colleagues and try to keep pace with them. Of course, if all of the past must be forgotten because it was a mistake, it is firstly necessary to listen to other. But this childish period of obedience somewhat drags on. Much can be said about our geophysicists, adherents of plate-tectonics, lagging behind the western colleagues and repeating in simplified form what was made in West long ago and now is already out of date.

Well, let them alone, our geophysicists. Geologists, with our national experience, could also say something new in the development of this “plate-tectonics”. But why are all the statements of our geologists, following these ideas, so frustratingly popular? There is kind of standard *a la brochure*, “Knowledge is power” (popular scientific Journal in USSR – L.I.) which is common not only for journal “Technics – to youth” but also for articles of member-correspondents of Ac. Science of USSR Khain. Do you not get apprehensive that personalities who pick up this popular tome will start managing our science when they are completely incognizant in Geology?

You could say that I confuse cheap popularity with simplicity and that verity is allegedly always simple. However, simple verity was never superficial nor light-minded. It does not tolerate superficiality.

And how do you substantiate your “simple verity”?

Speaking about the sea floor age, you passed over in silence the finding of the ancient rock in the Mid-Atlantic ridge. Do you consider such facts are not essential? Then prove their irrelevance! That would be a real contribution to “plate-tectonics”! But we have already got used to the veil of silence...

Your “deadly” argument against fixism – “the absence of the least signs of the assimilation of the continental crust” – sounds naive because the signs of such assimilation must inevitably be hidden in the deep interior where we do not see much. Nevertheless, on the microcontinent margins and sometimes on the margins of the big continents, the sedimentary cover is complicated by dikes, sill intrusions and basic nappes. As to indirect data on basification, there is any amount, especially in the Mediterranean and marginal seas. How will you deal with Mediterranean Sea? Maybe with Dewey’s construction?

But let’s have an open mind. Until we have reached the depths with apparent signs of the assimilation of the continental crust, you may claim that there is no such sign. Let this be a point in your favor at the moment. But how is it possible to get rid of such phenomena as equality of mean heat flow in no more than several lines of contemptuous remarks? Perhaps something was “underestimated”, or a harmonic analysis is to be blame… Well, as you yourself have not tried to understand this great problem, do you have the right to treat it more independently?

With the same ease, you settle the matter with another phenomenon, that of much larger importance. I mean the deep roots of superficial endogenous regimes. You have to admit the available facts indicating the connection of superficial structures with depths exceeding the usual depths of asthenosphere. There is a great deal of supporting data, far more than mentioned by you. But enough of these, because these facts force you to make an admission of great consequence, probably decisive in the whole problem. Though you evidently do not feel this decisive point, supposing that it is possible to assume sliding “plates” on the diverse depths at different times and at different places. Did not you really want to test yourself, at least by drawing on paper, a scheme of this kinematics at different depths? There is a probability that all of the concept may come crashing down at very this point...

You have no doubt that matter’s balance is provided by equality of spreading and subduction (if neglecting, as sometimes assumed by you, pulsation of the Earth globe volume). Let’s anyhow see what we are to overcome for this idea of spreading-subduction would be reasonable in some way. The Atlantic Ocean spreading requires the subduction somewhere in the North-East of the USSR or in the Alaska. By Heaven, you would do for “plate tectonics” a far greater service if, instead of repeating what was often said, you would show where and how this subduction occurs. But, mind you, without distortion of the real geology. We have already seen enough distorting of the real geology because of the light hand of “plate tectonics”, and this is a nuisance.

One should also consider the mechanism of subduction, especially relating to the observed distribution of the heat flow inside and outside the island arcs. It is necessary to explain what is under the Aleutian arc as well: in what way the rift was sunk there and how, being at the mantle depth, it can strangely deliver the new crust towards the general motion of the Pacific plate. Do you not think that clarification of these mysterious phenomena would be more useful than repetition of the basics?

It would be also possible to investigate the migration of the spreading axes together with plate motion. What has happened first: the ascent of the heated mantle material and then – as consequence – spreading? Or, firstly, spreading while ascending material flow can be always found? Instead of depending on Sorochtin’s opinion it would be more useful to examine critically the real cross-sections of the deep trench slopes and do not hastily employ an interpretation which is fine for your pet model.

You show the superficiality regarding the problem of ophiolite as well. Protecting your point of view is clearly outdated. There is mounting evidence that eugeosynclines were formed on the continental crust and that ultrabasites are the inseparable part of the initial magmatism (as it was postulated before). It is impossible to dismiss these data. Especially as geochemical results surely suggest geochemical diversity of the oceanic and continental rocks. This is a big problem and there is yet much unclear but it is dangerous to solve it in such straightforward way. The ophiolites of Mongolia, Southern Siberia and South-Eastern Asia may be hardly considered as unquestioning relics of the oceanic crust.

It is not so simple with stability of the trends of the diastrophism zones which you somehow reduce to a fault trend within cratons. In fact the question is about the channels of increased permeability, penetrating very deep, into depths beyond upper mantle!

I could give a lot of further examples of the extraordinary light attitude of the author to extremely complicated and profound problems. You glide over the surface grabbing at the first opportunity giving the appearance of explanation, avoiding to look deeper. It is no wonder that your article is full of such expressions as “evidently”, “obvious”, “logically thinking”, “it appears that”, “probably”, “natural to assume”, etc. Such expressions would not be necessary, if adherents of “plate tectonics” did not pass “the accursed questions”, by hiding behind a false appearance of “simple verity”. It would be preferable if they would deal with these issues more closely.

It should be remembered that science develops only where there are inconsistencies and where the researcher seeks not light success but real verity by first of all searching out not that what confirms his idea but what contradicts to it. Only overcoming contradictions he strengthens his idea.

We do not know many things not because we cannot know but because we do not want to. Why does nobody study thoroughly the condition of outcropping ancient rock in the Mid-Atlantic ridge? Why turn away from comparative geochemistry of the continental and oceanic rocks? Why the question about subduction compensating the Atlantic Ocean opening is ignored? Why all the oddities within the Aleutian arc are slurred over? It is because these questions are very dangerous for the whole concept of plate tectonics. Meanwhile, you and others like you create the impression of complete well-being for the general public by your hasty and superficial insight into these difficult and decisive problems.

Doctrine which does not tolerate contradictions and avoids inconsistencies inevitably falls outside the scope of science and becomes a dogma. Such a doctrine cannot be cognizable; it is possible only to believe in it. The “plate tectonics” is now an incognizable scientific dogma. That is why it is significant that deep mechanisms providing the whimsical kinematics of plate motion has not yet been understood successfully, despite all the efforts of the leading scientists. Everything is limited by the most uncertain reference to some multistage system of the convection currents, as in your article. Nothing remains but to trust that these systems may exist and this highlights the dogmatic character of your teaching.

Now about the key point. It is not about where the “most valuable” ideas of Beloussov may be occasionally evacuated but where in your edifice is all the scientific wealth of our national geotectonic school, accumulated over many decades, to be accommodated? After some effort I have envisaged this place. All our former science will be driven into so-called intra plate tectonics. Indeed, all our platforms with their complicated tectonic life, all activated zones, almost all folding belts, will turn out to be intra plate ones – at least from the position of “plate tectonics”. For it is now known that intra plate tectonics is, for “plate tectonics”, little more than some illegal and, besides, an ugly baby, a sin beguiled. And this baby, unfortunately, there is and you have to mess with him.

Well, how the intra tectonics could be otherwise interpreted? Must we recall the geosynclinal theory, tectonic-magmatic cycles, diverse types of folding, or specific geophysical data on the deep structure of the different tectonic zones or something of the general trend of the Earth’s crust evolution? Of course not! Again our wise teachers have invented for this case the much more superb reason – “the mantle jet”! - though these jets, never being appeared, turn out to be somewhat stained. It has happened that “his own received him not”. “Jets” were categorically rejected by one of the pillars of modern mobilism, Runcorn. All the same they are somehow closer to us than all the past! And if to boost India further under the Eurasiatic “plate”, not reflecting on the resulting monstrous physical, mechanical, geophysical and historical absurdities, then tectonics of our continent gains a perfect orderliness!

There is yet a lot of uncertainty about the deep interior of our planet, and the processes occurring there. Much of them seem to be contradictory and mysterious. But we have still studied these processes as naturalists striving to recognize the governing rules and their inner causal mechanism. Now we are assured that we must do it in another way, that relationship of the processes, historical succession and causes, are not as important as the developing of kinematical models. That is certainly an interesting approach and in some special cases it can be used. However there are more claims on us: the betrayal of our own history... We are to forget everything we had learnt from our abundant nature...

It is very sad and highly surprising that such people like you, Victor Efimovich, who made so much for real science, join the ranks of this strange crusade which carries us from living science to a dead dogma.

In conclusion I have a request to you. If you had mentioned my name critically, I would have no right to make such a request. But in context of your article my name appears to be irrelevant. This gives me the right to urge you to remove my name entirely. I do not want it to be figured in this article.
Yours,

Beloussov
October 15, 1977

Sir,

Non-biological hydrocarbons

In the last issue of this journal (v. 2, no 2), Johann-Christian Pratsch refers to one of my recent Letters to the Editor (see v. 2, no. 1) and says he is both surprised and even somewhat disappointed by reading the following passage: "*Both origin and migration of oil is very far from being understood, and these commodities are often found in crystalline basement rocks where they, according to prevailing theory, should not exist*". I further mentioned that, based on present evidence, there is ample reason for believing that the bulk of Earth's hydrocarbons is abiotic in origin – produced under mantle conditions. The possibility that oil and natural gas might have formed deep in the Earth has indeed been debated for more than a century. The abiotic school of thought has traditionally had its stronghold within countries of the former Soviet Union; already in the late 19th Century the renowned Russian chemist Dmitri Mendeleev concluded that crude oil and the overwhelming amount of natural gas are generated by hydration of iron carbides upwelling from the deep Earth. Today, the inorganic idea of hydrocarbons is frequently being referred to as the Russian-Ukrainian theory – which apparently is the dominating petroleum theory in these countries. In this context, it is important to point out that in the solar system methane is a major component of the atmospheres of the outer planets. On Saturn's moon Titan large areal depressions filled with liquid methane and ethane have recently been discovered, and much of the carbon material of carboniferous meteorites consists of hydrocarbons – in both solid and liquid forms (e. g. Stofan et al. 2007; Metri et al. 2007; Studier 1965; Gelphy and Oro 1970). These hydrocarbons cannot have biological origins.

Regarding the Earth, methane effusion through crystalline rocks is a worldwide phenomenon. An indication of the importance of methane flux from the mantle is given by the world-wide distribution of sedate mud volcanoes and associated gas hydrates – found either along fault-controlled deep continental margins or within major tectonic belts (see Milkov 2000; Hovland et al. 1997). The emitted gases permeating crystalline basement are usually flammable, comprising alkane hydrocarbons, hydrogen sulphide and hydrogen (for a summary, see Gold 1999). It has been argued that the enrichment of the chemically inert helium with hydrocarbons may serve as a good proxy for a mantle origin of fluid hydrocarbons – before they started their upward journey to near-surface reservoirs – besides serving as food for microbial life at levels of the upper crust.

Supporting the non-organic theory, a team of US researchers (Scott et al. 2004) demonstrated that methane readily forms by the reaction of carbonaceous rock material with iron-rich minerals and water under conditions typical of Earth's upper mantle. According to these authors, their "*study demonstrates the existence of abiogenic pathways for the formation of hydrocarbons in the Earth's interior and suggests that the hydrocarbon budget of the bulk Earth may be larger than conventionally assumed*." One of the researchers, Dudley Herschbach – the 1986 Nobel laureate in chemistry – stated that "*Some of the Earth's reservoir of oil may refill as they are pumped out*" (see O'Donnell 2005). In addition, recent high-pressure chemical experiments (Kolesnikov et al. 2009) suggested that 1) methane is stable under upper mantle conditions and 2) hydrocarbons heavier than methane can be produced by abiogenic processes in the upper mantle. As deep continental drilling (Kola and South Germany) has revealed that fracture spacing increases with depth, it seems likely that mantle hydrocarbons are slowly streaming up into near-surface reservoir structures. This may account for the fact that many major oil fields tend to have a much longer production history than initially estimated; some of them may even produce forever. For example, the Saudi oil fields apparently have no apparent proximal 'source rocks', and the production is experiencing little drawdown from virgin pressures despite decades of high capacity withdrawals (Mahfoud and Beck 1995).

In direct contrast to the abiotic theory, Johann-Christian Pratsch (NCGT, v.2, no.2) declare that the Earth's oils and gases are exclusively the result of organo-chemical processes. He states that: "*All occurrences, without a single exception, of oil and gas are intimately related to the existence of near-by generating basins, deep enough through their individual geological history to thermally mature hydrocarbons from original biological (animal or floral/planktonic) components*". He finds "*it rather surprising, and even somewhat disappointing*" that I argue against his favoured fossil, 'pressure-cooking' explanation. However, I am highly surprised at the stalled opinions he shows, disregarding the multitude of basic petroleum-related problems that await their solution; he must have skipped reading all published evidence pointing against his 'received opinion'. For example, petroleum geologist H. Wilson (2005) – reviewing a number of stumbling blocks surrounding conventional ideas on the origin, generation, migration and accumulation of hydrocarbons – states: *"The expulsion of oil from deeply buried source rock has never been satisfactorily explained and according to geochemists remains an enigma. The failure to decisively explain deep primary migration withdraws an essential foundation stone from the catagenic* [life-based] *edifice*".

Wilson refers to much field evidence contradicting the fossil-fuel hypothesis, arguing that ignorance of crucial facts have led to major misdirection in petroleum geological thinking. Too many aspects have simply been assumed rather than scientifically confirmed. Though the mass opinion in the West holds that hydrocarbons are formed from presumed sedimentary 'petroliferous beds', such as shale and carbonates, pinpointing their actual source beds have frequently remained an open question (e.g. Mahfoud and Beck 1995; Mahfoud 2000).

In Norway, professor of petroleum geology Per Arne Björkum, at the University of Stavanger, gives a very realistic description of the situation when he writes (Björkum 1998): "*Our understanding of how the oil moves in the subsurface is not satisfactory. One often discovers oil in unexpected places – by accident. When oil is found on a serendipitous basis – in unforeseen places and in crystalline rocks, one would think that it would be a priority task searching for more of this type of hydrocarbon deposits. However, this is normally not the case. If the prevailing theory suggests that there should be no oil there, one will explain away unexpected reservoirs by classifying them as something special, caused by unlikely circumstances which probably will not happen again. In other words, the experience is not taken seriously* […]. *The exceptions remain exceptions until a better theory can account for the unexpected discoveries* [without resorting to a multiplicity of ad hoc mechanisms].

The many surprises mean that one is in arrears in relation to nature – due to the absence of an inadequate theory on the part of nature with which one is concerned. Hence, one must resort to ad hoc approaches; in other words, one is practicing a kind of scientific 'hybrid methodology'. When one fails, it is not clear why [...].The fact that companies make money, does not necessarily mean that they are doing a good job. Use of the steam engine in the 1700s was profitable, although they were so poorly constructed that they only had an efficiency of approximately 1%. But as all steam engines were equally ineffective, they were still useful. Therefore, the fact that the oil industry makes money is objectively not a sign that they are doing a good job [due to lack of a satisfactory theory]. *Rather, it is a sign that the vast majority of oil companies thinks and operates in the same way."* [my translation].

During the last 15 years, I have given several talks and seminars (including some full day presentations) to oil companies – suggesting that the bulk of the world's petroleum resources have their origin in the mantle. For example, during a full day seminar at the Statoil Research Centre, Trondheim, in November 2003 – invited by a study group of 'unconventional resources', the main issue raised by team was why so many major oil fields were associated with masses of halite (in the form of salt stocks), and water. My answer to this question was that since crude oil, natural gas, water and salt were so closely connected, they were likely to be products that ultimately stem from Earth's internal degassing. I dare to say, that this conclusion was somewhat surprisingly received by the audience, but nevertheless the discussion was unusually relaxed and open-minded. In an email of thanks from the leader of the group, received a few days later, he admitted that in the aftermath of my seminar he had begun to rethink the question of Earth's oil and gas potential.

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Dear Editor,

I would like to thank you and your fellow editors for working hard to keep a flame of hope burning for those of us who feel suffocated by the orthodoxies of modern "science." I look forward to reading your journal because it is a source of ideas supported by evidence which I simply cannot find in other journals, and it is the breadth of material and ideas that I, and I am sure others, find so stimulating. I live in hope that one day many of the ideas which were first published in NCGT will become accepted as worthy of discussion and proper argument by a wider group. I hope too that you are in good health.
With kindest regards,

Stephen Foster
hero5.premiere@blueyonder.co.uk
8 August, 2014

****

Re: South Pacific-Siberia Geanticline (SPSG), *NCGT Journal*, v. 1, no. 3, p. 45-55.
(Extracted from a reviewer's comment, 22 September, 2013)

Is it possible to extend the Yunnan surge channel (YSC) further north into Mongolia? How does the YSC and the SPSG relate to the massive Siberian Traps? I strongly expect these surge channels to operate in pulses, is that possible? …If you draw a line from the western flank of the Ordos Basin you pass through Oyu Tolgoi, world class Cu-Au deposit… Implications: other potential world-class ore deposits along the length of the SPSG (!). The potential is clearly there to obtain funding from the natural resources industry to research new exciting concepts with clear economical implications.

The East Asia Reflective Axial Belt coming through from China, passing through the Dalanzadgad – Oyu Tolgoi region (depending on the width of the SPSG in this area). This is very interesting as in this area the easternmost extend of the Altai Mountains terminates. It is also the location of the world class Tavan Tolgoi coking coal and Bayan Obo REE deposits. Furthermore, this area is also characterized by a number of Permian intrusives as well as Tertiary extrusives, clear evidence of prolonged magmatic activity. Picture below shows well exposed Tertiary flood basalt just east of Dalanzadgad in the South Gobi Basin - ?related to the SPSG magmatic activity.

You are onto something Dong – please follow through with further ground breaking research. Many "scientists" spend their lives regurgitating others work, very few like you think outside the box. Upwards and Onwards

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ARTICLES

9/56 YEAR CYCLE: EARTHQUAKES IN THE PACIFIC RIM OF SOUTH AMERICA

"God does not play dice." Albert Einstein

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Abstract: 54/56 year grids were established for major earthquakes in Japan, Kamchatka and Alaska. They were also applicable for world mega quakes (M ≥ 8.6) since 1900. Given such findings, large seismic episodes in the Pacific Rim of South America were also hypothesised to fall preferentially in 54/56 year grids. This was confirmed after assessing all major quakes (M ≥ 7.8) in Chile, Peru, Ecuador and Colombia over the past three centuries. Other grids based on 56 years and multiples of 9 years yielded significance. Why 54/56 year patterns were so crucial in seismic activity around the 'Ring of Fire' was unknown. Lunisolar cycles were involved but that is all that can be stated with any degree of certainty.

Keywords: *9/56 year, 54/56 year, earthquake cycles, Chile, Peru, ring of fire*

Introduction

The 9/56 year grids were first established in the occurrence of financial panics in the USA and Western Europe (McMinn, 1993) and then extrapolated to seismic events (McMinn, 2011a; McMinn, 2011b). The importance of 54/56 year grids in the timing of world mega quakes was first established by McMinn (2011b). Major earthquakes in Japan, Kamchatka and Alaska were found to exhibit a similar 54/56 year effect (McMinn, 2014). Given such findings, these grids were also found to be relevant in the occurrence of large seismic events in Chile - Peru - Ecuador - Colombia over the past three centuries. Some commonalities were established in earthquake activity around the Ring of Fire, which were believed to arise from Moon Sun tidal harmonics as proposed by McMinn (2011a). The findings supported the non-random hypothesis for the timing of major earthquakes, contradicting the prevailing paradigm in seismology.

The 9/56 year cycle consists of a grid with intervals of 56 years on the vertical (called sequences) and multiples of 9 years on the horizontal (called subcycles). The 56 year sequences have been numbered in accordance with McMinn (1993), with 1817, 1873, 1929, 1985 being designated as Sequence 01; 1818, 1874, 1930, 1986 as Sequence 02 and so forth. McMinn (Appendix 2, 2002) presented the full numbering. In the accompanying tables, the dates have been expressed as YYYYMMDD and the year of best fit was taken as the year ending November 30. The database of the National Geophysical Data Center (NGDC) was accessed to compile a list of major earthquakes (M ≥ 7.8) taking place in Chile, Peru, Ecuador and Colombia since 1700 (see **Appendix 1**).

9/56 Year Cycle

The major earthquakes in western South America were plotted on the complete 9/56 year grid as presented in **Appendix 2**. This was divided into four equal sections – Grids A, B, C and D. Major earthquakes were most likely to fall in in Grid A. Of the 58 episodes listed in **Appendix 1**, 29 appeared in this grid compared with an expected 14.5 (significant $p < 10^{-4}$). These earthquakes were least likely to happen in Grid D which only experienced five events (significant $p < .01$). The latter correlate could be boosted by expanding Grid D to encompass a total of 19 56 year sequences (see **Table 1**). The enlarged table comprised 34% of the complete 9/56 year grid, yet it only contained 13% of the 58 earthquakes in **Appendix 1** (significant $p < .001$).

Table 1
9/56 YEAR CYCLE: THE MINIMUM CONCENTRATION OF QUAKES IN CHILE – PERU – ECUADOR - COLOMBIA Post 1700 M ≥ 7.8
Year ending November 30

Sq 31	**Sq 40**	**Sq 49**	**Sq 02**	**Sq 11**	**Sq 20**	**Sq 29**	**Sq 38**	**Sq 47**	**Sq 56**
			1706	1715	1724	1733	1742	**1751** 0525	1760
	1744	1753	1762	1771	1780	1789	1798	1807	1816
1791	1800	1809	1818	1827	1836	1845	1854	1863	1872
1847	1856	1865	1874	1883	1892	**1901** 0107	1910	1919 **1918** 1204	1928
1903	1912	1921	1930	**1939** 0125	1948	1957	**1966** 1017	1975	1984
1959	1968	1977	1986	**1995** 0730	2004	2013			
2015									
Sq 09	**Sq 18**	**Sq 27**	**Sq 36**	**Sq 45**	**Sq 54**	**Sq 07**	**Sq 16**	**Sq 25**	
					1702	1711	1720	1729	
		1731	1740	1749	1758	1767	1776	1785	
1769	1778	1787	1796	1805	1814	1823	1832	1841	
1825	1834	1843	1852	1861	1870	1879	1888	1897	
1881	1890	1899	1908	1917	1926	1935	1944	1953	
1937	**1946** 0802	1955	1964	1973	1982	1991	2000	2009	
1993	2002	2011							

There were no mega quakes M ≥ 8.6 in this layout. Events with M ≥ 7.8 and ≤ 8.5 were denoted in **blue**.
Source of Raw Data: National Geophysical Data Center.

Grid A **Appendix 2** can be reduced to give another pattern with intervals of 18 years on the horizontal (see **Table 2**). The 18/56 year arrangement represented only 13% of the complete 9/56 year grid, but it accounted for 33% of all major quakes (M ≥ 7.8) (significant $p < 10^{-5}$) and five of the 7 mega quakes (M ≥ 8.6).

Table 2
18/56 YEAR CYCLE:
CHILE – PERU – ECUADOR = COLOMBIA EARTHQUAKES
Post 1700 M ≥ 7.8
Year ending November 30

Sq 34	**Sq 52**	**Sq 14**	**Sq 32**	**Sq 50**	**Sq 12**	**Sq 30**
					1716 Feb06 **1716** Feb11	1734
	1700	1718	1736	1754	1772	1790
1738 **1737** Dec04	1756	1774	1792	1810	**1828** Mar30	**1846** Jun28
1794	1812	1830	1848	1866	1884	1902
1850	**1868** Aug13 **1868** Aug14 **1868** Aug15	1886	**1904** Jan20	**1922** Nov11	**1940** May24	1958
1906 Jan31 **1906** Aug17 **1906** Sep28	1924	**1942** May14 **1942** Aug24	**1960** Jan13 **1960** May22	1978	1996	**2014** Apr01
1962	1980	1998	2016			
2018						

The 56 year sequences are separated by an interval of 18 years on the horizontal.
Mega quakes M ≥ 8.6 highlighted in **red**. Large events with M ≥ 7.8 and ≤ 8.5 denoted in **blue**.
Source of Raw Data: National Geophysical Data Center.

54/56 Year Cycle

Major earthquakes (M ≥ 7.8) in western South America also tended to fall in a 54/56 year grid as shown in **Table 3** (significant p < .001). This arrangement aligned closely with Grid B **Appendix 3** for world mega quakes, as both patterns shared Sequences 34, 32, 30, 28 and 26. Another 54/56 year layout for world mega quakes was presented as Grid A **Appendix 3**, but it did not have an equivalent for western South America.

Table 3
54/56 YEAR CYCLE: CHILE – PERU – ECUADOR – COLOMBIA
EARTHQUAKES Post 1700 M ≥ 7.8
Year ending November 30

Sq 34	Sq 32	Sq 30	Sq 28	Sq 26	Sq 24	Sq 22
						1726
					1728	1782
				1730 Jul08	**1784** May14	1838
			1732	1786	1840	1894
		1734	1788	1842	1896	**1950** May16 **1949** Dec17 **1949** Dec17
	1736	1790	1844	1898	1952	2006
1738 **1737** Dec04	1792	**1846** Jun28	**1900** Sep18	1954	2008	
1794	1848	1902	1956	**2010** Feb27		
1850	**1904** Jan20	1958	2012			
1906 Jan31 **1906** Aug17 **1906** Sep28	**1960** Jan13 **1960** May22	**2014** Apr01				
1962	2016					
2018						

The 56 year sequences are separated by an interval of 54 years on the horizontal.
Mega quakes M ≥ 8.6 highlighted in **red**. Large events with M ≥ 7.8 and ≤ 8.5 denoted in **blue**.
Source of Raw Data: National Geophysical Data Center.

Another correlate could be realised based on a grid with 108 year intervals on the horizontal (see **Appendix 4)**. The 108/56 year configuration represented 14% of the complete 9/56 year grid, but it experienced 29% of all seismic events (M ≥ 7.8) (significant p < .01).

Discussion and Conclusions

54/56 year seismic grids have been established in various regions around the Pacific Basin.

* Chile - Peru - Ecuador - Colombia
* Japan - Kamchatka (McMinn, 2014).
* Alaska (McMinn, 2014).
* World Mega quakes (McMinn, 2011b).

Grids A and B for world mega quakes (see **Appendix 3**) overlapped with the 54/56 year patterns for western South America, Alaska and Japan - Kamchatka. The same sequences tended to show up in the various 54/56 year grids (see **Table 4**). There was an overall theme in the timing of these events, which contradicts the prevailing paradigm. Professor Shearer of UCSD believed that "*There is no plausible physical mechanism that would link a large earthquake in Chile with one in Japan, so it's most likely that these* [mega quake clusters] *are truly random events*" (Salleh, 2011). An alternative view is that these episodes were triggered by Moon Sun tidal harmonics (McMinn, 2011a) and were thus linked by time. This hypothesis can explain the commonalities in the 54/56 year grids around the Pacific in **Table 4**, whereas the random view cannot.

Table 4 SHARED 56 YEAR SEQUENCES IN THE 54/56 YEAR SEISMIC GRIDS			
	Grid A Sequence Numbers	**Grid B Sequence Numbers**	**Source**
World Mega Quakes	29, 27, 25, 23, 21	36, 34, 32, 30, 28, 26	**Appendix 3** this paper McMinn (2011b)
Japan - Kamchatka	29, 27, 25, 23, 21	na	Table 1, McMinn (2014)
Alaska	29 27	36, 34, 32, 30, 28, 26	Table 2, McMinn (2014)
Wn South America	na	34, 32, 30, 28, 26	**Table 3** this paper
World mega quakes had M ≥ 8.6, whereas all the other regions had quakes with M ≥ 7.8. **Sources**: World Mega Quakes. McMinn (2011b). Earthquakes in Japan – Kamchatka and Alaska. McMinn (2014).			

Sequence 12 was the most interesting series with five large Peruvian quakes (M ≥ 8.2) happening every 112 years (see **Table 5**).

Table 5 SEQUENCE 12 & PERUVIAN EARTHQUAKES		
Sq 12	**M**	**Location**
Nov 22, 1604	8.5	Peru: Arequipa Chile: Arica
+ 112		
Feb 6, 1716 Feb 11 1716	8.8 8.6	Peru: Pueblo De Torata In Tacna Peru
+ 112		
Mar 30, 1828	8.3	Peru: Lima, Callao
+ 112		
May 24, 1940	8.2	Peru
+ 112		
2052	??	

54/56 year grids could not be produced for historic large quakes in Indonesia (M ≥ 7.8), based on a listing sourced from NGDC. The outcome was anomalous when compared with other regions around the Ring of Fire.

The 54/56 year cycle was first established for financial panics (see **Appendix 5**) and then extrapolated to earthquakes (McMinn, 2011b and 2014). Why these particular grids were so important in the occurrence of large Pacific Basin earthquakes was unknown. Lunisolar cycles hold the key to unravelling the mystery (McMinn, 2011a), but it is impossible to explain how this effect actually functioned. Much more research is essential to decipher meaningful patterns in the apparent 'randomness' of seismic activity.

Acknowledgements
The author would like to thank the editor Dong Choi and the reviewers for their input in the publishing of this paper. As always, their efforts were very much appreciated.

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Appendix 1
MAJOR EARTHQUAKES IN CHILE – PERU - ECUADOR – COLOMBIA
Post 1700. M ≥ 7.8
National Geophysical Data Center

YYYY	MM	DD	Location	M
1730	7	8	CHILE: VALPARAISO	8.7
1737	12	24	CHILE: VALDIVIA, CHILOE	8.0
1751	5	25	CHILE: CONCEPCION, CHILLAN, TALCA, TUTUBEN	8.5
1819	4	3	CHILE: COPIAPO	8.0
1819	4	4	CHILE: COPIAPO	8.0
1819	4	12	CHILE: COPIAPO	8.5
1822	11	20	CHILE: VALPARAISO, QUILLOTA, CONCON	8.5
1831	10	9	CHILE: PERU: TACNA, ARICA, AREQUIPA	7.8
1835	2	20	CHILE: CONCEPCION	8.2
1837	11	7	CHILE: VALDIVIA	8.5
1868	8	13	CHILE: ARICA	8.5
1868	8	14	CHILE: ARICA	8.5
1877	5	10	CHILE: OFF NORTH COAST	8.3
1878	1	23	CHILE: TARAPACA	7.9
1906	8	17	CHILE: SOUTH CENTRAL	8.2
1918	5	20	CHILE: NORTHERN	7.9
1918	12	4	CHILE: COPIAPO	7.8
1922	11	11	CHILE: ATACAMA	8.7
1939	1	25	CHILE: CHILLAN	8.3
1943	4	6	CHILE: ILLAPEL	8.2
1946	8	2	CHILE: NORTHERN	7.9
1949	12	17	CHILE: TIERRA DEL FUEGO	7.8
1949	12	17	CHILE: TIERRA DEL FUEGO	7.8
1950	12	9	CHILE-ARGENTINA	8.0
1960	5	22	CHILE: PUERTO MONTT, VALDIVIA	9.5
1966	12	28	CHILE: TALTAL, CATALINA	7.8
1985	3	3	CHILE: CENTRAL COAST, SAN ANTONIO	8.0
1995	7	30	CHILE: ANTOFAGASTA, CALAMA, MEJILLONES	8.0
2010	2	27	CHILE: MAULE, CONCEPCION, TALCAHUANO	8.8
2014	4	1	CHILE: IQUIQUE	8.2
1716	2	6	PERU: PUEBLO DE TORATA IN TACNA	8.8
1716	2	11	PERU	8.6
1746	10	29	PERU: LIMA, CALLAO	8.0
1784	5	13	PERU: AREQUIPA, CAMANA, MOQUEGUA, TAMBO	8.0
1828	3	30	PERU: LIMA, CALLAO	8.3
1846	6	28	PERU	7.8
1906	12	26	PERU: OFF SOUTH COAST	7.9
1908	12	12	PERU: OFF COAST	8.2
1913	8	6	PERU: CARAVELI, CHUQUIBAMBA, OCONA	7.9
1940	5	24	PERU	8.2
1942	8	24	PERU: ICA, NAZCA	8.2
1950	5	16	PERU	7.9
1951	5	16	PERU	7.9
1960	1	13	PERU: AREQUIPA, CHUQUIBAMBA, CARAVELI	7.8
1966	10	17	PERU: LIMA, HUACHO, HUAURA, CHANCAY, SUPE	8.1
1970	5	31	PERU: NORTHERN, PISCO, CHICLAYO	7.9
1974	10	3	PERU: LIMA, CALLAO	8.1
2001	6	23	PERU: AREQUIPA, MOQUEGUA, TACNA	8.4
2007	8	15	PERU: ICA, PISCO, LIMA	8.0
1797	2	4	ECUADOR: RIOBAMBA, QUITO, CUZCO	8.3
1868	8	15	ECUADOR: EL ANGEL, CONCEPCION	8.0
1901	1	7	ECUADOR: ESMERALDAS	7.8
1906	1	31	ECUADOR: OFF COAST	8.6
1906	9	28	ECUADOR	7.9
1942	5	14	ECUADOR: GUAYAQUIL	7.9
1826	6	18	COLOMBIA: ENGATIVA, BOGOTA, RAMIRIQUI	8.2
1900	9	18	COLOMBIA	7.9
1904	1	20	PANAMA-COLOMBIA	7.9

Mega quakes M ≥ 8.6 highlighted in **red**.
Source: National Geophysical Data Center. Parameters: Earthquakes M ≥ 7.8.

Appendix 2
COMPLETE 9/56 YEAR SEISMIC CYCLE:
CHILE – PERU – ECUADOR - COLOMBIA Post 1700 M ≥ 7.8
Year ending November 30

Grid A													
Sq 34	**Sq 43**	**Sq 52**	**Sq 05**	**Sq 14**	**Sq 23**	**Sq 32**	**Sq 41**	**Sq 50**	**Sq 03**	**Sq 12**	**Sq 21**	**Sq 30**	**Sq 39**
									1707	**1716** 0206 **1716** 0211	1725	1734	1743
		1700	1709	1718	1727	1736	1745	1754	1763	1772	1781	1790	1799
1738 **1737** 1204	1747	1756	1765	1774	1783	1792	1801	1810	**1819** 04031 **819** 04031 **819** 0411	**1828** 0330	**1837** 1107	**1846** 0628	1855
1794	1803	1812	1821	1830	1839	1848	1857	1866	1875	1884	1893	1902	1911
1850	1859	**1868** 0813 **1868** 0814 **1868** 0815	**1877** 0510	1886	1895	**1904** 0120	**1913** 0806	**1922** 1111	1931	**1940** 0524	1949	1958	1967 **1966** 1228
1906 0131 **1906** 0817 **1906** 0928	1915	1924	1933	**1942** 05141 **942** 0824	**1951** 0516 **1950** 1209	**1960** 0113 **1960** 0522	1969	1978	1987	1996	2005	**2014** 0401	
1962	1971	1980	1989	1998	**2007** 0815	2016							
2018													
Grid B													
Sq 48	**Sq 01**	**Sq 10**	**Sq 19**	**Sq 28**	**Sq 37**	**Sq 46**	**Sq 55**	**Sq 08**	**Sq 17**	**Sq 26**	**Sq 35**	**Sq 44**	**Sq 53**
													1701
							1703	1722	1721	**1730** 0708	1739	1748	1757
	1705	1714	1723	1732	1741	1750	1759	1768	1777	1786	1795	1804	1813
1752	1761	1770	1779	1788	**1797** 0204	1806	1815	1824	1833	1842	1851	1860	1869
1808	1817	**1826** 0618	**1835** 0220	1844	1853	1862	1871	1880	1889	1898	1907 **1906** 1226	1916	1925
1864	1873	1882	1891	**1900** 0918	19091 **908** 1212	**1918** 0520	1927	1936	1945	1954	1963	1972	1981
1920	1929	1938	1947	1956	1965	**1974** 05	1983	1992	**2001** 0623	**2010** 0227			
1976	**1985** 0303	1994	2003	2012									
Grid C													
Sq 06	**Sq 15**	**Sq 24**	**Sq 33**	**Sq 42**	**Sq 51**	**Sq 04**	**Sq 13**	**Sq 22**	**Sq 31**	**Sq 40**	**Sq 49**	**Sq 02**	**Sq 11**
												1706	1715
						1708	1717	1726	1735	1744	1753	1762	1771
1710	1719	1728	1737	**1746** 1029	1755	1764	1773	1782	1791	1800	1809	1818	1827
1766	1775	**1784** 0514	1793	1802	1811	1820	1829	1838	1847	1856	1865	1874	1883
1822 1120	**1831** 1009	1840	1849	1858	1867	1876	1885	1894	1903	1912	1921	1930	**1939** 0125
1878 0123	1887	1896	1905	1914	1923	1932	1941	**1950** 0516 **1949** 1217 **1949** 1217	1959	1968	1977	1986	**1995** 0730
1934	**1943** 0406	1952	1961	**1970** 0531	1979	1988	1997	2006	2015				
1990	1999	2008	2017										
Grid D													
Sq 20	**Sq 29**	**Sq 38**	**Sq 47**	**Sq 56**	**Sq 09**	**Sq 18**	**Sq 27**	**Sq 36**	**Sq 45**	**Sq 54**	**Sq 07**	**Sq 16**	**Sq 25**
										1702	1711	1720	1729
							1731	1740	1749	1758	1767	1776	1785

1724	1733	1742	**1751** 0525	1760	1769	1778	1787	1796	1805	1814	1823	1832	1841
1780	1789	1798	1807	1816	1825	1834	1843	1852	1861	1870	1879	1888	1897
1836	1845	1854	1863	1872	1881	1890	1899	1908	1917	1926	1935	1944	1953
1892	**1901** 0107	1910	1919 **1918** 1204	1928	1937	**1946** 0802	1955	1964	1973	1982	1991	2000	2009
1948	1957	**1966** 1017	1975	1984	1993	2002	2011	2020					
2004	2013	2022											

The 56 year sequences are separated by an interval of 9 years on the horizontal.
Mega quakes M ≥ 8.6 highlighted in **red**. Events with M ≥ 7.8 and ≤ 8.5 denoted in **blue**.
Source of Raw Data: National Geophysical Data Center.

Appendix 3
54/56 YEAR CYCLE: WORLD MEGA QUAKES Post 1870 M ≥ 8.6
National Geophysical Data Center

Grid A
7.5 months ending March 31

Sq 29	**Sq 27**	**Sq 25**	**Sq 23**	**Sq 21**	
				1893	
			1895	1949	
		1897	1951 **1950** Aug15	**2005** Mar28 **2004** Dec26	
	1899	1953 **1952** Nov04	2007		
1901	1955	2009			
1957 Mar09	**2011** Mar11				
2013					

Grid B
9 months ending June 10

Sq 36	**Sq 34**	**Sq 32**	**Sq 30**	**Sq 28**	**Sq 26**
					1898 Jun05 **1897** Sep20 **1897** Sep21
				1900	1954
			1902	1956	**2010** Feb27
		1904	1958	**2012** Apr11	
	1906 Jan31	**1960** May22	2014		
1908	1962	2016			
1964 Mar28	2018				
2020					

WORLD MEGA QUAKES: 1870–2013 M ≥ 8.6
National Geophysical Data Center

Date	**Country**	**Mag**
1897 Sep 20	Philippines: North west Mindanao, Dapitan	8.6
1897 Sep 21	Philippines: Mindanao, Zamboanga, Sulu	8.7
1898 Jun 05	Japan: Offshore east coast Honshu	8.7
1906 Jan 31	Ecuador: Offshore	8.6
1922 Nov 11	Chile: Atacama	8.7
1946 Apr 01	Alaska: Unimak Island	8.6
1950 Aug 15	India-China	8.6
1952 Nov 04	Russia: Kamchatka	9.0
1957 Mar 09	Alaska	8.6
1960 May 22	Chile: Puerto Montt, Valdiva	9.5
1964 Mar 28	Alaska	9.2
1965 Feb 04	Alaska: Aleutian Islands, Rat Islands	8.7
2004 Dec 26	Indonesia: Offshore west coast Sumatra	9.1
2005 Mar 28	Indonesia: Offshore south west Sumatra	8.6

2010 Feb 27	Chile: Maule, Concepcion, Talcahuano	8.8
2011 Mar 11	Japan: Honshu	9.0
2012 Apr11	Indonesia: Offshore north west coast Sumatra	8.6

In Grids A & B, the 56 year sequences are separated by intervals of 54 years on the horizontal.
World mega quakes M ≥ 8.6 falling in Grids A and B are highlighted in **red**.
The NGDC listed some 17 world mega quakes (M ≥ 8.6) since 1870, of which 14 showed up in Grids A & B compared with an expected 3.3.
Source of Raw Data: National Geophysical Data Center.
Source: McMinn, 2011b.

Appendix 4
108/56 YEAR CYCLE: WESTERN SOUTH AMERICA Post 1700 ≥ M 7.8
Year ending November 30

Sq 50	**Sq 46**	**Sq 42**	**Sq 38**	**Sq 34**	**Sq 30**	**Sq 26**	**Sq 22**
							1726
							1782
						1730 0708	1838
						1786	1894
					1734	1842	**1950** 0516 **1949** 1217 **1949** 1217
					1790	1898	2006
				1738 **1737** 1204	**1846** 0628	1954	
				1794	1902	**2010** 0227	
			1742	1850	1958		
			1798	**1906** 0131 **1906** 0817 **1906** 0928	**2014** 0401		
		1746 1029	1854	1962			
		1802	1910	2018			
	1750	1858	**1966** 1017				
	1806	1914	2022				
1754	1862	**1970** 0531					
1810	**1918** 0520						
1866	**1974** 1003						
1922 1111							
1978							

The 56 year sequences are separated by an interval of 108 years on the horizontal.
Mega quakes M ≥ 8.6 highlighted in **red**. Events with M ≥ 7.8 and ≤ 8.5 denoted in **blue**.
Source of Raw Data: National Geophysical Data Center.

Appendix 5
54/56 YEAR CYCLE:
MAJOR US & WESTERN EUROPEAN FINANCIAL CRISES Post 1810
Year beginning March 1

Grid A						
Sq 11	**Sq 09**	**Sq 07**	**Sq 05**	**Sq 03**	**Sq 01**	**Sq 55**
						1815
					1817	1871
				1819	**1873**	1927
			1821	1875	**1929**	1983
		1823	1877	**1931**	**1985**	
	1825	1879	**1933**	**1987**		
1827	**1881**	1935	**1989**			
1883	1937	1991				
1939	1993					

1995						
Grid B						
Sq 04	**Sq 02**	**Sq 56**	**Sq 54**	**Sq 52**	**Sq 50**	**Sq 48**
						1808
					1810	1864
				1812	**1866**	**1920**
			1814	1868	1922	1976
		1816	1870	1924	1978	
	1818	1872	1926	**1980**		
1820	1874	1928	**1982**			
1876	1930	1984				
1932	1986					
1988						

The 56 year columns are separated by intervals of 54 years on the horizontal.
Years in **bold** experienced major financial crises as listed by Kindleberger (Appendix B, 1996).
Of the 25 major financial crises listed by Kindleberger (1996) for the 1810-1940 era, 14 appeared in the two 54/56 year grids (significant $p < .01$).
Source: McMinn, 1993.

SEISMO-VOLCANIC ENERGY PROPAGATION TRENDS IN CENTRAL AMERICA AND THEIR RELATIONSHIP TO SOLAR CYCLES

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Abstract: Earthquake and volcanic energies in Central America come from the outer core under the Caribbean Sea and transmigrate to the Pacific coast through the oceanized horst structures. It is concluded that the direction of energy movement is controlled by the level of thermal energy input into the Caribbean dome from the outer core, which is inversely correlated with the solar cycle: during the declining solar cycle, earthquake and volcanic swarms moved northward, but during the rising cycle, southward.

Keywords: *earthquake, volcano, energy propagation, Caribbean, Central America, solar cycle*

Introduction

Our recent studies have clarified the seismo-volcanic energy flow patterns in many parts of the Circum-Pacific regions (Blot, 1976; Meyerhoff et al., 1996; Tsunoda, 2009; Tsunoda et al., 2013 and 2014; Choi, 2014; Choi et al., 2014a and 2014b). These studies firmly established that the thermal energy that comes from the Earth's outer core to the surface spreads laterally along major deep fracture zones and mobile tectonic belts.

Central America (**Fig. 1**) is unique in various ways: 1) the area is occupied by a series of oceanized seas including the Caribbean Sea (Meso-Cenozoic) and Costa Rica Rift (Cenozoic), and possibly the Gulf of Mexico (Paleozoic oceanization? – Pratsch, 2010); 2) it is located in the axial area of the North-South American Geanticline (Choi, 2014); and 3) it has been tectonically and magmatically very active in the Cenozoic.

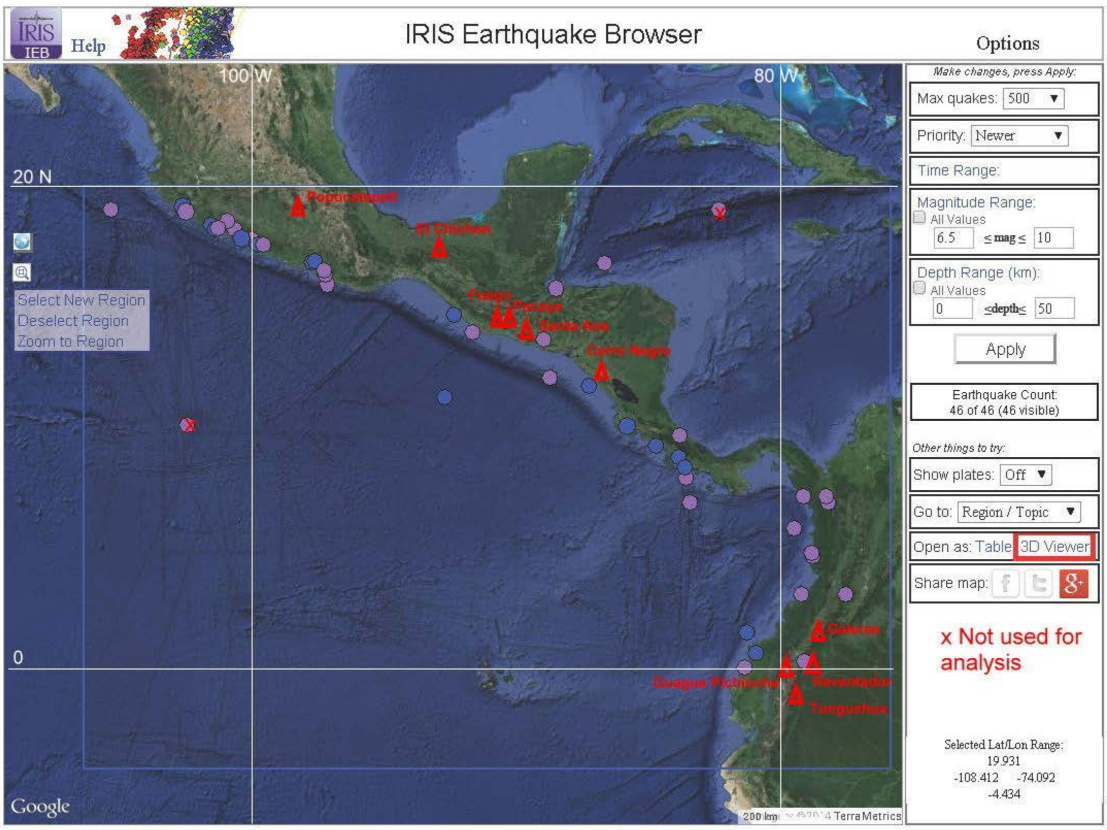

Figure 1. Earthquakes with magnitude 6.5 or greater, 50 km or shallower in depth, that have occurred since 1970 in Central America. Map generated by IRIS Seismic Monitor program (http://www.iris.edu/seismon/). Volcanoes indicated on this map erupted with the scale of VEI 3 or greater after 1970 and are listed in Table 2.

Furthermore, the present study clarified a complex energy propagation pattern with the deep-sourced energy coming from the east, the Caribbean Sea. A comparison of this pattern with the solar cycle fluctuation revealed very interesting facts in regard to energy movement. The author presents here some of the highlights of the study.

1. Earthquake propagation trends

Major earthquake (M6.0+) and volcanic eruption (VEI 3+) data were extracted from the IRIS (www.iris.edu/seismon/)/USGS NEIC (http://earthquake.usgs.gov/earthquakes/search), and USGS Volcanic Program websites (http://www.volcano.si.edu), respectively, and facilitated for the study (**Fig. 1**).

As seen in **Fig. 2** below, M6.0 to M6.9 quakes do not show particular trends, but the M6.5+ group quakes do show some significant patterns. The same fact was also observed in South and North American earthquakes (Choi, 2014; Choi et al. 2014a). Based on this, earthquakes with magnitude 6.5 or greater were used in this study.

Other parameters of extracted quakes are: depth range – 50 km or shallower; and those occurring in the coastal area of Central America between latitudes -4.5^{o} and 20^{o}. A list of earthquakes is shown in **Table 1**, and a list of volcanic eruptions in **Table 2**. Their geographic positions are illustrated in **Fig. 1**.

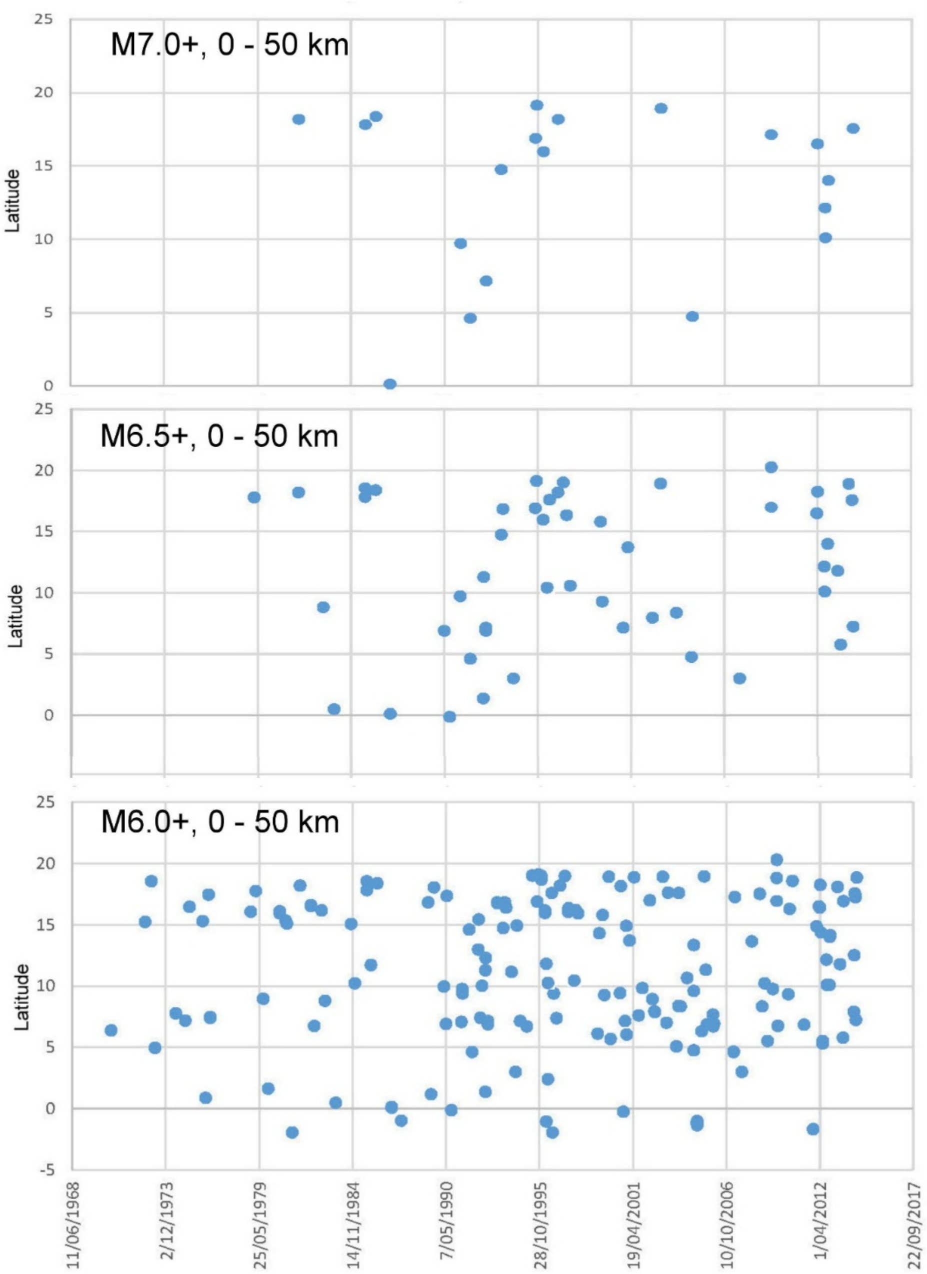

Figure 3. Latitude-time plots for comparison strong earthquakes of various magnitudes in Central America. No clear trend is seen in M6.0+ quakes (bottom), but trends appear in the M6.5+ quake group. Note the sudden appearance of numerous M6.5+ quakes from 1990 to 2004, and from 2012 to 2014. See also Figure 9.

Tale 1. List of very strong earthquakes (M6.5+) between the latitudes -0.5º and 20.00º, Central America.

Magnitude	Depth (km)	Year	Date	Time	Latitude	Longitude	Locality
6.6	41.2	1974	8/10/1974	9:50:57	17.37	-61.99	LEEWARD ISLANDS
6.5	2.8	1979	14/03/1979	11:07:10	17.76	-101.3	NEAR COAST OF GUERRERO, MEXICO
7.3	28.1	1981	25/10/1981	3:22:16	18.18	-102.01	MICHOACAN, MEXICO
6.5	43.8	1983	3/04/1983	2:50:02	8.8	-83.11	COSTA RICA
6.6	38.8	1983	22/11/1983	14:21:01	0.48	-79.79	NEAR COAST OF ECUADOR
6.5	20.2	1985	16/03/1985	14:54:01	16.98	-62.46	LEEWARD ISLANDS
6.8	29.2	1985	19/09/1985	13:17:50	18.54	-102.32	MICHOACAN, MEXICO
7.6	42.1	1985	21/09/1985	1:37:15	17.81	-101.69	NEAR COAST OF GUERRERO, MEXICO
7	37.8	1986	30/04/1986	7:07:19	18.37	-103.01	NEAR COAST OF MICHOACAN, MEXICO
7	10	1987	6/03/1987	4:10:42	0.12	-77.8	COLOMBIA-ECUADOR BORDER REGION
6.5	9.6	1990	8/05/1990	0:01:40	6.9	-82.63	SOUTH OF PANAMA
7.3	4	1991	22/04/1991	21:56:51	9.7	-83.07	COSTA RICA
7.1	14.9	1991	19/11/1991	22:28:50	4.6	-77.41	NEAR WEST COAST OF COLOMBIA
6.6	50	1992	26/08/1992	9:43:11	1.36	-80.19	OFF COAST OF ECUADOR
6.7	50	1992	2/09/1992	1:51:04	11.29	-93.08	OFF COAST OF MEXICO
6.6	14.3	1992	17/10/1992	8:32:40	6.88	-76.76	NORTHERN COLOMBIA
7.2	10	1992	18/10/1992	15:11:59	7.15	-76.84	NORTHERN COLOMBIA
7.2	35.7	1993	10/09/1993	19:12:55	14.74	-92.69	NEAR COAST OF CHIAPAS, MEXICO
6.7	39	1993	24/10/1993	7:52:18	16.83	-98.73	NEAR COAST OF GUERRERO, MEXICO
6.7	32.6	1994	6/06/1994	20:47:43	2.99	-76.03	COLOMBIA
6.5	36.6	1995	19/01/1995	15:05:06	5.09	-72.94	COLOMBIA
7.4	44.4	1995	14/09/1995	14:04:34	16.88	-98.6	NEAR COAST OF GUERRERO, MEXICO
7.9	42.3	1995	9/10/1995	15:35:55	19.12	-104.2	NEAR COAST OF JALISCO, MEXICO
6.6	10	1995	1/12/1995	5:20:28	10.16	-104.02	NORTHERN EAST PACIFIC RISE
7.1	7.1	1996	25/02/1996	3:08:13	15.96	-98.09	OFF COAST OF GUERRERO, MEXICO
6.8	48.2	1996	15/07/1996	21:23:37	17.57	-101.05	NEAR COAST OF GUERRERO, MEXICO
7.2	49.3	1997	11/01/1997	20:28:28	18.17	-102.83	MICHOACAN, MEXICO
6.9	15	1997	1/05/1997	11:37:34	18.99	-107.27	OFF COAST OF JALISCO, MEXICO
6.9	3	1997	9/07/1997	19:24:10	10.5	-63.55	NEAR COAST OF VENEZUELA
6.9	31.4	1997	19/07/1997	14:22:08	16.34	-98.19	NEAR COAST OF GUERRERO, MEXICO
6.7	6.4	1999	11/07/1999	14:14:16	15.79	-88.32	HONDURAS
6.8	44.9	1999	20/08/1999	10:02:23	9.26	-84.06	COSTA RICA
6.5	17	2000	8/11/2000	6:59:59	7.14	-77.84	PANAMA-COLOMBIA BORDER REGION
6.5	7.6	2001	13/02/2001	14:22:05	13.7	-88.87	EL SALVADOR
6.5	10	2002	31/07/2002	0:16:44	7.94	-82.81	SOUTH OF PANAMA
6.5	33.4	2003	25/12/2003	7:11:11	8.36	-82.82	PANAMA-COSTA RICA BORDER REGION
7.2	15	2004	15/11/2004	9:06:55	4.74	-77.47	NEAR WEST COAST OF COLOMBIA
6.8	29	2007	10/09/2007	1:49:14	3	-77.9	NEAR WEST COAST OF COLOMBIA
7.3	29	2009	28/05/2009	8:24:48	16.81	-86.24	NORTH OF HONDURAS
6.7	10	2009	28/05/2009	8:24:57	20.28	-86.6	YUCATAN PENINSULA, MEXICO
7.4	20	2012	20/03/2012	18:02:47	16.49	-98.23	NEAR COAST OF GUERRERO, MEXICO
6.7	20	2012	11/04/2012	22:55:10	18.23	-102.69	MICHOACAN, MEXICO
7.4	28	2012	27/08/2012	4:37:19	12.14	-88.59	OFF COAST OF CENTRAL AMERICA
7.6	35	2012	5/09/2012	14:42:07	10.09	-85.32	COSTA RICA
7.3	24	2012	7/11/2012	16:35:46	13.99	-91.9	NEAR COAST OF GUATEMALA
6.5	37.4	2013	15/06/2013	17:34:28	11.79	-86.91	NEAR COAST OF NICARAGUA
6.7	12	2013	13/08/2013	15:43:15	5.77	-78.2	SOUTH OF PANAMA

6.5	28.5	2014	13/01/2014	4:01:04	19	-66.83	PUERTO RICO REGION
7.2	24	2014	18/04/2014	14:27:26	17.55	-100.82	GUERRERO, MEXICO

Table 2. List of volcanoes in Central America with VEI 3+ since 1970.

Name	Year of major eruption	Latitude	Longitude	Eruption, VEI 3+
Cerro Negra, Nicaragua	1971	12.51	-85.7	3
Fuego, Guatemala	1971	14.47	-90.88	3
Reventador, Ecuador	1973	-0.08	-77.66	3
Fuego, Guatemala	1974	14.47	-90.88	4
El Chichon, Mexico	1982	17.36	-93.23	5
Pacaya, Guatemala	1990	14.38	-90.6	3
Cerro Negra, Nicaragua	1992	12.51	-85.7	3
Popocatepetl, Mexico	1996	19.02	-98.62	3
Guagua Pichincha, Ecuador	1998	-0.17	-78.6	3
Tunguahua, Ecuador	1999	-1.47	-78.44	3
Reventador, Ecuador	2002	-0.08	-77.66	4
Galeras, Colombia	2004	1.22	-77.37	3
Pacaya, Guatemala	2004	14.38	-90.6	3
Santa Ana, El Salvador	2005	13.85	-89.63	3
Galeras, Colombia	2007	1.22	-77.37	3
Galeras, Colombia	2008	1.22	-77.37	3
Tunguahua, Ecuador	2010	-1.47	-78.44	3

As seen in **Fig. 4**, the Central America region displays very interesting and complex trends: a major northing trend from 1990 to 1997, a southing trend from 1988 to 2001, a brief northward movement from 2002 to 2003, and southward from 2004 to 2007. Another major southward trend is also present from 2009 to 2013.

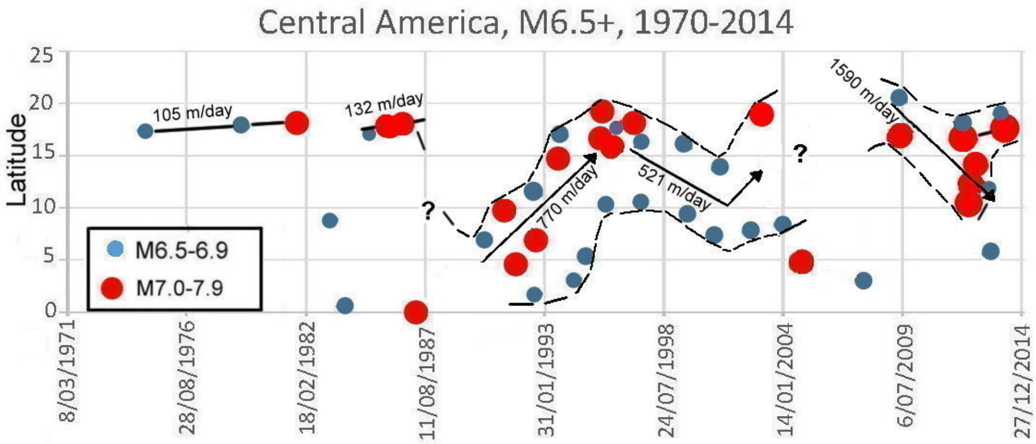

Figure 4. Time vs latitude plot of M6.5+ earthquakes in Central America. Lines indicate possible energy link.

Each group's average propagation speed was calculated with the Excel program (**Figs. 5 and 6** for example). The average speed of the major northward propagation (based on M7.0 quakes) from 1987 to 1997 is 280 km/year or 770 m/day. This group has numerous very strong quakes (M7.0 to 7.9). The major southward flow of the 1998-2001 group was calculated as 190 km/year or 521 m/day, much slower than the northward speed. Due to the paucity of samples, however, the speeds for the period prior to 1990, and the 2002 to 2007 fluctuation cannot be given here. The latest southward group from 2009 to 2013 (**Fig. 6**) is much faster than others, calculated as 580 km/year or 1,590 m/day.

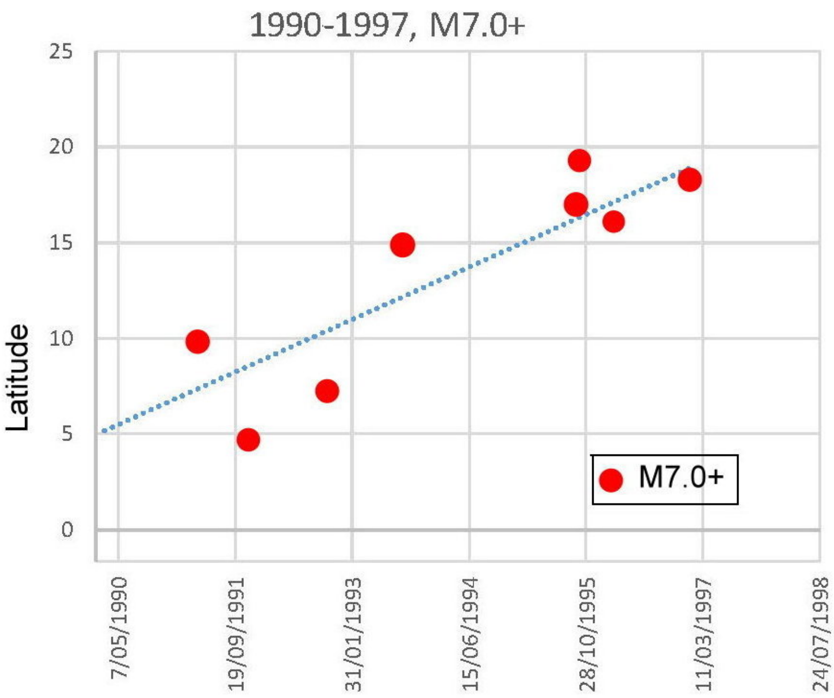

Figure 5. M7.0+ earthquakes from 1990 to 1997, northward propagation period. Average speed: 280 km/year or 770 m/day.

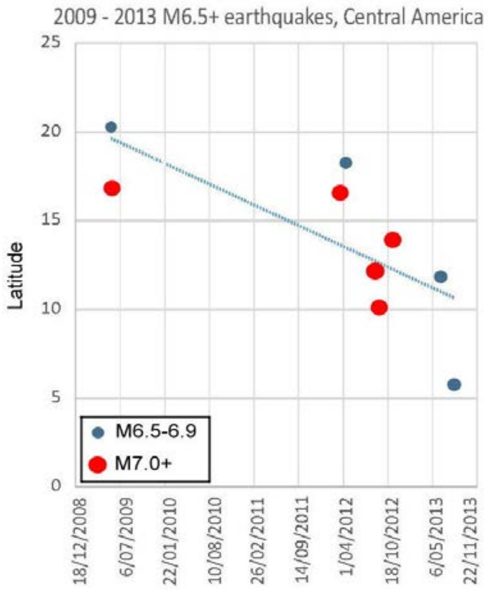

Figure 6. Time vs latitude plot of M6.5+ earthquakes from 2009 to 2013 in Central America with an average trend calculated by the Excel program (1,590 m/day). Note the absence of large quakes between 2010 and 2011 (rising period of solar cycle 24).

In addition, there is another trend – very slow northward flow, especially well seen in the quakes prior to 1987 north of 15 degrees latitude, Mexico (**Fig. 4**). Its speed ranges from 38 km/year (105 m/day) to 48 km/year (132 m/day) – both speeds are very close to or within the range of the North American propagation speed, 120-140 m/day (Choi et al., 2014b).

Propagation trend of volcanic eruptions

During the period from 1970 to 2013 a total of 17 major volcanic eruptions with VEI 3 or greater took place in Central America (**Table 2**; **Figs. 1 & 6**) in two areas: north of 10^{o} latitude and south of 2^{o} latitude. No major eruptions (VEI3 or greater) are recorded in the Smithsonian Volcanic Program (http://volcano.si.edu/) during the study period in Costa Rica, Panama and Colombia, despite the presence of numerous active volcanoes.

The largest eruption was El Chichon Volcano in Mexico with VEI 5 in 1982. The eruption occurred one year after the 1981 M7.3 Michoacan, Mexico quake, about 600 km west to the El Chichon Volcano. Other energy links are seen in 1990 to 1996 between eruptions and northward-moving earthquakes. In this group, numerous very strong earthquakes and VEI 3 eruptions coexisted in the same area.

Other outstanding features are very intensive volcanic activities in Ecuador to Colombia (near equator) from 1998 to 2010: during first three years (1998-2002) in Ecuador, then moving north to Columbia, and triggering a series of VEI 3 Galera Volcano eruptions from 2004 to 2008.

Another possible energy link is seen prior to 1983 in the series of northward-moving volcanoes in Nicaragua, Guatemala and Mexico (**Fig. 7**). It is noteworthy that its eruption scale increased with time. The energy propagation speed from Cerro Negro, Nicaragua, in 1971 to El Chichon, Mexico, in 1982 is 77 km/year or 102 m/day.

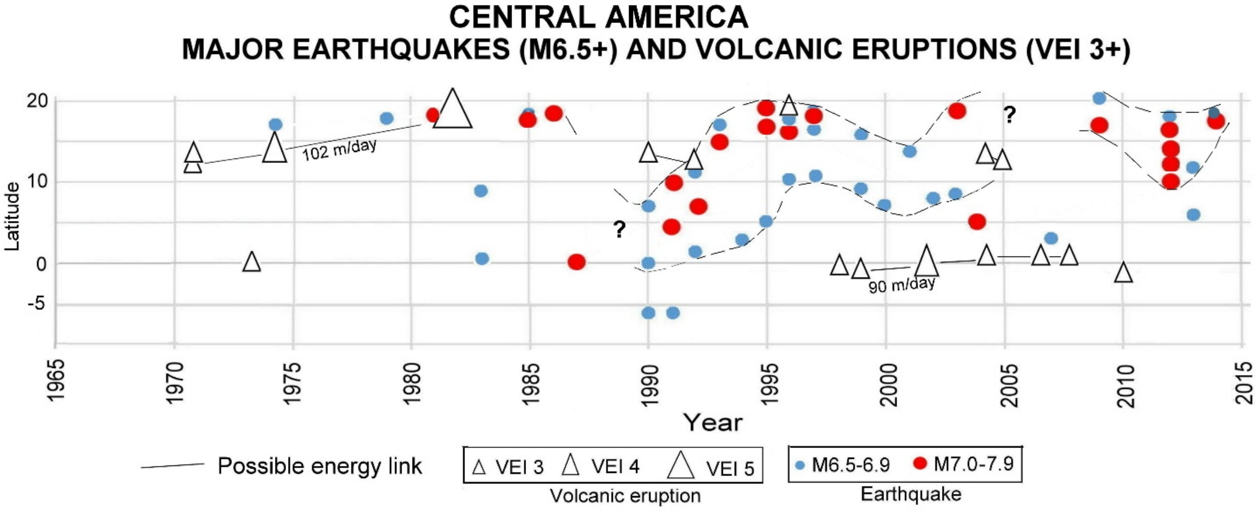

Figure 7. Year vs latitude plot of major earthquakes and volcanic eruptions in Central America from 1970 to 2014. Possible energy link of volcanoes and their propagation speed are indicated.

Discussion

Energy source of the Central American earthquakes and volcanoes

The author analysed the 2010 Haiti earthquake (Choi, 2010), in which he presented the oceanized Caribbean dome (**Fig. 7**). The paper contains a detailed account of his ideas and supporting data. The most important point that has to be emphasized here is the gigantic fracture system that is most likely connected with the lower mantle and possibly the outer core, as is well displayed in the mantle tomography by Karason and Van der Hilst (2000), **Fig. 8**.

The author has already discussed the deep-seated fracture systems coinciding with the high-angle, narrow, fast-mantle zones in tomographic images on many occasions (Choi, 2005 for example). Based on experiences in seismic data processing and interpretation for hydrocarbon exploration in the past, the author considers that the p-wave velocity contrast in the tomographic images is strongly related to the amount of fluid and/or gas present in the mantle. The deep fractures are open systems through which outer core–derived materials rise to the Earth’s surface and finally escape to the atmosphere, whereas the slow mantle is primarily a closed system filled with fluid and gas. Interestingly the fast mantle zone under the Caribbean Sea is steeply inclined eastward at about 50 to 60 degrees from the horizontal. Though obscured below 2,000 km, the continuation of the fast-velocity channel to the mantle bottom is almost doubtless if we take the relatively poor resolution in the deep mantle into consideration.

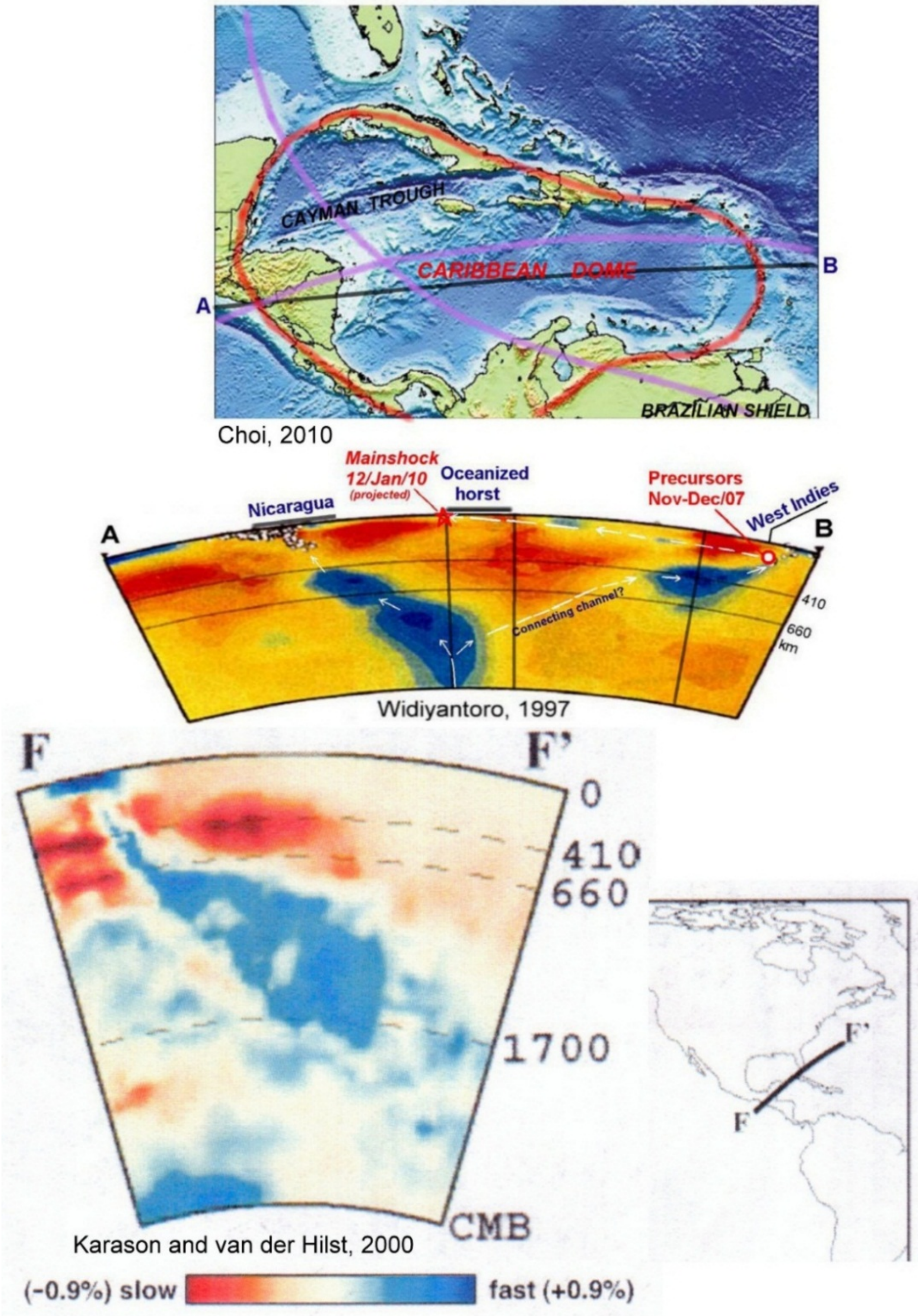

Figure 8. Seismo-tomographic images of the Caribbean dome. Note inclined, very distinctive, deep mantle-rooted fast zone.

As discussed above, the seismic tomographic images convincingly suggest that the energy for the oceanization of the Caribbean dome was supplied from the outer core through the deep fracture systems under the Caribbean Sea. This energy supply is still continuing today, generating earthquakes and volcanic eruptions.

The Caribbean dome is situated on the gigantic Precambrian geanticlinal system that runs from North to South America and extends into the Rio Grand Ridge, off Brazil, in the Atlantic Ocean (Choi, 2013a and 2013b). Like the SE Asian region, where extensive oceanization has taken place since the late Mesozoic near the axis of the geanticlinal trend, the Borneo-Vanuatu Geanticline (Choi, 2005 and 2013b), the Caribbean region has also been subject to intensive oceanization.

Considering the structural highs which have been most extensively oceanized (Choi, 2010), the author speculates that the outer core energy, after reaching the upper mantle, flows mainly along the horst structures: the NE-SW horst running through the middle of the Caribbean Sea from Hispaniola (Dominica) in the north to Panama in the south (which further runs into the Costa Rica Rift), and an E-W horst which forms

Cayman Trough today (**Fig. 9**).

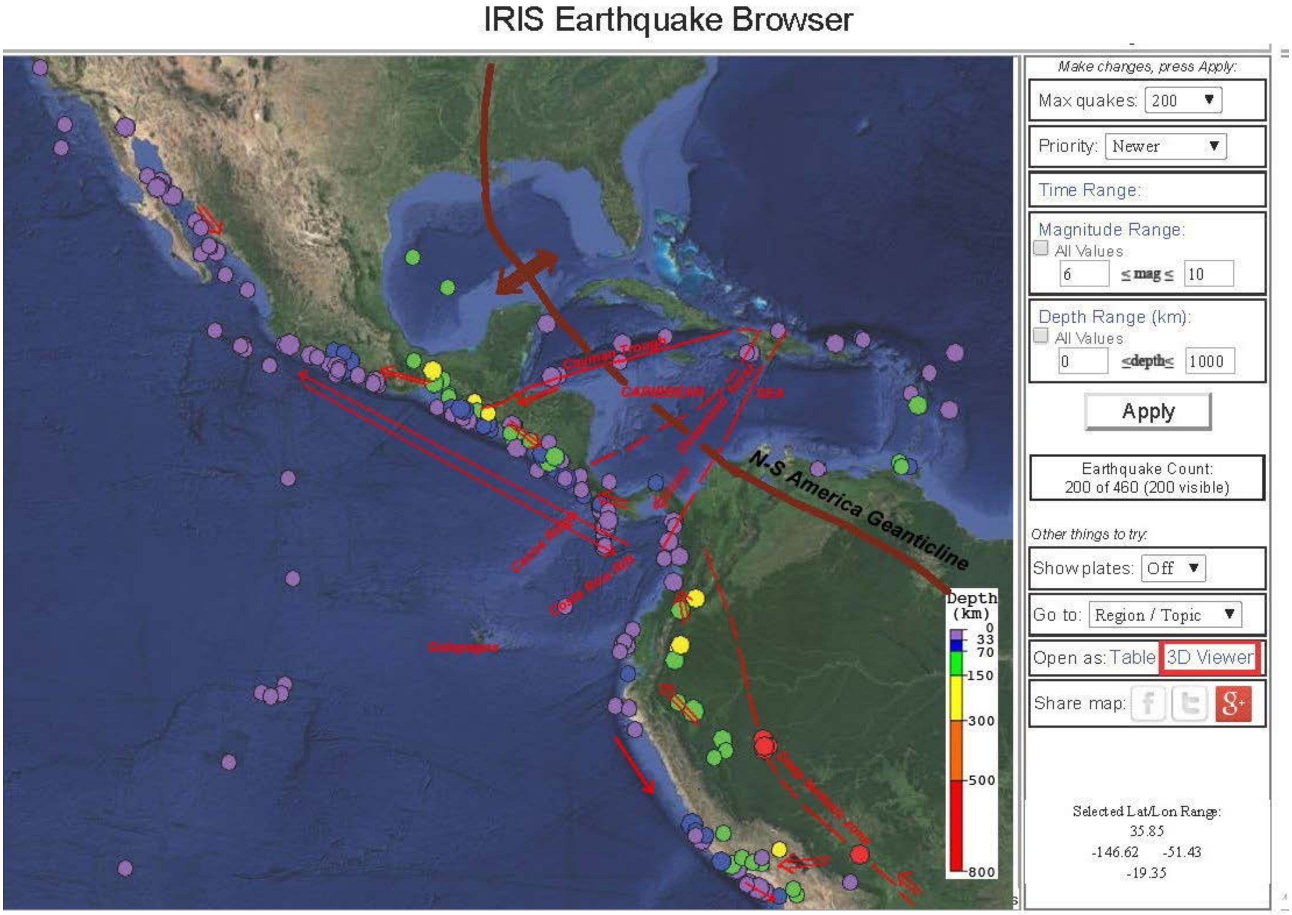

Figure 9. Suggested energy flow routes superimposed on the M6+ earthquake map (all depths, 1970-2014). For the deep tectonic zone in South America, see Choi, 2005.

Energy movement direction and solar cycle

The time-space distribution of strong earthquakes (**Fig. 3**) and its comparison with the solar cycle (**Fig. 10**) make it possible to reconstruct the following seismic history in the region. The history is especially intriguing in terms of the inverse correlation between the solar cycle and earthquake frequency (Choi and Maslov, 2012; Choi et al., 2004a):

The deep-Earth sourced energy first arrived at Panama from the Caribbean in 1990. From there, with the onset of the active core phase, it started to move northward along the coast from 1990 while generating numerous strong quakes and volcanic eruptions on the way (start of sharp declining period of solar cycle 22), reached the Mexico region, where it stayed from 1995 to 1997 (solar cycle trough), and triggered powerful volcanic and seismic activities in the region. It then returned southward from 1997 to 2000 (solar cycle 23 rising period – low energy period). The flow direction temporarily reversed and headed northward from 2003 to 2004 (solar cycle 23 declining period). It appears to have headed south again from 2004 to 2008 (trough between solar cycles 23-24), but due to the paucity of M6.5+ seismicity during the period, the energy movement is not traceable. The prolonged declining period of solar cycle 23, from 2002 to 2009 (7 years), longer than the usual cycle (5 to 6 years), and the deepening solar hibernation (Casey, 2012), may be responsible for this low seismic activity.

A new seismic cycle started from 2009 at the northern end of the study area (Mexico) – southward flow until 2012 (solar cycle 24 rising period), which reversed to northward in 2013 (declining cycle).

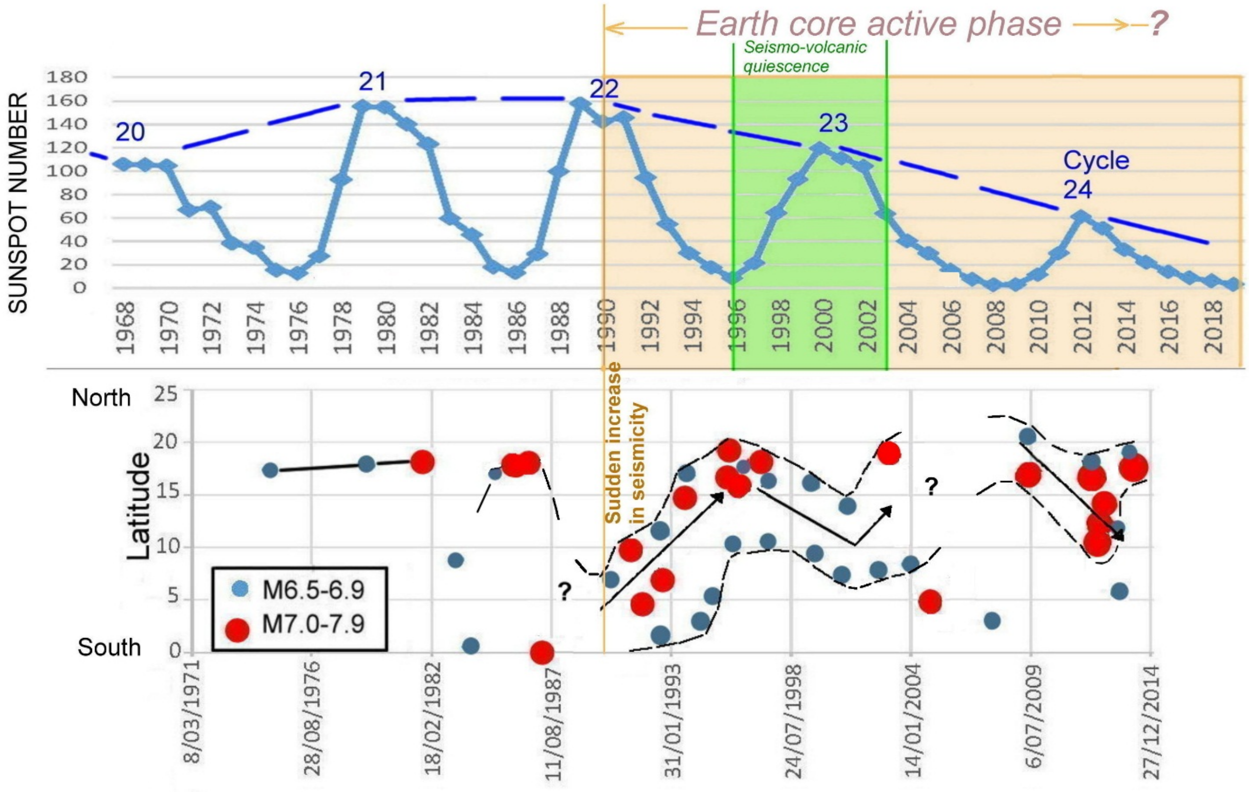

Figure 10. Solar cycles and earthquake propagation trend. Note a general trend in which earthquakes move northward when the solar cycle is in decline, but southward when the solar cycle rises, except for the period from 2005 to 2009 for which no data are available. The sudden increase in seismic activity from 1990 coincides with the start of the declining period of a longer cycle obtained by tying the peaks the 11-year cycle. See Choi et al., 2014a. Seismo-volcanic quiescence cited from Choi (2010) and Tsunoda et al. (2013), and the Earth core active phase from Choi and Maslov (2010).

The above history testifies to an intricate relationship between the solar cycles and energy movement direction: During the solar cycle rise (relatively lower energy input from the Caribbean), seismo-volcanic energy moves south, but the solar cycle is declining (high energy phase), it moves northward. This fact implies that the thermal level (which is inversely related to the solar cycle) in the Caribbean Sea controls the direction of seismo-volcanic energy along the Pacific coast of Central America.

Conclusions

This paper clarified the complex seismo-volcanic energy flow trend along the Pacific coast of Central America, which is characterized by a repeated cycle of northing and southing controlled by energy input fluctuation from the Caribbean Sea.

This pattern has not been seen in other areas of the Pacific Rim, such as the Izu-Bonin Volcanic Islands Chain (Tsunoda et al., 2014), North America (Choi et al., 2014) and South America (Choi, 2014). This is considered to be due to the unique geological setting of the Caribbean Sea: it forms a major dome structure formed by thermal energy directly derived from the outer core through gigantic deep fracture systems developed underneath. The Earth core activity is directly influencing the energy level fluctuation in the Caribbean Sea.

These facts have wide ramifications in understanding the Earth's geodynamic systems and internal workings that generate tectonic and magmatic activities at the Earth's surface.

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A NEW METHOD TO CALCULATE PALAEOGRAVITY USING FOSSIL FEATHERS

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Abstract: A recent paper on the ancient birds *Archaeopteryx* and *Confuciusornis* has shown that their feathers would not be able to support the weight of the birds in flight. The authors therefore suggested that the birds could not fly. This suggestion remains debatable since other authors have highlighted various features that indicate these birds did fly. In this paper, values for gravity which would enable these birds to fly with weaker feathers are calculated based on the controversial theory that palaeogravity was less. These values are then compared with earlier estimates of palaeogravity derived from a range of other life forms from approximately the same time period.

Keywords: *palaeogravity, Archaeopteryx, Confuciusornis, fossil feathers, feather evolution*

Introduction

Ancient birds had feathers that look a lot like those of living birds. Since most birds use feathers for flight it has been widely suggested that they must have flown: Norberg (1985), Olson (1979), Rietschel (1985), Wellnhofer (2004), and Yalden (1971). But many ancient birds seem too heavy to fly in today's gravity and it has also been argued that they didn't fly: Ostrom (1974), Senter (2006), Speakman and Thomson (1994), and Vazquez (1992). The whole issue remains controversial with some studies indicating that they would be incapable of flying while others conclude the opposite.

One bird that has been widely studied for many years is *Archaeopteryx* (see **Figure 1**). The recent fossil finds of the *Confuciusornis* bird have not been studied as intensively but this will probably change in the future since there are now hundreds of fossils. One interesting recent study of the flying ability of the ancient birds *Archaeopteryx* and *Confuciusornis*, by Robert L. Nudds and Gareth J. Dyke (2010a), analysed the strength of the fossil feathers of the birds to determine if the feathers could support the weight of the bird during flight. Based on the strength of the feathers they concluded that both birds 'were not capable of flight'.

The main strength in a primary flight feather is provided by a central shaft known as the rachis. All the barbs of the feather are attached to this central shaft so it must be strong enough to withstand the lift generated on the wing without buckling. During flight the feathers must be able to support a lift force that is at least equal to the weight of the bird (*mass* x *gravity*) so the authors calculated this buckling limit to infer the flight capabilities of the ancient birds. As modern-day birds change direction they generate forces greater than the weight of the bird, so modern-day birds have additional factors of safety to allow for this increased loading. Nudds and Dyke found that even if they assumed a small factor of safety and completely solid feathers the ancient feather was weaker than expected for a similar sized modern-day bird. The calculations revealed that the feathers of both birds were so weak that they would buckle if the birds attempted to fly.

The conclusion the authors reached was that both *Archaeopteryx* and *Confuciusornis* would not be able to flap their wings without buckling the feathers. Nudds and Dyke suggested that both birds may only have been capable of gliding down with their wings held outstretched to reduce the forces on the feathers of the wing. They would need to climb up a tree again to be able to repeat this technique. Flapping flight was not possible. Even this concept of a gliding bird produced very low safety factors which were well below those of modern birds.

Figure 1. Fossil *Archaeopteryx* showing feather impressions

The gliding technique suggested by Nudds and Dyke conforms to the well-known “top down” flight evolution model where flight developed as birds used their wings to glide down to the ground. Another widely known and debated evolution model is the “bottom up” version where flight evolved first on the ground as agile birds used their developing wings to help them run faster. An interesting alternative theory that is not as well known was suggested by Klaus Ebel (1996) almost 20 years ago. He presented evidence that originally flight did not occur above land at all but was the by-product of hunting under water. Thus, the first step to conquering the air was to learn “flying” under water. The long bony tail of *Archaeopteryx* is actually the part of its skeleton that would have helped it to fly under water. Real airborne flight only evolved later, as primitive birds began to glide over the surface of water and then developed powered flight. During this evolution towards true flight they lost their long bony tails and became more like *Confuciusornis.* A similar evolution, better backed up by fossil evidence, was also suggested for pterosaurs. However, while it can be suggested for various reasons that *Archaeopteryx* may not have fully developed the ability to fly in the Upper Jurassic, it seems that many palaeontologists believe *Confuciusornis* must have evolved to be a competent flier by the time it lived in the Lower Cretaceous, tens of millions of years later.

Both *Archaeopteryx* and *Confuciusornis* have feathered wings that appear to be capable of generating aerodynamic lift, so the conclusion that the feathers would be incapable of supporting the weight of the birds, as proposed by Nudds and Dyke, generated intense debate. Gregory S. Paul (2010) replied that the ‘total biology of the birds indicates that they could achieve flapping flight’ and suggested there might be errors in either the mass estimates of the birds or the shaft diameters of the feathers. Xiaoting Zheng et al. (2010) recognised that the innovative analysis offered important new insights into bird evolution. However, they believed that some recently collected data for *Confuciusornis* indicating thicker main shaft diameters, about twice those used in the original calculations, would mean that their conclusions would need to be further evaluated. But despite this they noted that ‘even our measurements are considerably smaller than the predicted rachis diameter of primary feathers with similar feather length in similarly sized extant birds’. Nudds and Dyke (2010b) responded to these two comments by agreeing that their model is of course dependent on the data fed into it, but calculating the maximum lift forces sustainable by a bird still provides a novel way of estimating the flight capabilities of ancient birds. Using the other values suggested still indicated the birds were not capable of flapping flight and the feather’s strength was barely enough for gliding.

Description

One alternative hypothesis that would enable a bird to fly with weak feathers is the notion that the weight of the bird was less because gravity was less. The hypothesis that ancient life has been significantly affected by a weaker palaeogravity is a controversial proposal that has only been seriously discussed by a small number of authors: Carey (2000), Davidson (1994), Erickson (2001), Hurrell (1994, 2011 and 2012), Kort (1949), Ciechanowicz and Koziar (1994), Koziar (2011), Mardfar (2000 and 2012), Maxlow (2005 and 2014), Scalera (2002, 2003a, 2003b and 2004), and Strutinski (2012 and 2013). However, the strength of fossil feathers has never been used to estimate the strength of ancient gravity so this would introduce an interesting new alternative method of calculation.

Nudds and Dyke calculated the body mass of *Archaeopteryx* and *Confuciusornis* respectively as 0.276 kg and 0.5 kg based on their size in relation to modern birds. They also estimated the downward force required so the feathers were 'strong enough to sustain a force equal to their body weight' as equivalent to a mass of 0.188 kg and 0.215 kg respectively in today's gravity. The authors calculated their weight assuming the prevalent concept that ancient gravity was equal to the present. They then rejected the possibility of flight because they thought 'their body masses are unlikely to be this low'. However, if gravity was less on the ancient Earth then the weight of the birds would also be less. This allows us to calculate palaeogravity relatively easily by assuming that the ancient birds were able to fly with weaker feathers by today's standards. In a reduced gravity the weight (*mass* x *gravity*) would be reduced to the appropriate level required for flight. So ancient gravity (g_a) can be calculated from:

$$g_a = F_a / M_p$$

where F_a is taken as the maximum ancient force produced by the birds' wings, and M_p is the mass of the bird.

For *Archaeopteryx*:

$$\begin{aligned} g_a &= F_a / M_p \\ &= 0.188 / 0.276 \\ &= 0.68\text{g} \end{aligned}$$

As at about 145 million years ago relative ancient gravity is calculated as 68% of present gravity.

For *Confuciusornis*:

$$\begin{aligned} g_a &= F_a / M_p \\ &= 0.215 / 0.5 \\ &= 0.43\text{g} \end{aligned}$$

As at about 120 million years ago relative ancient gravity is calculated as 43% of present gravity.

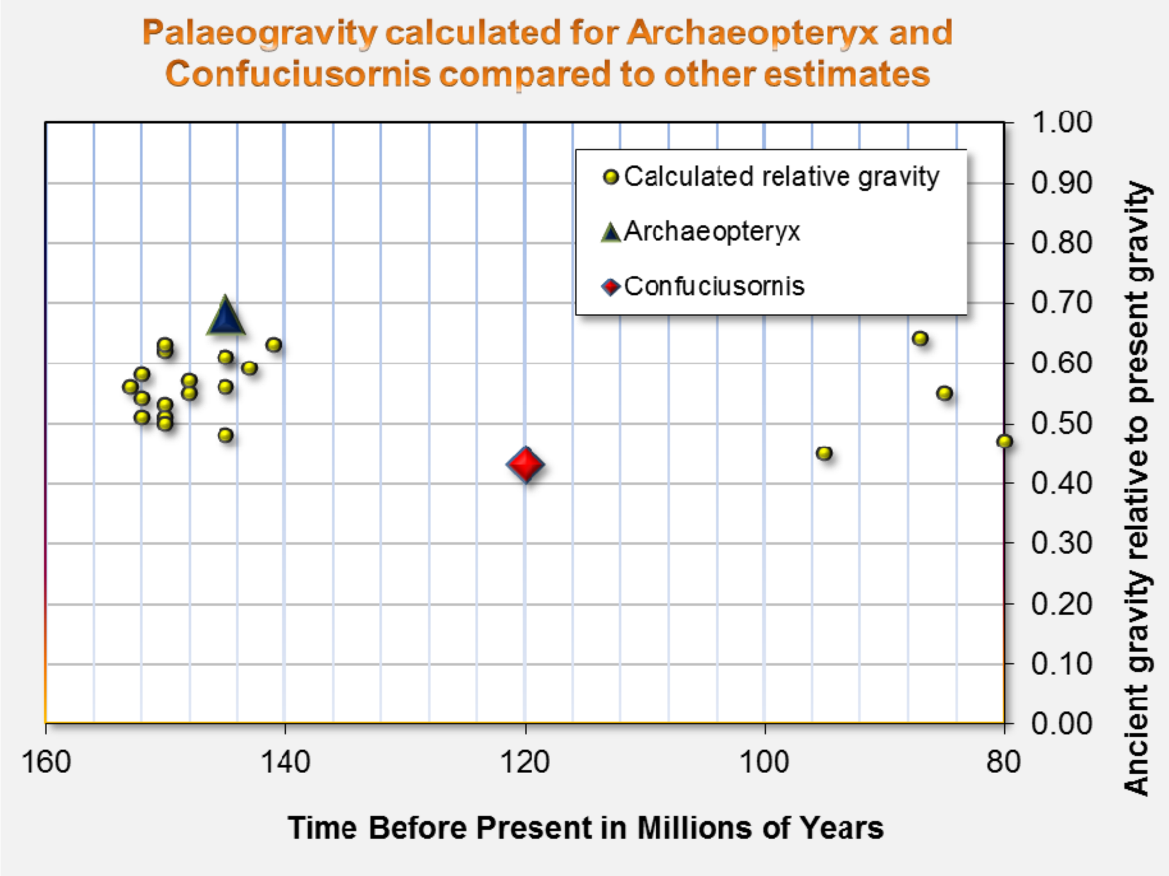

Figure 2. Palaeogravity estimates obtained from primary feather strengths of *Archaeopteryx* and *Confuciusornis* compared with a range of other estimates.

It should also be mentioned that Zheng et al (2010) queried the measurements of *Confuciusornis* and noted 'the rachises of primary feathers of confuciusornithids measure 2.1 mm to 2.3 mm in diameter on four Tianyu specimens, about twice as large as the measurements reported by Nudds and Dyke'. However, even these measurements are still less than modern birds of a similar size. For example, *C. palumbus* has a body mass of 0.49 kg with a rachis feather diameter of 2.97 mm, and the smaller *L. ridibundus* with a body mass of 0.284 kg has a feather rachis diameter of 2.6 mm. It is clear that even if the measurements need to be adjusted they are still considerably lower than modern birds require when flying.

Conclusion

Previous estimates of ancient gravity have been derived from a range of other life forms from approximately the same time period by Hurrell (2012). These previous estimates for palaeogravity can usefully be compared with the calculations obtained in this paper for birds. The leg bone strengths of *Diplodocus*, *Allosaurus* and *Giraffatitan* have given approximate values of palaeogravity as 0.51g, 0.5 g and 0.56 g relative to today's gravity. The neck ligament strength of *Diplodocus* has been calculated to indicate an approximate palaeogravity of 0.53 g. The blood pressure of *Giraffatitan* indicates an approximate palaeogravity of 0.48 g. The dynamic similarity of *Giraffatitan* compared with present-day life indicates a palaeogravity of 0.61 g. Previously unpublished additional estimates are also in a similar range for palaeogravity. Thus it appears that the estimates of ancient gravity from fossil bird feathers are in broad agreement with bone and ligament strength, blood pressure and dynamic-similarity calculations on a range of other life from that time. The values for palaeogravity obtained from *Archaeopteryx* and *Confuciusornis* are shown on a graph in **Figure 2** with the other estimates of palaeogravity mentioned above shown for comparison. The data used in **Figure 2** is shown in **table 2.**

Table 2. A table of palaeogravity estimates obtained from the primary feather strengths of *Archaeopteryx* and *Confuciusornis* compared with a range of other life using differing methods. These data are shown in graph form in Figure 2.

Animal type	Scientific Name	Time (Ma)	Calculated relative gravity	Estimation Method
Sauropod dinosaur	Supersaurus vivianae	153	0.56	Dynamic Similarity
Sauropod dinosaur	Apatosaurus ajax	152	0.58	Leg Bone Strength
Sauropod dinosaur	Apatosaurus louisai	152	0.51	Leg Bone Strength
Sauropod dinosaur	Apatosaurus (unknown)	152	0.54	Dynamic Similarity
Sauropod dinosaur	Diplodocus carnegiei	150	0.51	Leg Bone Strength
Theropod	Allosaurus fragilis	150	0.50	Leg Bone Strength
Sauropod dinosaur	Diplodocus carnegiei	150	0.53	Neck ligament
Sauropod dinosaur	Apatosaurus louisae	150	0.62	Dynamic Similarity
Sauropod dinosaur	Diplodocus longus	150	0.63	Dynamic Similarity
True Dragonfly	Isophlebia aspasia	148	0.55	Dynamic Similarity
True Dragonfly	Aeschnogomphus kuempeli	148	0.57	Dynamic Similarity
Bird	**Archaeopteryx**	**145**	**0.68**	**Feather Strength**
Sauropod dinosaur	Giraffatitan brancai	145	0.48	Blood Pressure
Sauropod dinosaur	Giraffatitan brancai	145	0.56	Leg Bone Strength
Sauropod dinosaur	Giraffatitan brancai	145	0.61	Dynamic Similarity
Sauropod dinosaur	Brachiosaurus altithorax	145	0.61	Dynamic Similarity
True Dragonfly	Prohoyaeshna milleri	143	0.59	Dynamic Similarity
True Dragonfly	Aktassia pritykinae	141	0.63	Dynamic Similarity
Bird	**Confuciusornis**	**120**	**0.43**	**Feather Strength**
Sauropod dinosaur	Argentinosaurus huinculensis	95	0.45	Dynamic Similarity
Sauropod dinosaur	Futalognkosaurus dukei	87	0.64	Dynamic Similarity
Pterosaur	Pteranodon longiceps	85	0.55	Dynamic Similarity
Sauropod dinosaur	Puertasaurus reuili	80	0.47	Dynamic Similarity

Nudds and Dyke found that the feather lengths of the two birds *Archaeopteryx* and *Confuciusornis* were very similar to present-day birds of a similar size but the rachises were narrower. They checked their calculations against a range of present-day birds and found they could predict the rachis size accurately for modern-day birds based on the feather length. Further work by Wang et al. (2012) seems to confirm this view. This is interesting for calculations of ancient gravity since it may be possible to use any fossil primary feather to calculate ancient gravity so long as the length of the feather and the diameter of the main shaft can be accurately determined. This indicates a useful area for future research.

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OLR AND AIR TEMPERATURE ANOMALIES PRIOR TO BIG EARTHQUAKES: A CASE STUDY ON AN ALASKAN EARTHQUAKE ON JUNE 23, 2014 (M7.9)

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ABSTRACT: Scientists have attempted to predict earthquakes using several methods like abnormal animal behavior, geophysical and geochemical precursory studies by specifying the three most important parameters, namely, location (in terms of latitude and longitude), time of occurrence and magnitude of an impending earthquake. In 1980, Russian scientists noted the appearance of thermal anomaly prior to devastating earthquakes. In this paper, we discuss the appearance of anomalous air temperature at various levels of the atmosphere and outgoing longwave radiation (OLR) above cloud levels 13 days prior to a gigantic Alaskan earthquake occurred on June 23, 2014 with the magnitude of 7.9. This gives hope to the society that the "Air temperature anomaly" (ATA) and OLR anomaly can be used as complementary methods to improve accuracy in predicting devastating earthquakes.

***Keywords**: precursor, outgoing longwave radiations, air temperature, thermodynamics.*

INTRODUCTION

Transient thermal anomalies prior to devastating earthquakes were first detected by Russian scientists (Gorny et al., 1988). Currently, many scientists from all over the world are involved in this line of research thanks to satellite technology. Thermal anomalies normally appear for short duration 3 to 30 days prior to the big earthquakes (magnitude greater than 5.0). Many scientists (Singh et al., 2010; Ouzounov et al., 2011; Pulinets et al., 2006a&b), based on their retrospective analysis of several devastating earthquakes, presented possible link between the atmospheric - surface parameters (ASP) and occurrence of the devastating earthquakes. In this paper we discuss anomalous thermal change at various altitudes of atmosphere and OLR anomaly above the cloud level, which are concomitant with major earthquakes.

Anomalous changes in surface temperature, surface latent heat flux, total ozone content and total electron content (TEC) were reported by several scientists prior to the Sumatra earthquake (December 26, 2004), the Wenchuan earthquake (May 12, 2008) and the Japan earthquake (March 11, 2011) (Dey and Singh, 2003; Dey et al., 2004; Okada et al., 2004; Trigunait et al., 2004; Genzano et al., 2007.; Ouzounov et al., 2011). These surface, atmospheric and ionospheric parameters are complementary in nature and can be used as an effective tool to predict devastating earthquakes. A low air temperature also has been observed prior to the occurrence of the big earthquakes. This anomalous air temperature is observed at various levels of atmosphere. Complementary to this, OLR anomaly is observed to be high, above +2 sigma level, which can be measured above the cloud level. National Oceanic and Atmospheric Administration (NOAA) satellites 15, 16 and 17 are measuring at the top of the atmosphere and the data can be downloaded from http://www.cdc.noaa.gov. Here we discuss a massive earthquake occurred in Alaska about 24 km SE of Little Sitkin Island (51.7972N latitude & 178.7604E longitude) on June 23, 2014 with the magnitude 7.9.

MATERIALS AND METHODS

National Oceanic and Atmospheric Administration Climate Prediction Center (http://www.cdc.noaa.gov) provides both the data and algorithm for examining the advanced very high resolution radiometer (AVHRR). The appearance of anomalous OLR radiation (positive deviation above 2σ level) on for transient period may be related to tectonic stress in a particular region and thermodynamic processes in the atmosphere. An anomalous eddy of OLR can be defined as dE_index (change in OLR), which signifies the maximum change in the rate of OLR for a given spatial location prior to the earthquake:

$$dE_{index} = \frac{Current\ field\ OLR - Base\ field\ OLR}{Standard\ Deviation}\ W/m^2$$

Air temperature for various levels of altitude (1000 hPa – 364 ft/111 m, 850 hPa - 4781 ft/ 1458 m, 700 hPa - 9882 ft/ 3013 m and 500 hPa - 18289 ft/ 5576 m) is also analysed. Anomalous thermal index (change in temperature) for a tectonically active area for given time can be calculated by downloading data from The National Centers for Environmental Prediction (NCEP; http://www.esrl.noaa.gov/psd/data).

$$dT_{index} = \frac{Current\ field\ air\ temperature - Base\ field\ air\ temperature}{(Standard\ Deviation)}\ K$$

It is found that air temperature is complementary to the OLR radiation. When OLR radiation with positive deviation above 2σ value is observed, there is drop in air temperature well below the 2σ value at various levels.

RESULTS OF LITTLE SITKIN ISLAND, ALASKA (JUNE 23, 2014)

An earthquake with M_w7.9 occurred at 24 km SE of Little Sitkin Island, Alaska (51.7972N latitude & 178.7604E longitude) on June 23, 2014 (USGS.gov). Anomalous OLR first appeared on June 15, 2014 in the nearby region (52.5N latitude & 177.5E longitude), **Fig. 1c & 1d**, just eight days prior to the earthquake. Before that the OLR was normal, **Fig. 1a & 1b)**. The OLR anomaly disappeared on the following day (June 16, 2014), **Fig. 1e &1f)**].

On the same day when OLR anomaly appeared (June 15, 2014), air temperature anomaly (ATA) was recorded. There was a sharp drop in air temperature well below the 2σ level at various altitude levels, **Fig. 2a, 2b, 2c & 2d**.

DISCUSSION AND CONCLUSION

The drop in air temperature prior to the earthquake may be due to the condensation of water, which is due to evaporation of water from the Earth’s surface into the atmosphere. The evaporated water is probably caused by heating up of Earth’s surface due to increased tectonic activity. The anomalous OLR value may be caused by the increased IR radiation due to the heating of Earth’s surface along the seismically active zones. Even though the change in OLR and air temperature values are governed by various other atmospheric factors, the variation of OLR and air temperature due to other atmospheric factors can be removed.

We have compared current OLR and AT data with the base value which was computed by averaging the values during the years from 2007 to 2013. The first OLR anomaly was recorded in the night pass satellite image on June 14, 2014. On the following day (June 15), a very strong OLR anomaly was recorded. On the same day the air temperature rapidly dropped below 2σ level, clearly indicating that the complementary nature of OLR anomaly and ATA for a particular day prior to the impending earthquakes. This fact provides strong evidence for the existence of link between these atmospheric parameters with devastating earthquakes.

The accuracy of earthquake forecasting can be elevated to an unprecedented level in positioning the location and more importantly time of occurrence, if scientists take an interdisciplinary approach we could also predict the depth of earthquake with more accuracy. This can be achieved by well-coordinated global networking with the help of satellite technology (Venkatanathan and Natyaganov, 2014), using low frequency anomalies as precursors; then forecasting earthquakes will no longer be considered as ‘impossible’.

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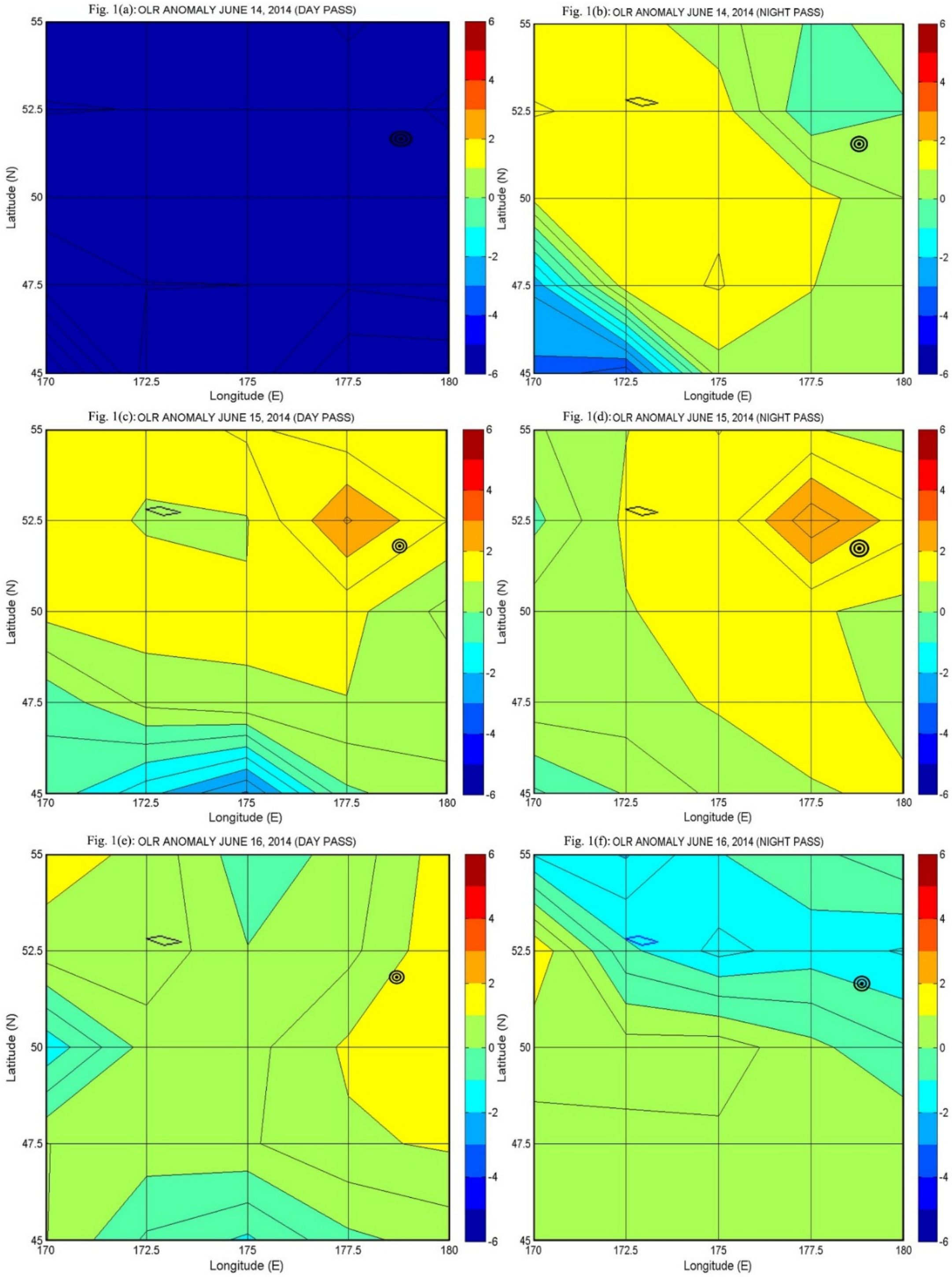

Figure 1. Maps showing OLR scenario for the Alaska region during June 14, 15 & 16, 2014. Appearance of OLR anomaly (above +2 sigma level) can be noticed on June 15, 2014 at the location (52.5N & 177.5E) close to the epicenter of the June 23, 2014 earthquake. The epicenter is marked with black concentric circles.

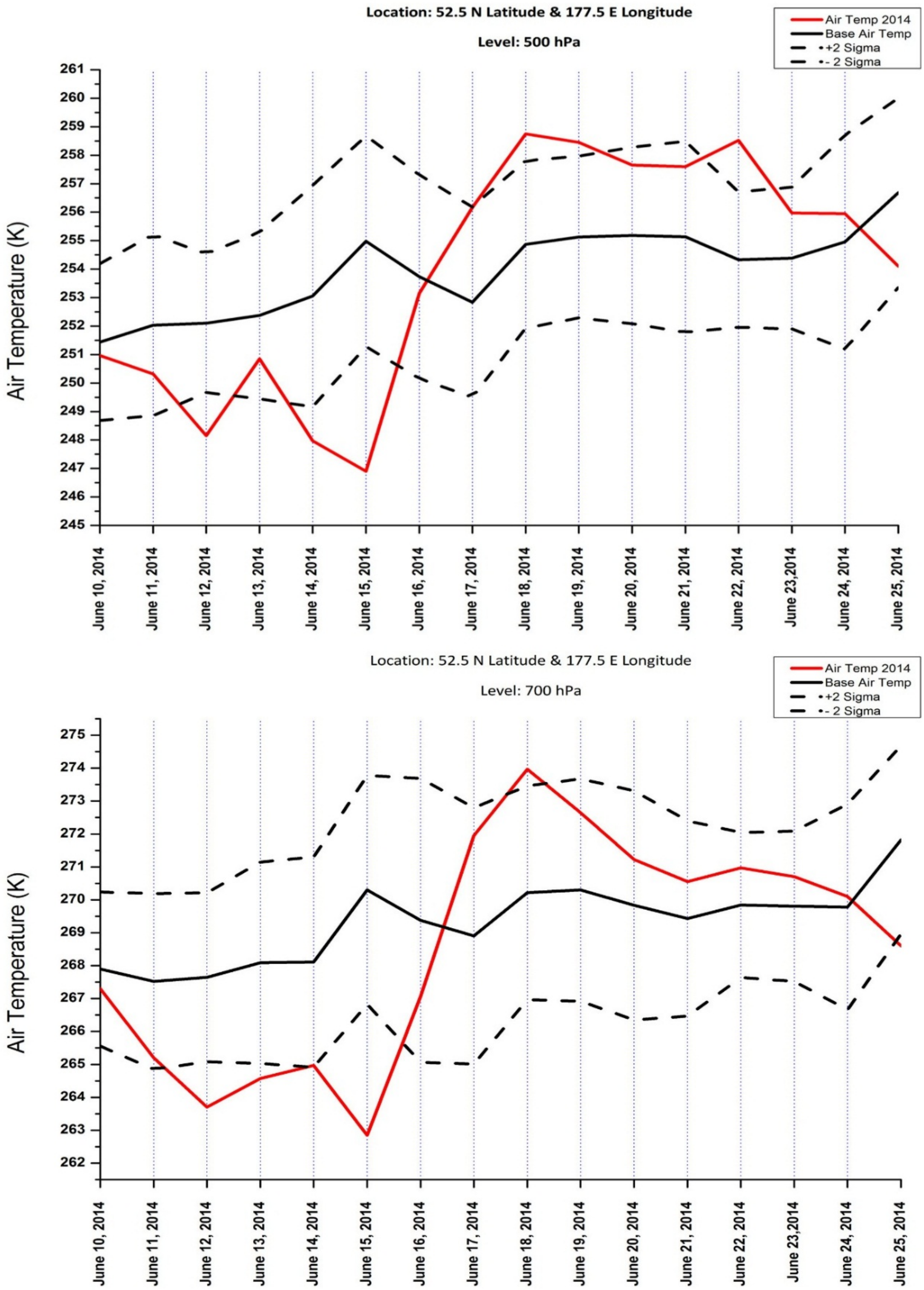

Figure 2 (a & b). Graphs showing anomalous drop in air temperature on June 15, 2014 at the levels 500 hPa (18289 ft/ 5576 m) and 700 hPa (9882 ft/ 3013 m). An earthquake occurred on June 23, 2014. The base air temperature is average seven years starting from 2007 to 2013.

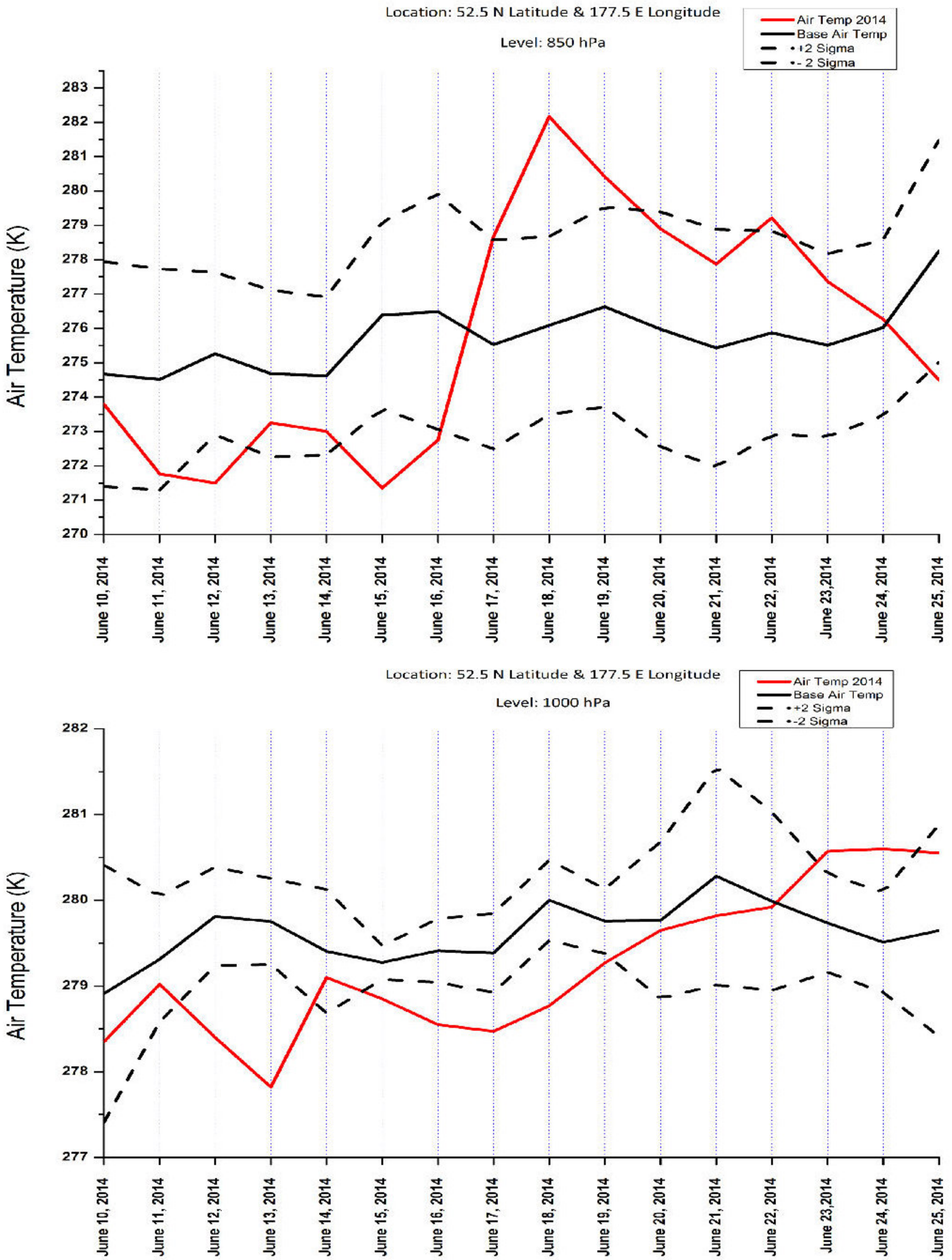

Figure 2 (c & d). Graph showing anomalous drop in air temperature on June 15, 2014 at the levels 850 hPa & 1000 hPa. The earthquake occurred on June 23, 2014 in nearby region.

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DISCUSSION

- DARWIN RISE -

SISYPHUS AND OCEANOGRAPHY

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Many of us have spent considerable time both at sea collecting the raw data and entire careers in house processing same. Some of us are old enough to remember, or have participated, in the evolution of the survey suites used for the collection of this data. Fewer of us have used this processed data in an attempt to try to enlighten those of us who, while enjoying the comforts of home, never had to go to sea to collect any of the data. Yet, many of those try to extrapolate that which they have learned from the real data collected by the seagoers, so much so, that they lose sight of the forest because of the trees (to use a popular expression). It is to that that I address this Letter to the Editor. As a one-time editor of the *NCGT Newsletter* myself, and a contributor of real data from time to time, I feel qualified to address the Sisyphusian task of trying to remind some would-be "scientists" about the subject of ocean floor geomorphology. Considering that this field comprises over 70% of Earth's surface, some of the gibberish I read in the newfound Journal is just that, not relying on, or building on, what we have learned in the past.

The most glaring example, at least to me, is this "paper" by Takao Yano in the *NCGT Journal* v. 2, no., 2, 2014. I began work in 1966 at the beginning of the multibeam sonar age and worked in the Ocean Survey Program until it disbanded in 1993. To that end the US Naval Oceanographic Office employed at least three ships doing deep ocean surveys. The bathymetry effort was generally at a line spacing to ensure total bottom coverage. I was selected by the hierarchy to release discrete pieces of that bathymetry to the public.

Now, to Yano's paper. I had been asked to review the paper, and I rejected it based on the Darwin Rise idea. Here is where Sisyphus came in. I had published many papers on the region outlined by Yano. The sections I had not published on, I had access to the bathymetry. This is what I found:

1. He has a rather dreamlike outline of the Rise which appears similar to the ones described by Menard and his crowd many years ago. After looking all through his Darwin Rise region, I find that the common bottom depth is between 3000 and 3100 fathoms (5550 and 5735 meters). This includes the regions around the Caroline and Marcus-Wake Seamounts, the Geisha Guyots, the Ontong-Java Plateau, and the Hess, Magellan and Shatskiy Rises. Therefore, one has to wonder where the contours came from to delineate a 4.5 km isobaths, a 4.0 km isobaths, and a 3.5 km isobaths. This concentric configuration delineating a rise is not defined by any accurate bathymetry I have ever seen. No bathymetry; no swell.
2. Yano makes the comment that …"another possible contouring…from the same height data by Smoot and King could reflect the rough relief of the Darwin Rise…" In order to make that statement, he would have to ignore several data sets; to wit, the double ridges of the Mendocino Fracture Zone (Yano's figures 3 and 4) which appear exactly in the center of his Darwin Rise diagram. He would necessarily also have to ignore the satellite altimetry data provided by the Geodetic Earth Orbiting Satellite (GEOSAT), which collected gravity data from 72ºN to 72ºS. Bruce Leybourne and I used the US Navy's high pass filtered data set from that to construct ocean floor structural trends. These also agree with the interpretation made by Bob King and I.

In my role as Sisyphus for all those productive years, I had to push that boulder up many a hill to overcome the likes of the Menards and Heezens to get the real data in print. When I see a paper like this in a Journal I was once associated with, it really irks me. Now I've got to push that boulder back up the same hill that a wee bit of a literature search would have prevented.

REPLY TO SMOOT COMMENT

LATE MESOZOIC TECTONO-MAGMATISM IN THE WEST PACIFIC OCEAN — DID THE DARWIN RISE DEMISE OR REVIVE? —

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Abstract: Reviewing the late Mesozoic volcanic rocks drilled and dredged in the West Pacific Ocean and the paleobathymetric models concerning with the Darwin Rise, this paper discusses whether the Darwin Rise hypothesis demised or revived. The revival is concluded to be much conceivable with regional and multiphase volcanic activities on the northwestern two-thirds on the rise and with an examination of the three bathymetric models already proposed.

Keywords*: West Pacific Ocean, Darwin Rise, rock evidence, volcanic interval, guyot, paleobathymetry*

Introduction

The thick layer of seawater brings about serious difficulties to geological surveys in deep oceans. Conquering technological and financial difficulties, great efforts have been made, since the Challenger Expedition in 1872-76, to get sediments and rocks from ocean floors. Thus deepsea specimens have been accumulated acceleratively since around 1960's, including ancient and continental rocks (Meyerhoff and Meyerhoff, 1974; Udintsev, 1990; Meyerhoff et al., 1992; Pratt, 2000; Rezanov, 2002; Wezel, 2005; Vasiliev and Yano, 2006, 2007; Yano et al., 2009, 2011; Vasiliev et al., 2012).

The Geologic Map of the World (Scale 1:15,000,000) by Jatskevich [ed.] (2000), published as the first geological map of the Earth, was successful to depict the world geology with the legends common on continents and in oceans. Although geological data are still fairly scarce in oceans (Vasiliev, 2006), now we are in a new era in which one should be based on rock evidences as much as possible when investigating geologic histories of oceans.

Menard (1964) proposed the Darwin Rise in the late Mesozoic West Pacific Ocean (**Fig. 1**) on the basis of the depths of wave-cut surfaces on numerous guyots (Hess, 1946). The proposed rise is a super-large swell, 4,000 km wide, 10,000 km long and 2 km high, with voluminous volcanoes and archipelagic aprons, owing its origin to an upward thermal bulge of substrative upper mantle. Plate tectonic hypothesis becoming generally accepted, attention was focused on horizontal motions of the Pacific lithosphere and thus the Darwin Rise was largely discredited. Accumulation of drilling and dredging data since the 1970s, however, became to confirm that widespread magmatic activities and vertical movements had taken place in the West Pacific Ocean during the late Mesozoic. Schlanger et al. (1981, p. 447) thus declared the "rehabilitating" of the Darwin Rise in a plate tectonic setting. On the other hand Smoot and King (1997) affirmed the demise of the Darwin Rise, because their linear paleobathymetry reconstructed in the central West Pacific Ocean was incompatible with the rise.

This paper aims to clarify whether the Darwin Rise has revived or demised, through (1) an examination on the actuality of the late Mesozoic regional volcanism in the West Pacific Ocean according to rock evidences and (2) a discussion on the late Mesozoic paleobathymetric models proposed in the West Pacific Ocean. It is also my response to Smoot (2014) holding in doubt about Yano (2014), which has investigated the late Mesozoic tectonic development of the Pacific Ocean driven by the gravity instability between a Pacific-wide superplume head and its lithospheric overburden under the influence of inertial asthenospheric flows.

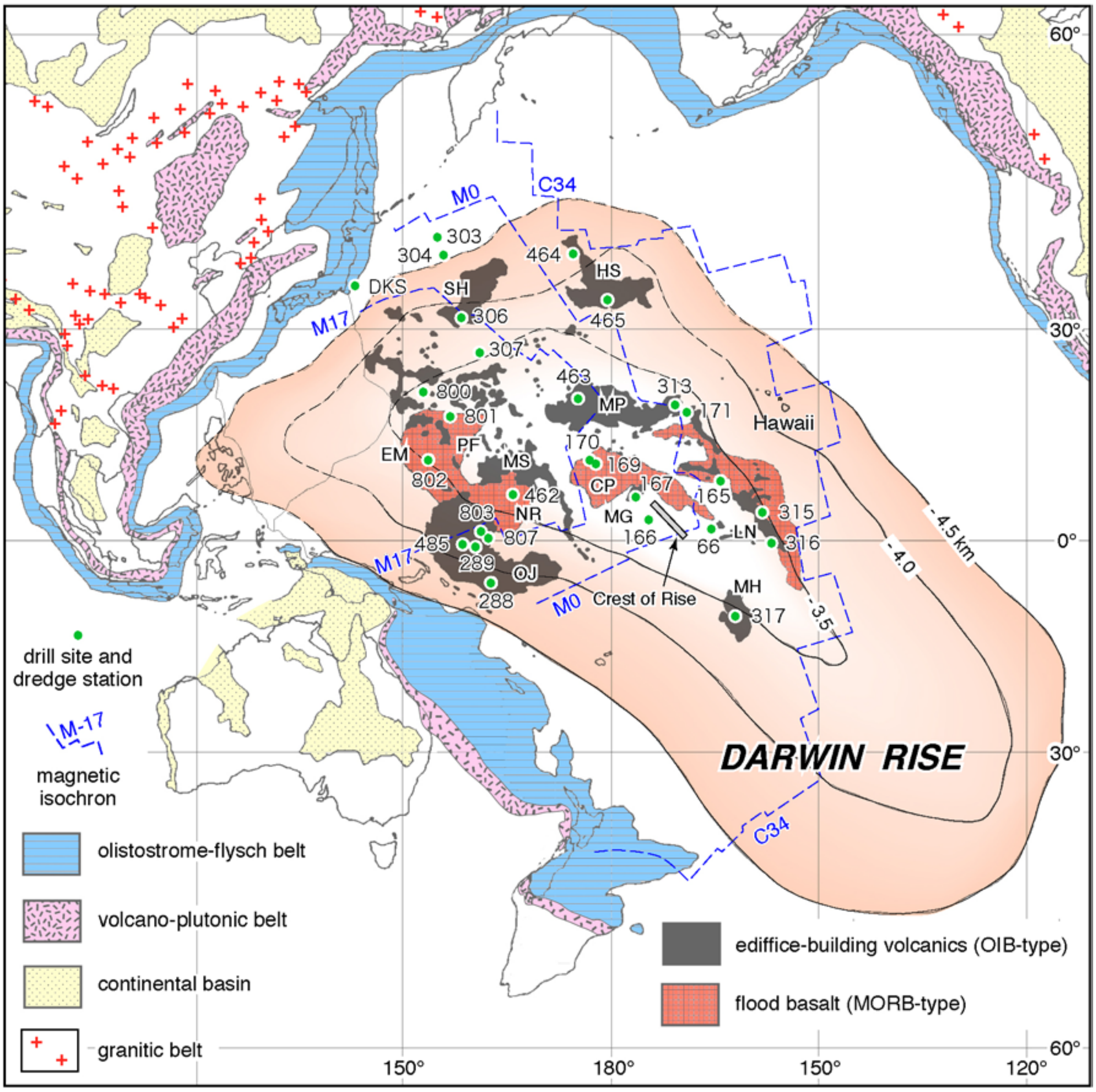

Fig. 1 Tectonic map of the West Pacific Ocean (a part of Fig. 3 in Yano, 2014), with the three representatives of magnetic isochrones after Vogt (1989) and the drilling and dredging locations of the late Mesozoic volcanic intervals shown in **Fig. 2**. CP: Central Pacific Basin, EM: East Mariana Basin, HS: Hess Rise, LN: Line Islands, MG: Magellan Rise, MH: Manihiki Plateau, MP: Mid-Pacific Mountains, MS: Marshall Islands, NR: Nauru Basin, OJ: Ontong Java Plateau, PF: Pigafetta Basin, SH: Shatsky Rise.

Late Mesozoic regional volcanism in the West Pacific

Late Mesozoic volcanic rocks

Late Mesozoic volcanic rocks recovered at a dredged station and 27 drilling sites in the West Pacific Ocean (**Fig. 1**) are collectively shown in **Fig. 2** (Daiichi-Kashima Seamount [DKS]: Research Group for the Daiichi-Kashima Seamount, 1985; Sites 303, 304, 307 and 306: The Shipboard Scientific Party, 1975c, d, f and e, respectively; Sites 464 and 465: Shipboard Scientific Party, 1981c and d; Sites 800-802: Shipboard Scientific Party, 1990a-c; Site 585: Shipboard Scientific Party, 1986; Sites 807 and 803: Shipboard Scientific Party, 1991b and a; Sites 289 and 288: The Shipboard Scientific Party, 1975b and a; Site 462: Shipboard Scientific Party, 1981a; Sites 169 and 170: The Shipboard Scientific Party, 1973d and e; Site 166: The Shipboard Scientific Party, 1973b; Site 66: The Shipboard Scientific Party, 1971; Site 167: The Shipboard Scientific Party, 1973c; Site 463: Shipboard Scientific Party, 1981b; Site 313: The Shipboard Scientific Party, 1975g ; Site 171: The Shipboard Scientific Party, 1973f; Site 165: The Shipboard Scientific Party, 1973a; Sites 315-317: The Shipboard Scientific Party, 1976a-c). They are divided lithologically and geochemically into tuff/volcanic glass, volcaniclastic turbidite, and basaltic lava flows (high alumina, OIB [oceanic island basalts]-type, transitional, and MORB [mid-oceanic ridge basalts]-type). The volcaniclastic turbidite beds obtained through drillings are up to 2 or 3 meters thick and contain shallow-water sands, reefal limestones and fossils, phosphorite, higher-plant fragments, and subaerially-weathered basalt debris. Thus they appear to have been supplied from nearby oceanic islands with carbonate reefs (Schlanger et al., 1981), as depicted in **Fig. 3-2** and **-3**.

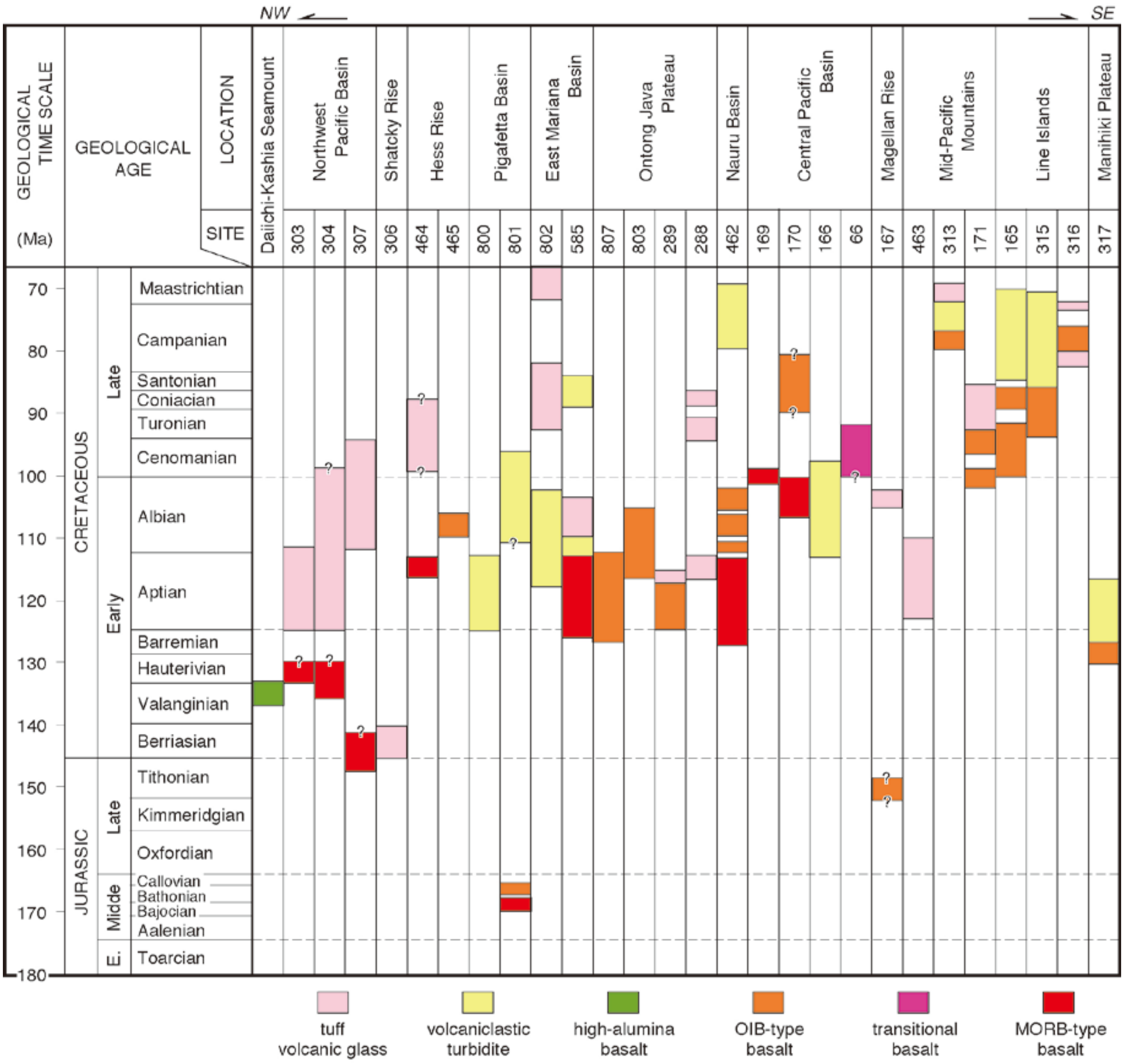

Fig. 2 Volcanic intervals at a dredge station and 27 drill sites in the West Pacific Ocean (modified after Yano and Wu, 1997; Yano et al., 2001). See Fig. 1 for localities.

Chronology of Late Mesozoic volcanic intervals

In the West Pacific there are many oceanic plateaus and numerous seamounts rising from the abyssal plain 5 to 6 km deep. They crowd particularly in the equatorial domain ranging from 30°N to 30°S. The abyssal plain is silled with plateaus, seamount groups and seamount chains and is separated into several ocean basins (**Fig. 1**). The volcanic intervals dredged and drilled indicate that Late Mesozoic magmatism in the West Pacific Ocean has occurred successively on the whole and is separable into four stages: the Middle Jurassic, the latest Jurassic to early Early Cretaceous, the late Early Cretaceous and the Late Cretaceous (**Fig. 2**).

Middle Jurassic

After the twenty-years attempt to recover the Jurassic oceanic crust oldest in the Pacific Ocean, ODP Leg 129 obtained a success to drill the Middle Jurassic basalts overlain with the radiolarites of the Late Jurassic to earliest Cretaceous (Callovian/Bathonian to Valanginian: Matsuoka, 1992) at the Site 801 (**Figs. 1 and 2**) in the Pigafetta Basin [PF], the equatorial West Pacific Ocean (Shipboard Scientific Party, 1990b; Lancelot, Larson, et al., 1990). The penetrated 131 m of basaltic flows consist of the upper sequence of OIB-type alkali olivine basalts dated at 157.4 ± 0.5 Ma and the lower sequence of typical N (normal)-MORB-type tholeiites dated at 166.8 ± 4.5 Ma (Floyd and Castillo, 1992; Pringle, 1992).

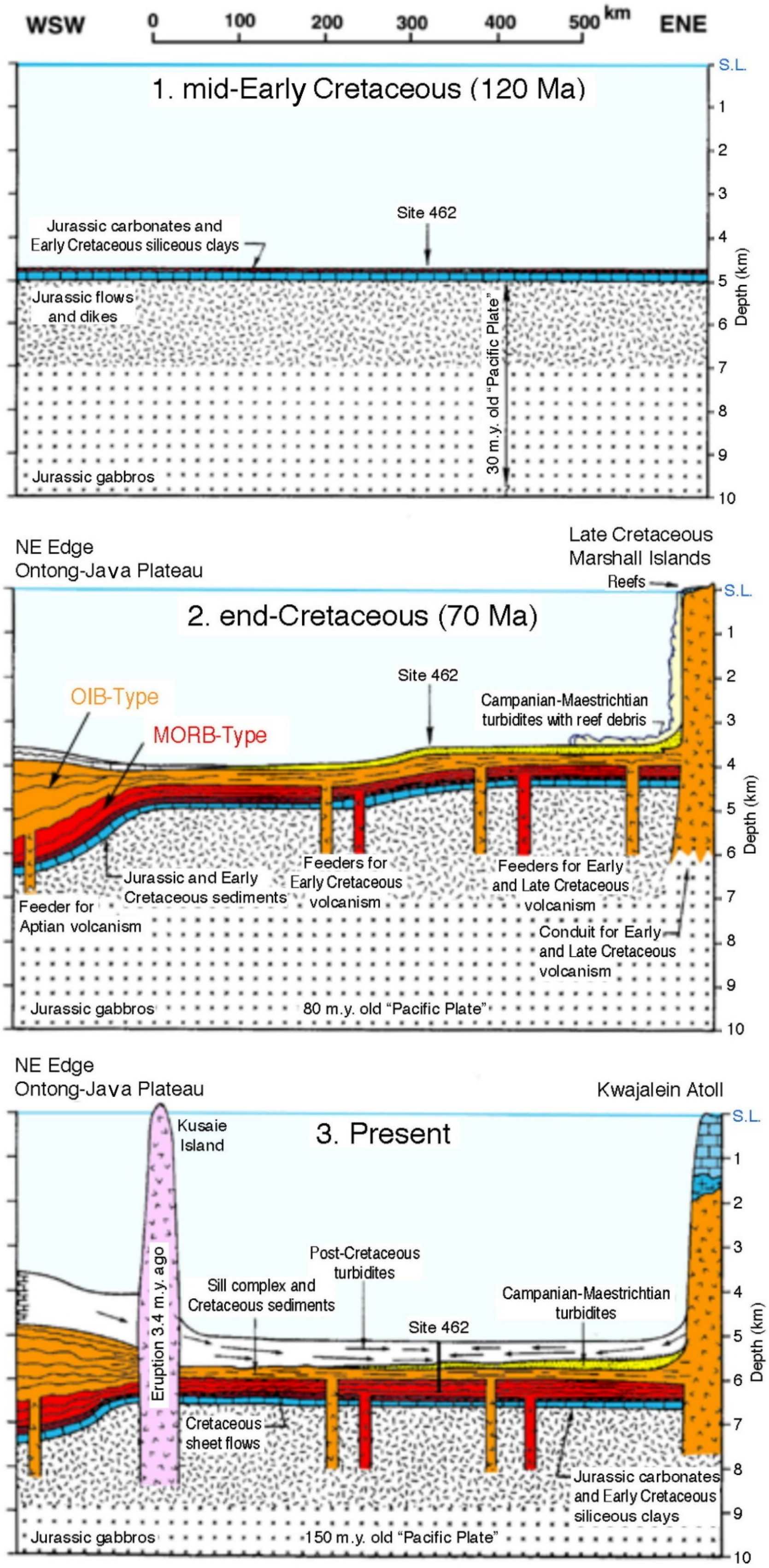

Fig. 3 Tectono-magmatic development of the Nauru Basin, equatorial West Pacific (modified after Larson and Schlanger, 1981). See Fig. 1 for the location of the basin [NR] and Fig. 2 for the volcanic intervals at the DSDP Site 462. Regional subsidence since the end-Cretaceous estimated to be 1.5 to 2.0 m.

From almost exact coincidence between the obtained age of the lower MORB-type basalt flows and the predicted age by extrapolation of M-sequence magnetic lineations, the MORB-type tholeiites are generally considered to have formed at a mid-oceanic ridge. However Abrams et al. (1992), who have synthesized the seismic stratigraphy and sedimentary history elucidated by Leg 129, prudently state that the igneous basement is "not necessarily layer 2". The reasons are that the late Mesozoic magmatism in the West Pacific Ocean is of long-term and multiphase nature (**Fig. 2**), and that, as mentioned below, Cretaceous MORB-type flood basalts have spread synchronously over extensive areas in the West Pacific Ocean (**Fig. 1**). Thus the Jurassic MORB-type basalts drilled at the Site 801 retain a possibility to be oceanic flood basalts having outpoured over ocean basins, not at a mid-oceanic ridge.

Latest Jurassic to early Early Cretaceous

Latest Jurassic to early Early Cretaceous volcanic intervals (**Fig. 2**), though their age controls are not good, are known or predicted at the following sites: MORB-type tholeiites at Sites 303, 304 and 307 in the Northwest Pacific Basin (Marshall, 1975; Moberly and Larson, 1975), undrilled acoustic basements under the Berriasian chert and nanno-chalk with volcanic glass at the Site 306 on the Shatcky Rise (The Shipboard Scientific Party, 1975e), and OIB-type alkali basalts at the Site167 on the Magellan Rise (Bass et al., 1973).

A noticeable existence in this stage is the Daiichi-Kashima Seamount in the southernmost Japan Trench, covered with the early Albian and the probable Barremian reef carbonates. Basement rocks dredged from the seamount are mostly of high-alumina basalts and small amounts of plagioclase rhyolites and perlites, and show a Rb-Sr isochron age of 125 ± 25 Ma (Research Group for the Daiichi-Kashima Seamount, 1985). The high-alumina basalts are considered to have formed probably in the setting of an active continental margin.

Late Early Cretaceous

Late Early Cretaceous (Aptian to Albian) volcanic rocks are most voluminous and spread over an extensive area in the West Pacific Ocean (**Figs. 1 and 2**), so far as confirmed. Their thicknesses are still unknown due to the limited penetrations less than ca. 500 m, but, for instance, the edifice-building complex underneath the Ontong Java Plateau [OJ] in **Fig. 1** attains presumably to several kilometers thick (Tarduno et al., 1991).

Volcanisms in this stage were differentiated into two styles, which are related closely to the genesis of topographic features (**Fig. 1**). One is the flooding volcanism that has erupted MORB-type basalt flows in oceanic basins, and another is the edifice-building volcanism that has piled up OIB-type basalts to form oceanic plateaus and seamounts. For instance, Larson and Schlanger (1981) depict these two styles of volcanism in the Nauru Basin (**Fig. 1** [NR] and **Fig. 2**) as below;

After the postulated Jurassic magmatism forming the oceanic crust of the basin (**Fig. 3-1**), the Aptian flood basalts (MORB-type) covered the floor of the Nauru Basin and the Albian edifice-building basalts (OIB-type) formed the Ontong Java Plateau on the southwest and the Kwajalein Atoll in the Marshall Islands [MS] on the northeast (**Fig. 3-2**). It is noticeable that the flood basalt lavas and associating sills of MORB-type have outpoured not at mid-oceanic ridges, but in oceanic basins (**Fig. 1**), e.g., in the East Mariana [EM] and southeast Pigafetta [PF] Basins (Abrams et al., 1992), the Nauru Basin [NR] (Larson and Schlanger, 1981), and the Central Pacific Basin [CP] (Winterer, 1973). The relative abundance of volcaniclastic turbidites in this stage (**Fig. 2**) indicates that many oceanic islands and some huge plateaus have formed in this stage, with carbonate reefs (e.g., **Fig. 3**). Thus, the late Early Cretaceous voluminous volcanic rocks (**Figs. 1** and **2**) could mark a climax of the late Mesozoic magmatic activities in the West Pacific Ocean.

Incidentally, in the Pigafetta, East Mariana, Nauru and Central Pacific Basins (**Fig. 1**), the MORB-type flood basalts and associating sills appear to have outpoured through feeder systems crowded in underlying presumable Jurassic sediments and oceanic crust (e.g., **Fig. 3-2**). The crowded feeders are inferred from that the basalt lavas drilled are generally the piles of thin flows, tens centimeters to a few meters thick, with many intricate chilled-margins (Shipboard Scientific Party, 1981a), eliminating the possibility of long-distant travels (Larson and Schlanger, 1981). The late Early Cretaceous flood basalts are confirmed to hold a uniform, normal magnetization obtained during the Cretaceous normal polarity superchron (Steiner, 1981). On the other hand, rather well lineated Middle to Late Jurassic "M-sequence" magnetic anomalies are identified in the basins.

If these are the case, two paradoxes are to arise (Larson and Schlanger, 1981): 1) Why are not the Jurassic magnetic lineations annihilated by the overlying late Early Cretaceous normally-magnetized flood basalts? 2) Why did not the regional and voluminous late Early Cretaceous magmatism, which occurred through crowded feeders in the underlying Jurassic oceanic crust, decay and destroy the Jurassic magnetic lineaments, thermally and structurally? In spite of the partial interpretation on the Phoenix set (M17-M29) by Nakanishi et al. (1992), the carrier(s) of the magnetic lineations should have still remained in the unsolved paradoxes

Late Cretaceous

Of the Late Cretaceous volcanic intervals, OIB-type volcanic rocks are predominant and volcaniclastic turbidites are relatively abundant (**Fig. 2**). These facts indicate that the final stage of the late Mesozoic magmatic activities is characterized by active edifice-building volcanism. These intervals are found mostly in the Mid-Pacific Mountains, the Line Islands and the Marshall Islands, not in northwestern marginal areas (**Fig. 2**).

Around the end-Cretaceous, edifice tops became covered with deep-water sediments or rapid-growing reef complexes, reflecting the beginning of regional and long-term subsidence (**Fig. 3-3**: Schlanger et al., 1981; Larson and Schlanger, 1981). Remnant edifice-building activities continued locally until e.g., the Eocene at Eniwetok (Schlanger, 1963) and the Pliocene at the Kusaie Island in the Nauru Basin (**Fig. 3-3**).

The predominance of OIB-type volcanism in the Late Cretaceous suggests a decrease in partial melting degree and/or an increase in segregation depth, indicating the cooling of source mantle. The rapidly declining volcanism and regional subsidence since the end-Cretaceous (e.g., **Fig. 3-3**) might be hence attributable to the subsequent general cooling of the mantle (Yano et al., 2001).

Paleobathymetric models concerning with the Darwin Rise

The height of a guyot as measured from the surrounding regional sea floor to the volcanic slope break at a wave-erosion surface records the sea level when the guyot has begun to submerge. Thus, guyot heights represent the paleodepths of surrounding ocean floors. The present depths of wave-cut surfaces on guyots are equivalent to the total amounts of subsidence since submergence. In case guyots on a regional swell experienced the subsequent flattening of the swell, a sag-shaped distribution of guyot-depths is expected (**Fig. 4**: Smoot and King, 1997).

Hereafter the terms "guyot-height" and "guyot-depth" are used to designate the height and depth of wave-cut surface on a guyot, not those of sediment surfaces. Concerning with the Darwin Rise, three bathymetric models are known;

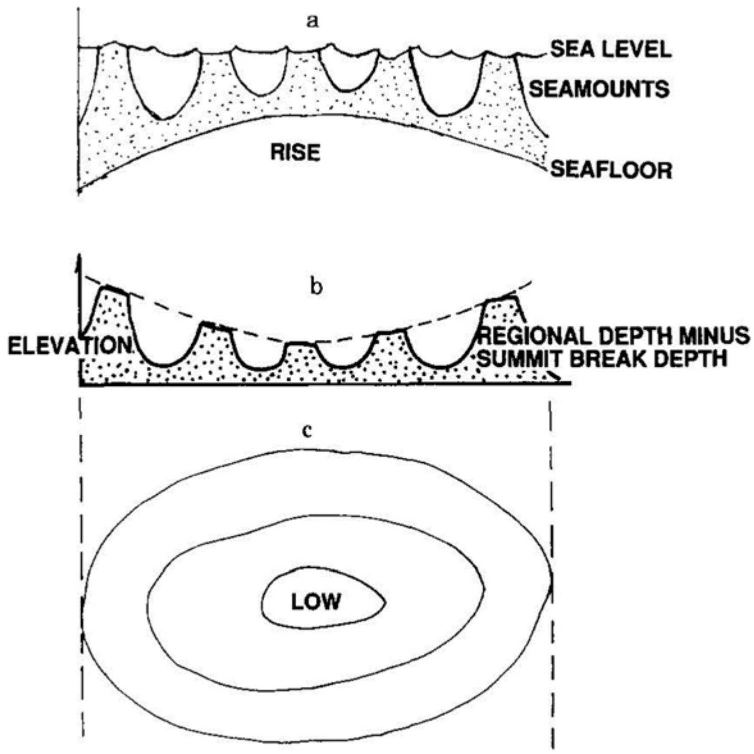

Fig. 4 Illustration for the subsidence mode of the Darin Rise (Fig. 1 of Smoot and King, 1997: cited with a permission of the publisher); (a) guyot heights were initially smaller in the center of than on the margin of the rise, (b) a remnant topography, and (c) a difference between summit plateau break depths and a regional depth.

Speculative model based on Menard (1964)'s Fig. 6.14

Menard (1964) presents a guyot-depth data in the whole Pacific Ocean, including the Northeast and Southeast Pacific Ocean (see his Figure 6.14 for details), without depth contours. In **Fig. 5** with contour lines, though being highly speculative, a huge sag-shaped subsidence of NW-SE trend in the West Pacific looms in the probable extent of the Darwin Rise. Additional possible features are the relative uplift in the area of the East Pacific Rise and the northwest and southeast marginal subsidence along the trenches at present (**Fig. 5**). While the guyot-depth data and the contouring should be, as a matter of course, revised fundamentally, this model could represent a prototype for the distribution of cumulative subsidence of the Pacific Ocean basin since the late Mesozoic.

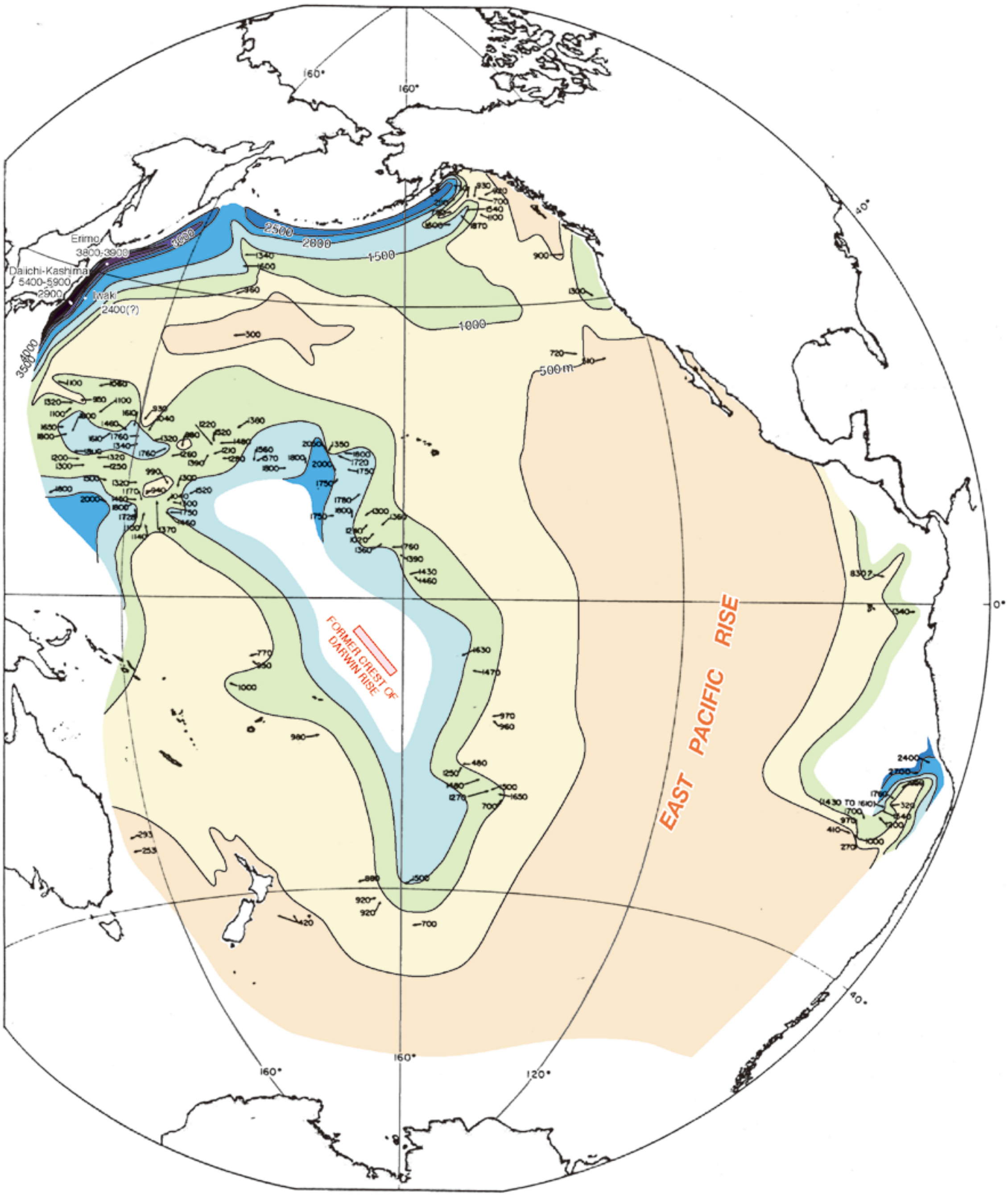

Fig. 5 Speculative contour lines added on the guyot depth data in the Pacific Ocean (after Fig. 6.14 of Menard, 1964) with additional guyot depths: 5,400-5,900 m and 3,900 m at the Daiichi-Kashima Seamount (Research Group for Daiichi-Kashima Seamount, 1976; Research Group for the Daiichi-Kashima Seamount, 1985; Cadet et al., 1987; Shiba, 1988; Konishi, 1989), 2400(?) m at the Iwaki Seamount (Masalu et al., 2001), and 3,800-3,900 m at the Erimo Seamount (Cadet et al., 1987; Dubois and Deplus, 1989).

Menard (1979)'s net subsidence model

The concept of the Darwin Rise (Menard, 1964), since its birth, has been controversial with that of the secular cooling subsidence of oceanic plates (e.g., Hess, 1962; Mckenzie, 1967). Menard (1979) once abandoned his own hypothesis, but Menard (1984) reprised back to that on the basis of the fact that the late Mesozoic extensive magmatic activities had occurred over the northwestern two-third of the Darwin Rise (e.g., Larson, 1991a: **Figs. 1** and **2**). When reprising, the dimensions of the rise was much reduced to an extent 1,000km wide, 2,000km long and 1.0-1.6 km high within central Micronesia, on the basis of the WNW-ESE trending sag-shaped net subsidence shown in **Fig. 6A** [colored in orange].

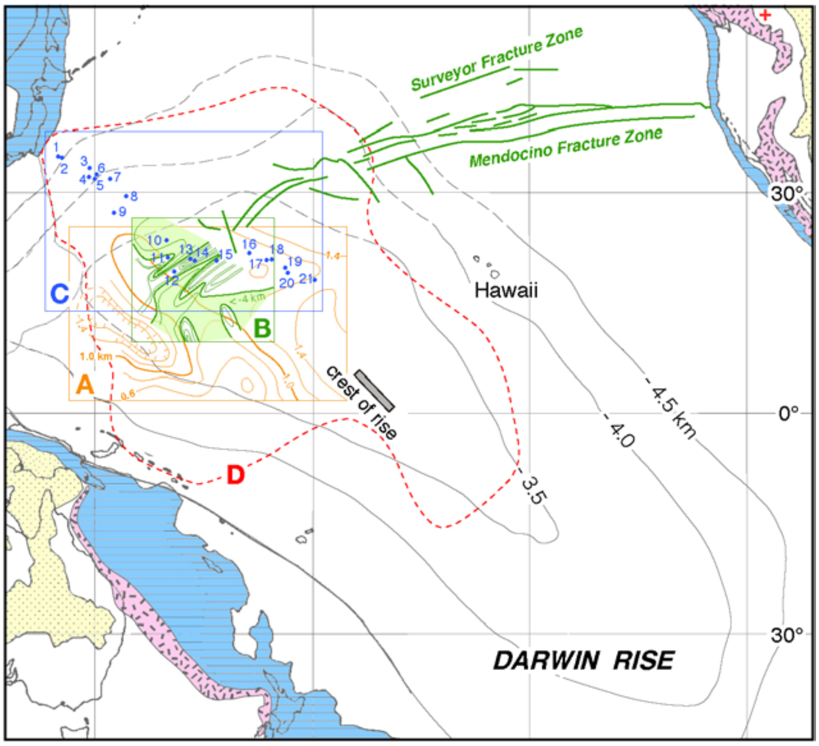

Fig. 6 Data sets A to D concerning with the Darwin Rise, superimposed on a part of **Fig. 1**. **A** [colored in orange]: the distribution of net subsidence of ca. 100 Ma guyots (after Fig. 4 of Menard, 1984). **B** [colored in green]: the contour map of remnant regional depth for 80-100 Ma guyot to the southwest of the Surveyor and Mendocino Fracture Zones (after Smoot and King, 1997: the contour interval 0.2 km and the shaded areas deeper than -4km). **C** [colored in blue]: the locations of the guyots with post-Alibian karstic unconformities (van Waasbergen and Winterer, 1993) , 1: Takuyo-Daini, 2: Takuyo-Daisan, 3: "Winterer", 4: "Charlie Johnson", 5: "Stout", 6: "Thomas Washington", 7: Isakov, 8: Makarov, 9: MIT, 10: "Scripps", 11:"Lamont", 12: "Pot", 13: "Vibelius", 14: "Wilde", 15: "Woods Hole", 16: Darwin. 17: "Heezen", 18: Resolution, 19: "Caprina", 20: "Jacqueline", 21: Allison. **D** [colored in red]: the extent of the recognized distribution of late Mesozoic volcanic rocks (see **Fig. 1** for details).

Smoot and King (1979)'s linear model

Smoot and King (1997) depicted a contour map of 60 guyot-depths in eastern Micronesia (the rectangular area bounded by 147°E to 173° and 10°N to 27°N: **Fig. 6B** [colored in green]) through detail bathymetric analyses. They affirmed the demise of the Darwin Rise, because the NE-SW linear pattern characteristic of their map is completely different from the sag-shape illustrated in **Fig. 4** and also incompatible with the NW-SE general trend of the Darwin Rise.

Demised or revived?

Did the Darwin Rise demised or revived? Let's consider here this issue based on (1) the above three paleobathymetric models proposed to the late Mesozoic West Pacific Ocean (**Figs. 5** and **6**) and (2) the late Mesozoic regional volcanism occurred in the West Pacific Ocean (**Figs. 1** and **2**). The reason is that the Darwin Rise hypothesis (Menard, 1964) is made up of the two components; a huge submarine swell and contemporaneous regional magmatic activities.

Demise of Darwin Rise

The demise of the Darwin Rise affirmed by Smoot and King (1997) appears to be not adequate for the four reasons;

Un-unique contouring
Yano (2014), showing another possible contour map of NW-SE trend (his Fig. 13) depicted from the guyot-height data of Smoot and King (1997), pointed out: “Their contouring appears to have attached too much importance to the ENE-WSW trend of the Surveyor/Mendocino Fracture” and “Anyhow the both are not the unique solution”.

In spite of the Smoot (2014)’s comment to my above pointing, their bias to the Surveyor/Mendocino Fracture is clear as shown with their own statements: “Once the heights were plotted, contouring them became a simple matter”, but also “The most recently synthesized lineations (Nakanishi et al., 1992) were used in part to guide the contouring effort” (both sentences in p. 228). This means that their contouring has needed the “guide” of magnetic lineations at least “in part”, although the “M-sequence” magnetic lineations must be the parameters independent from guyot heights and the carriers of the lineaments are uncertain (Larson and Schlanger, 1981) as mentioned earlier.

Mismatch of contouring area
Smoot and King (1997) stated: "The restricted geographical area is addressed in this study (Fig. 2)". It is clear that their "The restricted geographical area" indicates the central Micronesian sag-shaped area of the Menard (1979)’s net subsidence map (**Fig. 6A** [colored in orange], see p. 224 of Smoot and King for details). The contour map of Smoot and King (1997) (**Fig. 6B** [colored in green]) has nevertheless a little overlap with "The restricted geographical area" of the Menard (1979)’s map, making it almost impossible to compare with each other. The Smoot and King (1997)’s map also does not cover the extensive area of the original Darwin Rise (**Figs. 1 and 6**). Hence, the item possible to be compared is restricted only to the general trend of the depth contours. Their contouring however seems to be biased and not unique as mentioned above, and other cases of contouring are possible (e.g., fig. 13 of Yano, 2014). The demise assertion by Smoot and King (1997) is hence not definitive and is required to assure the uniqueness of their contouring and to enlarge the area of their guyot-hight map at least to the extent of the recognized distribution of late Mesozoic volcanic rocks (**Fig. 6D** [colored in red]).

End-Aptian regional uplifting
Reefal limestones on the 21 guyots distributed in an extent of ca. 5,000 x 1,500 km from the Takuyo-Daini Seamount to the Mid-Pacific Mountains (**Fig. 5C** [colored in blue]), obliquely across the Smoot and King (1997)'s contouring area (**Fig. 6B** [colored in green]). The limestones have common karstic unconformities formed during the post-Albian/pre-Turonian time (McNutt et al., 1990; Winterer and Sager, 1995; van Waasbergen and Winterer, 1993, Winterer, 1998). The unconformities indicate a regional fall of the relative sea level ca. 200 m (e.g., **Fig. 7**) and are attributed to a tectonic uplifting because of the absence of equivalent eustatic falls during the time. This end-Albian regional uplifting thus suggests the tectonic unity of and the huge-sale systematic tectonic control on a northwestern extensive area of the West Pacific Ocean. The regional uplifting appears to have marked the final climax of the mid-Cretaceous tectono-magmatic activization (**Fig. 2**) of the alive, not demised, Darwin Rise.

Ignorance of late Mesozoic regional volcanism
Smoot and King (1997) and Smoot (2014) have ignored one of the two components of the Darwin Rise hypothesis (Menard, 1964), i.e., contemporaneous regional magmatic activities. The actuality of the late Mesozoic regional volcanism in the West Pacific Ocean seems to have become evident with numerous rocks drilled and dredged, except for on the southeastern one-third of the Darwin Rise (**Figs. 1 and 2**). It should be remember that the declaration of the rehabilitated Darwin Rise (Schlanger et al., 1981) has been accorded to rock evidences, not to paleobathymetric data. The Darwin Rise hypothesis has thus obtained the substantial backup of numerous rock evidences. It is inevitably required to certify the demise assertion of Smoot and King (1997) and Smoot (2014) that they deny the very rock evidences, or revise their model to be able to explain the contemporaneous, regional magmatism spread over the West Pacific Ocean.

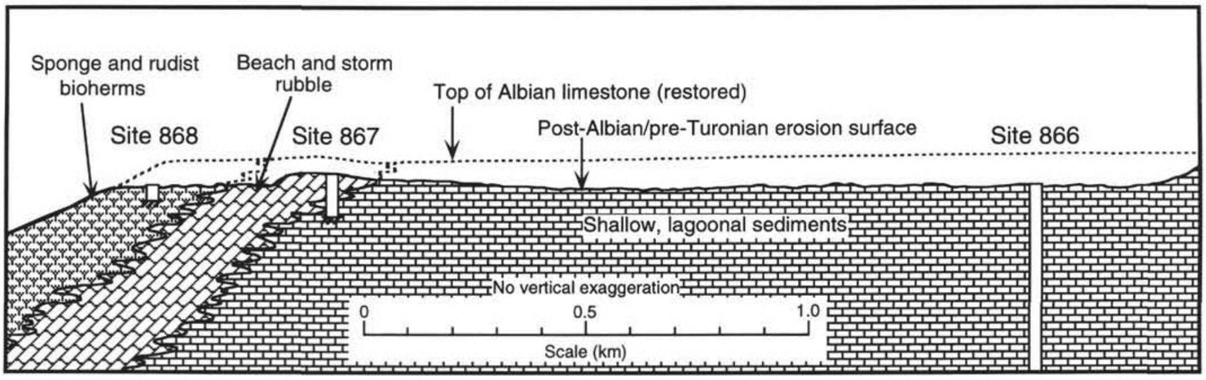

Fig. 7 Post-Albian/pre-Turonian erosional interval on the Resolution Guyot, the Mid-Pacific Mountains (from Winterer and Sager, 1995). See **Fig. 6**-18 for location. The thickness of eroded limestone is estimated from the remnant layers near the center of the guyot (see Figures 12 and 13 of Winterer and Sager, 1995).

Revival of the Darwin Rise

It appears to be conceivable that the voluminous volcanic rocks spread over an extensive area on the order of 10^7 km^2 (**Fig. 1**) in the West Pacific Ocean and range for 55 m.y. at least, or for 90 m.y. if the activity started in the Middle Jurassic (**Fig. 2**). Such synchronously widespread and long-lived volcanism (**Fig. 3**) is not compatible with Morgan's (1972) hot-spot concept (Schlanger and Premoli Silva, 1981; Larson and Schlanger, 1981), as well as with volcanism along mid-oceanic ridges. Hsü and Schlanger (1968) attributed such regional volcanic activities to major changes in the geothermal gradient within the subjacent upper mantle, probably originated from a superplume (Larson, 1991a, b). Thus Schlanger et al. (1981), as mentioned earlier, declared the revival of the Darwin Rise with numerous rock evidences.

The issues to complete the revival are (1) the drilling and dredging on the rest one-third and (2) the precise and overall reconstruction of the paleobathymerty of the late Mesozoic Pacific Ocean basin with updated data. The both issues, being not easy to be accomplished, will promise a considerable advancement in our study on the origin of the Pacific Ocean, as attempted and intended by Menard (1964) a half century ago.

Concluding Remarks

Reviewing the late Mesozoic volcanic rocks drilled and dredged in the West Pacific Ocean (**Figs. 1** and **2**) and the paleobathymetric models concerning with the Darwin Rise (**Figs. 5** and **6**), this paper discussed whether the Darwin Rise hypothesis (Menard, 1964) demised or revived and made the latter much conceivable. The issues to clarify the more detailed life history of the Darwin Rise are geological surveys on the southeast one-third of the rise, superdeep drillings down to upper mantle, paleobathymetric studies of the whole Pacific Ocean basin, and interdisciplinary syntheses among geology, geomorphology, geo-physics and -chemistry.

Probably before not so long, an overall revision on the origin and history of the world oceans will become inevitable due to the recent accumulation of the submarine rock evidences including the ancient and continental rocks found over 200 localities in the world oceans (e.g., Vasiliev et al., 2012). The late Mesozoic paleobathymetry and tectono-magmatism in the Pacific Ocean discussed above will be much more weighted during the coming revision.

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(* in Japanese; ** in Japanese with English abstract; ***in Russian with English abstract)

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REPLY TO YANO

SISYPHUS AND THE DARWIN RISE

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Abstract: Superswells have been in the literature ever since the advent of the plate tectonic hypothesis, which was based on little or no bathymetry. With 72% of Earth's surface covered by the sea/oceans, and with continental crust not having the option to subduct, then that idea was based on dreams. This is exactly what the Darwin Rise is-a dream feature. Superswells caused by huge magma plumes in the lithosphere have been proven not to exist by various researchers. The advent of total coverage bathymetry in the North Pacific allows one to investigate whether the Darwin Rise ever existed, or does so now. Actual charts, used for interpreting the guyot heights and trends in addition to the westward passage of the Mendocino Fracture Zone, refute the idea of any kind of superswell in the central Pacific basin. The intersecting megatrends refute the idea of large block rotations.

Keywords: *superswell, Darwin Rise, bathymetry, GEOSAT, megatrends, Pacific Ocean*

INTRODUCTION

The Darwin "Rise" (DR) has long been the subject of much speculation, beginning with Menard (1964 and 1984) and continued (McNutt et al., 1990). In 1997 (Smoot and King) put the nail in that coffin. In 2000 I reiterated the problems with the DR idea again only to discover a revival by Takao Yano (2014). While the dimensions of the superswell diminished with each investigation, Yano seems to have gone for Menard's entire feature again. His interpretation (see Yano, 2014a and 2014b) is the basis for this discussion as Sisyphus tries to push that rock up yet another hill.

Before the advent of relatively decent bathymetric maps in the late 1970s, this large feature (10,000 km long by 6000 km wide) was hypothesized to swell up to 2 km in height and be home to many of the mid-Pacific seamounts. The current summit is listed at 2400-3000 m, and the regional base depth is 5500 m. Many seamounts/guyots and rises reside in that region of the NW Pacific basin between 0^{o}N to 40^{o}N latitude and 140^{o}E to 170^{o}E longitude, with inclusive NW—SE dimensions spanning 45^{o}N to 40^{o}S up to 120^{o}E to 115^{o}W. In other words, the DR is the entire central Pacific basin.

Thought to have been formed at an extinct MOR, the DR includes the region from the Tuamotu Archipelago on the south through the Line Islands and Marshall-Gilbert Seamounts north to the Mid-Pacific Mountains and Geisha Guyots, and west to the Magellan Seamounts and the western Pacific trenches. Created during the Great Cretaceous Outpouring, hundreds of seamounts are thought by some to sit atop the Darwin Rise. This could have been a function of the available bathymetry at that time.

OCEAN FLOOR AGE

Yano cites many of the DSDP/ODP volumes in his paper. In order to ground truth the magnetic anomalies used to help formulate the plate tectonic hypothesis, an effort called the Deep Sea Drilling Project (DSDP) was begun in 1968 with the ship Glomar Challenger. The idea was to collect, and determine the age, of enough oceanic rock samples to prove seafloor spreading. In 1983 this effort was stopped. The idea was expanded to include international cooperation and started again in 1985 with a new ship, the JOIDES Resolution, this time called the Ocean Drilling Program (ODP). This effort was continued until 2004 (Leg 207) when it evolved into the Integrated Ocean Drilling Program (IODP). I looked at every site description, so I know the following is an accurate assessment, not by me, but by the principal investigators themselves:

The Shipboard Scientific Party on ODP Leg 179 wrote in 1999: “Since the inception of the Deep Sea Drilling Project (DSDP) and its successor, the Ocean Drilling Program (ODP), one of the principal objectives of the science community has been to penetrate an entire section of oceanic crust to reach the crust/mantle boundary. This objective has proven to be a difficult engineering task from the initial efforts of ocean drilling to the present. Attempts to initiate and drill holes in young, highly fractured basaltic rock with little or no sediment cover have proven unsuccessful (e.g., Legs 106, 109 and 142). However, efforts to initiate deep basement holes with considerable sedimentary cover have proven successful in yielding more stable holes. For example, Hole 504B reached a depth in excess of 2100 m before the hole was lost to further penetration. Approximately two-thirds of the crust below, however, remained unsampled, as did much of the drilled crust because of low recovery.”

Following this and other attempts at total crustal penetration, new strategies were adopted based on successful drilling in tectonic windows into peridotite during Leg 109 along the Mid-Atlantic Ridge near the Kane Transform (MARK Area, 1988) and into gabbroic rocks during Leg 118 near the Atlantis II Transform on the Southwest Indian Ridge (SWIR, 1989). With the exception of Hole 735B drilled during Leg 118 and 176 (1508 m of total penetration), which was initially spudded using a guide base on an highly unusual wave-cut platform along the Atlantis II Fracture Zone in the Southwest Indian Ocean, other ODP igneous legs conducted in young terrane, where there is little to no sedimentary cover, have been successful only in penetrating and coring to about 200 meters below the seafloor (mbsf) or less. They have also been characterized by far less recovery than experienced during Legs 118 and 176. Experience gained from the two legs that directly followed the inception of offset drilling strategy, Legs 147 (Hess Deep, 1993) and 153 (MARK region, 1995), indicates that the current hard-rock guide base design is not optimal for establishing boreholes in fractured igneous rock environments with moderate slopes and little or no sediment cover. These are typical environmental conditions for mid-ocean ridge and fracture-zone drilling targets. Thinly sedimented slopes that are covered with debris or rubble are also commonly encountered in young terrane at mid-ocean ridges. These areas have proven especially problematic from an engineering point of view because of the difficulties in keeping a stable open hole for deepening.

Even with these formidable engineering and technical problems, the scientific objective of total crustal penetration or developing a composite section by drilling offset holes in the ocean crust remains one of the highest priority thematic objectives of ODP. Therefore, new hardware and techniques need to be developed to establish boreholes in these environments to meet the scientific objectives of hard-rock drilling. The tool with the most promise of dramatically increasing ODP's ability to establish a borehole in hard-rock environments is the hammer drill-in casing system (HDS).

Interestingly, in a word from the then-reigning manager (1998) of the ODP, Tom Davies of TAMU: “…indeed, we have yet to find rocks older than 200 MA in deep oceans. These observations lead to a rapid acceptance of the plate tectonic hypothesis.” Really?

According to the Shipboard Scientific Party (2004), “prior to Leg 203, only three holes had been drilled during ODP/DSDP with penetrations >100 m in such "normal" Pacific crust. The Leg 203 coring and logging goals were intended to add to the limited inventory of baseline data about this understudied, yet common lithospheric setting.”

In fact, Leg 209 (2007) is the only successful leg to collect mantle peridotites, and that was from the crest of the Mid-Atlantic Ridge. It seems that chert was the problem heretofore.

A brief summary of the 1276 sites reveals that many pillow basalts, dikes, and volcanics were recovered. These were predominantly on large, igneous provinces (LIPs) or on-ridge. The only deep-ocean site, 801, did not reach basement. It did give an age of 151 Ma. Therefore, the use of DSDP and ODP information is only good for age determination down through the sediment layer. Not one drillsite has ever reached oceanic basement. What this also means is that the magnetic anomalies remain undated as they have not been ground-truthed by solid data.

As a point of age/composition reference, that has been addressed many times in the Newsletter/Journal, the first time by Choi and others (1992) and Dickins and others (1992). Paleozoic continental crust seems to be the consensus with later basalt emplacement and covered by marine sediments and coral caps.

Because of the presumed age, a large amount of sediment deposition has taken place. Theoretically, the first 1000 m on the DR are sediment. Removing the sediment cap, the actual swell was on the order of 1500-2000 m high. Of course, the seamounts and guyots are covered by sediment caps too, most of which have not been sampled. Depending entirely upon the age differences, the sediment regime is also ruled by both the northward flowing North Pacific Deep Water and the Kuroshio currents and their effects on the thickness of the sediment caps. For this exercise, we will assume that the guyot heights would remain relatively close to what I have determined no matter that depth.

EVOLUTION OF THE DARWIN RISE

Theoretically a large bulge first formed in the mantle so that longitudinal ridges and troughs formed atop this mantle plume. Volcanic buildups formed on the flanks to form paleo-islands about 110 Ma. Eventually volcanic activity ceased, and isostasy took over as the rise subsided. Bill Menard and Marcia McNutt (1964; 1984, 1990) had the ear of the investigative community from 1964 to 1990, saying that the guyots formed on 40 Ma crust, making this region on the order of 140 Ma. As previously noted, not enough information was gathered from the DSDP/ODP efforts to make that determination. The entire region is surrounded by the M20 magnetic chron (148 Ma). That is not ground-truthed either. In fact, at the time of the original idea, this region was in the Jurassic Magnetic Quiet Zone. Nobody had any idea what the actual age of any of this was.

A series of hotspots entered the picture, and a thallasocraton was proposed, but the Darwin Rise continued to be the explanation. A comparison of the results to the John Woodhouse and Adam Dziewonski heat flow diagram (1984) showed that the Darwin Rise region had no higher heat beneath it than any other place of elevated heat flow worldwide. Also, the entire region proposed for this exercise is in the positive gravity geoid region.

A large mantle plume seems to be one of the causes of the superswell (Yano, 2014). Large mantle plumes have been relegated to the trash bin of the excesses of the 1970s and 80s (see any of Don L. Anderson’s papers from the 1990s forward). McNutt (1998) thinks that what is under a superswell is melt rich, but that it is a hot layer above the transition zone and not a mantle plume. Careful there, Marcia, this starting to sound like heated channels.

Therefore, the only concrete information available for a study of the Darwin Rise is the gravity/GEOSAT altimetry data and comparative bathymetry. The bathymetry is only total coverage for the northern provinces included in the proposed Darwin Rise because dedicated bathymetric surveying apparently stopped in 1993 when the US Navy changed tactics to survey brown water areas. All other multibeam surveys only gather the postage stamp information without surveying the entire envelope.

As the GEOSAT does not show regions of elevated heat flow, one must then look at the structural features. The structural diagram of the Pacific basin based on the GEOSAT data (Leybourne and Smoot, 1997) shows the megatrends going through the Darwin Rise region. The 5500 m contour surrounds the southern features in the Darwin Rise region, the Magellan and Manihiki rises and the Ontong-Java Plateau for example (**Figure 1**). Thus, Yano’s 3500-m contour does not exist as such.

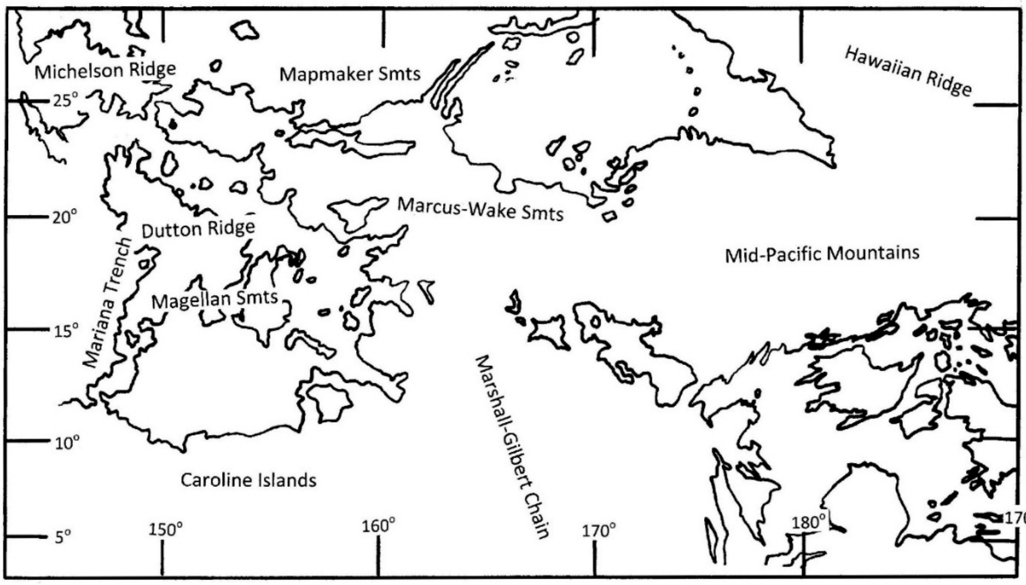

Figure 1. This figure is based on the DBDB-5 1978 bathymetric atlas compiled by SIO. I have traced the 3000 fm/5500 m isobaths through the LIP region for the Darwin Rise. This regional base depth predominates.

The Chinook (Smoot, 1995), Mendocino (Smoot and King, 1997), Molokai and Murray (Smoot, 1997 and 1999), Clarion, and Clipperton (Joseph et al., 1993; Smoot, 1994 and 2005) fractures/megatrends all pass through the region on a WSW-ENE azimuth. The Central Pacific Megatrend (Smoot and Leybourne, 2001) passes through the equatorial region from WNW-ESE. Added to that configuration, the Udintsev, Kashima/Eltanin, Mamua/Tubuai, and Emperor/Easter megatrends (Smoot, 1997) all pass through on a NNW-SSE azimuth (**Figure 2**). These fractures are all undated. Nevertheless, with few offsets along any of the trends, the ocean floor has remained in a fairly constant state for the duration of the fracture activity. Some of these fractures actually continue ashore, and they are dated at Paleozoic at the very least (Smoot and Choi, 2003). Because fracture zones have been repeatedly shown not to show the direction of plate movement because of these intersections, and because they are not offset by each other, and because investigators have determined the Pacific Ocean floor to be in the neighborhood of 600 Ma, then the guyots themselves coupled with the multibeam bathymetry are the determining factor in this exercise. The fact that almost no offset occurs in the cross-basin megatrends leads one to believe that any kind of block rotation for the entire basin since the formation of the fractures, whenever that may one-day prove to be, is negated.

By the 1990s, NAVOCEANO had surveyed enough of the region to make a more definitive study. The idea of a basin-crossing Mendocino/Surveyor fracture swarm was in its infancy. Flank rift zones on many of the features pointed to the WSW (Smoot, 1989), and traces of fracture zone ridges crossed way to the west of the proposed end of that fracture zone into the Marcus-Wake Seamount group. I had performed a study of the guyot heights, so I continued that study into the Marcus-Wakes all the way to the Mariana Trench. The study found 60 guyot heights, ranging from 4387 m to 3290 m. The summit plateau break depths range from 600 fm (1100 m) to 1300 fm (2380 m). The initial premise was that, if the guyots formed essentially coevally on a rise/swell, and the rise/swell subsided equally, the guyot heights, having been islands and eroded coevally, the guyot heights in the center of the rise would be less than those on the flanks of the rise, and the summit plateau break depths would lie deeper in the ocean. With these heights concentric circle contours would have been a reality. It was not.

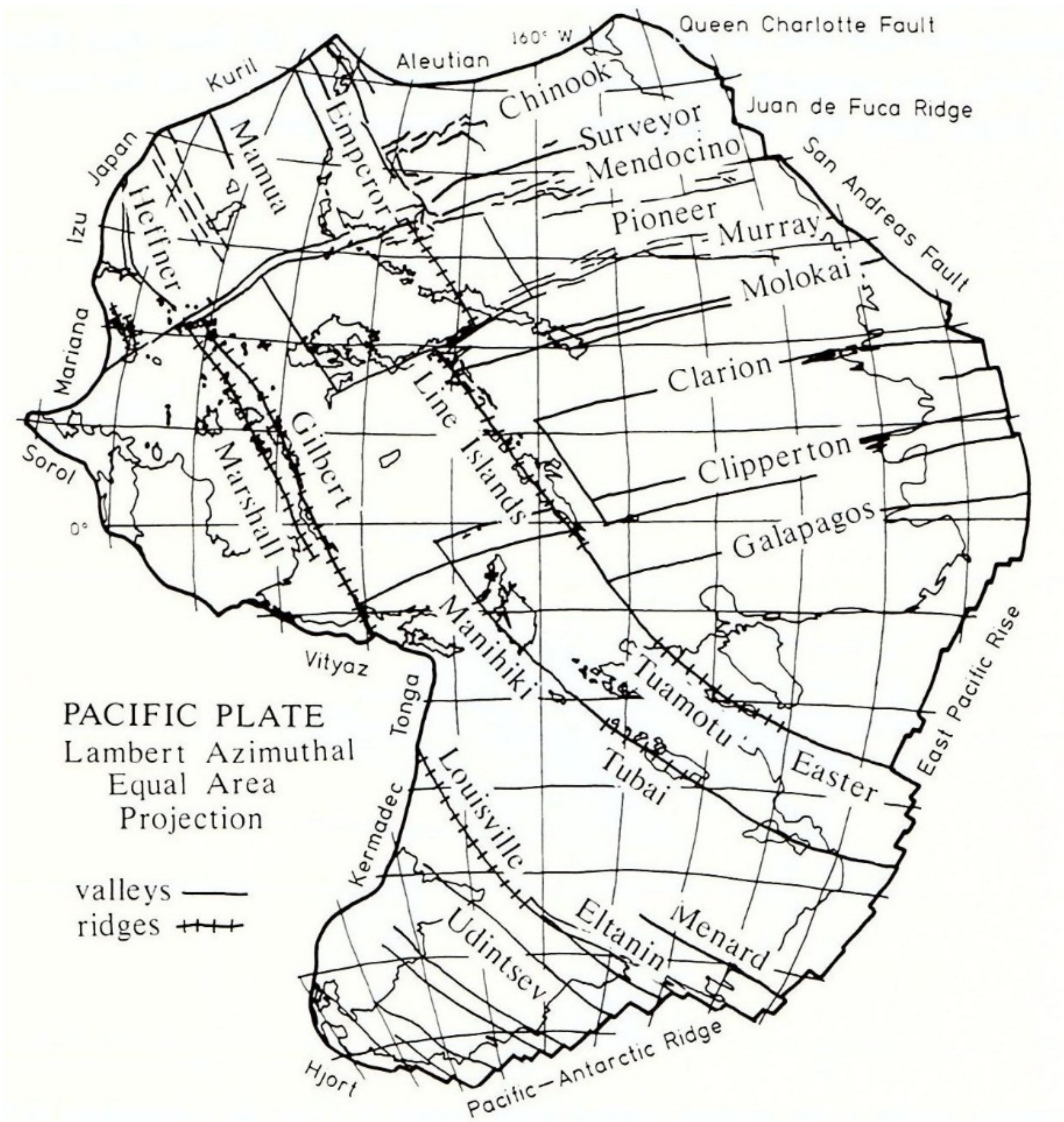

Figure 2. By 1988 enough good bathymetry was in place to draw this diagram of the major trends in the Pacific basin on a Lambert equal area projection. By ignoring the 43-Ma "bend" while realizing that many of the seamount chains were extensions or parts of the fracture zones, and continuing the lineaments, it was possible to see that the lineaments intersected orthogonally. Still the data were ignored, and it became increasingly difficult to get that information published in mainstream journals. Later constructions based on the GEOSAT data (Leybourne and Smoot, 1997) show this in much more detail.

As can be seen in the enclosed diagrams (**Figures 3 and 4**), rather than the concentric circles (contours) one would expect had guyots in this region formed coevally on an existing rise, one finds what appears to be a ridge/trough feature passing through the region on a WSW azimuth. This is exactly the appearance and strike of the incoming Mendocino Megatrend, and it continues all the way to the trench. Rather than a huge high at 3500 m, the 5500 m contour interval winds all through the proposed high plateau north of the equator (**Figure 1**). Had this been huge swell of 2000 m, all of the guyots would have summit plateau break depths had they all formed during the Great Cretaceous Outpouring. They do not. In fact, most would have been at least 1000 m above sealevel when they were flattened. By definition, guyots are flattened mostly by wave cutting and subaerial erosion.

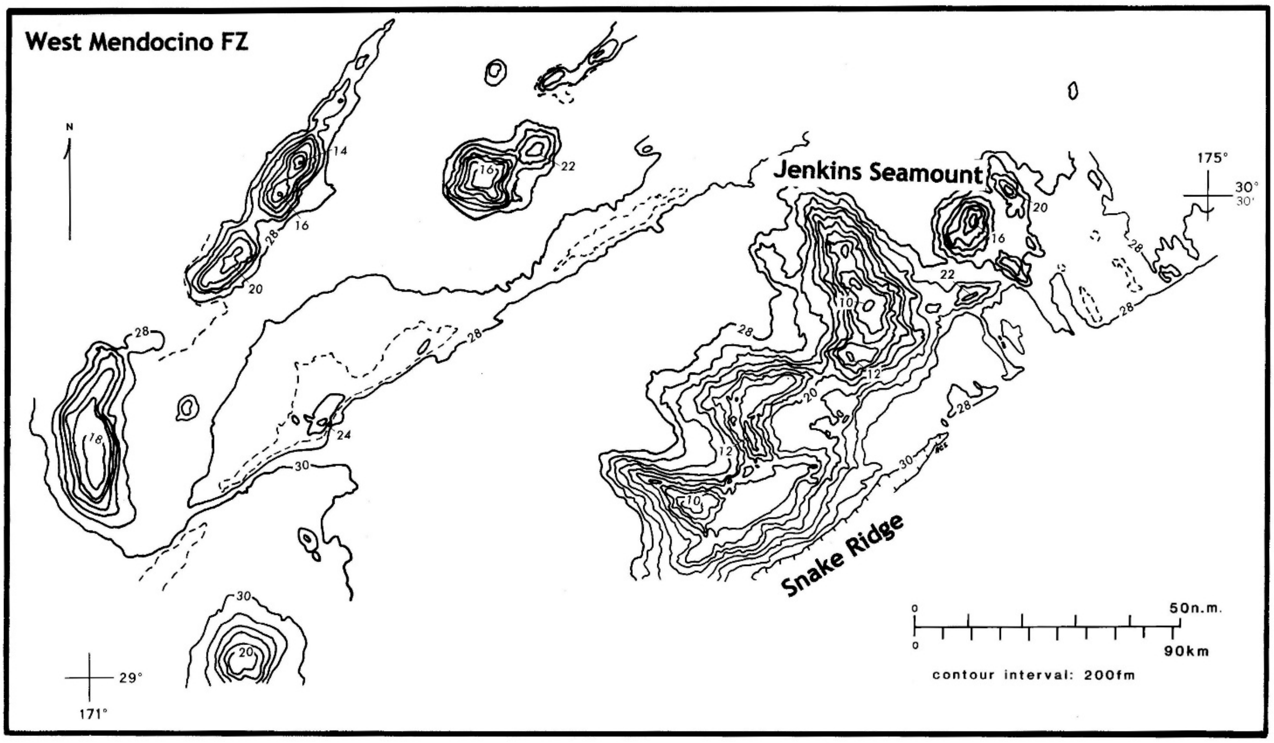

Figure 3. Total-coverage multibeam sonar bathymetry of the western extension of the Mendocino Fracture Zone double trace at a 200-fm contour interval. The Snake Ridge bathymetry has all the appearance of a linear ridge after having been pushed together by compression forces.

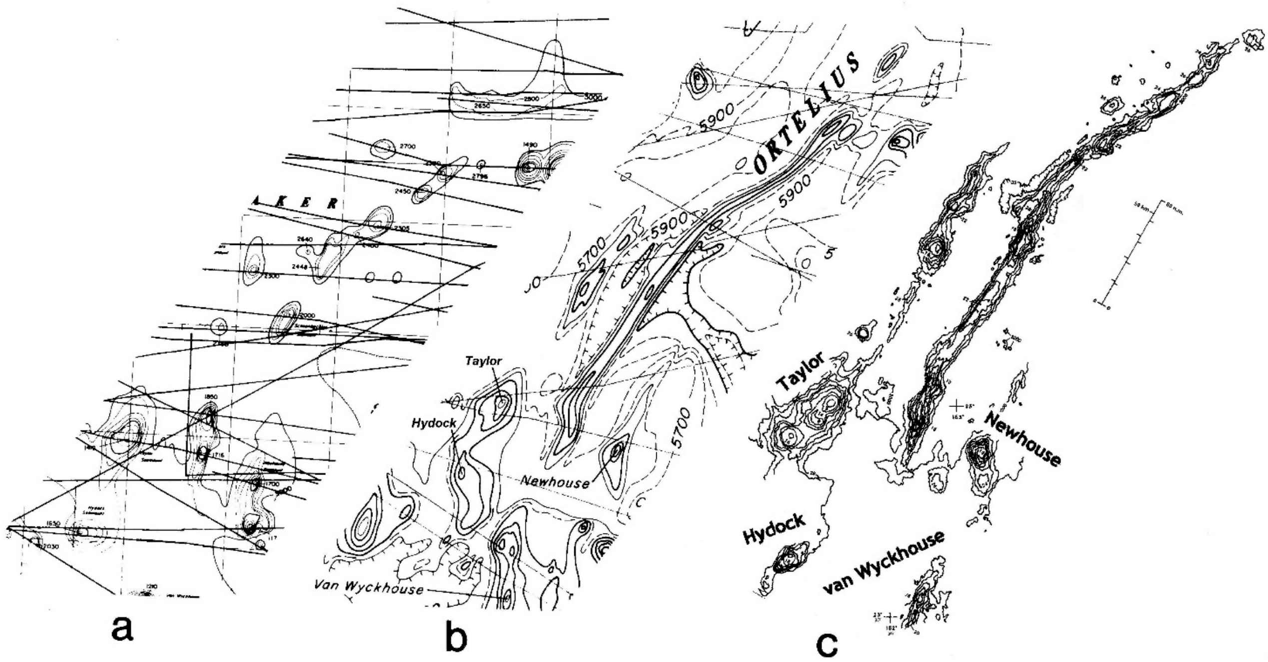

Figure 4. This diagram explains the fallacy of drawing circular contours. The Mapmaker Seamounts were "discovered" using this method (a). A later interpretation of that data showed the passage of the Ortelius Fracture Zone (b). The total coverage package provided by SASS dedicated surveys over the same region reveals the passage of two forks of the Mendocino Fracture Zone (c). This feature lies in the middle of the north-central Darwin Rise and is the start of the contouring effort using the guyot heights.

The main problem with Yano's paper is not poor scholarship, it is the fact that he has ignored the total coverage bathymetry of the two linear ridges crossing the entire basin and going through his proposed Darwin Rise region in the north. I include those features with a reminder that real data cannot be ignored, or shoved under the carpet, to suit one's needs. These kinds of things will always come back to haunt the investigator who is not careful.

Therefore, the DR does not now, nor has it ever, existed. It is a “dream feature,” the proposed “facts” become a null hypothesis. The mere fact that the multibeam bathymetry shown in Smoot and King is accurate and 100% totally covered should be enough to satisfy even the most myopic of researchers. If you want to contour circles, be a guest of mine. If you want to use inaccurate age data, be a guest of mine. But, don’t pour water down my back and tell me it is raining outside.

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– *ROTATION OF AUSTRALIA* –

The wrench tectonic history of Greater Australia: Further substantiation of evidence

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Synopsis: In a recent article in this journal we concluded that a significant Neogene counter-clockwise rotation of an extended Australia block is largely responsible for the present-day structural complexity of the Australasia region (Storetvedt and Longhinos 2014). Based on a multitude of geophysical and geological evidence, we inferred that late Cretaceous-Recent wrench tectonic motions of the Greater Australia block have produced a regional phenomenological interlinking not hitherto envisaged for Australasia. In a subsequent critique of our conclusions, Choi (2014) maintains that we have not clearly defined "the boundary of the rotated block, especially south and west of Australia", but his alleged counter-evidence arises in part from misunderstanding of the operating system of wrench tectonics. In addition, Dong Choi subscribes to a tectonically stationary Australia but without accounting for the origin of its principal tectonic belts, transcontinental fracture zones and the structural complexity of the broad quasi-continental or semi-oceanic border zones. In the present communication we give an extended portrayal of the tectonic history of Australia within the framework of wrench tectonics – including the origin of its late Proterozoic-Palaeozoic foldbelts and trans-continental tectonic lineaments. The additional facts substantiate our original conclusions.

The wrench tectonic platform

In his discussion paper, Choi (2014) – arguing against our mobilistic tectonic scenario for Australasia (Storetvedt and Longhinos 2014) – places great emphasis on the late Precambrian fracture zones across Australia and the presumed continuation of some of these lineaments beyond the present continental margins. There can be little doubt that old, steep mega-scale lineaments constitute deep lithosphere-cutting fracture zones which certainly, in many cases, have been subjected to repeated reactivation. An important question is what kind of dynamo-tectonic system that gave rise to these structures. Another principal question is the genetic relationship between continental and oceanic fracture zones, and whether a continuity of these structures across the present continental margins poses a relevant argument against our tectonic solution.

Within the world oceans, pre-Cretaceous deep sea sediments have rarely been sampled. On present evidence, oceanic depressions and continental margins seem to have formed primarily in the Upper Cretaceous – in response to accelerated sub-crustal eclogitization and associated gravity-driven loss to the upper mantle – which in turn have led to a certain planetary acceleration and related inertia-driven global lithospheric torsion (cf. Storetvedt 1997, 2003). In many regions, however, this chemo-mechanical break-down of an original thick pan-global continental crust are fairly incomplete – demonstrated, for example, by the many submerged relatively thick-crusted continental plateaus and aseismic ridges occurring in broader belts around Australia. Thus, van der Linden (1977), comparing geophysical structure and rock information from many quasi-oceanic regions – such as the complex New Zealand-Australia oceanic sector, argued that "attenuation of continental lithosphere plays an important part in the formation of continental margins and ocean basins".

To the east of Australia, the region bounded by New Zealand Plateau/Tonga Ridge – including Tasman Basin, Lord Howe Rise, Norfolk Ridge and South Fiji Basin – displays a highly diversified crustal structure. For example, the large Fiji Plateau – with crustal thicknesses in the range of 15-25 km – has been described as quasi-continental, a conclusion that has been strengthened by the character and distribution of earthquakes suggestive of a stable block surrounded by zones of active deformation in the North Fiji and Lau basins (e.g. Chase 1971; Malahoff et al. 1982). According to Shor et al. (1971) "the Lord Howe Rise and the Norfolk Ridge are topped by thick sediments and have deep crustal roots and thick layers of material with the same compressional wave velocity as the Australian crust".

The N-S continuity of the regional structure (see also below) from New Caledonia through the Norfolk Ridge to New Zealand, and the geological evidence from exposed parts (e.g. New Caledonia, New Zealand, and islands sitting on Campbell Plateau and Chatham Rise) suggested to Summerhayes (1969) that the entire plateau/ridge system east of Australia has continental affinity. It seems likely therefore that an original thick continental crust has been variably attenuated (by mantle processes) during which pre-existing fracture systems would have provided the morphological control (see also below). In our wrench tectonic scenario, it has been natural to consider Australia and its bounding quasi-continental regions as a united tectonic block (named Greater or Extended Australia), and there is no reason to suspect structural discontinuities in seismic lines across the present continental margins (one of Choi's principal arguments against our mobile tectonic scheme).

In fact, submerged continental plateaus/ridges and deep sea basins are not different in origin; they differ primarily in the degree of sub-crustal thinning and associated isostatic subsidence. Therefore, Choi's promulgation of dredged Precambrian-Palaeozoic rocks in the deep margin off South Australia (for locations, see his Figure 2) is irrelevant to the extent it is thought to contradict our tectonic model. In fact, the structural discontinuities resulting from our inferred counter-clockwise motions of Greater Australia is to be found farther away from the present continental margins – in the form of accumulated torsion within the deep Indian and Southern oceans (see below).

In tectonic terms the broad marginal region east of Australia can be regarded an extension of the continent; hence, indication that an old Precambrian fracture seems to continue across the eastern quasi-oceanic border zone (one of Dong Choi's arguments against our Neogene rotation – ref. his fig. 2) is just as might be expected. This extension of Precambrian lineaments may only tell us that the principal eastern boundary of the rotated Australia block is to be found along the predominant Tonga-Vitiaz trench system – today forming a marked disrupted (knee-shaped) Benioff Zone which indeed may be seen as a tectonic product of the counter-clockwise motion of an extended Australian block.

The primary lithospheric units of wrench tectonics are the two time-relevant palaeo-lithospheric 'caps' – this inertial motion has, in particular, given rise to shearing and foldbelt formation along palaeo-equatorial regions and occasionally to rifting at steep angles to these belts. By the Upper Cretaceous, due to the degassing-driven accumulation of melts, fluids and gases in the uppermost mantle, an irregular low-velocity (soft) asthenosphere had gradually been 'installed'. Hence, in response to resulting changes in planetary spin rate the overlying more brittle 'lithosphere' would have been prone to detach from the deeper Earth and, hence, events of latitude-dependent lithospheric wrench deformation, operating in concert with changes in the planet's moment(s) of inertia, would follow as a natural consequence (Storetvedt 2003).

There is no evidence that deep oceanic basins to any extent existed in pre-Cretaceous time. Therefore, land masses as we know them today are also newcomers in Earth history. Significant crustal loss to the mantle led to a certain increase in Earth's rotation rate, triggering latitude-dependent wrench deformation of the global lithosphere – giving rise to the widespread Alpine-age (late Cretaceous to Lower Tertiary) tectonic climax. During this process the relative thin and mechanically weak oceanic lithosphere was subjected to strong shearing deformation – besides resulting in magnetic mineral alteration and fault-aligned crustal susceptibility contrasts. Inferentially, this Alpine-age wrench deformation of the thin oceanic lithosphere can be regarded the principal agent of marine magnetic lineations – through induction by the ambient geomagnetic field (Storetvedt 2003, 2010).

As an integral part of the twisted global lithospheric, the modern continents underwent relative rotations (mostly moderate) – along with neighbouring oceanic tracts. In this process, broader oceanic regions became wide tectonic boundary zones – including the central rift along mid-oceanic ridges. A new, but much weaker, dynamo-tectonic event swept the Earth in Neogene time, during which the mid-oceanic ridges and all continental mountain ranges came into existence – a feature which does not seem to have existed in earlier geological times. During the Neogene, relative continental motions were overall too small to be recorded by palaeomagnetic data with respect to continental motions. A glaring exception is Australia for which the Neogene-Recent inertial wrenching has produced the world's most complex region – the 'enigmatic' Australasia conjoint.

The late Cretaceous to Recent events of inertia-driven continental motions would have depended primarily on factors such as palaeo-latitude and size, though tectonic interaction played an important role in specific cases (Storetvedt 2003). In late Cretaceous time Australia had, like today, a sub-tropical position (cf. Fig. 10, Storetvedt and Longhinos 2014). With its location just to the south of the corresponding palaeo-equator, the by then newly formed, and relatively small, Australian lithospheric block would have been subjected to fairly strong inertial effects; hence, with its position in low palaeo-latitudes in the southern hemisphere it was consequentially subjected to northward, counter-clockwise displacement/motion. Owing to the irregular asthenosphere, larger continents would just respond by *in situ* wrench rotations. But owing to the relatively small size of Australia, in combination with its low palaeo-latitudes, the underlying asthenosphere seems to have formed a detachment zone allowing the continental lithosphere to slid northward – relative to the underlying parts of the upper mantle – in the order of 2000 km (Storetvedt 1997, 2003; Storetvedt and Longhinos 2014). As demonstrated by **Fig. 1**, modern GPS velocity vectors are in full agreement with these inertia-based predictions: Characteristic velocities for sites in Australia are northerly but just to the north of the continent – along the present equatorial belt – the prevailing velocity field turns markedly west-northwest. We argue that the simple counter-clockwise, inertia-based motions readily explain the late Cretaceous-Recent tectonic complexity of the Australasia region.

Our inferred motions of Greater Australia are bound to have produced pervasive shearing across the tectonic unit, in addition to enlarging/broadening of asthenospheric low velocity zones. In this context, Hamburger et al. (1988), studying active tectonism within the Fiji Platform, emphasized the considerable evidence for Neogene volcanism in the Fiji Islands. They concluded that the seismic velocity structure and seismicity pattern are suggestive of the whole

Platform being an active participant in a pervasive regional deformation. Furthermore, they found that seismic velocities of the upper mantle beneath the Fiji Platform is anomalously low (7.55 km/s) – being similar to that observed beneath neighbouring marginal basins and island arcs. In addition, Hamburger et al. concluded that though earthquakes within the Platform occurred more sporadically then along the surrounding tectonic zones they were associated with strike-slip faulting – within the same stress field that is deforming the North Fiji and Lau basins. These conclusions are consistent with the predictions of wrench tectonics.

The 'first-order' tectonic picture

It is a well-established fact that the prevalent stress-imposed characteristics in brittle crustal material are in the form of conjugate fracture systems for which the bulk normally constitute two steeply dipping and near-perpendicular sets (for a review, see Scheidegger 1963, 1985; Hancock 1985). The fabric of these fundamental ruptures seems to date from the Upper Archaean (for discussion and references, see Storetvedt 2003); episodic global tectonic processes throughout subsequent geological history has apparently reactivated and intensified this fracture pattern so that today we see a complex system of rock discontinuities varying from small joints to mega-scale fault zones. Consistent with the wrench tectonic thesis, several authors have argued that the characteristic near-vertical joint planes, with their sharp intersections and smooth faces, are likely to have originated through crustal shear (e.g. Bucher 1921; Scheidegger 1982; Hancock 1985). The presence of occasional slickensides on joint surfaces clearly demonstrates that shearing has been involved during the history of these rock structures. The ubiquitous existences of sub-horizontal (undulating) fracture planes may be seen as additional evidence in support of lithospheric torsion.

Predominant sets of conjugate fracture surfaces often lie parallel to faults or fault zones – towards which the joint frequency may increase, suggesting that there is a causal relationship between faulting and jointing even though the majority of joints are undoubtedly younger than the principal age of faulting. However, joints are commonly seen cutting the lithological complexities of basement formation as well as penetrating flat-lying cover formations of all ages. The fact that Precambrian dykes often line up along one or both of the characteristic orthogonal fracture sets give substance to the view that neo-tectonic joints are inherited from a deep-seated fracture network/fabric – a fracture system that has been implanted into ever younger surface strata during planetary dynamo-tectonic events. Occasionally, global wrenching may have produced prominent fault zones in directions oblique to the predominant fracture network; such cases would notably occur within palaeo-equatorial settings where inertial processes will have had their strongest straining effect. These aspects bring us to the question of how the large-scale fault zones across Australia came into existence.

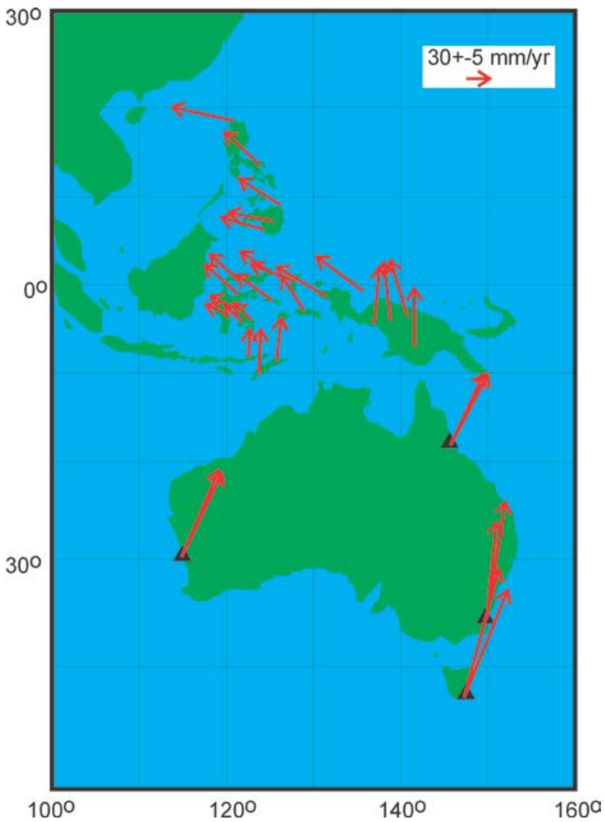

Fig. 1 Diagram demonstrates the characteristic GPS velocity picture for New Guinea and the outer archipelago of SE Asia and representative velocity vectors for Australia – with respect to continental SE Asia. Note the unusually large NNE-directed GPS-derived displacement for Australia, a velocity field which in the present equatorial region turns markedly counter-clockwise. GPS information is based on Puntodewo et al. (1994), Larson et al. (1997) and Rangin et al. (1999).

Fig. 2 shows the directional populations of near-vertical surface joints for 4 regions in Australia – for which the predominant directions are NNE/SSW and ESE/WNW respectively. When we restore Australia to its pre-Upper Cretaceous azimuthal orientation, using palaeomagnetic data (Storetvedt 1997), the subsequent rotation figure (the combined Alpine and Neogene counter clockwise wrenching events) will amount to ca. 80˚; in other words, the original joint axes will be practically interchanged. A comparison of the prevailing joint axes with the orientation of tentative large-scale fault zones across Australia, as given by Choi (his figure 2), suggests that in some cases there is a relatively close relationship between joint axes and large-scale faults zones. In other cases, however, the large-scale faulting seems to have intermediate orientations (relative to the prevailing joint axes). The fact that Australia's predominant lineaments inferentially are of Precambrian ages, it follows that the continent was affected by strong tectonic shear deformation at an early stage (cf. O'Driscoll 1980, 1986) – long before its late Cretaceous-Neogene rotation history. According to wrench tectonics, the cause of the major old fracture zones in Australia is to be found in the fact that it, in its pre-Upper Cretaceous azimuthal setting, was crossed by two palaeo-equator aligned fold belts: The late Proterozoic Adelaidean geosyncline/lithotectonic zone across SW Australia, and the younger Tasman fold belt along its present western margin.

In Australia's pre-Upper Cretaceous azimuthal orientation, the Proterozoic-Ordovician palaeo-equator ran across the continent, as envisaged in **Fig. 3**. During this nearly 3 Gy-long time span, intra-continental diastrophic processes would repeatedly have reactivated the Australian craton – producing palaeo-equator aligned sedimentary troughs with superimposed transpressive deformation. Thus, the late Proterozoic-early Cambrian Adelaide geosyncline/fold belt system, which extends through a major part of Southern Australia, contain thick sequences of sediments, up to 15 km of mainly shallow-water deposits (Brown et al. 1969; Preiss and Forbes 1981) – with equivalent rock sequences in western Tasmania (e.g. Williams 1978; Turner et al. 1993). The surface curvature, structural trends and a well-developed cleavage within the fold-thrust belt (e.g. Marshak and Flöttmann 1996; Turner et al. 1994) suggest that a significant amount of shearing has been involved. To the northwest of the strato-type Adelaide syncline, other thick late Proterozoic basins in Central and Western Australia (Canning Basin, Officer Basin and Amadeus Basin etc.) are considered sufficiently similar to suggest close interconnection with the Adelaide geosyncline to the southeast (cf. Preiss and Forbes 1981). This succession of intra-cratonic troughs across south-western Australia – here named the Adelaidean Belt – is consistent with their inferred late Proterozoic palaeo-equatorial location (**Fig. 3**). According to wrench tectonics, a palaeo-equatorial belt would be the region with the strongest inertial effects – giving rise to a globe-encircling succession of sedimentary troughs with superimposed shear deformation (Storetvedt 1997, 2003).

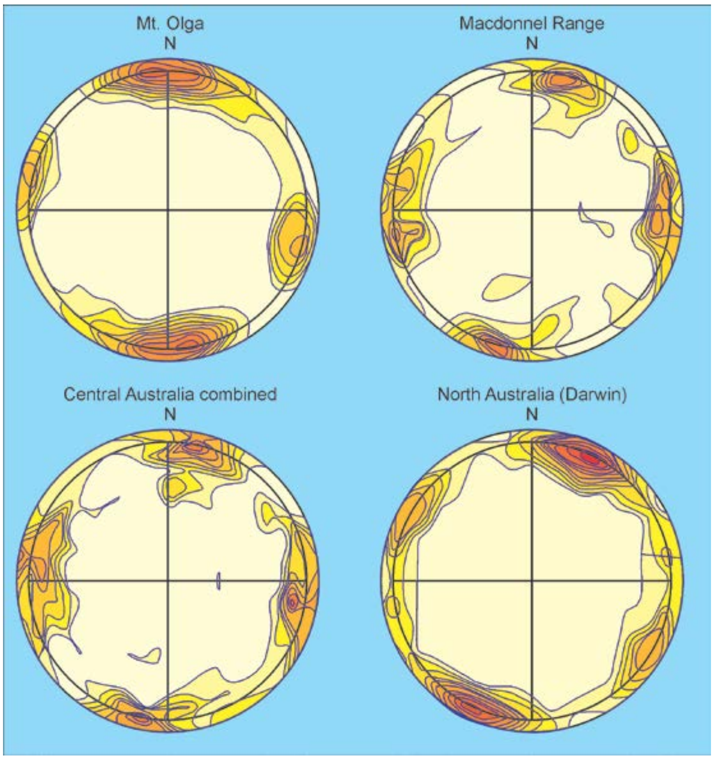

Fig. 2 Lambert projection depicting pole-density distribution of characteristic near-vertical joints from 4 regions in Australia – based on Scheidegger (1980). Coloured regions delineate statistical averages of joint population maxima. The inner circle of the projections limits the lower hemisphere, whereas the outer circle represents a 10˚ overlap of the upper hemisphere.

In the original orientation of Greater Australia – with the present Great Australian Bight pointing due west (in present grids) – the late Proterozoic palaeo-equator aligned fold belt would continue across north-western Pacific before passing along Arctic Canada. In this regard, Neo-Proterozoic palaeomagnetic poles from the northern cratonic regions define reasonable clusters at antipodal positions a ca. 65˚E, 20˚E and ca. 115˚W, 20˚S respectively (cf. Storetvedt 1997). This places the palaeo-equator along Arctic Canada and the Labrador Sea, with further continuation along present-day Central and South Atlantic, before cutting across Australia. Apart from a glaciogene sequence at higher levels of the Adelaidean succession (corresponding to a global cooling event), the occurrence of redbeds at various horizons and the widespread accumulation of carbonates, including stromatolitic reef sequences, may provide further evidence that Australia experienced tropical to sub-tropical conditions during late Proterozoic and Lower Palaeozoic times.

From, say, the Middle Ordovician, the palaeo-equatorial zone acquired a quite different orientation, lining up along the Tasman New England lithotectonic belt; the major shift of the relative equator across Australia was brought about by an event of 'Middle' Ordovician true polar wander (Storetvedt 1997, 2003). The palaeo-equatorial/tectonic junction at the south-eastern tip of Australian continent (see **Fig. 3**) has its antipodal equivalence in the Newfoundland-Labrador Sea region (Storetvedt 2003). This hemispherical geometrical/tectonic anti-podality may be seen as fairly strong evidence in support of Australia's original geographical orientation – i.e. with present-day Great Australian Bight facing westward.

The fact that two palaeo-equatorial fold belts have cut across the much older Australian craton, it seems likely that their detailed geometry and structure would be affected by the 'pre-set' Archaean-age orthogonal tectonic fabric – which for Australia are oriented 'N-S' and 'E-W' respectively. For example, the Tasman fold belt shows internal split-up structures with 'N-S' bearings, and some of the major fault zones have approximate 'E-W' orientation. However, the fact that Australia in late Proterozoic and Palaeozoic times had palaeo-equatorial settings – along which inertia-driven wrench tectonic processes would have had their strongest effect – intra-cratonic shearing and break-up of major fault zones, in some cases oblique to the underlying structural fabric, are likely to have been produced.

Furthermore, in a degassing Earth model, on which the wrench tectonic theory is based, the bulk of surface ores have probably been emplaced by upwelling streams of abiotic hydrocarbons from the mantle – in tectonically suitable locations along fracture zones. According to Gold (1999), the upward metal transport is likely to have been in the form of so-called organometallics; because of changes of pressure and other conditions along the flow route, at some point a particular metal association will have dissociated from the hydrocarbon molecule – forming an ore deposit. Anyway, the intersecting fault zones of Australia (see Choi 2014, fig.2) are likely to have formed vertical lithospheric break-up channels, forming effective flow routes to the surface; hence, it should not come as a surprise that such tectonic junctions may have major ore concentrations. The Earth's episodic changes of its moments of inertia (by way of variation in rotation rate or by events of true polar wander) will have acted as a kind of hydraulic pump for gases and fluids from the mantle –transporting a diversity of metal compounds to surface levels.

The question of tectonic boundaries

Dong Choi criticizes our Neogene rotation model, claiming that we have not clearly defined "the boundary of the rotated Australasian block, especially south and west of Australia". It seems necessary therefore to take a closer look at the tectonic 'boundary' problem. Upstanding continental masses will be more strongly affected by inertial effects than lower-lying oceanic regions, but normally continental margins will not turn out to be tectonic discontinuities of any importance. Thus, mobile continental 'entities' will include also broader adjacent, wrenching-deformed, oceanic regions; hence, the tectonic effect of relative rotation of lithospheric masses is basically taken up by the mechanically weak oceanic basement. In this process, the ubiquitous systems of orthogonal oceanic fractures may be strongly reactivated (and sometimes deformed), and the mid-oceanic rifts/ridges (molded by the Archaean-age fracture fabric) apparently owe their present shape and structure to post-Upper Cretaceous lithospheric torsion – topped up by Neogene uplift (Storetvedt 2003). In conclusion:

The limiting tectonic boundary between two continents in relative rotation will be defined by their intervening mid-ocean rift zone. Hence, tectonic boundaries between mobile lithospheric masses will be in the form of broad oceanic deformation zones – a new type of fold belts originating in Alpine time.

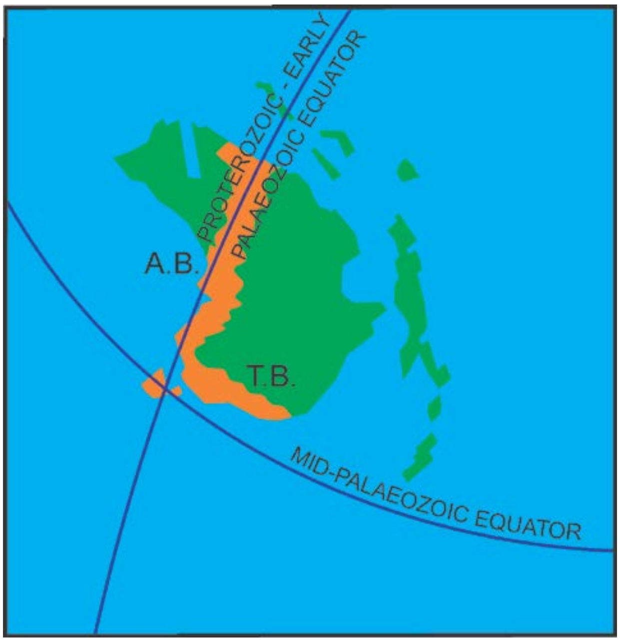

Fig. 3 In the pre-Upper Cretaceous azimuthal orientation of Greater Australia (with the present Great Australian Bight facing westward), the late Proterozoic-Lower Palaeozoic equator was co-linear with the Adelaidean lithotectonic belt (A.B); in the Tasmania region it is cut by the Middle-Late Palaeozoic Tasman tectonic zone of SE Australia (T.B.). The Tasman belt represents the landward segment of another palaeo-equator aligned geosyncline/fold belt – corresponding to the Caledonian-Appalachian zone of the Northern Hemisphere. The Tasman-Adelaidean junction has its northern antipodal counterpart in the Newfoundland region (cf. Storetvedt 2003).

In the case of Australia, its inferred major Neogene counter clockwise rotation is bound to have left significant tectonic discontinuities and other deformation structures in the surrounding thin-crusted oceanic tracts for which the mid-ocean rifts/ridges of the Indian and Southern oceans may be seen as kind of outer tectonic border zones. In this context the Miocene and Pliocene strata of the Central Indian Basin have been subjected to strong tectonic disturbance – forming the Indoysian Foldbelt of Wezel (1988), characterized by intense folding and high-angle reverse faulting. Wezel noted that linear magnetic anomalies in the Central Indian Basin run approximately parallel to the regional high-angle reverse faults, and according to Neprochnov et al. (1988), the younger regional shearing event (Neogene) has cut the Central Indian Basin and parts of the continental Ninety-East Ridge into a series of *en-echelon* blocks. Furthermore, the Neogene counter clockwise rotation of Greater Australia apparently affected the entire north-eastern Indian Ocean (see Fig. 4), during which the southern Ninety-East Ridge is likely to have acquired its slight counter clockwise bend.

The structural framework of frontier deep basins and abyssal plains of the western margin of Australia and major parts of the Wharton Basin constitutes a complex physiography and tectonic network. Differential subsidence of micro-continental masses (plateaus), bounded by the common network of orthogonal fractures, have given rise to the various physiographic features (cf. Falvey and Veevers 1974; Bradshaw et al. 2003; Williams et al. 2013). The fracture zones of the characteristic structural fabric have 'NE-SW' and 'NW-SE' orientations, but the tectonic lines and their enclosed crustal segments are generally curved and distorted (see **Fig. 4**) bearing witness to quite intense wrench deformation. For example, the elongate and fault-controlled Lost Dutchman's and Broken Ridge continental fragments line up with major shear zones along which the ocean floor is anomalously deep. During the major Australia block, the basement of the whole northwestern Indian Ocean was seemingly undergoing transtensive deformation producing an overall curved pattern of the prevailing fault zones – modified in part by the Precambrian orthogonal fracture systems.

In the rare of Australia's Neogene rotation, the complexity of tectonized deep sea basins and micro-continental ridges formed a wide oceanic 'tectonic boundary' belt – extending westward at least to the Ninety-East Ridge and the Central Indian Basin. To the south, the transtensive deformation of the extended Wharton Basin seems to be largely bounded by the major Broken Ridge/Diamantina Fracture Zone which continues eastwards into the deep sea basin of the Great Australian Bight. Major fracture zones and transtensive regions would provide routes of hydrous fluids-enforced sub-crustal eclogitization with associated delamination of the crust – from the bottom upwards (Storetvedt 2003) – giving rise to isostatic basin subsidence and formation of elongate basins. In this way, the deep sea basins off Western Australia – the West and East Perth, Gascoyne and Argo abyssal plains – can be explained.

Prominent fracture zones and deformed crust would be marked by deep linear troughs or oceanic plains. In our tectonic evaluation (Storetvedt and Longhinos 2014), we have distinguished two major inertia-driven tectonic phases of counter clockwise motions: a minor Upper Cretaceous-Lower Tertiary event, and a major Middle Miocene-Recent rotation. Hence, we might expect that these time intervals would show up by deep sea basin subsidence and accumulation rates. In a compilation of DSDP drilling data from sites in the Indian and Southern oceans, Quilty (1980) found that, overall, these two age intervals do in fact occur quite commonly in the episodic deep sea sedimentation histories.

The Upper Cretaceous-Lower Tertiary global tectonic climax is likely to be chiefly responsible for the marine magnetic lineations of the world oceans (cf. Storetvedt 2010) – formed through inertia-driven shearing of the evolving thin oceanic crust. The subsequent Neogene tectonic event apparently had, overall, only minor global significance. The only conspicuous, divergent regional example is Australia, which according to palaeomagnetic and diverse structural and geophysical evidence is likely to have undergone a significant Neogene-Recent rotation. **Fig. 4** demonstrates the resulting complex tectonic break-up in the surrounding oceanic regions. South of the tectonically deformed north-eastern Indian Ocean, the major Diamantina Fracture Zone apparently formed a major tectonic crush zone – a complex shear belt which in the deep sea basin off the Great Australian Bight cut across the dominant N-S striking fault system which characterizes the seafloor of the Southern Ocean; this predominant N-S fracture system – being perpendicular to the major W-E running shear belt off South Australia – is likely to have undergone Neogene shear reactivation – in places cutting into to the W-E shear belt off Southern Australia (see Fig. 4).

Resulting from the crustal break-up off Southern Australia, a wide deep sea basin, characterized by non-correlatable magnetic anomalies, was formed (cf. König and Talwani 1977); from their geophysical study, the latter authors suggested that the basement depths underlying the magnetic quiet zone, extending from west of Tasmania to the western border of the Great Australian Bight, were greater than on either side of the quiet zone. Veevers (1986) tried to define some magnetic lineations further landward, in terms of early seafloor spreading isochrons, but Sayers et al. (2001) suggested that at least some of the deep sea region concerned is likely to represent subsided continental crust, rather than oceanic crust formed by spreading. According to the scheme of wrench tectonics, the pre-late Mesozoic Southern Ocean was (like other oceans) continental or semi-continental, and its present N-S structured ocean floor is likely to have formed through Neogene shear reactivation of one of the orthogonal sets of old Precambrian lineaments. The strongly sheared Australia-Antarctica Ridge complies with this explanation.

In **Fig. 4** we have outlined the more intensely deformed crustal belt off the Great Australian Bight – believed here to represent a main southern segment of the principal tectonic boundary zone produced during the Neogene counter clockwise rotation. In the eastern sector of the Bight, the crust is exceptionally sheared, splitting into two sections – north and south of the South Tasman Rise respectively – before the two tectonic branches unite and turning counter clockwise before continuing along the dextral Alpine Fault of southern New Zealand. The tectonic boundary has a natural northern continuation along the deep-faulted Kermadec/Tonga trench; the complex tectonic knee and trench/Benioff splits up around the Fiji Plateau and the major concentration of deep, magnitude 7+ earthquakes in this region testifies to the compressive nature of this boundary region. In terms of our Neogene rotation model, the seafloor physiography of the NE Indian Ocean and the basins off Western Australia are consistent with the seafloor structure of the Tasman Sea. As we discussed in our original paper, the geoid high of the resulting Outer Melanesia tectonic front as well as the structural intricacy of the Australasia conjoint are natural products of the inferred Neogene counter clockwise rotation of the Australia block.

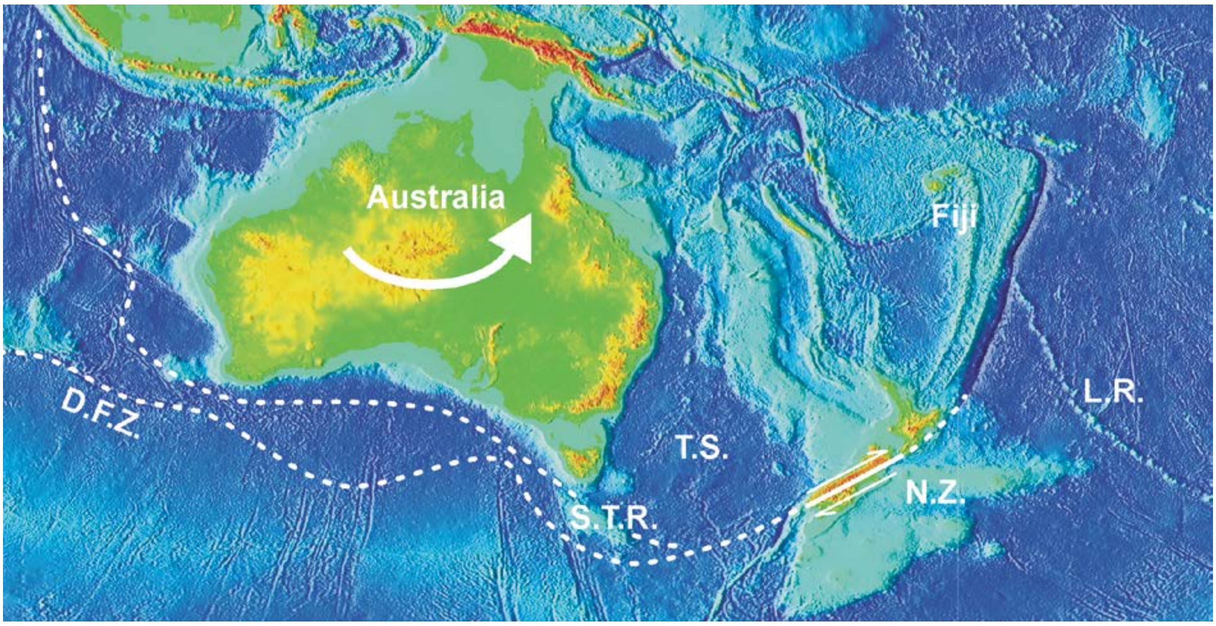

Fig. 4 Cut from the NOAA satellite altimetry global seafloor topography map. A predominant tectonic break-up zone, presumably produced by the Neogene counter clockwise rotation of the Australia block, is marked by white stippled lines. Note the similarity in seafloor structure in the NE Indian Ocean (off Western Australia) and the Tasman Sea (T.S.) for which the principal trends of the two regions are consistent with the inferred rotation. The major Neogene motion of the Australia block reactivated also the 'N-S' striking fundamental system of old fractures of the Southern Ocean; this major 'seafloor' reactivation has apparently been responsible for the strongly sheared Australia-Antarctica Ridge – in addition to occasionally having cut into the tectonic crush zone off Southern Australia. The dextral character of the major Alpine Fault of southern New Zealand (denoted by white arrows) is in accordance with the inferred rotation, and so is the resulting compressive front in the Fiji region. Note how the Louisville and other SW Pacific ridges are bent towards, and sharply cut by, the Kermadec-Tonga Trench/shear zone. D.F.Z: Diamantina Fracture Zone; S.T.R.: South Tasman Ridge; L.R.: Louisville Ridge.

Closing comments

The additional tectonic evidence presented in this communication strengthens our original conclusion (Storetvedt and Longhinos 2014) – that a major Neogene-Recent counter clockwise rotation of the Australia block is chiefly responsible for the structural complexity of the Australasia region. Continuing northward from the Alpine Fault of South New Zealand, the eastern boundary of the rotating block follows the tectono-volcanic Kermadec-Tonga Trench. In this rotation process, which apparently is still taking place, the Fiji Plateau has been turned into a compressive front characterized by unusually strong seismic activity. In addition, a predominant structural knee has been formed, during which the present-day complex trench/Benioff zone system has come about. In view of the inferred anti-clockwise rotation, the rare end of the moving block would give rise transtensive conditions; this means that the oceanic regions off Western Australia would be subject to increased gravity-driven sub-crustal delamination and basin subsidence (Storetvedt 2003). This ongoing crustal loss to the upper mantle is likely to be the mechanism behind the geoid low of the north-eastern Indian Ocean, while the overall (and currently) transpressive front – extending from the Kermadec/Fiji region to South China Sea – is likely to account for the present elongate, SE-NW trending, belt of geoid highs along the Melanesia region. The relatively modest clockwise rotation of Eurasia, producing a certain overriding effect along the outer Indonesian Arc (Storetvedt 1997, 2003), is unlikely to compensate for the southward lithospheric drag in the adjacent Indian Ocean – which would be the product of the relatively fast rotation of the Australia block. Hence, the overall tectonic effect along the Indonesian Arc (Neogene to present) would expectedly be transtensive – thereby paving the way for rising magma and mantle volatiles under high confining pressure. Hence, the unusually strong earthquake and volcanic activity along the Indonesian Arc may therefore find a natural explanation.

Acknowledgment: We thank Frank Cleveland for help with the illustrations.

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ESSAY

"DON'T THINK YOU CAN TEACH US ANYTHING"

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"Intellectual corruption can be a toxic variant of self-censorship. The distinction between what is intellectual corruption and what is self-censorship is often confused with the boundary between silence and cowardice"
William Nygaard (Norwegian Chief Publisher and freedom of speech advocate).

William Nygaard was responsible for publishing the Norwegian edition of Salman Rushdie's novel *The Satanic Verses* – two months after Khomeini's fatwa against Rushdie and his publishers. On 11 Oct. 1993, Nygaard was shot three times outside his home in Oslo; badly wounded he barely escaped the assassination attempt.

Background information

In a recent extended Letter (NCGT, v. 2, no. 2, 2014), I recounted a revealing observation during a conference field excursion in New Zealand in 1992 – at which the strong emotional component and a profound lack of appreciation of the demonstrably tenuous status of plate tectonics, was amply demonstrated. Shortly before my Australia/New Zealand trip, I had got an invitation to give the opening address at the forthcoming annual meeting of the Norwegian Geological Society. As it turned out, my presentation (in January 1993) triggered an enormous amount of media attention that came as a real shock to the national geoscience community. Radio, TV and newspaper commentators wondered whether plate tectonics was on its way out. If so, what were all the established geoscientists doing about this challenge and, indeed, what was to be done about the significantly flawed presentation of plate tectonics in school and college text books? Prominent members of the national geoscience establishment argued that all the publicity I had received was completely undeserved, but scientific arguments bolstering their view were not given. For them, my thesis was undermining the whole basis of their dedication to the plate tectonic paradigm; it was time for counter measures! Here I give a summary of these 'corrective' actions taken during the following few years orchestrated by some of my most implacable local opponents – within the solid Earth physics and geology institutes in Bergen. In 1990 there was a reorganization of the Earth Sciences in Bergen. In this process my Department of Geomagnetism – which since 1917 had been part of the Geophysical Institute, was amalgamated with the Department of Seismology (located in another building) to form the new Institute of Solid Earth Physics. Because of my new theory and the unrest that gradually, during the 1980s, had been building up around my exposure of some grave problems facing plate tectonics, the institutional reorganization would prove to be a professional 'disaster' for me (in a separate essay, I will come back to the growing local turmoil around my teaching). In order to save my scientific integrity and ability to complete my planned books – describing the inadequacy of plate tectonics and discussing my alternative explanation of the Earth's tectonic history – I had no other choice but to break out of the newly established institute. I succeeded, and from early 1992 I was part of my old Geophysical Institute – which after the 1990-reorganization had retained its old name but was now housing the fields of meteorology, oceanography and climate research. Officially I became part of the oceanography section. This was an ideal situation for me; I could now work in peace because my ideas did not interfere with the work of the Institute as such. As my professional opponents – within the solid Earth and geology communities – were located in another building, I was able to continue my work without too much professional hassle, for which I was profoundly thankful. This autobiographical account gives a sketch of the ups and downs in my scientific crusade during the most turbulent years – from early 1993 to early 1999; its title is taken from a novel by Axel Sandemose – *A fugitive crosses his tracks* (1936).

An unexpected invitation – and a publicity peak

In mid-October 1992, four weeks before I took off on an extended trip to Australia and New Zealand, I received a telephone call from the outgoing President of the Norwegian Geological Society – Professor Tore Torske. He was an old acquaintance from our student days in Bergen around 1960. He had read about my scientific breakthrough with a new global tectonic theory, discussed in the Bergen University press [the UiB news bulletin] a few months earlier. He said that the Organizing Committee of the forthcoming Annual Assembly – to take place in Tromsö 8-10 January 1993 – was curious to hear more about my work. For me, this was good news, and I accepted the invitation unhesitatingly. When I returned from my long journey 'down under', the December issue of the Society's house journal (*Geonytt* 3/92) had an amusing news cutting on the forthcoming event. The eye catching article, written by Tore Torske, had the following headline: ***The new hairstyle of Plate Tectonics; can it rescue the national meeting 1993?*** The note read as follows (my translation):

> *In the Bergen University Press, and later this summer in a regional newspaper, interviews with Karsten Storetvedt, well-known palaeomagnetist at UiB, have cast fire flares and other destructive materials against the foundations of plate tectonics and all its consequent and constituent parts. Plate tectonics, as he sees and rejects is, is characterized as the most serious mistake in the geosciences to date. Early in his career, he says, he lay 'doggo' for ill-founded scientific thought.*
> *The explanation for this was his contact with geophysical celebrities in Newcastle. As he would term it: "…my original choice of scientific conceptual dressing was not significantly different from the 'mechanism' that leads to our choice of a favorite football team." – Oh, boy! Among geoscience colleagues the heretic has met surprising, unpleasant and sometimes shocking reactions. Most surprising were the many hateful and malicious reactions, which he regarded as a substitute for the lack of scientific arguments. He must surely have provoked the plate tectonics hooligans? However, it comes to my mind that maybe the National Meeting in 1993 would benefit from clearing space in the programme for a presentation and critical debate, as well as specific interpretations, of the more fundamental aspects of our scientific 'reality' which is being challenged. If so, it is tacitly presumed that a "positive, well-educated and tolerant" tone would be exercised, in the same way as the news was received by the Newcastle geophysics group in 1988. Or can it be lucky that NSB [the National Railway] rail tracks have not yet been extended to Tromsø, with the risk that plate-hooligans will be wrecking the trains during their mass migration toward the National Meeting -93?*

This looked promising. Just before my departure for Tromsö on January 7th I received a telephone call from the Norwegian Broadcasting Corporation (NRK) asking me to be present for a live interview in their studio in Tromsø early the following day. According to rumors that circulated during the meeting, it was one of the science directors at Rogaland Research (now University of Stavanger) who had tipped off NRK about my opening lecture. This interest turned out to be the start of a media attention that lasted for months. The official opening of the Tromsö meeting was by professor Ole Danbolt Mjös – rector at the University of Tromsö. Next in the program was my non-traditional presentation entitled: "*What is the Earth's tectonic system?" The* one hour talk was followed by an unusually humorous discussion. During the subsequent coffee break, I was contacted by another NRK-reporter who asked for an interview. In my hotel room later that day I heard Rector Mjös, in the regional radio broadcast, commenting on the opening session of the geological meeting. It had been an unusual event, he said; in spite of the serious character of the theme, the tone of the discussion had been characterized by warm humor – something the audience had appreciated through multiple bouts of spontaneous applauses. It had been a busy day, so I wanted to drop out of a getting-together event at a local pub later in the evening. I mentioned this to a couple of geologists, but they put up a warning finger. "*You must be present - if not, rumours will be spread that you were afraid to stand face to face with the geological expertise*", one of them emphasized. I followed their advice.

When I was back in Bergen, Professor Jan Mangerud commented that my opening presentation in Tromsö was "*the lecture everyone was talking about*". Further development came in rapid succession. On February 2, Norway's largest newspaper – *Verdens Gang* – described the academic 'event' in a full-page look-up, on page 2. In the early afternoon the same day, Norwegian Television called requesting my attendance for a live news broadcast from Oslo the same evening.

My appearance on television led to a wave of curious inquiries from geography teachers across the country – in addition to further lecture invitations to university institutes and academic societies. The University's News Bulletin – *På Høyden* – was quickly out with an article entitled "*He dares when others keep silent"*. During the following week I was interviewed by a number of local radio stations. Such was the news coverage that geophysicists from Bergen who were on an expedition in Antarctica later told that they had been informed about the development via NRK's overseas broadcasts. The case had taken a completely unexpected turn. Members of the university's technical staff were amused by the embarrassing situation that had arisen in the local geological-geophysical research community. "*We see that they are not feeling well"*, they told me. I had never been part of the sociable aristocracy at the university, and as an 'outsider' with too much media attention, I suddenly had become a 'young cuckoo' in the local nest of prestige – a situation that soon turned out to have consequences.

The positive attitude cracks

A few days later, on 5th February, the university arranged its Open Day with a wide range of lectures – mainly professional updating lectures primarily intended for high school teachers, during which I gave a 2-hours seminar entitled "*Dissemination of knowledge or destructive indoctrination?*". The lecture was held to an audience of around 50 high school and college teachers, and by the response, I judged that my message was well received. At the back of the auditorium, however, an 'unexpected guest' – one of my toughest local opponents over many years, had sneaked in; he obviously did not have the noblest of intentions. Immediately after the seminar was over, the 'intruder' asked for a comment. I stepped aside to hear what he had to say. It started rather badly for my colleague. With an insecure voice he told the audience that what they had just heard was in a way OK, "*but in Iceland…*", he continued. But suddenly he was sharply interrupted by someone in the audience, a stout and authoritative person, who apparently had perceived the situation. He stood up and with a loud voice he concludes the session. Turning to my colleague he said: "*We do not need you to protect us from Professor Storetvedt's new thoughts; we are fully able ourselves to assess the message! On behalf of myself and the rest of the audience I would like to thank him for a very interesting lecture!*" The audience then applauded and ostentatiously got up to go. My colleague, who stood irresolutely at the podium, quickly disappeared. For me, this incident came as a great surprise. My colleague who obviously had entered the arena with the intention of putting me down had instead been repudiated by the audience. A student at the local Teachers Training College later told me that the incident had been discussed in a lecture at the college – as a demonstration of the emotional resistance, of a 'well-established' science community, to new scientific ideas.

Shortly after this incident, I got a telephone call from the Editor of *Geonytt* – Halvdan Carstens. He encouraged me to write articles for his magazine, an invitation that I promptly accepted. We agreed on a summary article for the forthcoming summer issue – based on my presentation in Tromsö. I immediately completed a short article and submitted it to the editorial office in Trondheim, but it did not appear in the journal's summer edition, as had been promised. Instead, the issue had an article and a few shorter contributions that indicated a marked change in attitude. The journal issue contained both condescending and sarcastic words that pertained to my work and seemed to me to be totally misplaced in a civilized academic debate. Fresh opposing forces had entered the field; the peace pipe from Tromsö had clearly been put aside.

Most surprising was the commentary, in that issue of the journal, by Tore Torske, the former President of the society (who had invited me to Tromsö) and which now used phrases like *inedible, breezy performance, controversial, uncritical, dogmatic, unfair, uncompromising, all-knowing,* etc. - either about me personally or about my new global geophysical synthesis. I then called him asking the reason for his sudden change of attitude, and the low standard of objectivity he had shown. During our brief conversation it became clear that after the Tromsø meeting he had come under pressure from some of the more prominent figures in Norwegian geology who resented the amount of publicity I had received. But the condemnation was nevertheless not without exceptions because Professor Tore Prestvik, newly elected President of the Society, wrote:

> *"I think Karsten Storetvedt's invited opening lecture will be remembered for quite a long time. He came with many surprising solutions. The debate is now taking place between colleagues both in the offices, on the phone and in lunch canteens".*

Throughout the 1980s, my academic activities had prospered - both in teaching and research, which had resulted in a relatively great influx of new research students to my department (Geomagnetism). This success had not been happily received by some local colleagues, and a 'campaign' had started to psyche me out of my favourable position [I will come back to this development in a later essay]. So far I had remained rather passive to these underground activities, but with the new wave of 'academic' dirty tricks – triggered by the publicity my work had generated after Tromsö, my patience was beginning to run out. It was time for a little exposure of the black side of science. I therefore phoned the editor of *Geonytt* and informed him that I would use my right to reply to the attacks that they just had published. In addition to the scientific article that had already been accepted, I would submit a piece about *the Human Face of Science* – as a response to the attacks his journal had published. "*Yes, that could be an interesting contribution*", the editor replied.

The discussion article about general aspects of science as a human activity, in which I also replied to the attacks in *Geonytt*, was submitted to the journal. The article focused on our often glossy picture of science which is in contrast to the real situation. Among other things, I took up aspects of the dominant anonymity and professional alienation that prevails in a research community in relation to its ruling mindset. I had put some embarrassing issues on the agenda. While I was waiting for the reaction, I was busy with book writing and giving invited seminars. In mid- September 93, I gave an evening lecture to the Norwegian Botanical Society in Oslo. At this meeting there were also a couple of senior geologists. Just before my lecture one of them came to me with a plea: *"Please do not take away my plate theory! I need it so badly in class!"* Another from the audience told me later that during the lecture he had surreptitiously glanced at the strained faces of the geologists. In retrospect, he felt guilty for 'peeping behind the curtain'.

In November, I was on lecture tour in the U.S. and Canada. During my stay at the University of Western Ontario my wife called to tell me that the latest issue of *Geonytt* had been received, and that my two articles in the journal had led to new lecture invitations – including an evening talk to the Norwegian Petroleum Society in Stavanger. Back in Bergen I quickly learned that my discussion article about the 'secret' psycho-social aspects of science had become a very hot potato in the national geoscience community. My accusations of a lack of readiness to confront unpalatable facts in the geosciences hierarchy uncovered a hornet's nest; the elder statesmen in the Earth sciences were clearly discomfit that the varnish covering up the imperfections of the existing holy grail of plate tectonics had been exposed. In order to regain authority, members of the resistance group were getting ready to use unconventional methods.

The opposition changes tactic

In December 1993 we often noticed a car that was parked illegally at the end of a bus stop immediately below our private residence. We became aware of it – a dark colored car of a type which is often used by university departments – on our late night walks. Every time we passed the car, we wondered what it was doing there. But we eventually accepted that it probably belonged to late evening customers at a bridal salon that was located just across the road. Even so the car could be parked for hours in the same place, and it turned out later that others in the immediate surroundings had marveled at the continuing wrong parking – as did the owners of the bridal salon. Even the police told later that, on their late evening routine patrols, they had observed the illegally parked car, but had done nothing about it. There were also other strange local happenings.

Our bedroom faces the bus stop where the unknown car was parked. Nearly an hour after we had gone to bed for the night, we were on several occasions awakened by noise immediately below our bedroom windows. We had moved into our new house in the spring of 1988, but there was still some unfinished landscaping work on the side facing the main road below. Our house has a ground floor overhang along its entire length (see photo), and since this front was then without outside lights much could obviously happen there under cover of darkness. I tried to calm my wife that the noise was caused by cats or dogs, but she insisted that it had to be people outside. Basically, I had to agree that it seemed as if someone tripped over the uneven ground below the house. Even though I had experienced a little of almost everything in science, I did not believe that there could be elements within my own academic circles that could be responsible for this form of criminal intrigue. On one occasion, the noise was so striking that I rushed out of bed, got my pants on in a jiffy and was heading out of the house. My wife stopped me at last minute and begged me not to go out. She was afraid of what might happen. "*After all we have experienced in recent years, I no longer have the slightest confidence in the morals of your scientific colleagues; some of these guys can certainly do rather unexpected things*", she said with a trembling voice.

Photo of our private residence taken from the approximate location, at the end of a bus stop, where a mysterious vehicle was periodically parked illegally – in the winter months between December 1993 and November 1995. Photo: Rune Andre Storetvedt

Oddly enough, I was still not quite able to connect the unknown car to what was happening outside our bedroom windows at midnight. Most likely, I was fed up with all the problems that for years had surrounded my work and I wanted above all to work in peace as I could see light at the end of the tunnel of my thesis. Getting a modicum of peace turned out to be wishful thinking. Time passed and the situation seemed to have calmed down, but the errant vehicle was still observed now and then. Then the activists struck again.

On March 1st 1994, I gave a two-hour seminar in Stavanger for the Norwegian Petroleum Society – for an audience of around 120 people from the international petroleum community. According to a letter I received a few days later, from a Norwegian petroleum geophysicist, my seminar had come as a great surprise to many in the audience. Two members of the audience had been particularly cocky before the talk, but afterwards they left the auditorium in silence. During the short social gathering after the lecture, some admitted they were very provoked by my lecture, but what actually had caused their emotional imbalance was not revealed.

My return to Bergen was in the afternoon the following day. Having dined we sat down to watch the 7 o'clock news. Suddenly we hear a bang on one of the windows behind us. We jumped up to see what had happened. A quick survey of the window glasses shows no signs of a breach, but my wife discovered that the familiar car was again parked at the end of the bus stop below our property. Then there came another blow on another of our windows! As we opened the balcony door, the car's lights were being put on, the vehicle made an abrupt U-turn, accelerated rapidly and disappeared from view. Perhaps this latest 'geoscientific' incursion was meant as a kind of greeting because my lecture in Stavanger the evening before had been successful? Seemingly, my scientific activity had triggered some criminal countermeasures. We reported the incident to the police. "*I never thought I would experience science at that level*", my wife said – clearly shaken by the incident.

We gradually got to know that others in our neighbourhood had been wondering about the mysterious car. Our closest neighbours – Knut and Kate Böe – told us of a couple of strange observations events they had witnessed which had been the subject of discussion between them. On one occasion – earlier in the winter – they had from their living room windows seen a car stopping on the road immediately below our property. The car's windows had been rolled down and some kind of apparatus or instrument had been directed towards our house. "*What's so interesting about Storetvedt's house*", Kate Böe had said to her husband. This event had taken place several weeks before we had begun to wonder if we were being subjected to monitoring. Now that the case had been so clearly exposed, I contacted people with knowledge of electronic surveillance equipment. They came to the conclusion that one or more audio sensors may have been placed on the columns along the basement floor of the house (under the overhang). Our private conversations would then have been registered in the parked car about 60 metres away. The noise that awakened us on several occasions probably came from someone stumbling on the unlit and rocky ground, in conjunction with dismantling and removing what would have been incriminating equipment after the 'evening duty'. A specialist in ballistics believed that the shots against our living room windows on March 2nd may have come from an air gun or similar weapon. Because of the oblique angle at which the projectiles struck our windows, it was likely that the shots had been reflected without damaging the glass.

Immediate reactions – at the campus and abroad

Chicanery and dirty trick activity, triggered by my scientific work, had now been going on for many years, and had gained such a primitive character, that I found it necessary to speak out about the conditions to my closest contacts at home and abroad. Thus, on April 14th I gave a 3-hour lecture for visiting scientists as well as interested people from other departments. The Vice-Rector of the University, Ian Dundas, was also present. When I told him about the latest developments, he became very upset over the situation. "*You must not let this terror break you*", he exclaimed. News of the nasty tricks played against me by members of the geosciences 'club' was now spreading around the campus – but as it turned out the reactions were not in my favour. My own faculty took a rather weak stance on the issue; the case was of course embarrassing, but it could hurt the reputation of the University and therefore had to be hushed up. The Geophysical Institute, on the other hand, suggested me a far more purposeful strategy.

"*Forget your university and* the *Norwegian* geological community; do *all your lecture* activities *abroad, where they will not be able to reach you*"! This was the recommendation of the Head of the Geophysical Institute – Professor Øystein Hov. But some worrisome letters came from abroad. An American colleague wrote:

> *"I was very sad and disheartened to read your letter of May 5. This is awful! We are now back to the Galilean time. You should be very careful, my friend. Throughout history, scientists and creative people have been ostracized for their heretic views. Yet, I believe, life is too valuable to sacrifice for this noble cause. You have to change your strategy since your opponent is such a powerful global enterprise. Instead of giving popular talks across the country, you should concentrate on writing monographs and books, putting all your ideas together, and leave the result for the posterity. Side by side, you should start working on something which is equally interesting but based on safe ground".*

In other words, my American colleague suggested that, in light of the methods being used by certain activists within my Norwegian opposition I would be exposed to excessive risk if I continued to lecture domestically and should therefore focus on something less contentious. In his Christmas greetings, he clarified his recommendation:

> *"Teach something conventional, 'garden-variety' geophysics, so that your job is secured. Once your book is published, you will have a wider audience".*

My American friend thus went so far that he suggested I should hibernate with my tectonics book project – for safety reasons my theory ought to be completed in secrecy. But to do that would simply be a surrendering to the terrorists as well as losing my self-respect by pretending I had changed my work to focus on more conventional aspects of geophysics – of the non-provocative type my friend had called 'puzzle-in-the garden'. I had been concerned with small palaeomagnetic projects for most of my academic career, and as long as I did not come up with challenging ideas my university life had passed quietly. But my primary research interests were now aimed at greater and more fundamental questions – a change that had flipped my university life upside down. I had not done anything that was illegal, only espoused research and educational activities that should be absolutely central and obvious to the idealized role as a university professor. It has been said that "*If you once have been struck down, but manages to get up again, you will hardly be beaten down once more.*" It was just such a mental strengthening process I had passed through, gaining a fighting spirit that was about to be crowned with quite a strong come back. I had no intention whatsoever to become an intellectual coward.

I had become aware of the necessity of struggle. I therefore continued my domestic and international lecture activity as before. As I saw it, the intimidatory non-academic activities were regarded as mere 'warning shots' – attempts to destabilize our family life, presumably with the intention of stopping my travel and lecture activities. The bugging had probably had as its main objective to gain knowledge of the timing of my absences, and also to obtain any information about our family that potentially could be useful in the ongoing fight.

A phone call from an executive officer at the Norwegian Research Council advised me not to send further applications to the Council: "*Every time you apply, you become the victim of a massive slander campaign; it may not be in your best interests that this continues*", he said. Although this information was far from pleasant, I had to reconcile myself with this new reality. My attack on what had been established as fundamental thinking in the geological sciences had generated such turbulence that the flora of primitive defence mechanisms apparently flowed freely in some of my colleagues. Until 1989, I had been supported generously by the Research Council, but after having officially dismissed plate tectonics and, particularly, by launching my alternative theory, most financial doors had been closed. A combination of jealousy, ambition, backbiting and suppression of dissent has a long story in science funding. For example, biologist Lynn Margulis – famous for her invention of the endosymbiosis theory – describes the reaction to a request she made for funds from the National Science Foundation (NSF) for further work on her theory (Margulis 1977):

> *"I was told by an NSF grants officer (after having been supported nicely for several years) that 'important' scientists did not like the theory presented in a book I had written and that they would never fund my work. I was actually told that I should never apply again to the cell biology group at NSF".*

A few days after I had had the discouraging conversation with the grants officer of the Norwegian Research Council, I met Sigve Tjøtta – formerly professor of mathematics at UiB, in town. We had a long talk about disturbing aspects of academic culture. He admitted that much was wrong in the universities. He emphasized particularly the widespread camaraderie as one of the greatest problems. After I had summarized some of my personal experiences, he commented: "*I am not the least surprised by what you tell me, for scientists have no higher morals than most people!* He further said he understood quite well why I had to decline well-meaning initiatives to get me elected to the Norwegian Academy of Science. I understood that this had been a discussion topic in certain academic circles, but at least some had realized my awkward situation. Professor Wilhelm Bjerknes and his Bergen school of meteorology revolutionized meteorology and weather prediction in the 1920s and since then Norwegian natural science had hardly produced ideas of wide-reaching paradigmatic importance. And besides, my work had gained more publicity than most researchers could even dream of. My case was therefore quite unusual and troublesome for some, I understood; had I got myself on a high horse? – Some wondered.

I had fought for years for my integrity and intellectual freedom, and now it seemed that the arduous struggle was about to have a successful end. Earlier in life I had seen too many examples of how people had been 'bought to silence'. The sociological mechanisms are well known; if someone wants to be nice to you there is an ingrained desire in most of us to reciprocate that kindness. And the only way I could repay was by giving up my global tectonics theory – presumably re-establishing peace and quiet both in the University and within the national geoscience community. But I was approaching a scientific peak in my career, so I had to avoid any restraint of my intellectual freedom. I felt I had no choice in the matter. Another mathematics colleague in Bergen later put it quite succinctly: "*I understand*! *You were tied up* [like Gulliver] *by your own situation*".

After the shooting incident in 2nd March 1994 there was a long time without nocturnal disturbances around our house. But a new development came in early January 1995 while I was on a trip to Holland. About an hour after midnight, my wife was awakened by the intense ringing of the phone. It was our young tenants in the basement flat who said they were frantically trying to wake her up. "*We were now close to calling the police ourselves"*, they exclaimed. They had been sitting up late talking, but suddenly the silence had been broken by someone rummaging outside their windows. They had been so terrified that they were unable to find out what was going on. My wife had immediately called the police who were on the spot 15 minutes later. However, by then the troublemakers had disappeared though leaving clear foot prints in the snow-covered ground. The incident had shaken up everyone in the house. When I got home a few days later we had another conversation with the police. They mentioned that they were well informed about the illegally parked car by the bus stop below our house the winter before. Everything indicated now that someone had been informed about my recent trip abroad, and thus tormenting activists had gone into action again – against an innocent party: my wife.

Ten months then passed without anything unusual, but then struck the activists again – in the middle of November 1995. I was away in Warsaw as a guest lecturer invited by the Polish Academy of Science. A few days after my departure from Bergen, at half past nine in the evening, my wife – who was alone at home – was suddenly surprised by sharp blows against the front door; someone was using considerable physical force against the door handle as well as demonstratively rummaging around in the shingle outside. Petrified she called the police. By the time the police arrived 10 minutes later the 'birds' had flown – but again they had left their marks on the frost covered ground. After this episode my wife found shelter with our daughter, until I was back from Poland. We then decided to install more lighting around the outside of the house.

The Head of the Geophysical Institute – Øystein Hov, professor of meteorology – now went into action. He was in no doubt what measures had to be implemented. "*We will 'anonymize' you! Henceforth no one shall know about your absences and travels – not even your colleagues in oceanography and meteorology*"! He maintained that this was the only way to end the scourge against my family and my scientific activity. The science faculty had to agree. Moreover, the Faculty Director suggested that my wife ought to have hotel accommodation during my absences. Since this extraordinary situation had arisen directly in connection with performing my work, I suggested that the Faculty should cover the unexpected expenses. But the Faculty was not ready to go that far. As I understood it, although the problem brought shame onto the university, they would avoid giving it an official stamp. However, it turned out that the new scheme of keeping my travels secret ended the blatant harassment around our property. No more mysterious vehicles or other suspicious activity was observed. A leak had been stopped.

By now, my experiences had become widely known within the university. Such situations are ideal for 'academic' gossiping. I was soon being blamed for the local turmoil: among high level bureaucrats, I was a troublemaker and disloyal colleague who spread falsehoods about the university. For example, it was later claimed that scientists who went their own way in research and did not follow the institution's priorities deserved no salary increase. In other words, wage increases should be reserved for those obedient to the ruling clique. But colleagues from other science institutes made fun of my scientific opponents. For example, at the annual university dinner, in early March 1996, I got to converse with a group of biologists who amused themselves that some geologists apparently lacked a rational debate culture. In particular, I remember the following comment from professor of botany Dagfinn Moe: "*If your colleagues here are so confident that plate tectonics is a durable theory, why do they then have to shoot at your windows*?"

From the Bergen arena: lectures and reactions

Towards the end of 1995 I realized that a completely new situation in the socio-scientific process had developed. Health wise, I was practically back to normal, as the tropical infection I had incurred during a field trip to Madagascar in the summer of 1987 was about to fade away. The university seems to have noted this change with some 'concern'. After I had begun to show my rejuvenated strength by outspoken statements in articles and at seminars, a top level bureaucrat at the University said: "*I have feared it would only be a matter of time before you would start 'compensating' for what you have been through*"! And maybe even more precisely put, by the same person: "*If you publish your story, people will begin to wonder if the University knows what it is doing"*. In the last statement, there was perhaps a certain threat lurking in the background. The hypocritical protectionism would eventually become enveloped in a tragi-comical light.

During the spring of 1996, my work spirit was impeccable and my first monograph on Earth evolution (published in 1997 under the title of *Our Evolving Planet*) proceeded with great inspiration. I suddenly felt an enormous freedom. I did not understand it at first, but gradually I realized that I had lost my fear of the authorities. At the same time, I made multiple trips abroad during which I constantly registered the profound lack of freedom of speech within the geosciences. The plate tectonics dogma had brought the geosciences into a coercive situation; younger researchers and new students in the field were apparently persuaded not to challenge the ruling dogma. In addition, I received multiple lecture invitations from non-geological institutes and societies in Norway. Thus, I gave a talk in the lecture series in Science and Philosophy at my university – entitled *Scientific Theories and the Human Factor*. For the announcement I had submitted the following short summary:

> *"Logical and rational thinking has traditionally been thought to be the trademark of science, but such ideals seem primarily confined to technical and observational aspects. Theories, especially those that form bridging structures across specialist disciplines, has a much weaker intellectual foundation and are often heavily infiltrated by sociological, psychological and political factors. The lecture will be illustrated with simple examples from the geosciences"*.

It hardly came as a surprise that I would focus on non-intellectual aspects of the scientific enterprise. In fact, colleagues at the Centre for Philosophy of Science could tell that the controversy surrounding my global geophysical theory had triggered a broad discussion in the university – about practices and methods in science. In lectures and at seminars, I had argued that the plate tectonic revolution at the end of the 1960s had been little more than a cultural invasion that came forth from Anglo-American research institutions. The combination of authoritarianism, the repetition principle, wishful thinking, and the many socialization mechanisms had been the driving forces behind this ‘conversion processes’. To the hard core of geoscientists this, my proposition, was worse than swearing in church.

The lecture began by highlighting a growing problem: the tendency to glorify certain popular natural science hypotheses, at the expense of factual information. Big-bang cosmology and the plate tectonic thesis could be regarded as flashy examples of this phenomenon. Presentations of such major scientific viewpoints are nowadays often given in the form of colourful and glorified mass entertainment. Textbooks, right down to the primary level, as well as TV presentations for the general public, give the impression that these ideas are absolute truths, and many widespread but unproven statements have been repeated so often that they have now achieved almost authoritarian status in many parts of society. On the other hand, most specific questions never seem to be satisfactorily resolved. This is a great paradox.

It is often asserted that the major aspects of Nature are known, and have been adequately described by some popular theory. But when it comes to understanding the observations and phenomena, which the wide-reaching theories are meant to explain, researchers frequently ignore uncomfortable inconsistencies and sail on serenely as though the foundation of their belief system was entirely secure. In other words, the expectations are not consistent with reality! Or put in another way, the theories do not work as intended. If such conditions are not openly acknowledged and taken into account, much scientific effort will inevitably be based on misleading assumptions. The result is accumulation of fictitious information, with stagnation as a consequence.

I went on to say that within science there are many delusions about the origin of theories and their intellectual status. It is often alleged that a theory has been “proven” by some specific set of measurements or observations, but this speaks only about lack of insight. Any limited data set can normally be accommodated in a variety of bridging theories – the number of such possible theoretical abstractions is only limited by our imagination. It is only when a number of different types of observation (and phenomena) finds their natural positions within the framework of a theory – meaning that the range of phenomena are connected by means of progressive prediction – that there might be hope that it has an acceptable probability. Such pioneering events are, however, very rare of science. It is a fact that most scientists never go in depth into their fields. The ‘burden of proof’ is conveniently left to others. This academic alienation coupled with the authoritarian dissemination of scientific knowledge is apparently the reason why popular theories are defended with an almost fundamentalist vigour.

I went on to say that major scientific progress is often not a matter of more and better measurements, but the ability to sort phenomena, thoughts and ideas; in other words, to structure the ‘world’ through good theoretical inventions. Although theories are the weakest part of science, they nevertheless form the foundation of rules and terminology that govern its operations. Therefore, new creative thoughts will always interfere with this ‘balance’. Cultural and psychological influences and barriers, as well as cut-and-dried opinions will always form strong barriers against major changes in basic thinking. Often the scientific content of the theories get subordinate importance, if they are being seriously considered at all by the scientific majority. In my seminar I referred to many well documented and perceived cases which support this description of science at work; it is also completely in line with Thomas Kuhn’s statement that “*science students accept theories on the authority of teacher and text, not because of evidence”*.

Thomas Kuhn was very polite when he only referred to students, because the same claim of the remoteness from scientific fundamentals might as well be said of most researchers. It is both time-consuming and capacity demanding to penetrate the depth of theories. Also, pondering basic theories yields little if anything in today’s mass-producing culture of competition. Being guided by current popular perceptions is thus the way many science communities work. Conventions are often so ingrained, and the consequences of innovation so severe, that a scientific community will have trouble imagining that there may be a different reality than that advocated by the current ‘beliefs’. To conceal the lack of confirmatory data for a popular theory – the hallmark of an ethical breakdown in science – is something that has haunted plate tectonics from its beginning. I finished my lecture with some words about the declining interest in science in many Western countries. The problems seem to be primarily related to uninspired and unsuccessful teaching guided by contemporary lightweight values.

Surprisingly enough, the lecture – which was rather unusual in a scientific context, was well received and created a very good mood. The audience chuckled on several occasions, and the subsequent 45-minute question-and-discussion round was also characterized by good humour and a relaxed atmosphere. My lecture had fallen on fertile soil. The next day I received several enthusiastic telephone calls from non-Earth science people who had been present. I was also encouraged to get involved in the activities in the Centre for Philosophy of Science. These positive reactions told me once more that it is good for scientists to have 'pressure valves' – open chat forums that can lessen the stress caused by the academic territorial battle.

In late fall the same year I also gave a philosophy of science lecture at an evening meeting of the Bergen Geophysical Society. The lecture was entitled: *The pathological-pedagogical problems of science*. The next day I met one of the participants – State meteorologist Karstein Eitrheim, who commented: "*To judge by the reactions you got yesterday you said the right things at the right time. I'm sure many got a lot to think about, and in my opinion this kind of lecture should frequently be repeated.* "

In the mid-1990s, my situation was a favourite topic of conversation and gossip within the university. Colleagues from institutes other than geosciences shook their heads at the behavior and methods exercised by some of my opponents. On the other hand, to justify the turbulent situation a number of colleagues from Earth science departments argued that my problems were entirely self-inflicted. Why couldn't I be a good professional 'mate' – sticking to and teaching the conventional beliefs? Some even argued that I was disloyal and that I brought disrepute to my university. I was described as both a renegade and traitor. Foreign colleagues reported that rumours from Norway said that the university had forced me into early retirement and that I stood tilted in my grave. Therefore, they were quite surprised when they found me in good shape guest lecturing abroad.

After a lecture at Berlin Technical University in October 1994, on global-geophysical theories, I was invited to emphasize some of my points by giving, on the following day, a seminar on science-related philosophical questions. Both presentations were well received and sparked off exemplary discussions – a response very similar to which I experienced in Laramie, Wyoming, a month later. My host in Berlin, Professor Johannes Schröder was, during the subsequent dinner, unusually open about the 'pretended game' (as he called it) with which he had to deal within his own everyday academic life. Even with other guests present, he told me how he had to 'zigzag' so as not to be squeezed out of the 'good company'. Johannes Schröder encouraged me to write a science-based biography about my experiences. He considered that, since my theoretical work embraced such a large segment of the Earth sciences, and 'confronted' it on a broad front, I would necessarily have a broad base of experience that few others could emulate.

Eventually, at my own university, many requests surfaced that I should publish my scientific history and all the non-academic consequences that followed in its wake. Thus, during a lunch break in spring 1994, Knut Maartman-Moe – professor of chemistry, said to me resolutely:

> *"If you do not already keep a dairy of all the irrational reactions you have encountered in your scientific activities, then you have to start immediately! Write down everything that passes on your way, and when you have collected sufficient material you must write a book about your experiences. I will be the first buyer"*.

At the Centre for Philosophy of Science it was claimed that my story could be an interesting contribution to the philosophy of science literature. It is likely that my experiences would include many tough questions about the science culture – aspects of which are rarely broached. When I recounted what had happened around my professional activities, there were many 'under the counter' confirmatory echoes or reactions. Statements such as "*what you say seems familiar*" were also heard now and then. But the Vice-Rector – Professor Ian Dundas, was the most forthcoming of all:

> *"You should write a book about your scientific developments and about your experiences as an independent researcher. Particularly, you need to tell how such a comprehensive theory as yours came about. You simply owe science your personal and candid story!"*

It was nice to hear that I had supporters within the governing bodies of the university, but a couple of years later I regrettably got to know that the hard core of opponents to my work actually worked within, or close to, the Central University administration; to a major extent these were people from academic branches other than the natural sciences.

"*You should still be happy that you are based in Norway! Had you been employed at a North American university your job would have been in jeopardy,*" said Professor Jim Wright of the Memorial University of Newfoundland.

From a trip to India

In January 1996, I attended a volcanological meeting in Mumbai, India, with a subsequent excursion in the famous Deccan complex. According to the scheduled programme, my 30-min. talk had been placed near lunch time on the first day. The opening lecture was to be by an American geologist, but due to a delayed flight this lecture had to be postponed. On short notice, I was asked to take the available space in the programme. Given the non-traditional character of my lecture such a prominent position in the schedule was very brave of the organizer – Professor Subbarao; unconventional contributions at conferences are normally 'hidden' – appearing either as the last talk before lunch, or placed at the final presentation of the day. I think it was Professor Keith Cox (Oxford), with whom I shared room, who had whispered to the meeting management that my presentation could be suitable as an introductory lecture. During the break after the lecture, I was surrounded by curious Indian geologists and geophysicists who expressed great astonishment as the plate model really had so many grave problems as I had demonstrated, and that it was actually possible to view geological developments within the context of a brand new theory.

Later that day, Keith Cox pointed out that, because the new theory covered so many different disciplines, he had had trouble forming an overall picture of the new ideas that I had presented. He readily admitted that most scientists have expertise in only narrow fields, and therefore it becomes difficult to deal with scientific theories that lie outside these specialties. Later during the meeting, Keith Cox and I had many frank discussions about the fact that most geologists have a rather minimal understanding of larger over-arching concepts. He agreed that most geologists have a very distant relationship to the pertinent theories, and eventually admitted that he himself had only peripheral overview of the current status of plate tectonics. His time was so heavily loaded with small projects, often in collaboration with colleagues, that he left the 'big thinking' to others, he said. Such a frank 'confession' from a respected British geologist was liberating.

At this meeting, it did not prove to be easy to make contact with the other participants from Western Europe and the United States, so it was impossible to know what they really thought about the questions I had raised. Conversations during the subsequent excursion were mostly of the detailed variety pertaining to field-related issues that came up at the various locations, but some wanted to know how my life as a scientist really was, after I so strongly had set my face against the conventional ideas in global tectonics. Professor A. De from Calcutta commented that it could not have been easy. His wife referred to the Indian writer Rabindranath Tagore who had given a good description of the situation for those who swim against the stream. She believed Tagore's words could be comforting for a scientific iconoclast like me. Thus, Professor and Mrs. De called home to get the precise wording on Tagore's poetic text, which was then presented to me as personal words of comfort. Tagore's poem reads:

Akla chalo, Akla chalo, Akla chalo, Re!
If no-one responds at your call,
Walk alone, walk alone, oh! Walk alone,
If no-one talks
If everyone looks aside
If everyone is afraid
Then open your heart and open your mouth
To speak the words of your mind, you alone.
If everyone goes back
At the time of going through the difficult path
No-one looks back at you.
Then the thorn of the path, oh! You alone,
Crush under your blood smeared feet.

On the first night during the excursion we stayed in a small provincial hotel, and the evening buffet dinner was served in the hotel garden. The food was placed on a long table with dim lighting from kerosene lamps. Otherwise it was pretty dark, and we had to tread carefully in the uneven ground. While I stood in the dim light enjoying my food, one of the American geologists came up to me and began a chat. He first thanked me for an article I had sent him the year before at his request. But he soon switched to questions that were occupying his mind – the role of plate tectonics in today's Earth sciences. In this regard, he made the following surprising statement which is still ringing in my ears:

> *"It is quite right as you say that plate tectonics is in big trouble; the model is now so full of special cases that it is doubtful whether it really has given us the answer to a single one of the major problems in global geology. Consequently, it seems that a new theoretical revolution is necessary!"*

This concession came almost like a lightning bolt; I had not expected that, at this meeting, I would hear such a 'confession' from a well-known American geologist. It seemed indeed as if he needed me to ease his 'conscience'; it appeared as if I had served as his pressure valve and thereby relieved his inner discomfort with respect to problems he could not talk openly about with colleagues in general. This and other conversations during the excursion gave me increased confidence and belief that my iconoclastic project was a step in the right direction. Back in Mumbai, it was nice parting with the other Western participants; for a moment I thought I had acquired at least a couple of important western contacts. But scientists do not live in an open and free intellectual world – they must neatly fall in line with the tyranny of prevailing opinions in their respective environments, a fact of life of which I would soon be reminded.

Some little time after the meeting in Mumbai I was to take a trip to Canada. As my travel schedule was routed *via* New York, I therefore wrote to my American colleague (whom I had met in India) that I could well stopover for a few days in the New York area before continuing to my final destination. I said it would be nice to visit his institute, and maybe give a seminar if that could be arranged. There was never any answer, and the explanation for his silence was remarkably simple. In our private conversation under the dark Indian night, he had been able to act as an open and free person and, without any risk, say what he really thought. But at his home base, however, he was bound and gagged. It might have had unforeseen consequences for his academic activities if he had shown signs of doubt about plate tectonics. As is well known, disloyal people are commonly pushed out of the 'good society '.

Behind a European scene

A shocking death hit the geoscientific community in early December 1995. The legendary Keith Runcorn – the prime advocate of Alfred Wegener's hypothesis of continental drift after the 2nd World War – had been killed in San Diego. It had been arranged that he would come to Bergen a couple of months later to give a lecture about the controversy between Galileo Galilei and the Pope. In several ways, Keith had been a beacon in European geophysics since the early 1950s, and he had taken the initiative to establish the European Geophysical Society in the early 1970s. It was natural therefore that some of the symposia at the then upcoming EGS General Assembly, in The Hague in May 1996, were given a certain professional re-direction so that they could serve as "Runcorn memorial symposia". In addition, the executive director of EGS, Dr. Arne Richter, thought that since it had been known in geophysics circles for many years that Keith no longer sympathized with the plate tectonics theory, this information had somehow to be exposed in the memorial programme. As I had been in close contact with Keith on these aspects, Arne Richter proposed that it would only be natural that I should be the one who would make known this information, both verbally and in writing.

As an old palaeomagnetist, Keith was course a mobilistic crustal geophysicist. It was impossible to get away from the accumulated fact that some form of relative motion between the continents had to have taken place relatively late in the geological past. Moreover, modern space geodetic measurements indicated that slow movements were still continuing – not only between continents but also between regions within individual land masses. Keith reasoned that these movements were most likely caused by dynamic principles other than those underlying the plate tectonic model. Besides, he had little patience with the overall thrust of most of the massive efforts then going into geophysical and geological research that took plate tectonics for granted – driven by people who he, with a certain glint in the eye, called 'the orthodox'. Global tectonics appeared confusingly unclear – "completely muddled" as he often expressed it. The growing scepticism with which he, in his old age, regarded plate tectonics was widely known by his more intimate acquaintances. The ever increasing problems facing this model were constantly aired in personal discussions and letters, but as far as I know he never publically proclaimed this distrust.

There can be little doubt that the plate model was a promising geophysical idea at the time it was established (in the 1960s). It gave the promise that global geology would be lifted out of the ditch in which it always had laid, but after a few years problems began to rear up. For an established research community such a situation would naturally create unease in the ranks. When even one of the professional 'guarantors' of the thesis (Keith Runcorn) no longer gave his unreserved support, the discomfort became even greater. But science should be an arena for honesty and integrity, not a scene for fake games! In a special *Runcorn Memorial Volume* I therefore published the last two letters he sent me - dated Fairbanks, Alaska, 25 October and 21 November 1995 (Storetvedt, 1998). After the publication of these letters, I received some grumpy comments from a couple of plate tectonic supporters; by my publication of Keith's opinions they apparently felt 'harassed'. Keith's acidulous allusion to the current dominance of plate tectonics is evident in the first letter, within which he had written:

> *"Incidentally why don't you study the surface of Venus; the Magellan data is* [sic] *wonderful, but it is being studied by people imbued with plate tectonics ideas and I feel sure that Venus needs a mind uncluttered by* <u>*geo*</u> *scientific orthodoxy."*

At the EGS meeting Arne Richter was aware that certain 'forces' had tried to derail the memorial symposia in honour of Runcorn. "*Even before the man is under the turf, the struggle for 'power' flares up*", said a very indignant Arne Richter.

But the ceremonial symposia went its course. In my two lectures, I presented some basic problems with plate tectonics - including Keith's latest 'kick' at the orthodox - as well as a loose sketch of my own replacement framework. In the audience, sitting in the front row, there were a couple of senior American geophysicists for whom plate tectonics formed the very basis of their academic careers. As expected, they challenged me immediately after my two consecutive talks. One felt that the marine magnetic anomalies were 'indisputable proof' of seafloor spreading. I replied that this claim could easily be refuted by reference to existing data from deep sea drilling. When a new model comes 'on the market', all kinds of data, including marine magnetic anomalies, will be looked at with new eyes and often given a brand new context. I think he understood my point. My second opponent did not present a single concrete argument, but he claimed that he would need a full day to refute what I had conveyed (in 30 minutes). I took him at his word and agreed to further discussion. We would have plenty of time at the EGS meeting, but he did not respond to my invitation.

Professor Ken Creer (Edinburgh) was far more positive in his comments. But he claimed that I had embarked on such an extensive research programme that I would need an entire legion of employees to reach my goal. Dr. David Tozer (Newcastle) gave full support to my description of the many human factors in science. During the coffee break, a female Romanian seismologist came up to me to find out what had set me off towards an entirely new theory of the Earth. "*Oh, that's a long and complicated story", I replied. But in the same moment an American colleague, Professor Michael Fuller, interrupted our conversation with the following remark: "*I know exactly when your scientific independence began: it started at the end of the 1960s when you had trouble understanding the magnetization of the Old Red Sandstone*". [My reconsiderations of experimental palaeomagnetism actually led to an intense debate for many years (1968-1976), but the aspects and problems I had pointed at were finally accepted – after a topical symposium at the EGS Assembly in Amsterdam in 1976]. I was impressed by the clear understanding of my colleague about the technical-scientific issues that sparked off my long-term scientific development; he probably recalled the intense debate at the NATO Geophysics meeting in Newcastle in April 1968.

Moments of joy

1996 was an unusually productive year, particularly during the period March-December when I wrote as much as in the previous three years combined. Immediately before Christmas I was able to go to the publisher Knut Lie in Alma Mater publishing house, with a complete manuscript of my first book. I had struggled for years against the overwhelming majority view, but I now had taken a decisive step towards promoting territorial claims over my own island of knowledge. It was with a feeling of great relief, satisfaction and a certain pride that I went home that day. It was time for a break.

A few days later we had a pre-Christmas visit by Harald Vik-Mo and Kari Todnem – a professor of medicine and chief physician respectively, near neighbours. "*If I had done something similar to what you have now done, I would have been completely isolated in my medical community"*, said Harald. And his wife Kari added: "*But think of all you have experienced in these years, and which you can laugh at when you grow old*!"

After the finished book manuscript had been delivered in December 1996, it took a whole year before my monograph – *Our Evolving Planet* – was released. During this year I undertook several lecture trips to academic institutions in Europe and overseas. Early in December my publisher called and with ill-concealed delight told me that the pallet with the first copies was ready for 'inspection': this had been a long-awaited day. It was common practice that the publisher's latest releases stood foremost among the book pallets in the front office. But when I arrived, however, the pallet with my book was not located in the lobby, but was in a flashy location in the publisher's own office – next to his desk so that all his visitors had the book right in front of them. It took several months before I realized why he had given my book such a prominent placing. The book immediately got great publicity in Norway – with several newspaper articles. After a presentation on 16 December 1997 in Norway's largest newspaper – *Verdens Gang* – Glen Ostling from the news magazine *Norway Now* called. In the telephone interview that followed, which became a humorous affair, Ostling referred to several quotes in the book that he thought would 'outwit weak souls'. Under the title *Measure less - think MORE*, the journal gave a full page to the matter (*Norway Now*, no. 2/98).

Based on his long experience in academic publishing, Knut Lie said he would be very parsimonious in sending out review copies to journals that had not directly intimated any interest in the matter. He referred to several cases where books with new ideas simply had been 'misappropriated' in the reviewing process. For political, collegiate and other reasons, many journal editors showed pathological caution with anything that smacked of support for non-conventional ideas that eventually could make their own positions unsafe. A few periodicals made a direct request to inspect my book – including the Norwegian popular science magazine *Naturen*. For a book that massacred the current thinking of a whole scientific discipline, besides outlining a new mind-set for understanding individual natural phenomena in a larger context, the assessment in *Naturen* was quite unusual. The reviewer, Professor and plant-geographer Per Magnus Jørgensen wrote (my translation):

> *"This is not a popular science book, but even so I would like the readers of Naturen to be aware of it, since Professor Storetvedt has told us about his struggle to get a paradigm shift in the understanding of our planet's geological-geophysical development. Even with the minimal knowledge I have of the subject, I do not find the book difficult to read. There is not a range of convoluted reasoning, rather it is remarkably simple what Storetvedt has to tell; it is so simple that it is difficult for an outsider to understand why Earth scientists have for so long not seen that the movement of the land masses, which have taken place on our planet, is not the result of complex, undocumented movements of the Earth's deep interior (convection currents), but rather a product of inertia forces associated with the earth's rotation around its own axis (Storetvedt's wrench tectonics theory). Of course, there are certainly a considerable number of details that need to be discussed, but this must be done in geophysical journals. However, as a plant geographer I see much (in Storetvedt's theory) that will solve problems we long have struggled with in the southern hemisphere and in the Pacific. Otherwise I find great similarities between this book and Darwin's 'Origin of Species'. Like Darwin, Storetvedt considers simple observations in many fields and adds them together into a compelling whole. He has many more reliable data to build on than Darwin, and can therefore be even more direct and secure, and so he does not need to wrap up his message for fear of ecclesiastical sanctions. It is therefore easier to follow Storetvedt's arguments than Darwin's. The first part of the book deals with the old theories and shows how they run aground with observations that are not consistent with them, and strangely enough, have long been widely known. It is much easier to criticize than to find alternative theories. Storetvedt have for years strived with the latter and demonstrates in the book how he has built a new theory; like Darwin he finds a unifying factor that can explain the totality - in Storetvedt's case: the Earth's rotation. It is easy to see that this book will become a classic – whatever the theory's destiny! It is entirely appropriate to congratulate both author and publisher."*

The next book publicity event came in the Bergen University news bulletin (1/98). Under the headline: *Continental Drift - simply nonsense*? The book was allotted significant space in this issue. A number of key aspects with regard to science in general, and the reactions of the established research communities to innovation, in particular, were discussed. For example, Professor Ragnar Fjelland, Centre for theory of science, considered the fact that academic communities rarely address issues of fundamental importance. Therefore, he claimed there is a great danger that many sciences are being turned into a state of trivialization. He further emphasized the importance of professional disagreement as an essential prerequisite for the advancement of science. In response, a few defenders of the conventional plate model immediately launched general counter attacks of the type: 'Storetvedt's hypotheses lack foundation in mathematical and physical logic '. Their assertions were however so superficial and misleading that I responded with a contribution entitled: *These burdensome theories* (UiB news bulletin, 2/98). Later, however, I realized that the attacks had been so superficial that I might best have ignored them and treated them with the contempt they deserved.

A couple of months after publication of my book, I received telephone call from Arne Richter who, in view of my many years of honorary services to the European Geophysical Society (EGS), offered me a free advertisement of my book (*Our Evolving Planet*) in the program book for the upcoming EGS General Assembly in Nice. He emphasized that this would give the publication wide publicity in the international geoscientific community. This was an offer I was glad to accept.

The first person I met when I arrived at the meeting in Nice was Professor David Gee (Uppsala) who had been previously informed of my lectures in Moscow and St Petersburg (a few months earlier). The positive reactions there, and the content of my book, had been a great surprise both to him and others, he said. During the first evening, participants began to flock to the conference centre, and there were pleasant reunions with many European and American colleagues. Fred Spilhaus, administrative director of the American Geophysical Union, said that he had studied my book and discussed the various aspects it raised with scientists in the United States. During these conversations it had been admitted that plate tectonics struggled with many unsolved problems, but it was asserted that "it is nevertheless the best model currently available".

Next day the convention center seethed with life. Professor Ken Creer (Edinburgh) asked if he could borrow a copy of my book in order to study it further in his hotel. When back at the convention center in the late afternoon, he made the following comment: "*Owing to the extensive documentation you have given, the inventive rumors about you and your work will now slow down. I think your situation from now on will be far better. Henceforth it will be primarily the scientific arguments your colleagues have to deal with"*. Ken further told that he had carefully read the introductory chapters. "*Your writing is straightforward and is hitting well, so no one should feel offended by the content. I was particularly amused with what you have written in a later chapter about Bullard's 'Atlantic fit'. Your presentation covers very well the actual balance of power at the time".*

Ken, however, believed that it was politically unwise for me to attempt to 'climb the mountain' from the front. It would be smarter if I, unnoticed, tried to climb up the back of the mountain and in the dead of night ascended to the top from that side. He had probably a point where there was talk of being 'king of the hill'. But the lust for power was far beyond my interests. As a researcher, my joy in life was primarily that of being an idea- and trend-setter. The career-related 'highs', I would leave with joy to others.

During the first day of the EGS meeting, Ken Creer, David Stone (Fairbanks, Alaska) and David Symons (Windsor, Ontario), independently, came to see me with expressions of sympathy. The essence of their statements was that no matter what happened to my theory in the years to come my independent work was "*a great service to science*". When I the next day told Arne Richter about these statements, he was quite taken aback. "*You cannot expect to get anything closer than that to a concession*", he proclaimed.

Professor Luis Mendes Victor (Lisbon) passed by and addressed me as 'the new Wegener', but without explaining what he really meant by that statement. But it was obvious that there were many opposite reactions. These were, however, not delivered verbally – but with a cool body language. Some pretended that they did not see me, while others said 'hello' without smiling – as if their mood had been left behind in the shower. As I experienced it, the attitude at the meeting was superficial; conversation was free and easy going as long as we talked about the 'weather' – but as soon as the discussion touched upon more demanding issues, many pretended to be in a hurry to reach 'the next talk'. All in all, it was not possible to initiate discussions on fundamental questions at the 1998 EGS meeting. It seemed as if colleagues neither would nor could relate to the many unresolved issues in global tectonics. They apparently would not admit that there might exist something of interest outside their pre-defined plate tectonic world. The reserved attitude was probably an expression of a kind of pathological fear of discussion – of anything that was beyond the daily technical and observational world. Science evidently has its share of *The Emperor's New Clothes.*

When I returned to Bergen, I met the new head of the Geophysical Institute – Professor Herman Gade. He said he was very surprised by the positive reactions I recently had received. "*I just read a couple of reviews of your book and I am overwhelmed. If this continues, you really have made it in life. When I think back on all the resistance you have had here locally and nationally, this is very well done. Just to get your case up to an open debate is an achievement in itself"*, he remarked.

Revealing information

In early March 1998, I attended the annual university dinner for professors and higher administrative personnel. During the dinner party I came to converse with two people from the University's central administration; with serious faces they told me that within, the central sections of the university, there were many who strongly repudiated the validity of my professional crusade. A little later I met one of my informants in the city, and again he repeated what I had been told during the dinner; he thought it was important that I was aware of what was going on behind my back. With my knowledge of university life – with its sharp elbows and pressing need to maintain a high profile – this discouraging information was not unexpected. I was fully aware that the extensive media attention my theory had attracted had created a certain degree of discomfort in the local hierarchal nest of prestige; hence, some ugly countermoves were to be expected. From the Norwegian Petroleum Directorate (NPD) in Stavanger I received an e-mail from a high rank employee (a former student) who said that "*some in the NPD lose their cool completely when your book is being informally discussed*". I would soon learn more about the many faces of resistance.

My contact with Knut Lie, my publisher, had begun at the end of 1995 – 2 years before he published my book *Our Evolving Planet.* But when I came to see him in 1995, he could tell that he already had been waiting for me for a few years. "*After all the media hype that has surrounded your work there must be a basis for an interesting book*”, he said. As a publisher, he had a very wide network of contacts at the university – and of course a good overview of the institution's inner life. What he possibly knew about my situation, he had so far kept to himself. As a publisher, he had of course to exhibit a necessary discretion with his authors. However, when I called at his office in October 1998 I happened to mention that I had received information that the opposition to my professional activity stretched far beyond the geoscience ranks. Then it suddenly dropped out of him:

> "*Yes and Amen! Guess that I have heard them"!*

Then followed a long story about how people at the university, without any knowledge of the scientific aspects concerned, had warned him against publishing my book. The pressure had been both lengthy and uncomfortable, even though he had not been subjected to direct threats, he said. And he continued:

"The opposition against you knew no professional boundaries. But as the calumnies against you became stronger, the more sure I became that Alma Mater would publish your book. I revealed their ulterior motives at an early stage. In conversations, I understood that their defamation of you had nothing to do with science. Their false accusation play was only a means for positioning of their prestige and trumpeting for their own excellence".

Knut Lie went on to say that as he had followed the developments since the early 1990s, it was the extraordinary attention around my person and my work that primarily had brought the activists into play. I had simply gained too much press coverage, and thus I had 'stolen' some of the attention to which they themselves felt entitled. As a protest against their evil tactics, and by declaring his independence, Knut Lie ostentatiously had placed some of the remaining copies of my book next to his desk and right in the face of the visitors.

“When you present such a major theory as in your case, you will have to be prepared for envious glances. Thus, every possible weapon will be adopted. When power-riddled people and those who cannot get enough attention feel slighted, they will soon revolt”, he stressed.

Knut Lie illustrated the situation by telling me about a recent visitor who had made negative comments about me and my professional activities, to which Knut Lie had retorted:

> *"Karsten has had many invitations, both before and after his book came out, and has also recently given several seminars to the petroleum industry. The fact that the university has reacted so strongly against Karsten's work is the stupidest thing the university has ever done. What do you intend to do now?"*

After this surprising countermove, the visitor looked sheepishly down and answered: “*I understand your point! I think you're right!”*

Evidently, the prestige- and publicity-hungry academics were not going to have everything their way.

I had experienced some strange things in connection with my scientific work, but that the university community could stoop to such a low level of behaviour was still surprising. Back at the Geophysical Institute I headed straight to see the head of the institute, Herman Gade, and informed him about what I had just been told. A baffled Gade burst out:

> "*You have not done anything other than what you are hired to do. Your 'transgression' is certainly that you have received more attention than most scientists normally get. Incidentally, what have they over there* [the central university leadership] *to do with your scientific work*?"

A few months later (in early 1999) another colleague at the Geophysical Institute, Professor Sigbjörn Grönås, came forth with the strong recommendation that I ought to ignore the university's negative attitudes. He recommended that I should put all my energy into finalizing my new book projects, and otherwise be active as a guest lecturer and seminar leader, primarily abroad. "*Do not be deterred by a degrading culture which we, as individual researchers, can hardly do anything about*", he concluded.

A new lecture, in the series 'Science and Philosophy', followed already on February 23, 1999. This talk was entitled: *When Science Staggers; about the status of popular theories, academic alienation and presentation of self in everyday life*. I was determined to put the secrecy aspects of university life once again and this was my chosen way of doing just that. In my diary I recorded that the discussion after the lecture was open, lively and enjoyable. Then followed an evening lecture on March 17, 1999, for the ‘Society for the Promotion of Science’, to which I had given the title: *These Strange Theories. Some longstanding problems with understanding the Earth's development.*

The only thing that turned out to be unusual that evening was that the President of the Society reported , just before the meeting started – with an undisguised amazement in her voice – that the very day a call had come from a person (unnamed) in the university, requesting anything sensitive I said that evening should be noted down. Thus, because of my openness about embarrassing aspects of academic culture in my home university environment, my lecture activity had come under scrutiny. The ‘fabled’ intellectual freedom had been replaced by control and monitoring. But I had already decided to fight against the ‘dirty tricks’ in academia. In that respect, I would never come to ‘trim my sails to the wind’. After years of struggle, I had gradually gained what our former Vice-Rector Ian Dundas called *the artist's freedom*. Thus, I was in fact well vaccinated against the combined sanctions and reward syndrome. As freedom of speech is an essential prerequisite for freedom of thought, I had won ample scope for further development of my theory of the Earth’s history, development and structure.

Acknowledgement
I am extremely thankful to Chris Argent, London, for his critical and meticulous reading of the original manuscript, suggesting a great number of language/syntax improvements and other necessary changes.

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GLOBAL CLIMATE CORNER

RISING SEA LEVEL FORECASTS: FACT OR FICTION?

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(**Editor's note:** The following article was recaptured from http://www.principia-scientific.org/rising-sea-level-forecasts-fact-or-fiction.html with permission of the author. It also appeared in Council for the National Interest, Western Australian Committee, Winter Newsletter, July 2014, www.cniwa.com.au)

Australia is a great place to study sea level at the present time. Fremantle, along with Sydney has the longest record of sea level measurements in Australia, going back to 1897**.**

Nearby Rottnest Island is the site of famous studies of sea level variation over the past few thousand years, including some sea levels a few metres higher than present.

In the last two years several papers have been published showing there is no reason to be alarmed over sea level rise and several alarmist papers also appeared (see references). Some alarmists claim there will be a rise of several metres by the end of the century. In this article, prepared for **The Council for the National Interest WA (CNI**),***** we see why cherry-picking of data is a real cause for concern.

There are two basic methods of estimating future changes in sea level: direct **observation** by tide gauges or by satellites, and computing by **models**.

Tide gauges are set up at stations to measure sea level at regular intervals. Technology has improved over time. While it is commonly assumed adjacent land is stable, it is known that some land is moving either up or down relative to the sea. The longer and more complete the record the better. The Fremantle record starts in 1897. Another station was set up at Hillarys in 1992. The records are shown in Figs. 1 and 2 (bottom of article).

Without statistical manipulation the Fremantle data appears to show a slight rise but nothing alarming, although there are some exceptionally high tides, and low tides (note 1941 and 1993).

Direct studies of sea level are showing only small rises. Sea level data for the United States and a few other countries can be sighted at: *http://tidesandcurrents.noaa.gov/sltrends/sltrends.shtml*. Most stations show a rise of sea level of about 1.7mm per year, but there is considerable variation even within a single state. Australian records can be sighted in Parker et al. 2013.

Several satellites measure the global sea level elevation. The European satellite, Evisat, provided possibly the best available data. It showed *falling* sea level since its launch in 2002, and for the last two years the decline was 5mm/yr. Unfortunately, Evisat broke down on April 8th 2012.

A recent review of sea level change is provided by Morner (2012), including analysis of satellite data. He writes that the raw data from the TOPEX/POSEIDON sea-level satellites, which operated from 1993-2000, show a slight uptrend in sea level, but if the distorting effects of the Great El Niño Southern Oscillation of 1997/1998 are excluded the sea-level trend is zero. The GRACE gravitational-anomaly satellite data shows that sea level fell slightly from 2002-2007.

Models, predictions and projections

The public think political decisions concerning climate are based on scientific predictions. This is untrue: what the politicians get from IPCC and CSIRO are *projections* based on models. Models depend on assumptions, what you put in (data), the program, and conclusions drawn from the output.

The UN's main adviser, the Intergovernmental Panel on Climate Change uses adjusted data for the input (mostly from the UK University of East Anglia's discredited Climate Research Unit), and their computer models and codes remain secret – not a scientific procedure. How does the IPCC get a runaway greenhouse effect? It assumes an enormous amount of compounding feedback to make CO2 warming heat water vapour, turning one Centigrade degree of heating into 6.4 degrees.

A 2009 CSIRO report "The Effect of Climate Change on Extreme Sea Levels in Port Phillip Bay" for the

Victorian government's *Future Coasts Program* is an example of its modelling. The model is based on temperature projections to 2100 of up to 6.4^{o}C. That comes from the most extreme scenario of the IPCC with unbelievable CO2 concentration of 1550 parts per million. Using up all known fossil fuel reserves would achieve only half this amount. The result was a projected sea-level rise for Port Phillip Bay by 2100 of 82cm. With the help of the Bureau of Meteorology, a further increase due to wind raised it to 98cm.

Cherry picking scares

An example of how to scare people by ‘cherry picking’ - the use of selected data - is shown in The Department of Infrastructure’s State of Australian Cities Report, which claimed that the ocean near Perth was rising at “three times the global average”. They did not use all the data available (from 1897), but from 1993, which happens to be the lowest level in local tide gauges since 1941. This is shown on Fig. 1, but it is clearer on Fig. 2 for Hillarys. If it had used 1999 instead of 1993, it would show that sea level was falling. By cherry picking suitable dates you can get any rate – and outcome - you want.

Stability of the land, and GPS

Sea level does not just rise and fall relative to a dipstick of stable coast. Earth movement can also have an effect, so we have to try to separate local effects from global (eustatic) effects. The sea level of the last interglacial(about 120,000 years ago) is generally around 2 m a.s.l. in Australia, but is raised to 26 m at Mount Gambier and lies below sea level at Adelaide indicating tectonic movement both up and down.

Baker *et al*. (2004) found notches on Rottnest Island marking former high sea levels. Calcareous organisms left deposits within a narrow range that could be carbon dated. The higher notch at about 1.8 m is about 3600 yr BP.

The lower notch at about 1 m is about 2000 yr BP. This level can be found elsewhere, including China and Brazil.

Parts of the Perth region are subsiding due to groundwater extraction (Featherstone *et al*., 2012). Records from a GPS receiver at Gnangara and nearby artesian boreholes show that the land subsidence rate has slowed from about -6 mm/yr to about -2 mm/yr since the reduction of groundwater extraction from the Yarragadee Aquifer around 2005. Hillarys and Fremantle are only 20 km apart but have quite different rates of sea level rise. Part of the apparent sea level rise is due to gauge sinking. The accuracy of estimates of vertical velocities of the GPS domes is still above 1 mm/year, very close to the average relative sea level velocity.

Geological studies

Jones (2005) examined the geological (and other) aspects of coastal erosion in the region. He concluded:

“The majority of the Mandurah to Fremantle sector does not appear to be susceptible to coastal erosion over the next century, despite the fact that the Tamala Limestone is preserved below sea level across the majority of the area. This is due to the fact that this sector has been the primary depositional province for the Swan coast over the last 8,000 years”.

“The Hillarys to Yanchep sector does not appear to be susceptible to erosion over the next century as Tamala Limestone is preserved above sea level along the majority of the coast, and the beaches are well sheltered by three lines of offshore reefs”.

The cause of sea level rise

There are many possible causes of sea level rise, including tectonic deformation of the sea floor, or production of new water at the hundreds of undersea volcanoes, but it is generally attributed to two mechanisms.

1. Warming of the ocean causes expansion of water and therefore a sea level rise. Since 2004 we have the ARGO scheme where over 3,600 buoys measure temperatures down to a depth of two km. They reveal no warming of the oceans. This, of course, is consistent with the lack of global warming over the past 17 years.

2. Melting of the icecaps – yet the icecaps in Antarctica and Greenland are actually increasing. Sea ice does not affect global sea level as it floats (Archimedes Principle). But the fact that Antarctic sea ice reached the greatest extent ever recorded in June 2014 suggests that this part of the world at least is *getting colder, not warmer*.

It takes a further act of faith to think that ocean temperature and icecap melting are in any way related to carbon dioxide.

The sun

It used to be thought that the sun controlled the Earth's climate, but the IPCC and the CSIRO ignore the sun and relate climate change to CO2 and so-called greenhouse gases. Outside that group many still think that the sun controls climate. There is a very good correlation between sunspot cycles and climate. We have entered sunspot cycle 24, and if things happen as they did in the past we are in for global cooling, which could lead to a fall in sea level. For those keen on the 'precautionary principle', we should take the necessary precautions for cooling and a drop in sea level.

Outside our region

Two favourites of sea level alarmists are the coral islands of Tuvalu and the Maldives. Sea level measurements for Tuvalu (and ten other stations) can be seen on Fig. 13 on the Australian Bureau of Meteorology website at:

http://www.bom.gov.au/ntc/IDO60101/IDO60101.200809.pdf">*website http://www.bom.gov.au/ntc/IDO60101/IDO60101.200809.pdf.*

You can see that sea level here is virtually stable. Yet as they are close to sea level it was repeatedly claimed that these islands are in imminent danger of drowning. Webb and Kench (2010) presented the first quantitative analysis of physical changes in 27 atoll islands in the SW Pacific (including Tuvalu) over a 19 to 61 year period. They found that 43% of islands remained stable and 43% *increased* in area. Coral islands are increasing in size because coral grows: the reef is a living thing. Affected by erosion and deposition the coast is modified, but there is no danger of drowning. The Maldives were studied by a team of geomorphologists led by the doyen of sea level studies, Niklas Axel-Morner, and they found no evidence of sea level rise (Morner et al., 2004).

Responsibility

The IPCC does not give genuine ***predictions***, only computer ***projections.*** Furthermore, it does not assume any responsibility for its alarmism.

Australia's CSIRO has legal disclaimers for its scary projections:

"*This report relates to climate change scenarios based on computer modelling. Models involve simplifications of the real processes that are not fully understood. Accordingly, no responsibility is accepted by the CSIRO for the accuracy of forecasts or predictions inferred from this report or for any person's interpretations, deductions, conclusions or actions in reliance on this report*".

Any allegedly scientific document that needs this kind of legal disclaimer is clearly not science. Australian government ministers (and their advisers) claim that their decisions are based on a scientific "consensus", yet they only use the advice of IPCC and CSIRO. But both these organisations deny making predictions, and refuse to be responsible for their computer projections. Computers cannot take responsibility, so presumably it is the government, through lack of due diligence, that is responsible for the expensive and ineffective actions it is now implementing to *combat* the alleged *human-induced dangerous global warming or climate change.* The argument can be extended to local governments that may be impoverishing citizens by their lack of due diligence and imposing extravagant policies based on a very partial view of the available evidence.

The references in *The City of Joondalup Climate Change Strategy: 2014 – 2019*, for example, suggest that the data here is almost entirely from IPCC and CSIRO, and a few other alarmist sources. I suggest it would be wise to have broader sources of information. Direct action against an alleged sea level threat should be delayed until the authorities have examined all the data, not just that provided by modellers.

Conclusions

Mörner and Parker conclude that the Fremantle tide gauge is likely to include a local subsidence factor of about 1.4 mm/year so there is not much left for sea level rise. They claim virtually stable conditions over the last 60 years and full stability over the last 14 years, implying there are no traces of any present day acceleration.

Carter and de Lange conclude that policy guidelines should include (1) abandoning 'let's stop global sea-level rise' policies; (2) Recognising local or regional nature of coastal hazard; and (3) flexible and adaptive planning controls.

I would only add that policy makers should show due diligence and assess the source and quality of their data.

*****The Council for the National Interest WA** (CNI) is a non party political group who recently hosted Professor Cliff Ollier with a presentation titled “Rising Sea Level Forecasts: Fact or Fiction?” Professor Ollier's address was so comprehensive and well received that his speech was the basis of CNI's most recent newsletter. The CNI regularly promotes discussion on topical subjects covering a broad range of issues effecting Australia's national interest. CNI prepares policy papers for distribution to influential policy makers that can be viewed at www.cniwa.com.au. For participation or commentary, contact the Executive Committee at admin@cniwa.com.au.

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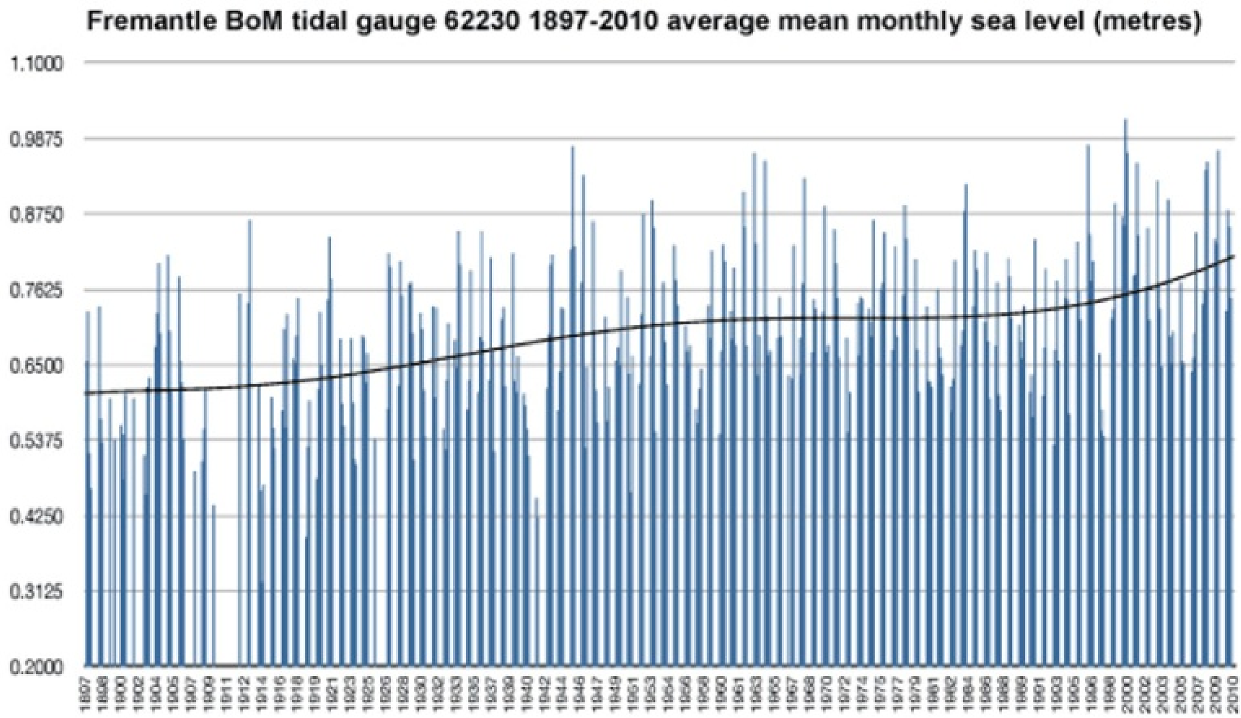

Figure 1. Tidal gauge record, Fremantle, WA

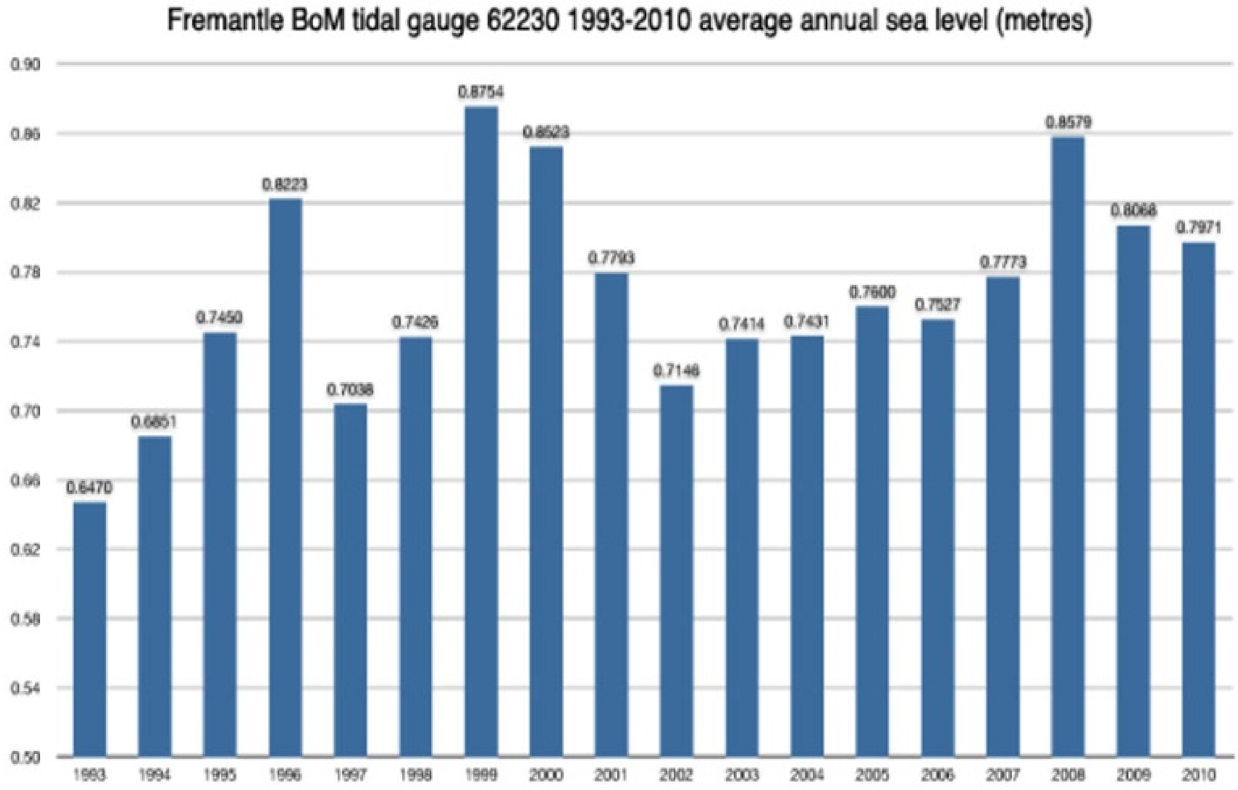

Figure 2. Tidal gauge record Hillarys WA

Summary of Sea Level Predictions by N.J. Ford

Actual sea level rises to date, may be somewhere between 1.4mm per year (Sceptic scientists) and 1.7mm per year (IPCC position). In calculating the prediction errors, the IPCC figure has been used. The errors would be larger if the sceptical scientists' figure was used. These figures assume that the natural sea rises are included in their predictions.

1. IPCC First Report (1990-1992), Summary for Policy Makers, p.52. Sea levels will rise by one metre by the year 2100 (110 years-times). This is 9.1mm per year. **Error to date is 535%**
2. IPCC Second Report (1995), Summary for Policy Makers, p.23. Sea levels will rise by 95cm by the year 2100 (105 years-times). This is 9.0mm per year. **Error to date is 532%**
3. IPCC Third Report (2001), Summary for Policy Makers, p.32. Sea levels will rise by 88cm by the year 2100 (99 years-times). This is 8.9mm per year. **Error to date is 523%**
4. IPCC Fourth Report (2007), Summary for Policy Makers, p.7-8. Sea levels will rise by 59cm by the year 2100 (93 years-times). This is 6.3mm per year. **Error to date is 373%**
5. United Nations Environmental Program (UNEP) in 1988 predicted sea levels would rise two metres by the year 2100 (112 years-time). This is 17.9mm per year. **Error to date is 1,050%**
6. Al Gore and his NASA scientific advisor James Hansen predicted sea levels would rise six metres by 2050 in 1988, a metre each decade (62 years-time), with the Florida Keys being one metre under water by the year 2000.. This is 96.8mm per year. **Error to date is 5,693%**
7. NSW Councils (e.g. Gosford, quoting the best international scientists including CSIRO and ANU) in 1995 was advising residents with water front properties that by 2015 (20 years-time) sea levels would rise by 6 metres. This is 300mm per year. **Error to date is 17,647%**
8. NSW Councils (e.g. Gosford, quoting the best international scientists including CSIRO and ANU) in 2011 was advising residents with water front properties that by 2100 (89 years-time) sea levels would rise by 90cm. This is 10.1mm per year. **Error to date is 595%%**
9. United Nations Environmental Program (UNEP) in 1995 predicted sea levels rises would result in 50 million climate refugees by the year 2010. No climate refugees by that year. **Very large Error**
10. United Nations Environmental Program (UNEP) in 2011 ***re-predicted*** sea levels rises would result in 50 million climate refugees by the year 2020. This is likely to be another - **Very large Error**
11. For the last twenty years the Greens and their scientists have been telling us the islands of the Tuvalu and Maldives were sinking into the ocean. In 2011 aerial photographs taken 60 years apart show the land area of all Tuvalu islands have grown by 5-30%. No discernible change in the Maldives. **Very large Error**
12. In 2005, Professor Flannery, a climate advisor to the Australian Government, predicted Sydney would be covered by 20 metres of water by the year 2050 (45 years-times). This is 444mm per year. **Error to date is 26,144%**

PUBLICATIONS

Earth's crust of oceans. According to the materials of international programs of deepwater drilling in the World Ocean.

Author: Boris A. BLUMAN.
Publisher; VSEGEI Press, St. Petersburg. 344p. 192 figures. 2011. ISBN 978-5-93761-176-5. In Russian.

The work deals with the composition, structure, and relationships of the first (sedimentary), second (volcanic), and third (crystalline) crustal layers of the oceans. The work is based on the author's translation of drill core descriptions from two programs of deepwater drilling in the World Ocean (DSDP, ODP) implemented in the period from 1968 to 2003. Emphasis was placed on the translation of core descriptions of the wells that penetrated volcanics of the second layer of the oceanic crust. Translated descriptions of drill cores under DSDP and ODP programs: for the Indian Ocean - 48, Atlantic - 117, and Pacific – 161. In total for the World Ocean, the work presents a short description of 326 wells of 1,276 drilled under these programs.

Core descriptions are organized by type structures of the World Ocean: passive margins, abyssal zones, mid-oceanic ridges and their flanks, intra- and marginal oceanic uplifts, etc. Maps showing the location of wells for individual legs of DSDP and ODP programs, colour photographs of core, as well as maps showing the location of different-type weathering crusts developed on basalt, places of relic ground vegetation discovery in well core and areas where kaolinite weathering crusts were penetrated on the sialic crystalline basement are appended.

Some sections of the work contain information from recently published papers and monographs that complement the data in the deepwater drill core descriptions. Information on sedimentation gaps between the second and first layers of the oceanic crust is analyzed. The history of passive continental margins and the World Ocean is compared; geodynamic consequences of the presence of weathering crusts in basalts of the second layer of oceanic crust, as well as the nature and significance of a gap between the basalt and sedimentary crustal layers in the World Ocean are considered.

The book is intended for geologists of Russian Interior Industrial Enterprises, as well as employees of branch and academic research institutions, teachers and students of mining and geological universities.

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Comment by Prof. O. I. Suprunenko, Doctor of Geology and Mineralogy, Editor of the Bluman book.

Work, which the reader will study, is unusual, especially in terms of content. And this unusualness is in the fact that the author of this monograph invites readers interested in the structural and evolutionary issues of the World Ocean and the Earth in general to abandon for a while (at least equal to the time of reading this book) from the ready-made global concepts, including the most popular ones, and to rethink perhaps once again over relatively old primary materials of international deepwater drilling projects (programs DSDP, 1963 – 1984 and ODP, 1985-2003). Some of these materials, which characterize the structure of basalt "basement" and lower horizons of the overlying sedimentary cover, supplemented by commentaries of the author and translator (in one) represent the major part of the monograph. Namely such structure of work is precisely defining its unusualness. The author's address particularly to primary drilling data contained in the "Initial Reports ... " volumes for each leg was caused by a natural desire to familiarize himself and now to acquaint the reader with the most detailed description of the raised core and those "primary", non-judgmental conclusions that have been made by experts who conducted the study of core on board of drilling ship, above all, about the possible causes of "basement" basalt alterations, and in some cases – on the conditions of formation of sedimentary rocks immediately overlying basalts or lying between their covers.

One would think, why is the attention drawn to the data published relatively long time ago, and a part of them is translated by the author? The point, however, is that the analysis of geological literature published over the past decade both in the Soviet Union/Russia and abroad definitely shows that it does not practically discuss or take into account at least some basic results of the international deepwater drilling programs – one of the largest successful projects of the world geological community in the late XX - early XXI centuries. And if in our country this fact can be explained to some extent by the limited availability of primary deepwater drilling materials for wide circles of geologists, especially in the absence of their translation into Russian, their ignoring by our foreign colleagues, especially in English-speaking countries, cannot have a logical explanation.

That is why initiative of the author of this monograph to draw attention of professionals to the structural problem of basalt "basement" in the oceanic floor and directly overlying sedimentary formations, as well as the great work that it took to sort the accumulated materials, translate "favourite places", and make a brief commentary on the received results based mainly on the findings of experts who carried out the initial core description – all of them deserves the widest possible support with publication of the prepared monograph as a material embodiment of public approval.

Considering the results of deep-water drilling in various parts of the Earth's oceans - Atlantic, Indian, and Pacific, including passive margins, intraoceanic plateaus and uplifts, abyssal zones, mid-oceanic ridges and their flanks, marginal seas and back-arc basins, island systems, as well as intraoceanic troughs and deep trenches of the Pacific (Chapter 1), and then presenting a concise summary of these results (Chapter 2), the author in the final Chapter 3, "Geology of the World Ocean and tectonics of lithosphere plates", leads the reader to several important conclusions clearly enough resulting directly from the primary core description and from the implications made by researchers who carried out these descriptions. These conclusions are as follows:

1. Weathered crusts of several types (East Pacific – Line and "halo"; West Pacific type; kaolinite crusts) are widespread in all the above areas of the Atlantic, Indian, and Pacific oceans, and a relation between the type of weathering crust, beginning, and duration of its formation is observed;
2. A global break is observed between basalt (or, as specifically noted, volcanic in a broader sense) basement and overlying layer of the World Ocean. Sufficiently long breaks are present inside the sedimentary layer as well;
3. Clastic horizon (rubble layer) is present almost in all wells penetrating the basalt basement; its fragments differing in size carry clearly-defined oxidation rims. This horizon is found over large areas of the World Ocean and is on the site of its formation and transformation

"The most important consequence of the analysis and structure of deepwater drill core in the World Ocean is that the boundary of the first and second layers of the oceanic crust, in essence, is a global break between them, and formation of weathering crusts in the upper basalt basement and the presence of clastics at the bottom of the first layer sequence are a particularly clear expression of this break" (p. 229).

All materials in the monograph demonstrate together that in the previous stages of geological history in many parts of the Earth's surface now occupied by the World Ocean, including the modern deep-water areas, terrestrial or shallow-marine conditions had existed. Lateritic weathering crusts are associated with cover volcanics of terrestrial origin, and near the roof of individual flows, weathered crust formed before the overlying flow effusion have been observed. Such weathering of crusts are known in cover basalts of the Outer Hebrides, Deccan, Karoo, Columbia Plateau and other land provinces. It is very important that in some cases cover basalts of continents with weathered crusts are traced by deepwater drilling wells from land to the World Ocean (shelves of Greenland, Outer Hebrides etc.). There is also evidence of a direct tracing of the Deccan traps for 100 km in the Bay of Bombay limits of the Arabian Sea, and the Parana traps - to 600 km within the Atlantic Ocean waters.

Replacing of subaerial conditions by submarine in different areas of the World Ocean took place at different times: from the Triassic-Jurassic (north-east Australia, Falkland Plateau, west Africa, north-west of the Pacific Ocean) to the Cenozoic (Yamato, Lau basins etc.), and immersion differed in various areas in direction and intensity.

Considering the widespread shallow-marine and continental environments in the World Ocean history, the author of the monograph involves an extremely interesting and relatively new question on the role of bacteria in the formation of ancient weathered crusts. According to Shtaudigela et al. (2008) cited by the author, bioalterations of volcanic glass are widely manifested in different areas of the World Ocean and could happen, apparently, for many hundreds of years, acting as a kind of "bioreactor". The effect of this "reactor", as it turns out, leads to the change in remanent magnetization of oceanic basalts. It is easy to see that the researchers of the World Ocean are faced with an entirely new direction in the study of the processes occurring in the upper oceanic crust.

Analysis of deepwater drilling results performed by the author of the monograph has led him to a belief that they are completely incompatible with the fundamentals of plate tectonics. However, he does not appeal for the reader to agree with him immediately. His task, as he sees it, is to make as much as possible number of experts evaluate independently the presented data "accumulated by gigantic efforts of the international geological community". I want to believe that these author’s hopes will come true, and he can find grateful general readers.

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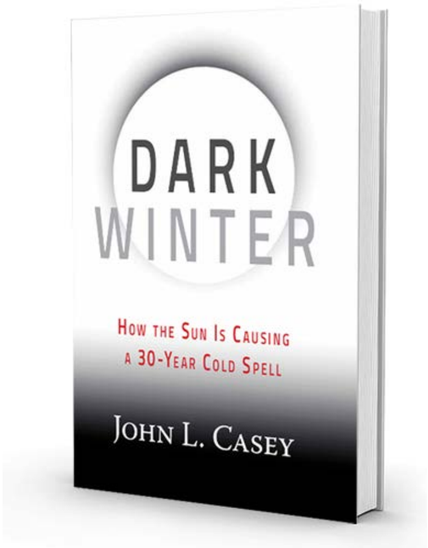

DARK WINTER

Climate change has been a perplexing problem for years.
In **Dark Winter**, author John L. Casey, a former White House national space policy adviser, NASA headquarters consultant, and space shuttle engineer, tells the truth about ominous changes taking place in the climate and the Sun.

In **Dark Winter**, Casey provides evidence of the following:

- The end of global warming
- The beginning of a "solar hibernation," a historic reduction in the energy output of the Sun
- A long-term drop in the Earth's temperatures
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- The high probability of record earthquakes and volcanic

A sobering look at the Earth's future, **Dark Winter** predicts worldwide, crop-destroying cold; food shortages and riots in the United States and abroad; significant global loss of life; and social, political, and economic upheaval.

The book, “Dark Winter” is now available from Amazon.com and other book sellers.

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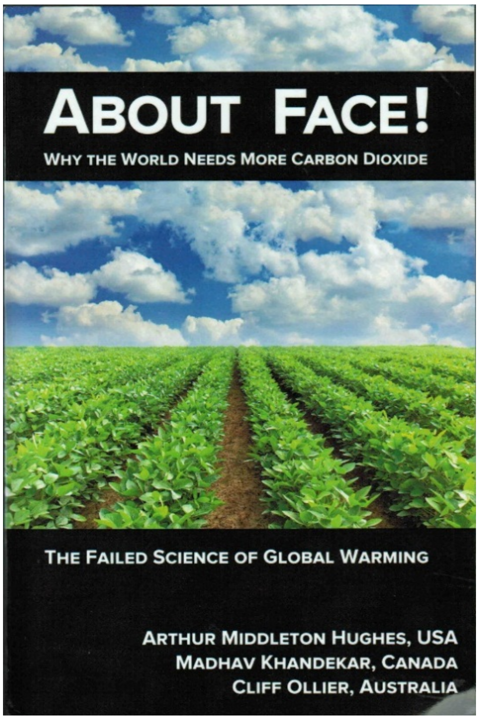

ABOUT FACE! Why the world needs more carbon dioxide. The failed science of global warming.

Authors: Arthur Middleton Hughes, Madhav Khadekar and Cliff Ollier.
Publisher: Two Harbors Press, Mineapolis, MN, USA.
Book order: www.TwoHarborsPress.com; http://www.twoharborspress.com/b-About-Face!_20333
Price: Hard copy, U$19.99.
ISBN-13: 987-1-62652-989-2.

Cited from Introduction:
…For the past fifty years, the scientific community and the governments of most of the countries of the world have been in the grip of monstrous misconception: that the world is becoming dangerously warm due to carbon dioxide. The misconception has been funded by billions of dollars in governmental grants to unveristy professors and corporations…

…CO2 is good. We need more of it, not less. DDT is good. We need to use it to eradicate malaria. Energy Independence is impossible and wrong-headed. We should stop subsidizing ethanol, wind power and solar. Coal plants are urgently needed to produce electricity and increrease CO2 in the atmosphere. We need to stop programs that are causing millions of deaths worldwide, and spending billions of dollars wastefully at home.

BOOK REVIEW

The history of micro-expanding Earth - History of the Earth from viewpoint of sea level rise

Author: Michihei HOSHINO
Published by E. G. Service Press, Sapporo, Japan. 2014. 234p. press@egs.co.jp. ISBN 978-4-9903950-5-6.
Price: JY5,000 (about U$45) plus tax and shipping charges. Available only in hard copy format.

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Prof. Michihei HOSHINO

The author, Michihei HOSHINO, Emeritus Professor of Tokai University and Ocean University of China, is now 91 years old. This book is his life work, which compiled into this book all the fruits of many years of his geotectonic study from viewpoint of sea level rise.

First, he studied the sediments of the continental shelf around Japan, and showed clearly that the sea level of the last glacial epoch was about 100 m lower than the present. Next, he hypothesized that submarine canyons originated by the submergence caused by a 2,000 m sea level rise after the late Miocene. This hypothesis was proved by the discovery of Messinian Evaporates in the western Mediterranean basins.

In early 1970s he commented that the oceanic trench is not the place of crustal depression but is an abandoned furrow left behind on the uplifting of ocean side crust. Since the summit depth of guyots which have the coral reef of the middle Cretaceous in trenches are 4,000 m, he presumed that the sea level of the mid Cretaceous was 4,000 m lower than the present. He explains that by extensive uplift of crust including ocean floor and sea level rise by the tholeiitic basalt activity of the asthenosphere origin after the Jurassic Period, the present continents and oceans have formed.

In 1991, he reported that the Baikalian and Variscan remnant basins, which have ancient peneplain (sea level) basement, and the peneplains of the Baikalian remnant basins are about 11 km below the present sea level. And he divided the history of the Earth into three stages; the Granitic Stage (Archean), the Transitional Stage (Proterozoic and Paleozoic), and the Basaltic Stage (Mesozoic and Cenozoic).

From these studies he now believes that "the fundamental problem of geology is the crustal uplifting (micro-expansion of the Earth)", and that crustal "subsidence" is a fictitious phenomenon and the true process is crustal submergence by the rising sea level due to crustal uplift. For me it is considered to be a very reasonable view.

In this book, the history of the crust and eustasy is shown from the Earth creation to the present. It is dominated by crustal uplifting and rising sea level by less than 50 km. Each time the crust was formed, the position on the Earth surface upheaved, and so did the Earth surface topography. This book tells in a dramatic way the history of micro-expansion of the Earth as follows:

Eucrite chondorite gathered at the earliest stage of Earth's birth, followed by enstatite chondorite, which formed the primeval Earth surface. The former formed the asthenosphere and the latter formed the uppermost part of the mantle (lower lithosphere).

In the Granite Stage (Archean), the lower lithosphere differentiated into atmosphere, hydrosphere and granitic crust. The tectonic pattern of this stage consisted of domes of concentrically arranged granitoids and gneisses, which were surrounded by greenstone belts. The diameter of the domes attained 100 km to 800 km.

In the Transitional Stage (Proterozoic and Paleozoic), the activities of the layered igneous body that had been produced by the mixing of the Mg-rich ultra-mafic and Ca-rich mafic magmas formed elevated plateaus. The high temperature atmosphere that contained carbon dioxide and water steams in the Proterozoic Era eroded the elevated plateau violently, and built the vast peneplains. Then, sediments filled the seas which were surrounded by the plateaus. As sea level rose, the plateau region became covered by shallow seas. The thick layers of Stromatolites deposited in the shallow seas, forming dolomite, and emitted a vast amount of oxygen into the atmosphere simultaneously.

The seas between the plateaus were filled with thick sediments supplied from elevated plateaus, having become an incipient geosyncline. The bottom of many present-day geosynclinal belts is situated at a depth of about 50 km beneath the present sea level. Hoshino considers that this depth was at the sea bottom in the late Archean. In the latest Proterozoic (Baikalian Stage), one billion years before the present, sea level had risen to about 11 km below the present level, and then the peneplains had been formed everywhere on the Earth. The greater part of these peneplains constitute the present Moho surface under the ocean floor.

In the case of wide geosynclinal basins, when the central plateau, a composite of old and young plateaus, was elevated, reverse faults in the shape of a bidirectional petal were produced. After that, as the reverse faults have remained active, geosynclinal rock formations were pushed out above the rims of the peneplanated plateaus (Moho surface under the ocean floor). The marginal sea (central plateau) - island arc (surrounding the geosynclinal-orogenic belt) - trench (compression zone) system was formed by this process. In the narrow geosynclinal basin without a central plateau, the elevation of the geosynclinal-orogenic belts has no extrusive rocks accompanied by reverse faults. The oceanic ridges consist of such geosynclinal-orogenic belts.

The Basaltic Stage, the latest stage in the history of the Earth, from the Mesozoic Era to the present, is characterized by the activity of Ca-rich basaltic plutonic magmas. This resulted in the upheaval of land and ocean basins (epirogenic movement) with rising sea level. The basaltic plutonic rocks, mainly high-temperature and high-pressure magmas originated from the asthenosphere, increased in volume and produced linear deep faults along the weak belts of the lithosphere (Archean lineaments). They rose and intruded under the plateau basement by spreading horizontally. Magmas erupted on continents are plateau basalts, and those extruded on the sea floors covered the peneplanated upper Proterozoic basement and formed the present-day oceanic crust. The deformed and elevated sedimentary layers formed island arcs and ridges. Due to the rise of the ocean basin floors caused by the addition of ocean basaltic layers since the Mesozoic, sea level has risen by 6 km.

The feature of the tectonics of the Basaltic Stage is characterized by fault blocks and tilting, and especially, it evolved in the last period of the Basaltic Stage into the period with distinctive characteristics, called the Neotectonic Period, and formed the geographical features as seen on the present surface of the Earth.

Hoshino refers to the history of the Earth in this book; "The history of the Earth is neither an uniformitarian-cyclic-concept (Charles Lyell) nor dialectic evolutionary process (ZHANG Wenyou, 1984), but it may be the developmental process from birth to death. When shall the death of the Earth arrive? Is it the day of the ending of the driving force for an expanding Earth, or the day of out-burst of stable Si-combined crust? The story of the death day of the Earth is far future, because the half-life of ^{232}Th is 14.1 billion years."

Zhang, W.J. 1984. An introduction to Fault-block tectonics. Petrol. Industr. Press, Beijing, 385p.

NEWS

Commission on Tectonics of Ore Deposits (CTOD) of the International Association on Genesis of Ore Deposits (IAGOD)
(国际矿床成因协会大地构造与成矿专业委员会)

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21 August 2014

In 1956, the activated tectonic theory (DIWA) and its metallogeny were proposed by Prof. Chen Guoda, who was a well-known geotectonic and metallogenic geologist in China. In 1988, "International Research Center for Activated Tectonics (DIWA) and Metallogeny" was founded in China. In 1990, when the "Activated Tectonics (DIWA) Group" (**CTOD WG4: Tectono-Magmatic Activation (DIWA)**) was established as a working group of the Commission on Tectonics of Ore Deposits (CTOD) of the International Association on Genesis of Ore Deposits (IAGOD), Prof. Chen Guoda became the vice chairman of the CTOD and the Chairman of DIWA Group. It was Prof. Chen Guoda who brought up the new idea of tectonometallogenic type, i.e. the mineralization in activated regions, and established the concept of multi-genetically formed ore deposits, which not only provides a new approach to understand ore genesis of complex ore deposits but also opens up our mind to the prospecting of new mineral resources. Despite Prof. Chen Guoda's passing away in 2004, his thoughts on the relationship of activated tectonics and metallogeny have had great impact on academic sector and mining industries.

On August 20, 2014, the Commission on Tectonics of Ore Deposits (CTOD) part was formally reestablished by IAGOD during the 14TH Quadrennial International Association on the Genesis of Ore Deposits (IAGOD) Symposium in Kunming, Yunnan Province, P.R. China. The Commission on Tectonics of Ore Deposits (CTOD) of the IAGOD aims to understand metallogenetic processes associated with tectonism and magmatism, to further reveal the relationship between activated regions and super-large ore deposits, and to facilitate the exchange of ideas, research methods, and reporting of progress in studies on activated tectonics and associated metallogeny, and thereby to help exploration for multi-genetically formed ore deposits in activated regions and to improve the understanding on ore mineral systems related to tectono-magmatic activity. The Commission on Tectonics of Ore Deposits (CTOD) will organize meetings, sessions at conferences, short courses/workshops and publications.

The CTOD of the IAGOD has a new committee composed of a Chairman, five vice Chairmen and two Secretary Generals. The current committee members are:

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Prof. Chen Shengzao, U.S. Geological Survey, szchen@usgs.gov
Prof. Chen Zhenglei, Institute of Geomechanics, Chinese Academy of Geological Sciences, chenzhengle@263.net

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Papers presented at the 14th IAGOD Symposium, 19-22 August 2014, Kunming, South China
Session: Tectono-magmatic activity and associated metallogeny

Conveners: Ge LIN, Dong R CHOI, Guoneng CHEN, Tagen DAI, Deru XU, Guoxang CHI and Yongjun SHAO

(*keynote paper)

***Behaviour of Elements during Granite Formation and Its Relationship to Mineral-Zonation**
CHEN Guoneng, CHEN Zhen and Grapes H. RODNEY

***Ore-Controlling Structures of the Heishan Iron Deposit, Damiao Anorthosite Complex in North China Craton and Ore-Prospecting**
CHEN Zhengle, DU Weihe, CHEN Bailin, HAN Fengbin, WANG Yong, SUN Yue and WU Yu

***Relationships between Hydrodynamics of Mineralization and Tectonic Settings**
CHI Guoxiang and LIN Ge

***An Archean Geanticline Extending from the South Pacific to Siberia**
CHOI Dong R.

Analysis of the Geodynamic Mechanism of the Large-Scale Gold Mineralization in Northeastern Hunan Province
DENG Teng, XU Deru, CHEN Genwen, WANG Zhilin, ZHANG Junling and ZHOU Yueqiang

Zircon U-Pb Geochronology and Hf Isotopic Geochemistry of the Liwan Cu–Mo Deposit, Guichi Ore Cluster Field
GU Huangling, DUAN Liuan and YANG Xiaoyong

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Granite and Mineralization of Qinghai Province in the Hoh Xil Wuxuefeng Area
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Mesozoic Monzogranites and Related Mo-Pb-Zn Polymetallic Mineralization in Southeastern Lesser Hinggan Area
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Baolun Gold Deposit, an Orogenic Gold Deposit in Hainan Province, South China

HOU Maozhou, XU Deru, SHAN Qiang, WU Chuanjun and YU Liangliang

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The New Concepts in Global Tectonics Newsletter, the predecessor of the current NCGT Journal, was initiated on the basis of discussion at the symposium “Alternative Theories to Plate Tectonics” held at the 30th International Geological Congress in Beijing in August 1996. The name is taken from an earlier symposium held at Smithsonian Institution, Washington, in association with the 28th International Geological Congress in Washington, D. C. in 1989. The NCGT Newsletter changed its name to NCGT Journal in 2013.

Aims include:
1. Forming an organizational focus for creative ideas not fitting readily within the scope of Plate Tectonics.
2. Forming the basis for the reproduction and publication of such work, especially where there has been censorship or discrimination.
3. Forum for discussion of such ideas and work which has been inhibited in existing channels. This should cover a very wide scope from such aspects as the effect of the rotation of the Earth and planetary and galactic effects, major theories of development of the Earth, lineaments, interpretation and prediction of earthquakes, major times of tectonic and biological change, and so on.
4. Organization of symposia, meetings and conferences.
5. Tabulation and support in case of censorship, discrimination or victimization.